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PART VII

PHILOSOPHY OF THE SCIENCES

CHAPTER 26

SCIENTIFIC REALISM

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1. Introduction

What is scientific realism? The literature provides a bewildering variety of answers. I shall start by addressing this question (Section 2). I shall go on to discuss the most influential arguments for and against scientific realism. The arguments for are the 'success argument' and related explanationist arguments (Section 3). 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 (Section 4). My approach is naturalistic.

2. What Is Scientific Realism?

Science appears to be committed to the existence of a variety of unobservable entities—to atoms, viruses, photons, and the like—and to these entities having certain properties. The central idea of scientific realism is that science really is committed and is, for the most part, right in its commitments. As Hilary Putnam once put it, realism takes science at 'face value' (1978: 37). So, for the most part, those scientific entities exist and have those properties. We might call this the 'existence dimension' of realism. It is opposed by those who are sceptical that science is giving us an accurate picture of reality.

Scientific realism is about *un*observable entities. Science appears also to be committed to lots of observable entities—to a variety of plants, molluscs, moons, and the like. Folk theory appears to be committed to observables like stones, trees, and cats. A scepticism that extends to observables is extreme, 'Cartesian,' scepticism. It yields the issue of 'realism about the external world'. This issue is both different from and prior to the issue of scientific realism. It addresses doubts about the very clearest cases of knowledge about observables, doubts occasioned by sceptical hypotheses such as that we are manipulated by an evil demon. The issue of scientific realism arises only once such doubts about the observable world have, somehow or other, been allayed. Given the obvious truth of the following weak underdetermination thesis,

(WU) Any theory has rivals that entail the same actual given observational evidence,

allaying those doubts will involve accepting some method of non-deductive ampliative inference. 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 scepticism behind us and gain any knowledge about the world, we need some ampliative method of inference. 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 defence of scientific realism does not require that we refight the battle with extreme scepticism, just that we respond to this special scepticism about unobservables. We shall see that this point has not been kept firmly enough in mind.

The general doctrine of realism about the external world is committed not only to the existence of this world but also to its 'mind-independence': it is not made up of 'ideas' or 'sense data' and does not depend for its existence and nature on the cognitive activities and capacities of our minds. Scientific realism is committed to the unobservable world enjoying this independence. We might call this the 'independence dimension' of realism. The very influential philosophers of science Thomas Kuhn and Paul Feyerabend think that scientific entities are not independent but are somehow 'constructed' by the theories we have of them. This 'constructivism' has its roots in the philosophy of Kant and is extremely influential. An important feature of constructivism, for the purposes of this chapter, is that it applies in the first instance to observables: there is no special problem about the independence of unobservables (as there is thought to be about their existence).[1]

[1] But what about quantum theory? The notorious Copenhagen interpretation responds to the mysterious picture of the world suggested by quantum theory by taking the quantum world to be observer-dependent. This would offend against the independence dimension of realism. But, of

The struggle between constructivism and realism is appropriately conducted at the level of observables. I shall therefore not engage in it here.[2]

Before attempting a 'definition' of scientific realism, some further clarification is called for. 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 Section 4.2. Secondly, the realist holds that science is right, 'for the most part'. It would be foolhardy to hold that current science is not making any mistakes and no realist would hold this. Thirdly, this caution 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 sceptical than the scientist: she is committed only to the claims of the latter 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.

More clarification would be appropriate but this will have to do. Utilizing the language of the clarification we can define a doctrine of scientific realism as follows:

(SR) Most of the essential unobservables of well-established current scientific theories exist mind-independently.

With a commitment to the existence of a certain unobservable goes an implicit commitment to its having whatever properties are essential to its nature as that unobservable. But, beyond that, SR is non-committal on the properties of the unobservables, on the scientific 'facts'. Yet the scientific realist is often committed not only to the entity realism of SR but to a stronger 'fact' realism:[3]

(SSR) Most of the essential unobservables of well-established current scientific theories exist mind-independently and mostly have the properties attributed to them by science.

course, this interpretation of the theory is not obligatory. Many interpretations have been proposed that do not involve observer-dependence. I'm told that all of these are somewhat weird in one way or another. Some philosophers respond to the mysteries by taking quantum theory instrumentally and hence not as an accurate guide to reality; so, the existence dimension of realism is not embraced for the theory. But the enormous success of the theory makes this a difficult choice (see Sect. 3 below on the significance of success). In brief, controversy rages (see e.g. the papers in Cushing and McMullin 1989; Cushing *et al.* 1996). This situation is both fascinating and worrying. What conclusions should we draw from it about scientific realism? In my view, we should draw none until the dust begins to settle.

[2] Elsewhere (Devitt 1991, 1999, 2001) I take a very dim view of constructivism.

[3] The scare quotes around "facts" are to indicate that the use of the term can be regarded as a mere manner of speaking, not reflecting any commitment to the existence of what many regard as very

The existence dimensions of these doctrines are opposed by those who are sceptical of what science is revealing; the independence dimension is opposed by the constructivists.

Although not generally sceptical of scientific theories, SR and SSR do reflect some scepticism. By varying the amount of scepticism, we could define some other doctrines; for example, instead of claiming that most of the unobservables exist we could claim that *a large proportion* do or, even weaker, that *some* do. Clearly there is room for argument about how strong a position should be defended against the sceptic. Related to this, but less interesting, there is room for argument about which doctrines warrant the label 'scientific realism'. But this does not prepare one for the bewildering variety of definitions of scientific realism in the literature, many of them very different from SR and SSR.

SR and SSR are about what the world is like, they are *metaphysical* (or *ontological*). Some philosophers favour *epistemic* definitions of scientific realism (for example, Kukla 1998: 10; see also Psillos 1999, pp. xix–xxi). Thus, instead of claiming that most of the unobservables of science exist, one could claim that a belief that they do is justified; or, instead of claiming SSR, one could claim that SSR is justified. This illustrates that epistemic definitions are generally parasitic on metaphysical ones. And although the epistemic ones are clearly different from metaphysical ones, they are not different in a way that is significant for the realism debate. For, if one believes that, say, SSR is justified, one should believe SSR. On the other hand, if one believes SSR, one should be able to produce a justification for it. And someone who urges SSR in the realism debate would produce (what she hopes is) a justification because she would argue for SSR. A metaphysical doctrine of scientific realism and the epistemic one that is parasitic on it stand or fall epistemically together.[4]

It is common to propose what may seem to be *semantic* definitions of scientific realism, definitions using the terms "refer" and "true" (e.g. Hesse 1967: 407; Hooker 1974: 409; Papineau 1979: 126; Ellis 1979: 28; Boyd 1984: 41–2; Leplin 1984*b*; Fales 1988: 253–4; Jennings 1989: 240; Matheson 1989). For example, we might propose: 'Most of the theoretical terms of currently well-established scientific theories refer to mind-independent entities and the theories' statements about those entities are approximately true.' This should be seen as simply a paraphrase of the metaphysical SSR, exploiting only the 'disquotational' properties of "refer" and "true"

dubious entities. Ian Hacking (1983) calls this sort of doctrine 'theory realism'. I prefer to talk of 'facts' rather than theories to emphasize that the doctrine is about the world itself not our account of it.

[4] Indeed, one can generalize: $(p)((p$ is justified) is justified iff p is justified). (Talk of justification here should be construed broadly so that 'externalist' accounts of knowledge are not ruled out.) Jarrett Leplin (1997: 26) has defined an epistemic doctrine, 'minimal epistemic realism', that is not parasitic on a metaphysical one. This doctrine does not claim that a belief in any of the claims of science *is* justified, just that such a belief *could be* justified. A realism that concedes this much to the sceptic is indeed minimal (although still too strong for an anti-realist like Bas van Fraassen).

captured in the schemas '"F" refers iff Fs exist' and '"S" is true iff S'. Such paraphrases are often convenient but they do not change the subject matter away from atoms, viruses, photons, and the like. They are not *in any interesting sense* semantic. In particular, they do not involve commitment to a *causal theory* of reference or a *correspondence theory* of truth, nor to any other theory of reference or truth. Indeed, they are compatible with a totally deflationary view of reference and truth: a deflationist can be a scientific realist (Horwich 1998).

So there are epistemic and apparently semantic definitions of scientific realism which do not differ in any significant way from straightforwardly metaphysical definitions like SR and SSR. However, there are others that do differ significantly. Most important are the ones that really have a semantic component. "Scientific realism" is often now taken to refer to some combination of a metaphysical doctrine like SSR with a correspondence theory of truth (see e.g. Putnam 1978: 18–20, 123–5; 1987: 15–16; Fine 1986*a*: 115–16, 136–7; Miller 1987; Kitcher 1993: 127–33; Brown 1994).[5] The combination is strange. Scepticism about unobservables, which is indubitably at the centre 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.[6] Of course there may be evidential connections between the two issues: there may be evidential connections between *any* issues (Duhem–Quine). But no doctrine of truth is constitutive of metaphysical doctrines of scientific realism.[7] In what follows I shall be concerned simply with the latter, using SR and SSR as my examples.

[5] Arthur Fine dismisses the realism issue altogether because he takes it to involve an issue about truth. Despite this dismissal, Fine urges the mysterious 'Natural Ontological Attitude' (1986*a*, *b*), which often seems to be a realist doctrine like SSR! However, some passages (1986*b*: 163–5) make it hard to take Fine as a realist.

[6] This objection also counts against definitions of realism that include the idea that truth is the *aim* of science (e.g. van Fraassen 1980: 8) *wherever this talk of truth is taken to commit realism to correspondence truth*. Even if the talk does not have that commitment, and so is acceptable to a deflationist, such definitions have problems. On the one hand, if the idea that truth is the aim of science is added to a doctrine like SSR, the addition is uninteresting: if science *is* discovering the truth, nobody is going to propose that it is not aiming to, that truth is a happy accident. On the other hand, if the idea is not added to a doctrine like SSR, the definition will be too weak: realism will require that science aims for truth without any commitment to it ever having achieved that aim. Indeed, if science had never achieved that aim despite the efforts of the last few centuries, it would hardly be rational now to have the aim. In the distant past, of course, the situation was different (as Howard Sankey has emphasized to me). *Then* the realistically inclined philosopher should have had the aim without the commitment. But *now* she should have the commitment with the result that the aim goes without saying.

[7] For more on this and other matters to do with defining realism, see Devitt (1991, particularly chs. 2–4; 1997, pt. i; 1999, pt. i). Here are two excuses for the intrusion of semantics into the definition of scientific realism. (1) We seem to need semantics to capture the 'non-factualist' anti-realism of classical positivistic instrumentalism (for a learned account of the history of this instrumentalism, see Psillos 1999, ch. 3). This instrumentalism is like the moral anti-realism of 'non-cognitivism' in claiming that what appear to be descriptive and factual statements are not really so. So these 'statements' are not really committed to what they appear to be committed to. I argue that these anti-realisms are, nonetheless, at bottom metaphysical not semantic (Devitt 1997, pt ii). Aside from that, positivistic instrumentalism is no longer a player in the dispute over scientific realism (although instrumentalism

We move on to consider the explanationist arguments for scientific realism, and the underdetermination argument and the pessimistic meta-induction against realism.

3. Arguments for Scientific Realism

3.1 The Success Argument

The most famous argument for realism is the argument from the success of science. The argument has its origins in the work of Grover Maxwell (1962) and J. J. C. Smart (1963) but its most influential expression is by Putnam (1978: 18–19) drawing on Richard Boyd. 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—SR—and the theories are approximately true—SSR: the world is observationally as if S because, approximately, S.[8] 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 weren't approximately true. Realism does not just have the best explanation of success, it has *the only good* explanation.

Larry Laudan (1981, 1984, 1996) has 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 favourite 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 (McAllister 1993). 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

in general certainly is, for it simply involves doubting the theoretical claims of science without reinterpreting them). (2) Although a doctrine like SSR need not be combined with a correspondence theory of truth, it very likely cannot be plausibly combined with an epistemic theory of truth. But still a non-epistemic theory is not constitutive of SSR (Devitt 1991: 4.3).

[8] Note that although this argument is usually stated using "refer" and "true", this is not essential. And such usage should be seen as exploiting only the disquotational properties of the terms with no commitment to a robust correspondence relation between language and the world. The realist argument should be that success is explained by the properties of unobservables, not by the properties of truth and reference. ('Truth, like Mae West's goodness, has nothing to do with it'; Levin 1984: 124.) So the argument could be urged by a deflationist (Devitt 1991: 113–17).

survive. Secondly, 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 (Kitcher 1993: 140–9). But surely some theories on the list will survive this test too. Thirdly, 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 (McMullin 1984).

Still, what about the theories that survive on Laudan's list? The realist must offer some other explanation of their success. So even if the approximate truth of most theories is the best, perhaps only, explanation of their success, it cannot be so for all theories. But then the realist should not have needed to struggle with Laudan's list to discover this need to modify the success argument. After all, the sensible realist does not suppose that no well-established scientific theory has ever been very wrong in its entities and its claims about them. And some theories that have been very wrong have surely been successful; indeed, scientists sometimes continue to use theories known to be false simply *because* they are so successful. So the realist must offer some explanation of this success that does not depend on the rightness of the theories.

Here is a suggestion that is very much in the spirit of the original success argument. The success of a theory T that is very wrong is explained by the approximate truth of a replacement theory T′.[9] It is because the unobservables posited by T′ exist and have approximately the properties attributed to them that T is successful. Indeed, we expect the very same theory that shows T to be wrong also to explain T's observational success.[10] So the realist modifies the success argument: the best explanation of a theory's success is mostly that its unobservables exist and have approximately the properties specified by the theory; otherwise, the best explanation is that the unobservables of a replacement theory exist and have approximately the properties specified by that theory. Furthermore, the realist may insist, the only way to explain the success of a theory is by appeal to its unobservables or those of another theory.

The three earlier realist responses greatly reduce the challenge that Laudan's list poses for the original success argument. Still, some theories on the list will survive these responses. The modified argument offers an explanation of the success of those theories and is sufficient to support SR and SSR.[11]

[9] Laudan is scornful of the realists' appeal to the explanatory role of approximate truth. He complains that the notion is undefined. He acknowledges that many scientifically useful notions are undefined, but thinks that approximate truth is so specially unclear that the realists' appeal to it is 'so much mumbo-jumbo' (1981: 32). This is excessive. First, approximately true theories can have fully true parts that do the explaining. Secondly, the talk of approximate truth is simply a convenience (n. 11). The claim that the approximate truth of "*a* is F" explains an observation amounts to the claim that *a*'s being approximately F explains it. Science and life are replete with such explanations; for example, *a*'s being approximately spherical explains why it rolls.

[10] This expectation should not be construed as a *requirement* on a replacement theory: that no theory should replace T until it has explained T's success. Laudan rightly objects to this requirement (1981: 44–5).

[11] In so far as we have reason to believe that the success of a current theory is to be explained by some as yet unknown future theory, that success provides no reason to believe in the current theory's entities or approximate truth. So the modified success argument's support for SR and SSR does

Now, of course, this modification that accepts past mistakes raises the spectre of the pessimistic meta-induction.[12] We shall consider that later (Section 4.2). Meanwhile, the modification does seem to save the success argument.

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

1. Bas van Fraassen has 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 (Lipton 1991: 170–2; Devitt 1991: 116; Kitcher 1993: 155–7; Leplin 1997: 7–9).
2. Arthur Fine claims that anti-realism can explain success as well as realism can by appealing to a theory's instrumental reliability (1986*b*; Fine is not committed to this anti-realist explanation). Jarrett Leplin (1987, 1997) 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' (1997: 26). Leplin goes on to argue, in my view convincingly, that the surrealist explanation is not a successful alternative to the realist one.[13]

In the second prong of his attack on realism, Laudan (1981: 45–6) has criticized the realist's success argument for its dependence on *inference to the best explanation*, or 'abduction'. Fine (1986*a*: 113–22) has made a similar criticism.[14] In presenting this criticism they charge the realist with 'question-begging'. This charge is not apt. The realist argument could be question-begging only if it assumed abduction and the dispute with the anti-realist was over abduction. But the primary dispute, at least, is not over abduction but over a doctrine like SSR. So, presented in this way, the criticism seems to miss its target. (One wonders if the cause of this mistake is that the many definitions of scientific realism have left the target unclear.)

depend on the three earlier responses showing that it is mostly the case that the success of a past theory can be explained by the reference of *its* terms and *its* approximate truth. (I am indebted to Mathias Frisch for drawing my attention to this.)

[12] Peter Lewis (2002) summarizes Laudan's discussion as a meta-induction (which differs from the meta-induction we shall be considering):

> Many false past theories were successful.
> So the success of a theory is not a reliable test for its truth.

Lewis argues persuasively that Laudan's meta-induction exemplifies 'the false positives paradox'. To establish his conclusion, Laudan needs to find periods of science where most theories were successful and yet most were false.

[13] For more on this, see Kukla (1998: 20–4); Psillos (1999: 90–3).

[14] Realists tend to be fond of abduction; e.g. Glymour (1984); Boyd (1984); McMullin (1984); Musgrave (1985); Leplin (1997).

The criticism should be simply that the realist argument relies on abduction and this is a method of inference that an anti-realist might reject. Van Fraassen (1980, 1989), for one, does reject it. Is the realist entitled to rely on abduction? 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'.[15] The realist starts by reminding the anti-realist that the debate is not over extreme scepticism: the anti-realist claims to have knowledge of observables (Section 2). 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 scepticism about unobservables produced by the anti-realist is no better than the case for scepticism about observables, a scepticism 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—and van Fraassen (1989) has made an attempt—then the realist seems to face the task of justifying abduction.

How concerned should the realist be about this? Perhaps not as much as many suppose. After all, the anti-realist must rely on some methods of ampliative inference, even if not on abduction, to overcome extreme scepticism. 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?[16] The naturalistically inclined will have trouble with any attempt at an a priori justification. And it is hard to see how an a priori approach could be effective either for or against abduction. What about an empirical justification within a naturalized epistemology? Any justification must of course use some methods of inference. If it uses the very method it seeks to justify, circularity threatens. Stathis Psillos argues that this circularity is not vicious (1999: 81–90). Be that as it may, Neurath's famous image of rebuilding a boat while staying afloat on it suggests another procedure for justifying a method: we hold fast to all other methods and use them to justify the method in contention. So, perhaps we can justify abduction using the methods of induction and deduction. Indeed, perhaps the very success of abduction in science and everyday life—its tendency to

[15] For examples of this strategy, see Churchland (1985); Gutting (1985); Musgrave (1985); Clendinnen (1989); Devitt (1991: 147–53); Psillos (1999: 186–91). Van Fraassen (1985) responds to the first three of these.

[16] One problem in finding it is that we cannot give a precise specification of any of these methods: as Georges Rey says, 'no one yet has an adequate theory of our knowledge of much of anything' (1998: 29).

produce conclusions that are later observationally confirmed—provides the basis for such an inductive justification.[17] In any case 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.

3.2 The Success of Methodology Argument

Our scientific methodology is 'instrumentally reliable' in that it leads to successful theories, theories that make true observational predictions. Everyone agrees that our methodology does this. Why does it? What is the explanation? Boyd (1973, 1984, 1985) has posed this question and offered an answer that is both realist and naturalist: the methodology is based in a dialectical way on our theories and those theories are approximately true. He argues that anti-realists of various sorts cannot explain this methodological success satisfactorily and so his realist explanation is the best. I think that he is probably right.

Like the earlier success argument this argument relies on abduction, but it has a different explanandum. Where the earlier argument sought to explain the success of theories, this one seeks to explain the success of scientific methodology in producing successful theories.

3.3 The Basic Abductive Argument

The two abductive arguments for realism that we have considered are somewhat sophisticated. A more basic argument is strangely overlooked: by supposing that

[17] The suggestion is that experience, according to the empiricist 'the sole legitimate source of information about the world' (van Fraassen 1985: 286), supports abduction. For arguments in favour of abduction, see Boyd (1984); McMullin (1984); Lipton (1991); Devitt (1991: 111–13); Leplin (1997: 116–20). Analogous problems arise, of course, over the justification of deduction; see Field (1996, 1998); Rey (1998); Devitt (1998).

Van Fraassen (2000: 261–71) seems to misunderstand the relation of naturalized epistemology to science. It goes without saying that epistemology implies the methods of science. But van Fraassen seems to take the naturalist view to be that basic science, or special sciences like biology, medicine, and psychology, imply the methods of science, a view that he rejects. This view misrepresents naturalism. Naturalism holds that epistemology is itself a special science. As such it is no more simply implied by another science than is any other special science: it has the same sort of relative autonomy, and yet dependence on basic science, as other special sciences. Naturalized epistemology, like any special science, applies the usual methods of science, whatever they may be, mostly taking established science for granted, to investigate its special realm. In the case of epistemology that realm is those very methods of science. The aim is to discover empirically how we humans learn, and should learn, about the world (Devitt 1991: 75–9). We have no reason to suppose that the methods that have yielded knowledge elsewhere cannot yield knowledge in epistemology.

the unobservables of science exist, we can give good explanations of the behaviour and characteristics of observed entities, behaviour and characteristics which would otherwise remain inexplicable. This basic argument differs from the success argument in the following way. Where the success argument uses realism to explain the observational success of theories, the basic argument uses realism to explain observed phenomena. This is not to say that observational success is unimportant to the basic argument: the explanation of observed phenomena, like any explanation, is tested by its observational success. So according to the basic argument, realism is successful; according to the popular one, it explains success.[18]

In sum, there are some good arguments for scientific realism provided the realist is allowed abduction. Some critics reject abduction but this rejection seems dubious. Perhaps our knowledge of observables depends on abduction. In any case, abduction seems to be on an equal footing, at least, with other ampliative methods of inference.

4. Arguments Against Scientific Realism

4.1 The Underdetermination Argument

There is an appealing and influential empiricist argument against scientific realism that starts from a doctrine of empirical equivalence.[19] 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.

[18] Devitt (1991: 113–17). Hacking's arguments (1983) for the reality of entities manipulated in experiments and perceived under a microscope are persuasive examples of the basic argument (although he, strangely, does not regard them as abductions). Hacking's point about manipulation is clearly related to Alan Musgrave's (1988) insistence that novel predictions give us the best reason for believing a theory. Ernan McMullin (1991), responding to Fine (1991), provides some nice examples of the basic argument in geology, biology, and astrophysics. An advantage of the basic argument is that it makes clear that, contrary to Fine's frequent claims, the use of abduction to justify realism is not at some 'philosophical' level above science: 'the argument is properly carried on at one level only, the level of the scientist' (McMullin 1991: 104).

[19] My discussion of this argument draws on a more detailed one in Devitt (2002).

So, realist doctrines like SR and SSR are unjustified.[20]

Some preliminaries. First, what exactly is it for two theories to be 'empirically equivalent'? The basic idea is that they have the same observational consequences. We shall later see the importance of looking very closely at this basic idea.

Secondly, where EE talks simply of T having equivalent rivals, the premiss of the argument is sometimes that T has *indefinitely many* rivals (e.g. Kukla 1998: 58) and sometimes that it has *at least one* (e.g. Psillos 1999: 164). For convenience, I shall mostly treat EE as if it were only committed to one rival because its commitment to more does not seem to make a significant difference to the conclusions we should draw.

Thirdly, SU should not be confused with various other underdetermination theses[21] including the weak and obviously true one, mentioned in Section 2, that leads to the challenge of extreme scepticism:

(WU) Any theory has rivals that entail the same actual given observational evidence.

SU is stronger than WU in two respects. First, SU concerns an *ampliative* relation between theories and evidence and not merely a deductive one. Secondly, SU is concerned with T's relation to *all possible* evidence not merely to the given evidence.[22] If we are to avoid scepticism 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 on the given evidence nor 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 scepticism behind us, realism is threatened.

Now, consider EE. 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. Still, it may not count as a rival because it is consistent with T. That is easily fixed: T* is the theory that T_o is true but T is not. 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 (Laudan and Leplin 1991; Hoefer and Rosenberg 1994). But this response

[20] The argument has no one clear source. But see Duhem (1906); Quine (1960, 1961 ('Two Dogmas'), 1975); van Fraassen (1980); Putnam (1983 ('Equivalence')).

[21] My presentation reflects the influence of Laudan's excellent discussion of this variety (1996, ch. 2).

[22] So the premiss about empirical equivalence that is supposed to support SU in the underdetermination argument must also concern all possible evidence. Psillos's version of the argument fails on this score. It starts: 'for *any* theory *T* and *any* body of observational evidence *E*, there is another theory *T′* such that *T* and *T′* are empirically equivalent with respect to *E*' (1999: 164). The quantifiers need to be reordered if this is to support SU: for *any* theory T, there is another theory T′ such that for *any* (possible) body of observational evidence E, T and T′ are empirically equivalent with respect to E.

seems question-begging and unconvincing, as Andre Kukla argues (1998: 66–81). A better response is that, in counting theories generated by the empiricist algorithm as rivals, EE, as it stands, is too weak to sustain SU. For, with extreme scepticism behind us, we are justified in choosing T over T*.

In considering this choice, the first half of T*, T_o, is key. In van Fraassen's terminology, T_o is the claim that T is 'empirically adequate'. He has some famous remarks comparing this claim with the bolder claim that T is true: 'the empirical adequacy of an empirical theory must always be more credible than its truth' (1985: 247); 'it is not an epistemological principle that one may as well hang for a sheep as for a lamb' (p. 254). The extra boldness of T comes, of course, from its realist commitment to certain truths about unobservables. Because van Fraassen thinks that T takes no further empirical risk than T_o, he claims that this extra boldness 'is but empty strutting and posturing', a 'display of courage not under fire' (p. 255). We should prefer the weaker T_o.

Now if van Fraassen were right about this, no evidence could justify a move from T_o to the bolder T. So it could not justify a preference for T over its rival T* (=T_o & not-T). SU would be established.

Here is a reason for thinking that van Fraassen is not right. If it were really the case that we were only ever justified in adopting the weakest theory compatible with the possible evidence for T, we would have to surrender to extreme scepticism. For T_o is far from being the weakest such theory. For example, consider T_e, the theory that T is 'experientially adequate'. Where T_o claims that the observable world *is* as if T, T_e claims only that the observable world *appears to be* as if T. T_e is much weaker than T_o: it does not require that there be an observable world at all; perhaps an evil demon is at work. Those, like van Fraassen, who believe theories of the observable world are displaying courage not under fire all the time.[23]

This argument exemplifies the negative side of the realist strategy described earlier: arguing that the case for scepticism about unobservables produced by the anti-realist is no better than the case for scepticism about observables.

We can apply the positive side of the strategy too. Any methods of ampliative inference that support the move from T_e to T_o and free us from extreme scepticism must justify the dismissal of the evil-demon hypothesis and a whole lot of others. The methods must justify many singular hypotheses about unobserved objects and many general hypotheses that cover such objects ("All ravens are black" and the like). Whether or not these methods alone support the further move to T, hence support scientific realism, they will surely justify the dismissal of T's rival T*, produced by the empiricist algorithm. And they will justify the dismissal of another empirically equivalent rival produced by Kukla's algorithm according to which the world changes when unobserved (1993). It would be nice to know, of course, what these methods are. But it is a strategic error for the scientific realist to attempt to

[23] I develop this argument more thoroughly in Devitt (1991: 150–3).

say what they are in responding to the anti-realist. For, the anti-realist believes in observables and *whatever* ampliative inferences support that belief will justify the dismissal of the likes of T*.

The anti-realist might, of course, simply insist that inferences that work for observables do not work for unobservables. Certainly there is no logical inconsistency in this insistence.[24] Nevertheless, the insistence is arbitrary and unprincipled. The realist need say no more.[25]

We conclude that EE as it stands cannot sustain SU: T is indeed justified 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 rival to T that can be dismissed by whatever ampliative inferences enable us to avoid extreme scepticism. Let us say that the rivals that can be thus dismissed are not 'genuine'. T* and the output of Kukla's algorithm are surely not genuine. Precisely how far we can go in thus dismissing rivals remains to be seen, of course, pending an account of how to avoid extreme scepticism. And, given the realist strategy, the account that matters is the one given by the anti-realist.

With EE now restricted to genuine rivals, the next step in assessing the underdetermination argument is a careful consideration of how to interpret EE's talk of empirical equivalence. The basic idea is that empirically equivalent theories have the same observational consequences. What does this amount to? A natural first stab at an answer is that the theories entail the same observations. This yields the following version of EE:

(EE1) T has genuine rivals that entail the same possible observational evidence.

Whether or not EE1 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). By failing to take account of these joint consequences, EE1 leaves many ways in which evidence could favour T over its rivals, contrary to SU. To sustain SU and challenge realism, we need another interpretation of EE.

Consider Laudan and Leplin's influential critique of the underdetermination argument (1991). They propose the thesis 'the Instability of Auxiliary Assumptions' according to which 'auxiliary information providing premises for the derivation of observational consequences from theory is unstable in two respects: it is defeasible and it is augmentable' (p. 57).[26] As the accepted auxiliaries that can be conjoined

[24] Kukla emphasizes this (1998: 25–6, 84).

[25] However, I think that an examination of the epistemic significance of observation helps to bring out the arbitrariness (Devitt 1991: 143–7).

[26] See also Ellis (1985); Devitt (1991: 117–21).

with T change, so do its consequences. So, any determination of T's empirical consequence class 'must be relativized to a particular state of science', the state that supplies the auxiliary hypotheses. Thus 'any finding of empirical equivalence is both contextual and defeasible' (p. 58). To determine the consequences of T we need more than logic, we need to know which auxiliaries are acceptable, an '*inescapably epistemic*' matter (p. 59).

To avoid the consequences of this argument, Kukla (1993) proposed an answer to our interpretative question along the following lines: for two theories to be empirically equivalent at time *t* is for them to entail the same observations when conjoined with A*t*, the auxiliaries that are accepted at *t*. This yields:

(EE2) T has genuine rivals which are such that when T and any of the rivals are conjoined with A*t* they entail the same possible observational evidence.

Set aside for a moment 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 At*, to the auxiliaries accepted at a certain time. 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: for two theories to be empirically equivalent is for them to entail the same observations when conjoined with any (possible) acceptable auxiliaries.[27] This yields:

(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.[28]

[27] This demand arises out of a liberal and, it seems to me, intuitive view of what counts as 'possible evidence'. Quine and van Fraassen have a more restricted view, which I discuss in Devitt (2002, sect. 13).

[28] Tim Williamson pointed out to me a problem with EE3 as it stands. Suppose that T_1 and T_2 are two allegedly equivalent rivals and that A_1 is an acceptable auxiliary *relative to* T_1 but no T_2 and A_2 is an acceptable auxiliary *relative to* T_2 but note T_1. Thus A_1 might be a theory of a testing instrument from the perspective of T_1, and A_2 a theory of that instrument from the perspective of T_2. So the acceptability of the auxiliaries is not independent of the theories being tested. Now suppose that T_1 & A_1 and T_2 & A_1 have different observational consequences. That alone should not show that T_1 and T_2 are not empirically equivalent. For, T_1 & A_1 and T_2 & A_2 might have the same observational consequences. Clearly, what needs to be assessed for empirical equivalence are theories *together with their*

The main point of Laudan and Leplin's critique can be put simply: we have no reason to believe EE3.[29] 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 (Devitt 1991: 119), when we note our capacity to invent new instruments and experiments to test theories. With a new instrument and experiment comes 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.[30] 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.

This argument against EE3 does not depend on any assumption about the breadth of T. So EE3 cannot be saved by taking it to apply to 'total sciences' (Boyd 1984: 50). Should such a broad conjunction of theories seem to face an equivalent rival at a certain time, we are unlikely to have sufficient reason for believing that experimental developments will not enable us to discriminate the conjunction from its rival by supplying new auxiliaries. *There is no known limit to our capacity to generate acceptable auxiliaries.*

I have argued that we have no reason to believe EE3. But suppose, nonetheless, that EE3 were true. Would this establish SU and undermine scientific realism? It might well do so.[31] If EE3 were true, realists would have to appeal to 'non-empirical virtues' to choose between empirically equivalent theories. Empirical virtue is a matter of entailing (in conjunction with accepted auxiliaries) observational truths and not entailing observational falsehoods. The non-empirical virtues are explanatory power, simplicity, and the like. For the reason indicated earlier in discussing abduction (Section 3), I think that the realist is entitled to appeal to explanatory virtues, at least. But if it really were the case that all theories faced genuine rivals equally compatible with all possible evidence, the appeal to these virtues would

dependent auxiliaries. And EE3 should be taken as referring to any possible *independently* acceptable auxiliaries.

29 Note that this is not the claim that EE3 is 'demonstratively false'; cf. Kukla (1998: 58).

30 For some theories where we may have sufficient reason, and for some past ones where we wrongly thought we had, see Psillos (1999: 166–8 and the works he cites).

31 Laudan and Leplin (1991: 63–8) think not, arguing that T can be indirectly supported over its rival by evidence that confirms another theory that entails T but not its rival; and that some consequences of T and its rival might support only T. But, as Kukla points out (1998: 84–90), this argument begs the question: if EE3 really were true, this evidential support would seem to disappear.

seem epistemologically dubious.[32] For, in those circumstances, there could be no way to judge the empirical success of these virtues, no way to show, for example, that theories that provide the best explanation tend to be observationally confirmed. So the defence of realism might well depend on there being no good reason for believing EE3.

What about EE2? We have already seen that EE2 will not sustain SU. But perhaps it is otherwise threatening to realism. So, first, we need to consider whether it is true; then, whether, if it were, it would undermine realism.

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? EE2 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 scepticism will count many rivals as not genuine, so many as to make this guarantee seem baseless. How could we know a priori that T must always face such a genuine rival?

Suppose, nonetheless, that EE2 were true. So, if T and its rivals are restricted to the accepted auxiliaries at a certain time, T could not be justified over some rivals on the basis only of the observations that the theories and auxiliaries entail and the ampliative methods that save us from extreme scepticism. So, without recourse to some further ampliative methods, T would be underdetermined by the evidence that the restriction allows into play. Of course, once new acceptable auxiliaries were discovered and the restriction changed, the further methods might well not be needed to justify T over those old rivals. So this underdetermination would not be as serious as SU, but it would be serious enough: at any time, we would not know what to be realist about. But then perhaps the realist would be entitled to the further ampliative methods that would remove this underdetermination. For the reasons already indicated, and given that the case for EE3 has not been made, I think that the realist might be so entitled.[33]

In sum, we have no reason to believe EE2 or EE3 and so the underdetermination argument fails. However, if EE3 were true, it might well undermine scientific realism and if EE2 were true, it could. Once we have set aside extreme scepticism, then, contrary to received opinion, the non-empirical virtues are not central to defending realism from the underdetermination argument; the rejection of the equivalence

[32] I emphasize that since it has not been established that all theories do face such rivals, it might well be appropriate to appeal to explanatory virtues, or indeed to the evidential support mentioned by Laudan and Leplin (1991: 63–8), to prefer some theory that does face such a rival.

[33] In a reply to Kukla (1993), Leplin and Laudan (1993: 10), in effect, doubt EE2 but in any case emphasize that EE3 is what matters to the underdetermination argument. Kukla disagrees, claiming, in effect, that EE2, when applied to total sciences, 'brings in its train all the epistemological problems that were ever ascribed to the doctrine of EE' (1998: 64). According to my discussion, EE2 would bring some epistemological problems if it were true, but they are not as extreme as those that would be brought by EE3 if it were true.

thesis is. In drawing these conclusions I have mostly construed EE2 and EE3 as if they were committed only to T having at least one genuine empirically equivalent rival. Their actual commitment to more rivals does not significantly change the conclusions we should draw.

4.2 The Pessimistic Meta-Induction

The most powerful argument against scientific realism, in my view, is what Putnam (1978) calls a 'meta-induction'. It does not rest on a prejudice against abduction or exaggerated concerns about underdetermination. It rests on plausible claims about the history of science. The basic version of the argument is aimed at an entity realism like SR: the unobservables posited by past theories do not exist; so, probably the unobservables posited by present theories do not exist. Another version, largely dependent on the basic one, is aimed at a 'fact' realism like SSR: past scientific theories are not approximately true; so, probably present theories are not approximately true. (This is an example of the convenience of exploiting the disquotational property of "true" to talk about the world.) Both versions of the argument rest on a claim about past theories from the perspective of our present theory.[34] And the pessimistic suggestion is that, from a future perspective, we will have a similarly critical view of our present theories. Laudan (1981, 1984, 1996) has supported these claims about the past with a list of theoretical failures.

Laudan's list is the one used to discuss the realist's success argument (2.1), but the purpose of the list is different here. The purpose before was to show that past theories were successful without being true, thus undermining the argument for realism that 'realism explains success'. The list's purpose here is to show that past theories were not approximately true and their unobservables did not exist, thus establishing the premiss of an argument against realism, against the view that present theories are approximately true and their unobservables exist.

Scientific realism already concedes something to the meta-induction in exhibiting *some* scepticism 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 scepticism 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

[34] So there is a 'tension' in the argument: it seems to rest on a realist view of present science and yet concludes that this realist view is mistaken; see Leplin (1997: 141–5). suppose that we should see the meta-induction as some sort of *reductio*.

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 (Worrall 1989;[35] Kitcher 1993: 140–9; Psillos 1999, chs. 5–6). 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 (McMullin 1984).

In the light of history, some scepticism about the claims of science is clearly appropriate. The argument is over how much, the mild scepticism of the realist, or the sweeping scepticism of the meta-induction. Settling the argument requires close attention to the historical details. This is not, of course, something that I shall be attempting. However, I shall make some general remarks about the attempt.

How can we *tell* whether Fs, posited by a past theory, exist? Given the disquotational schema ' "F" refers iff Fs exist,' many approach this question by considering another: How do we tell whether "F" refers? This common approach would be harmless if it exploited only the disquotational property of "refer" captured by the schema, a property acceptable to the deflationist. For then the reference question is just a paraphrase of the existence question. However, it is usual (and, in my view, right) to take "refer" to pick out a substantive semantic relation between "F" and the world, a relation that needs to be explained by a *theory* of reference. So it is natural to take the reference question to concern this substantive relation and to be answered by appealing to some theory of reference. But then the common approach is far from harmless.

The first problem is that the theory of reference appealed to on this approach is usually a description theory. According to this theory, the reference of "F" depends on the descriptions (other terms) that its containing theory associates with it: it refers to whatever those descriptions pick out. It is likely that, from our present scientific perspective, those descriptions do not pick anything out. So, the conclusion is drawn that "F" does not refer and hence there are no Fs.[36] Yet, the arguments of Saul Kripke (1980) and others have made it likely that reference for some terms, at least, is to be explained not by a description theory but by a theory that links a term to its referent in a more direct causal way.[37] So it may well be that a description theory is the wrong theory for "F". This points to the second, deeper, problem with the common approach: in attempting to answer the existence question by answering the reference question, the approach has its epistemic priorities all wrong. For, *we know far less about reference*, particularly about when to apply a description theory and when to apply a causal theory, than we know about what exists. In light of this,

[35] Worrall takes the truth to be not about the nature of entities but about structures that contain the entities. For a critical discussion of this 'structural realism', see Psillos (1999, ch. 7).

[36] See Kuhn (1962) for an argument along these lines. Stephen Stich (1983) and others have argued similarly for various forms of eliminativism about the mind. Stich has since recanted (1996: 3–90).

[37] For a summary and development of these and other moves in the theory of reference, see Devitt and Sterelny (1999).

the rational procedure is to let our view of what exists guide our theories of reference rather than let our theories of reference determine what exists.[38]

So, we should not use a theory of reference to answer our existence question. How then should we answer it? Consider how, in general, we argue directly for the non-existence of Fs. On the basis of the established view of Fs, we start, implicitly if not explicitly, with an assumption about the nature of Fs: something would not be an F unless it were G. Then we argue that nothing is G. So, there are no Fs. Very often this argument is persuasive and generally accepted. But someone might respond by denying the assumption about nature. 'Fs do not have to be G, they are just mistakenly thought to be G. So the argument proves nothing.' How do we settle this disagreement? It may be difficult. We can try saying more about the established view of Fs, but this may not do the trick. After all, the responder does not deny that Fs are thought to be G, just that being G is part of the nature of being an F. And the established view may not be clear on the nature issue. We may be left with nothing but a 'clash of intuitions' over that issue. In such a situation, we should wonder whether there is a genuine issue to settle: there may be no determinate matter of fact about the nature issue. If there is not, then there is no determinate matter of fact about whether the absence of G things establishes the non-existence of Fs.

Consider two humdrum examples. Most people are anti-realist about witches because they believe that nothing casts spells, rides on a broomstick through the sky, and so on. Some people may be anti-realist about God because they are convinced by the Problem of Evil that nothing is both all powerful and all good. But these are grounds for anti-realism only if casting of spells, riding on broomsticks, and so on, and being all powerful and all good, are essential to witches and God, respectively. There may be disagreement about that. And there is room for worry that disagreement may not be entirely over matters of fact.

In light of this, we can expect that close attention to the historical details about past unobservables will reveal some ontologically determinate cases but very likely some indeterminate ones too. The determinate cases will surely include some of non-existence; phlogiston is a good candidate. But it will surely also include some of existence; the atoms posited in the nineteenth century are good candidates.[39] 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.

[38] In Devitt (1991) I argue for this priority. There is, of course, a truth underlying the mistaken approach: to determine whether the posits of a theory exist we have to know what those posits are and for that we have to understand the language of the theory (pp. 50–3). But understanding a language is a practical skill that does not require theoretical knowledge about the language, else we would understand very little (pp. 270–5).

[39] Also molecules and microbes; see Miller (1987).

I think (Devitt 1991: 162–5) that there is a good reason for being dubious about the inference. Suppose that our past theories have indeed 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. A naturalized epistemology would surely show that science has for two or three centuries been getting better and better at this. 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 have now the sort of confidence reflected by SR.[40] And if we have been improving, but not fast enough for SR, 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 significantly righter. It seems to me most unlikely that this case can be made.

5. Conclusions

Scientific realism is best seen as a straightforwardly metaphysical doctrine along the lines of SR or SSR. Various explanationist arguments for scientific realism succeed provided that the realist is entitled to abduction. I have suggested that the realist is entitled. 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.

[40] So this realist response does not take the failures of 'immature' science to be irrelevant to the defence of realism, thus threatening the defence with 'vacuity' (Laudan 1981: 34). Rather, it takes the relevance of a science's failures (and successes) to that defence to increase with *the degree of* that science's maturity, a degree assessed by an empirical epistemology.

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. This is a hard case to make.[41]

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