21
REALISM/ANTI-REALISM

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The main realism/anti-realism issue in the philosophy of science is the issue of *scientific realism*, concerned with the unobservable entities of science. However, there is also a more general issue, often known as "realism about the external world," concerned primarily with the observable entities of common sense, but which spreads to scientific entities, both observable and unobservable. The issue of scientific realism is best approached from a perspective on the more general issue.

What are the realism issues?

The literature provides a bewildering variety of answers to this question, far too many to discuss here. I provide answers along what seem to me the right lines and then allude briefly to others.

I think that we should take these issues to be concerned with realism doctrines having two dimensions. The *existence dimension* of the general doctrine is a commitment to the existence of, primarily, the observable physical entities posited by common sense: stones, trees, cats, and the like. The existence dimension of scientific realism is a commitment to the existence of most of the unobservables posited by science: atoms, viruses, photons, and the like. Idealists, the traditional opponent of realists, have typically not denied this dimension; or, at least, have not straightforwardly denied it. What they have typically denied is *the independence dimension*. According to some idealists, the entities identified by the first dimension are made up of mental items, *ideas* or *sense data*, and so are not external to the mind. In recent times, under the influence of Kant, another sort of idealism has been much more common. According to these idealists, the entities are not, in a certain respect, *objective*: they depend for their existence and nature on the cognitive activities and capacities of our minds; we partly *construct* them by imposing our concepts. Furthermore, since we often differ in our worldview and hence differ in our concepts, we construct different worlds. This *constructivism* is the view of the very influential philosopher of science Thomas Kuhn (1970). Realists reject all such mind dependences.

Though the focus of the debate has mostly been on the independence dimension, the existence dimension is important. First, it identifies the entities that are the subject of the dispute over independence. In particular, it distinguishes a realism worth

fighting for from a commitment to there merely being *something* independent of us. Second, in the discussion of unobservables – the debate about *scientific* realism – the main controversy has been over existence.

We can capture the general doctrine's commitment to observables well enough as follows:

> *Common-sense realism*: Most of the observable physical entities of common sense and science exist mind-independently.

Scientific realism is our main concern and we need to be a bit more careful before defining it. So here are some clarifications. First, talk of the "commitments of science" is vague. In the context of the realism debate it means the commitments of *current* scientific theories. The realist's attitude to past theories will be the concern of the section "Arguments against scientific realism." Second, the realist's commitment is to *most* of the unobservables posited by science. It would be foolhardy to hold that current science is not making any mistakes, and no realist would hold this. Third, this cautiousness does not seem to go far enough: it comes too close to a blanket endorsement of the claims of science. Yet scientists themselves have many epistemic attitudes to their theories. These attitudes range from outright disbelief in a few theories that are useful for predictions but known to be false, through agnosticism about exciting speculations at the frontiers, to a strong commitment to thoroughly tested and well-established theories. The realist is not less skeptical than the scientist: she is committed only to the claims of the tested and established theories. Furthermore, realism has a critical aspect. Theories may posit unobservables that, given their purposes, they need not posit. Realism is committed only to *essential* unobservables. In brief, realism is a cautious and critical generalization of the commitments of well-established current theories.

Utilizing the language of these clarifications we can define a doctrine of scientific realism well enough as follows:

> *Scientific realism*: Most of the essential unobservables of well-established current scientific theories exist mind-independently.

This is a commitment only to the existence of unobservables. Realists often want a stronger doctrine than this *entity*-realism: they want a *fact*-realism committed to scientific theories mostly being right about the properties of those entities. But to keep it simple my focus is on the weaker doctrine.

According to definitions like these, the realism issues that concern us are *metaphysical* ones about the nature of the world. The literature contains a bewildering variety of other definitions, many of which seem very different. I have discussed these matters at length elsewhere (1997: Chs 2–4, 2005) and must be very brief here. Some of this variety are *epistemic* definitions about what we know about the world. Others are *apparently semantic* definitions about the truth and reference of our theories. These definitions do not differ in any *significant* way from straightforwardly metaphysical

ones. However, there are others that do differ significantly. Most important are those that really have a semantic component. "Scientific realism" is often now taken to refer to some combination of a metaphysical doctrine like scientific realism with a correspondence theory of truth (Putnam 1978; Fine 1986a; Kitcher 1993). The combination is strange. Skepticism about unobservables, which is indubitably at the center of the realism debate, is simply not about the nature of truth. The issue of that nature is surely fascinating but is orthogonal to the realism issue. No doctrine of truth is constitutive of metaphysical doctrines of scientific realism.

I turn now to the metaphysical issues. I start with common-sense realism because, manifestly, anyone who rejects that will reject scientific realism: if one has doubts about the independent existence of observables one will surely have doubts also about the independent existence of unobservables. So, scientific realism arises as a *distinct* issue only once common-sense realism has been accepted.

Common-sense realism

Realism about the ordinary observable physical world is a compelling doctrine. It is almost universally held outside intellectual circles. It is aptly named "common-sense realism" because it is the core of common sense. What, then, has persuaded so many philosophers out of it? The tradition provides a clear answer: the problem of extreme skepticism. In the *First Meditation* Descartes famously doubted the evidence of his senses. Is he right to believe that he is sitting by the fire? Perhaps he is suffering from an illusion, perhaps he is dreaming, perhaps he is being stimulated by an evil demon. In the face of such doubts, how can it be rational to believe realism?

Idealists think that it is not rational. They see an unbridgeable *gap* between the knowing mind and the independent world the realist believes in. They propose to close the gap between us and the world by abandoning the independence dimension: the world is made up of ideas or is partly constructed by the knowing mind. Only thus, it is thought, could the world be knowable.

A semantic variant of this argument can be abstracted from contemporary anti-realist discussions (Kuhn 1970; Putnam 1978, 1981). Just as traditional philosophers argued for epistemological doctrines that show that we could not *know* the realist world, we can see contemporary philosophers as arguing for semantic doctrines that show that we could not *refer to* the realist world. So the world we refer to cannot be that world but must be a world we make.

Abandoning realism and adopting idealism is, however, very costly. Idealism strikes many as bizarre. Thus, consider constructivism, according to which we partly make the familiar world by imposing our concepts. But how could we literally make dinosaurs and stars? It seems fantastic to suppose that we do.

I have argued elsewhere (2002) for two other responses we might make to the arguments against common-sense realism. First, there is a Moorean response that the arguments proceed in the wrong direction. The arguments are based on speculations about what we could know and refer to. Yet surely realism is much more plausible than these epistemological and semantic speculations that are thought to undermine it. So

we should *put metaphysics first* and argue from realism against these speculations. The second response stems from naturalism. From a naturalistic perspective, these speculations cannot be supported *a priori* and they do not come close to having the empirical support enjoyed by realism. The arguments against realism use the wrong method and proceed in the wrong direction.

One final point about the issue of common-sense realism is very important to the issue of scientific realism. Extreme skepticism demonstrates that the evidence we have for any of our beliefs about the external world is *logically compatible* with other views of the world, for example, with the view that we are manipulated by an evil demon. So the following *weak underdetermination* thesis is true:

> WU: Any theory has rivals that entail the same actual given observational evidence.

Not even a theory about observables can be simply *deduced* from any given body of evidence; indeed, not even the very existence of an observable can be deduced *from experience*. If we are to put extreme skepticism behind us we must rely on some *non*-deductive, or *ampliative*, method of inference that will support common-sense realism over the likes of the evil-demon hypothesis. This reliance might appeal to *a priori* insight or to empirical considerations, but without it there is no escape from extreme skepticism. Now, given that scientific realism arises as a distinct issue only once common-sense realism has been accepted, it follows that the issue arises only once we have adopted *some* ampliative method of inference that is sufficient to escape from extreme skepticism. The issue then arises because, armed with that method, and confident enough about the observable world, there is thought to be a *further* problem believing what science says about unobservables. So the defense of scientific realism does not require that we refight the battle with extreme skepticism, just that we respond to this special skepticism about unobservables.

We turn now to the most influential arguments for and against scientific realism. The arguments *for* are the "success argument" and related explanationist arguments (see next section). The arguments *against* are the "underdetermination argument," which starts from the claim that theories always have empirically equivalent rivals; and the "pessimistic meta-induction," which starts from a bleak view of the accuracy of past scientific theories ("Arguments against scientific realism").

Arguments for scientific realism

The most famous argument for scientific realism is the argument from the success of science (Putnam 1978: 18–19). Scientific theories tend to be successful in that their observational predictions tend to come out true: if a theory says that *S*, then the world tends to be observationally as if *S*. Why are theories thus successful? The best explanation, the realist claims, is that the theories' theoretical terms typically refer – scientific realism – and the theories are approximately true: the world is observationally as if *S* because, approximately, *S*. For example, why are all the observations

we make just the sort we would make if there were atoms? Answer: because there *are* atoms. Sometimes the realist goes further: it would be "a miracle" that theories were so successful if they were not approximately true. Realism does not just have the best explanation of success, it has *the only good* explanation.

Larry Laudan (1981) mounted a sustained attack on this argument. In the first prong of this attack, Laudan offers a list of past theories – phlogiston theory is a favorite example – that were successful but are now known not to be approximately true. The realist has a number of responses. First, the success of a theory can be challenged: although it was thought to be successful, it was not really so. But unless the criterion of success is put so high that not even contemporary theories will qualify, some theories on Laudan's list will surely survive. Second, it can be argued that a theory was not, in the appropriate sense, well-established and hence not the sort that the realist is committed to; or that entities it posited were not essential to its success. But surely some theories on the list will survive this test too. Third, the realist can insist that there are many other past theories, ones *not* on Laudan's list, for which the realist's explanation of success works fine.

Still, the realist faces a problem with the theories that survive on Laudan's list. In my view (2005), the realist should modify the explanation for such a surviving theory, explaining its success by appealing to the unobservables of replacement theories.

But perhaps anti-realists *can* explain success? There have been attempts:

- Bas van Fraassen offered a Darwinian explanation: "any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive" (1980: 39). But this explanation is not relevant because it is not explaining the same thing as the realist's success argument. It is explaining why we humans hold successful theories. It is not explaining why those particular theories are successful.
- Arthur Fine (1986b) claimed that anti-realism can explain success as well as realism can by appealing to a theory's instrumental reliability (Fine is not committed to this anti-realist explanation). Jarrett Leplin develops this proposal and labels it "surrealism." The basic idea is that although the world has a *deep structure* this structure is *not experientially accessible*. "The explanation of the success of any theory … is that the actual structure of the world operates at the experiential level as if the theory represented it correctly" (Leplin 1997: 26). Leplin goes on to argue that the surrealist explanation is not a successful alternative to the realist one.

In the second prong of his attack on realism, Laudan has criticized the realist's success argument for its dependence on inference to the best explanation, or *abduction*. Fine (1986a: 113–22) has made a similar criticism. Abduction is a method of inference that an anti-realist might reject. Van Fraassen (1980), for one, does reject it. Is the realist entitled to rely on abduction? Richard Boyd (1984: 65–75) has argued that the anti-realists are not in a position to deny entitlement because scientists regularly use abduction to draw conclusions about *observables*.

Boyd's argument illustrates an important, and quite general, realist strategy to defend unobservables against discrimination, to defend "unobservable rights." The

realist starts by reminding the anti-realist that the debate about *scientific* realism is not over extreme skepticism: the anti-realist claims to have knowledge of observables (see "Common-sense realism"). The realist then examines the anti-realist's justification for this knowledge. Using this justification she attempts to show, positively, that the epistemology it involves also justifies knowledge of unobservables. And, she attempts to show, negatively, that the case for skepticism about unobservables produced by the anti-realist is no better than the case for skepticism about observables, a skepticism that all parties to the scientific realism dispute have rejected.

So the anti-realist's criticism of the success argument leaves him with the task of showing that he can save his beliefs about observables without using abduction. If he cannot manage this, the criticism fails. If he can, then the realist seems to face the task of justifying abduction.

How concerned should the realist be about this? Perhaps not much. After all, the *anti*-realist must rely on some methods of ampliative inference, even if not on abduction, to overcome extreme skepticism. How are those methods justified? The anti-realist may well have little to say about this, relying on the fact that these methods are widely and successfully used in science and ordinary life and on there being no apparent reason to abandon them. But, of course, that seems to be true of abduction as well. If further justification for a method is required, where could we find it? Any such justification would have to be either *a priori* or empirical. Either way, it is not obvious that the justification of abduction will be more problematic than the justification of the methods of inference relied on by the anti-realists.

The literature contains two other explanationist arguments for scientific realism:

1 Why is our scientific methodology *instrumentally reliable* in that it leads to successful theories, theories that make true observational predictions? Boyd (1984) offers the realist explanation that the methodology is based in a dialectical way on our theories and those theories are approximately true. He argues that anti-realists cannot explain this methodological success.
2 I have offered elsewhere (1997: 113–17) a very basic argument: by supposing that the unobservables of science exist, we can give good explanations of the behavior and characteristics of observed entities, behavior and characteristics which would otherwise remain inexplicable.

In sum, there are some good arguments for scientific realism provided the realist is allowed abduction. It is not obvious that anti-realists are in a position to disallow this.

Arguments against scientific realism

The underdetermination argument

This empiricist argument starts from a doctrine of *empirical equivalence*. Let *T* be any theory committed to unobservables. Then,

EE: *T* has empirically equivalent rivals.

This is taken to imply the *strong underdetermination* thesis:

SU: *T* has rivals that are equally supported by all possible observational evidence for it.

So, doctrines like scientific realism are unjustified.

What is it for two theories to be *empirically equivalent*? The basic idea is that they have the same observational consequences. We shall soon see the importance of looking very closely at this basic idea.

SU should not be confused with other underdetermination theses, particularly the earlier (section 2) obviously true WU that leads to the challenge of extreme skepticism. SU is stronger than WU in two respects. First, SU concerns an *ampliative* relation between theories and evidence and not merely a deductive one. Second, SU is concerned with *T*'s relation to *all possible* evidence not merely to the given evidence. If we are to avoid skepticism in the face of WU, we noted, some ampliative method of inference must be accepted. But if SU is true, we face a *further* challenge: ampliative methods do not support *T* over its rivals either on the given evidence or even on all possible evidence. So what *T* says about the unobservable world can make no evidential difference. Surely, then, commitment to what the theory says is a piece of misguided metaphysics. Even with extreme skepticism behind us, realism is threatened.

A good reason for believing EE is that there is an empiricist algorithm for constructing an equivalent rival to *T*. Consider T_o, the theory that the observational consequences of *T* are true. T_o is obviously empirically equivalent to *T*. Now form *T** by combining T_o with the negation of *T*. *T** is an empirically equivalent rival to *T*. So EE is established.

It is tempting to respond that *T** is produced by trickery and is not a *genuine* rival to *T*. But this response seems question-begging. We need a principled basis for dismissing rivals as *not genuine*. Following the earlier-described realist strategy, I have argued for such a basis (1997: 150–3, 2002, 2005): in counting the likes of *T** as rivals, EE as it stands is too weak to sustain SU. For, with extreme skepticism behind us, we *are* justified in choosing *T* over empirically equivalent rivals like *T**. If the underdetermination argument is to work, it needs to start from a stronger equivalence thesis, one that does not count any theory as a genuine rival to *T* that can be dismissed by whatever ampliative inferences enable us to avoid extreme skepticism. Precisely how far we can go in thus dismissing rivals remains to be seen, of course, pending an account of how to avoid extreme skepticism. And, given the realist strategy, the account that matters is the one given by the anti-realist.

With EE now restricted to such *genuine* rivals, the next step in assessing the underdetermination argument is a careful consideration of how to interpret EE's talk of empirical equivalence. Given the basic idea of empirical equivalence, a natural interpretation is:

> EE1: *T* has genuine rivals that entail the same possible observational evidence.

Whether or not EE*1* is true, it is easy to see that it is inadequate to support SU. This inadequacy arises from the fact that *T* is likely to entail few observations on its own and yet the conjunction of *T* with auxiliary hypotheses, theories of instruments, background assumptions, and so on – briefly, its conjunction with *auxiliaries* – is likely to entail many observations. *T* does not face the tribunal of experience alone (Duhem–Quine). As Laudan and Leplin (1991) point out in their influential critique of the underdetermination argument, by failing to take account of these joint consequences, EE*1* leaves many ways in which evidence could favor *T* over its rivals, contrary to SU. To sustain SU and challenge realism, we need another interpretation of EE.

Consider this interpretation:

> EE2: *T* has genuine rivals which are such that when *T* and any of the rivals are conjoined with A_t, the auxiliaries that are accepted at a time *t*, they entail the same possible observational evidence.

Whether or not EE2 is any threat at all to realism, it is clearly too weak to sustain the threat posed by SU. Let *T′* be an empirically equivalent rival to *T* according to this interpretation. So $T\&A_t$ and $T'\&A_t$ entail the same observations. This sort of equivalence is *relative to* A_t. It amounts to the claim that *T* and *T′* cannot be discriminated observationally if conjoined only with those auxiliaries. But this does not show that *T* and *T′* could not be distinguished when conjoined with *any* acceptable auxiliaries at *any* time. And that is what is needed, at least, to sustain the claim that *T* and *T′* cannot be discriminated by *any possible* evidence, as SU requires. SU demands a much stronger answer to the interpretative question:

> EE3: *T* has genuine rivals which are such that when *T* and any of the rivals are conjoined with any possible acceptable auxiliaries they entail the same possible observational evidence.

If *T* and *T′* were thus related they would be empirically equivalent not just relative to certain auxiliaries but *tout court*, *absolutely* equivalent. Only then would they be observationally indiscriminable. So if EE is to support SU, it must be interpreted as EE3.

The main point of Laudan and Leplin's critique can be put simply: we have no reason to believe EE3. If *T* and *T′* cannot be discriminated observationally relative to, say, currently accepted auxiliaries, they may well be so relative to some future accepted auxiliaries. Some currently accepted auxiliaries may cease to be accepted and some new auxiliaries are likely to become accepted. This point becomes particularly persuasive, in my view (1997: 119), when we note our capacity to invent new instruments and experiments to test theories. With a new instrument and experiment come new auxiliaries, including a theory of the instrument and assumptions about the

experimental situation. Given that we can thus *create* evidence, the set of observational consequences of any theory seems totally open. Of course, there is *no guarantee* of successful discrimination by these means: a theory may really face a genuine empirically equivalent rival. Still, we are unlikely to have sufficient reason for believing this of any particular theory. More importantly, we have *no reason at all* for believing it of *all* theories, as EE3 requires. We will seldom, if ever, have a basis for concluding that two genuine rivals are empirically equivalent in the absolute sense required by EE3. *There is no known limit to our capacity to generate acceptable auxiliaries*.

What about EE2? We have already seen that EE2 will not sustain SU but maybe it could otherwise threaten realism. But is it true? There are surely some theories that face a genuine rival that is empirically equivalent relative to the accepted auxiliaries at a certain time. But do *all* theories face such rivals at that time, let alone at *all* times? EE*2 guarantees* that all theories do at all times. But the ampliative methods, whatever they may be, that support our knowledge of the observable world and avoid extreme skepticism will count many rivals as not genuine, so many as to make this guarantee seem baseless. There is no basis *a priori* for supposing that *T* must always face such a genuine rival.

In sum, we have no reason to believe EE2 or EE3, and so the underdetermination argument fails.

The pessimistic meta-induction

A powerful argument against scientific realism, called a "meta-induction" by Putnam (1978), runs as follows: the unobservables posited by past theories do not exist; so, probably the unobservables posited by current theories do not exist. The argument rests on a claim about past theories from the perspective of our current theories. And the pessimistic suggestion is that, from a future perspective, we will have a similarly critical view of our current theories. Laudan (1981) has supported these claims about the past with a list of theoretical failures.

Scientific realism already concedes something to the meta-induction in exhibiting *some* skepticism about the claims of science. It holds that science is more or less right, but not totally so. It is committed only to well-established theories not exciting speculations. It leaves room for a theoretical posit to be dismissed as inessential to the theory. According to the meta-induction, reflection on the track record of science shows that this skepticism has not gone nearly far enough.

The realist can respond to the meta-induction by attacking the premiss or the inference. Concerning the premiss, the realist can, on the one hand, resist the bleak assessment of the theories on Laudan's list, claiming that while some of the unobservables posited by these theories do not exist, others do; or claiming that while there is a deal of falsehood in these theories, there is a deal of truth too. On the other hand, the realist can claim that the list is unrepresentative, that other past theories do seem to be approximately true and to posit entities that do exist.

Clearly, settling the status of the premiss requires close attention to the historical details. What would such an attempt be likely to reveal? I think that it would reveal a good deal of indeterminacy about what does or does not exist, but also much deter-

minacy. Among the determinate cases there will surely be some of non-existence: phlogiston is a good candidate. But there will surely also be some of existence: the atoms posited in the nineteenth century are good candidates. So, we should conclude that the premiss of the meta-induction is overstated, at least. But how much is it overstated? That depends on the *success ratio* of past theories, the ratio of the determinately existents to the determinately non-existents + indeterminates. Where is this ratio likely to leave scientific realism? To answer this we need to consider the meta-induction's inference.

The first point to note is that even if history were to show that *most* of the unobservables posited by past theories do not exist that would *not* be sufficient to show that, probably, most of the unobservables posited by current theories do not exist. The problem is what Marc Lange (2002) calls "the turnover fallacy." Because false theories turn over much more often than true ones, the premiss might be true even though, at any time, most of the unobservables posited at that time exist. So, if the inference is to be good, and so threaten scientific realism, it must start from the premiss that most of the unobservables posited by theories *at all – or most – past times* do not exist.

I think (1997: 162–5) that we have good reason for doubting the inference even from this stronger premiss. If the premiss were right it would show that our past theories have failed rather badly to get the unobservable world right. Why would that show that our present theories are failing similarly? It clearly would show this if we supposed that we are *no better* at finding out about unobservables now than we were in the past. But why suppose that? Just the opposite seems more plausible: we are now *much better* at finding out about unobservables. Science has for two or three centuries been getting better and better at this. Indeed, scientific progress is, to a large degree, a matter of improving scientific methodologies often based on new technologies that provide new instruments for investigating the world. If this is so – and it seems fairly indubitable – then we should expect an examination of the historical details to show improvement over time in our success ratio for unobservables. If the details do show this, it will not matter to realism that the ratio for, say, two centuries ago was poor. What will matter is that we have been improving enough to now have the sort of confidence reflected by scientific realism. And if we have been improving, but not fast enough for scientific realism, the realist can fall back to a more moderate commitment to, say, a high proportion of the unobservables of currently well-established theories.

Improvements in scientific methodologies make it much harder to mount a case against realism than seems to have been appreciated. For, the appeal to historical details has to show not only that we were nearly always wrong in our unobservable posits but that, despite methodological improvements, we have not been getting increasingly right. It seems to me most unlikely that this case can be made.

Conclusions

The realism doctrines that concern philosophy of science are best seen as straightforwardly metaphysical. Extreme skepticism poses the background issue: it threatens realism about observables. Sustaining this *common-sense realism* requires adopting

some ampliative method of inference. Only then does a realism about unobservables, *scientific realism*, arise as a distinct issue. Various explanationist arguments for scientific realism succeed provided that the realist is entitled to abduction. The underdetermination argument against realism fails because we have no good reason to believe an empirical equivalence thesis that would serve as its premiss. The pessimistic meta-induction, with its attention to past theoretical failures, does pose a problem for realism. But the problem may be manageable. For, the anti-realist must argue that the historical record shows not only that past failures are extensive but also that we have *not improved* our capacity to describe the unobservable world sufficiently to justify confidence that the accounts given by our current well-established theories are to a large extent right. That is difficult to argue.

See also Empiricism; Inference to the best explanation; Models; Naturalism; Theory-change in science; Underdetermination; The virtues of a good theory.

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Further reading

Leplin (1984) is an excellent collection containing many of the arguments discussed. Paul M. Churchland and Clifford A. Hooker (eds) *Images of Science: Essays on Realism and Empiricism, with a Reply from Bas C. van Fraassen* (Chicago: University of Chicago Press, 1985) is another helpful collection. Paul Feyerabend's *Against Method* (London: New Left Books, 1975) is an influential source of constructivism (along with

Kuhn 1970). J. J. C. Smart's *Philosophy and Scientific Realism* (London: Routledge & Kegan Paul, 1963) is a classic defense of realism. Ian Hacking's *Representing and Intervening* (Cambridge: Cambridge University Press, 1983) is a lively and persuasive defense of entity-realism. Laudan's *Beyond Positivism and Relativism: Theory, Method, and Evidence* (Boulder, CO: Westview Press) reprints Laudan and Leplin (1991) and contains much else of interest. Stathis Psillos's *Scientific Realism: How Science Tracks Truth* (New York: Routledge, 1999) is a thorough discussion, and sustained defense, of scientific realism. André Kukla's *Studies in Scientific Realism* (New York: Oxford University Press, 1998) is a careful critical analysis of the arguments both for and against scientific realism. J. Worrall's "Structural Realism: The Best of Both Worlds," *Dialectica* 43 (1989): 99–124, is an interesting defense of a weaker form of realism.