5 Resisting the pessimistic induction

The explanationist defence of realism (EDR) has suffered a rather serious blow from Laudan's contention that the history of science itself destroys the credibility of realist explanation of the success of science. For it is full of theories which were once empirically successful and yet turned out to be false. Laudan's argument[1] against scientific realism is simple but powerful. It can be summarised as follows:

> The history of science is full of theories which at different times and for long periods had been empirically successful, and yet were shown to be false in the deep-structure claims they made about the world. It is similarly full of theoretical terms featuring in successful theories which do not refer. Therefore, by a simple (meta-)induction on scientific theories, our current successful theories are likely to be false (or, at any rate, are more likely to be false than true), and many or most of the theoretical terms featuring in them will turn out to be non-referential.
>
> Therefore, the empirical success of a theory provides no warrant for the claim that the theory is approximately true. There is no substantive retention at the theoretical, or deep-structural, level and no referential stability in theory-change.

Laudan has substantiated his argument by means of what he has called 'the historical gambit': the list that follows – which, Laudan says, 'could be extended *ad nauseam*' – gives theories which were once empirically successful and fruitful, yet were neither referential nor true. These theories were *just* false:

- the crystalline spheres of ancient and medieval astronomy
- the humoral theory of medicine
- the effluvial theory of static electricity
- catastrophist geology, with its commitment to a universal (Noachian) deluge

- the phlogiston theory of chemistry
- the caloric theory of heat
- the vibratory theory of heat
- the vital-force theory of physiology
- the theory of circular inertia
- theories of spontaneous generation
- the contact-action gravitational ether of Fatio and LeSage
- the optical ether
- the electromagnetic ether.

If Laudan is right, then the realist's explanation of the success of science flies in the face of the history of science: the history of science cannot possibly warrant the realist belief that currently successful theories are approximately true, at least insofar as the warrant for this belief is the 'no miracle' argument. In what follows, I analyse the structure of Laudan's argument and show how scientific realism can be defended.

Laudan's *reductio*

The 'pessimistic induction' is a kind of *reductio*. The target is the realist thesis that:

(A) Currently successful theories are approximately true.

Laudan does not directly deny that currently successful theories may *happen* to be truth-like. His argument aims to discredit the claim that there is an *explanatory connection* between empirical success and truth-likeness which warrants the realist's assertion (A). In order to achieve this, the argument compares a number of past theories to current ones and claims:

(B) If currently successful theories are truth-like, then past theories *cannot* have been.

Past theories are deemed not to have been truth-like because the entities they posited are no longer believed to exist and/or because the laws and mechanisms they postulated are not part of our current theoretical description of the world. Then, comes the 'historical gambit':

(C) These characteristically false theories were, nonetheless, empirically successful.

So, empirical success is not connected with truth-likeness and truth-likeness cannot explain success: the realist's potential warrant for (A) is defeated. As Laudan put it:

> Because they [most past theories] have been based on what we now believe to be fundamentally mistaken theoretical models and structures, the realist cannot possibly hope to explain the empirical success such theories enjoyed in terms of the truth-likeness of their constituent theoretical claims.
>
> (1984a: 91–92)

Hence, the pessimistic induction 'calls into question the realist's warrant for assuming that *today's* theories, including even those which have passed an impressive array of tests, can thereby warrantedly be taken to be (in Sellars' apt image) 'cutting the world at its joints' (Laudan 1984b: 157).

No realist can deny that Laudan's argument has *some* force. It shows that, on inductive grounds, the whole truth and nothing but the truth is unlikely to be had in science. That is, all scientific theories are likely to turn out to be, strictly speaking, false. This is something that realists seem to have to concede. However, a false theory can still be *approximately true*. The notion of approximate truth is discussed in detail in Chapter 11. For the time being, let me note that a theory is approximately true if it describes a world which is similar to the actual world in its most central or relevant features. So, what realists need to show is that past successful theories, although strictly speaking false, have been approximately true. This is the defensive line in which realists regroup and start their counter-attack.

Laudan's immediate challenge is that a theory cannot be said to be approximately true unless it is shown that its central terms refer (1981: 33). This requirement seems plausible. But one should be careful here. The intended realist claim is that from the genuine empirical success of a theory one can legitimately infer that the entities posited by the theory are real – they inhabit the world we live in. Without this assumption we cannot adequately explain the empirical success of a theory. There is, however, no way in which any proponents can 'step outside' of their theories and check whether these entities exist. We should simply have to rely on our theories as our best guide to what the furniture of the world is. What Laudan observes is that, given the past track-record of science, we simply cannot do that: the radical changes in the central ontological claims made by theories over the centuries suggest that any such claim is as likely to go as any other. None of them, in other words, enjoys any privilege over any other. Mary Hesse has put the same thought in the form of the 'principle of no privilege', which, she says, follows from an 'induction from the history of science'. According to this principle, 'our own scientific theories are held to be as much subject to radical conceptual change as past theories are seen to be' (1976: 264). In order to rebut the 'principle of no privilege', realists should show that:

1 the theoretical discontinuities in theory-change were neither as widespread nor as radical as Laudan has suggested;

2 instead, there has emerged a rather stable and well-supported network of theoretical assertions and posits which is our best account of what the world is like; and
3 theoretical terms that can be legitimately taken to have been central in past theories can still be referential, i.e. they can still be taken to refer to entities which feature in science's current theoretical ontology.

In sum, realists should try to reconcile the historical record with the realist claim that successful theories are typically approximately true. How can this be done?

Realist gambits

Before discussing this, let me make two preliminary points. First, one should note that scientists are not prone to acquire only false beliefs. As science progresses, they accumulate more evidence, further and fresh empirical data, which they can then use to update and modify their beliefs and theoretical commitments. Besides, scientists can come to know how to better test their theories and, in particular, how to identify those methods of theory-construction which are likely to generate false and unwarranted beliefs. Hence, they can form better-supported theoretical beliefs. They can learn how to gauge the requisite evidence for their beliefs, how to improve their methods, and how to avoid unreliable methods. There is no guarantee, of course, that this process of learning from past experience will lead from false to truer theories. However, if scientists can positively learn from past experience, they are in a better position to abandon false theoretical claims in favour of new ones that are better supported by the evidence. Hence, these claims have a better chance of being truth-like than did those now abandoned. Second, even a quick glance at current science suggests that there is a host of entities, laws, processes and mechanisms posited by past theories – such as the gene, the atom, kinetic energy, the chemical bond, the electromagnetic field etc. – which have survived a number of revolutions to be retained in current theories. That is, one can quickly see that Laudan has overstated his case against scientific realism. In its crudest form, the pessimistic induction boils down to the claim that, as science grows, we can certify only the accumulated theoretical falsehoods, while we invariably have no good reasons to believe that we have hit upon some theoretical truths. But this is far-fetched and implausible.

Success too-easy-to-get

It is now time to attempt a conclusive refutation of Laudan's *reductio*. In light of the structure of his argument outlined earlier, one way to block Laudan's *reductio* is to target the 'historical gambit' or premiss (C). One can substantially weaken premiss (C) simply by reducing the size of Laudan's

list. If we manage to restrict the meta-inductive basis, it no longer warrants the conclusion that genuine success and approximate truth are unconnected. Therefore, the 'historical gambit' is neutralised.

The form of Laudan's 'historical gambit' is this. It claims that all past theoretical conceptualisations of the several domains of inquiry $T_1, \ldots, T_n$ Laudan has sampled have been empirically successful yet false, and it concludes, inductively, that *any* arbitrarily successful scientific theory T_{n+1} is likely to be false (or, at any rate, more likely to be false than true).

This kind of argument can be challenged by observing that the inductive basis is not big and representative enough to warrant the pessimistic conclusion (cf. Devitt 1984: 161–162; McMullin 1984: 17). The basis for Laudan's induction can be eroded by querying whether all of the listed theories were, as a matter of fact, successful and whether they were representative of their disciplines at stages of development sufficiently advanced as to be reckoned theoretically mature.

One can dispute the claim that all theories in Laudan's list were successful. Laudan suggests that a theory is successful 'so long as it has worked reasonably well, that is, so long as it has functioned in a variety of explanatory contexts, has led to several confirmed predictions, and has been of broad explanatory scope' (1984a: 110). To be sure, he thinks that this is precisely the sense in which realists claim scientific theories to be successful when they propose the 'no miracle' argument (ibid.). However, the notion of empirical success should be *more* rigorous than simply getting the facts right, or telling a story that fits the facts. For any theory (and for that matter, any wild speculation) can be made to fit the facts – and hence to be successful – by simply 'writing' the right kind of empirical consequences into it. The notion of empirical success that realists are happy with is such that it includes the generation of novel predictions which are in principle testable.[2] Consequently, it is not at all clear that all theories in Laudan's list were genuinely successful. It is doubtful, for instance, that the contact-action gravitational ether theories of LeSage and Hartley, the crystalline spheres theory and the theory of circular inertia enjoyed any genuine success (cf. McMullin 1987: 70; Worrall 1994: 335). A realist simply would not endorse their inclusion in Laudan's list. On the contrary, the real question for a realist is this: are theories which were *genuinely* successful characteristically false?

Given the centrality of novel predictions in my defence of realism, it is prudent to analyse this notion a bit further so that it becomes clearer and certain misunderstandings are avoided. A 'novel' prediction is typically taken to be the prediction of a phenomenon whose existence is ascertained only *after* a theory suggests its existence. On this view a prediction counts as novel only if the predicted phenomenon is *temporally* novel, that is, only if the predicted phenomenon was hitherto unknown. This, however, cannot be the whole story. For one, theories also get support from their ability to explain already known phenomena. For another, why should the provenance

of the predicted phenomenon have any bearing on whether or not the prediction supports the theory? One can easily imagine a case in which, unbeknown to the theoretician whose theory made the prediction of a temporally novel phenomenon, the phenomenon had already been discovered by some experimenter. Would or should this information affect the support which the predicted fact confers on the theory? If we thought that *only* genuine temporally novel predictions can confer support on theories, then we would have to admit that once we were aware that the fact was known, the predicted fact would become impotent to support the theory. In order to avoid these counter-intuitive pitfalls, the notion of novelty should be broader than what is meant by 'temporal novelty'. Following Earman (1992: Chapter 4, section 8) we should speak of 'use novelty', where, simply put, the prediction P of a known fact is use-novel *relative to a theory* T, if no information about this phenomenon was used in the construction of the theory which predicted it.[3]

But how exactly are we to understand the claim that a theory T makes a use-novel prediction of a known phenomenon? I think that in order to appreciate the issue at stake, one must follow Worrall (1985; 1989c) and provide some analysis of the ways in which a known fact E can be accommodated in a scientific theory T. Generally, there are two such ways:

- Information about a known fact E is used in the construction of a theory T, and T predicts E.
- A phenomenon E is known the time that a theory T is proposed, T predicts E, but no information about E is used in the construction of T.

Tidal phenomena, for instance, were predicted by Newton's theory, but they were not used in its construction. Let me, then, call *novel accommodation* any case in which a known fact is accommodated within the scope of a scientific theory, but no information about it is used in its construction. Let me, moreover, contrast novel accommodation with *ad hoc accommodation*. Although the Lakatosian school has produced a fine-grained distinction between levels of ad hocness, (cf. Lakatos, 1968: 399; 1970: 175; Zahar, 1973: 101), I shall take the most general case, namely:

> *Conditions of ad hocness*: A theory T is ad hoc with respect to phenomenon E if and only if either of the following two conditions is satisfied:
>
> 1 A body of background knowledge B entails the existence of phenomenon E. Information about E is used in the construction of a theory T, and T accommodates E.
> 2 A body of background knowledge B entails the existence of phenomenon E. A certain already available theory T does not predict/explain E. T is modified into theory T' so that T' predicts E, but the *only* reason for this modification is the prediction/explanation of E. In particular T' has no other excess theoretical and empirical content over T.[4]

Given this analysis, novel accommodation (or use novelty) of known facts can be explicated as follows:

> *Use novelty*: A prediction P of a phenomenon E is use-novel with respect to a theory T if E is known before T is proposed, T does not satisfy either of the ad hocness conditions and T predicts E.

The real issue then is whether use novelty and temporal novelty have different bearings on the empirical support of a theory. I do not want to enter here the subtleties of this debate, for my purpose is to contrast novel accommodation with ad hoc accommodation. But, briefly, my view is that both use novelty and temporal novelty, so long as they are sharply distinguished from any ad hoc accommodation, are *complementary* aspects of theory confirmation. For, one can demand that a theory should accommodate known phenomena in a *non* ad hoc way, and *in addition* to this that it must yield temporally novel predictions. When, however, it comes to the *support* that use-novel and temporally novel predictions confer on a theory, that is, when it comes to the degree to which they confirm a theory, we may well assign different weights to these two sorts of prediction. It is natural to suggest that any temporally novel predictions which obtain carry an *additional* weight, because a theory that suggests new phenomena takes an extra risk of refutation. For there is always the possibility that a known fact can be 'forced' into a theory, whereas a theory cannot be forced to yield an hitherto unknown fact. Hence, predicting a new effect – whose existence falls naturally out of a theory – makes the theory more risky and susceptible to extra experimental scrutiny which may refute it.[5]

In sum, I want to stress that it is important *not* to contrast use novelty and temporal novelty, but both are to be contrasted with ad hoc accommodation. For, if anything, there is at most a difference in *degree* between use novelty and temporal novelty, whereas, there is a difference in *kind* between novel accommodation and ad hoc accommodation.[6]

Besides making the notion of empirical success more rigorous, another way to reduce the size Laudan's list is to suggest that *not all* past theoretical conceptualisations of domains of inquiry should be taken seriously. Realists require that Laudan's list should include only *mature* theories; that is, theories which have passed the 'take-off point' (Boyd) of a specific discipline. This 'take-off point' can be characterised by the presence of a body of well-entrenched background beliefs about the domain of inquiry which, in effect, delineate the boundaries of that domain, inform theoretical research and constrain the proposal of theories and hypotheses. This corpus of beliefs gives a broad identity to the discipline by being, normally, the common

ground that rival theories of the phenomena under investigation share. It is an empirical matter to find out when a discipline reaches the 'take-off point', but for most disciplines there is such a point (or, rather a period). For instance, in the case of heat phenomena, the period of theoretical maturity was reached when such background beliefs as the principle of impossibility of perpetual motion, the principle that heat flows only from a warm to a cold body and the laws of Newtonian mechanics had become well entrenched. If this requirement of maturity is taken into account, then theories such as the 'humoral theory of medicine' or the 'effluvial theory of static electricity' drop out of Laudan's list. Once Laudan's list is restricted to those past theories which were *mature and genuinely successful*, then it is no longer strong enough to warrant the pessimistic conclusion.

Although it is correct that realists should not worry about all of the past theories that Laudan suggests, the present move is not enough to defeat the 'pessimistic induction': for it does not account for the fact that at least *some* past theories which pass both realist tests of maturity and success are nevertheless considered false. Relevant examples are the caloric theory of heat and the nineteenth-century optical ether theories. If these theories are false, despite their being both distinctly successful and mature, then the intended explanatory connection between empirical success and truth-likeness is still undermined. How then can we defend this explanatory connection?

The *divide et impera* move

The crucial premiss in Laudan's *reductio* is (B) (see p. 102): if we hold current theories to be truth-like, then past theories are bound not to be truth-like since they posited entities that are no longer believed to exist, and posited laws and theoretical mechanisms that have now been abandoned. Without this premiss the pessimistic conclusion does not follow.

Can we defeat (B)? Here is a suggestion: it is enough to show that the success of past theories did not depend on what we now believe to be fundamentally flawed theoretical claims. Put positively, it is enough to show that the theoretical laws and mechanisms which generated the successes of past theories have been retained in our current scientific image. I shall call this the *divide et impera* move. It is based on the claim that when a theory is abandoned, its theoretical constituents, i.e. the theoretical mechanisms and laws it posited, should not be rejected *en bloc*. Some of those theoretical constituents are inconsistent with what we now accept, and therefore they have to be rejected. But not all are. Some of them have been retained as essential constituents of subsequent theories. The *divide et impera* move suggests that if it turns out that the theoretical constituents that were responsible for the empirical success of otherwise abandoned theories are those that have been retained in our current scientific image, then a substantive version of scientific realism can still be defended.

This move dissociates genuine empirical success from characteristic falsity. Moreover, it paves the way for the 'right kind' of explanatory connection between success and truth-likeness. Laudan, realists should say, has taught us something important: on pain of being at odds with the historical record, the empirical success of a theory cannot issue an unqualified warrant for the truth-likeness of everything that the theory says. Insofar as older realists have taken this view, they have been shown to be, to say the least, unrealistic. Yet, it would be equally implausible to claim that, despite its genuine success, everything that the theory says is wrong. The right assertion seems to be that the genuine empirical success of a theory does make it reasonable to believe that the theory has *truth-like constituent theoretical claims*.

Moreover, if the theoretical constituents that were responsible for the empirical successes of past theories have been retained in subsequent theories, then this gives us reason to be more optimistic about their truth-likeness: that all these theoretical constituents have been shown to be invariant and stable elements of our modern scientific image; they have survived several 'revolutions' and have contributed to the empirical success of science. I think realists should follow Philip Kitcher's lead (1993) and suggest that the best way to defend realism is to use the generation of stable and invariant elements in our evolving scientific image to support the view that these elements represent our best bet for what theoretical mechanisms and laws there are.

This preamble for the *divide et impera* move may resonate with two recent reactions to the 'pessimistic induction', those of Kitcher (1993) and of Worrall (1989; 1994). Both have defended the analogous view that realists should characterise which kinds of statement are abandoned as false and which are retained. Kitcher suggests a distinction between 'presuppositional posits' and 'working posits', while Worrall draws the line between the 'content' of a theoretical statement, which gets superseded, and its 'structure', which is retained. The position I defend is akin to Kitcher's, although some differences will be discussed shortly. However, the *divide et impera* move is not meant to reflect or capture Worrall's distinction between structure and content. The latter distinction and Worrall's position deserve a more detailed discussion and criticism, to which Chapter 7 is devoted.

How should realists circumscribe the truth-like constituents of past genuinely successful theories? I must first emphasise that we should really focus on the specific successes of certain theories, like the prediction by Fresnel's theory of diffraction that if an opaque disk intercepts the rays emitted by a light source, a bright spot will appear at the centre of its shadow; or Laplace's prediction of the law of propagation of sound in air by means of the hypothesis that sound's propagation is an adiabatic process. Then we should ask the question: how were these successes brought about? In particular, which theoretical constituents made essential contributions to them? It is not, generally, the case that *no* theoretical constituents contribute

to a theory's successes. Similarly, it is not, generally, the case that *all* theoretical constituents contribute (or contribute equally) to the empirical success of a theory. (What, for instance, was the relevant contribution of Newton's claim that the centre of mass of the universe is at absolute rest?) Theoretical constituents which make essential contributions to successes are those that have an indispensable role in their generation. They are those which 'really fuel the derivation' – to use one of Laudan and Leplin's recent expressions (1991: 462).

When does a theoretical constituent H indispensably contribute to the generation of, say, a successful prediction? Suppose that H together with another set of hypotheses H' (and some auxiliaries A) entail a prediction P. H indispensably contributes to the generation of P if H' and A alone cannot yield P and no other available hypothesis H^* which is consistent with H' and A can replace H without loss in the relevant derivation of P. Clearly, there are senses in which all theoretical assertions are eliminable, if, for instance, we take the Craig-transform of a theory, or if we 'cook up' a hypothesis H^* by writing P into it. But if we impose some natural epistemic constraints on the potential replacement – if, for instance, we require that the replacement be independently motivated, non ad hoc, potentially explanatory, etc. – then it is not certain at all that a suitable replacement can always be found. Worrall has recently noted that whenever a theory is replaced by another, 'the replacing theory alone offers a constructive proof of the "eliminability" of the earlier one' (1994: 339). There should be no doubt that the old theory as a whole gets eliminated. Yet, Worrall's observation does not establish the eliminability of the specific theoretical constituents that contributed to the empirical successes of the superseded theory. If the *divide et impera* move is correct, then these constituents are typically those that 'carry over' to the successor theory (admittedly, sometimes, only as limiting cases of the relevant constituents of the replacing theory).

So, when it comes to explaining the specific successes of a theory by means of the claim that the theory has truth-like constituent theoretical claims, realists should argue that the truth-like constituents are (more likely to be) those that contribute essentially to, or 'fuel', these successes. Realists need care only about those constituents which contribute to successes and which can, therefore, be used to account for these successes, or their lack thereof. Analogously, the theoretical constituents to which realists need not commit themselves are precisely those that are 'idle' components, impotent to make any difference to the theory's stake for empirical success.

What is required to successfully perform the *divide et impera* move? The key to this question lies in the careful study of the structure and content of past genuinely successful theories. What is needed are careful case-studies that will

- identify the theoretical constituents of past genuine successful theories that made essential contributions to their successes; and
- show that these constituents, far from being characteristically false, have been retained in subsequent theories of the same domain.

If all kinds of claims that are inconsistent with what we now accept were essential to the derivation of novel predictions and in the well-founded explanations of phenomena, then one cannot possibly appeal to their truth-likeness in order to explain empirical success. Then, Laudan wins. However, if it turns out that the theoretical constituents which were essential are those that have 'carried over' to subsequent theories, then the 'pessimistic induction' gets blocked. Settling this issue requires detailed study of some past theories that qualify as genuinely successful.

The good news for realism, as we shall see in detail in the next chapter, is that relevant studies of the several stages of the caloric theory of heat and the nineteenth-century optical ether theories suggest that both of the foregoing requirements can be met. However, as regards the *general* argument thus far, the details of these studies – illuminating though they may be – are not necessary. This argument has aimed to show that if realists successfully perform the two tasks outlined above, then a case can be made for scientific realism; it has also indicated how these tasks can be performed, in particular, what role the suggested case-studies are to play, what issues they should focus on and how they are relevant to settling the argument between scientific realism and the 'pessimistic induction'.

Is the *divide et impera* move perhaps too close to Kitcher's approach? Could one not simply identify the idle constituents of a theory with Kitcher's 'presuppositional posits' and the essentially contributing constituents with his 'working posits'? These identifications may be pertinent. However, there are differences. My distinction between idle and essentially contributing constituents is meant to capture how the successes of a theory can differently support its several theoretical constituents. Kitcher's distinction between presuppositional and working posits, however, is meant to capture the difference between referring and non-referring terms. Working posits are said to be 'the putative referents of terms that occur in problem-solving schemata', while presuppositional posits are 'those entities that apparently have to exist if the instances of the schemata are to be true' (Kitcher 1993: 149). But, so put, the distinction is problematic. For, in effect, we are told that the success of a problem-solving schema does support the existence of the referents of some of the terms featuring in it, but it does not support the existence of a putative entity the presence of which is required for the truth of the whole schema. But unless one shows how it is possible that the empirical success of the theory can lend support only to some, but not all, existence claims issued by the theory, then Kitcher's contention seems to be just grist to Laudan's mill. Kitcher suggests that the putative referents of presuppositional posits, such as the ether, were apparently only presupposed for the truth of the relevant schemata; in fact, they turned out to be eliminable without derivational loss (1993: 145). This suggestion is

retroactive and open to the charge that it is ad hoc: the eliminable posits are those that get abandoned. Yet, as we are about to see, the *divide et impera* move can improve on Kitcher's views by avoiding this charge.[7]

A central objection to my line thus far is the following: with the benefit of hindsight, one can rather easily work it out so that the theoretical constituents that supposedly contributed to the success of past theories turn out to be those which were, as it happens, retained in subsequent theories. So, the realists face the charge that they are bound to first identify the past constituents which have been retained and then proclaim that it was those (and only those) which contributed to the empirical success and which enjoyed evidential support. Can realists do better than that? Retention aside, can we independently identify the theoretical constituents that contribute to the successes of a given theory and show that it is only those that we deem truth-like?

In response to this objection, it should be pointed out that eminent scientists do the required identification all the time. It is not that realists come, as it were, from the future to identify the theoretical constituents of past theories that were responsible for their success. Scientists themselves tend to identify the constituents which they think were responsible for the success of their theories, and this is reflected in their attitude towards their own theories. This attitude is not an all-or-nothing affair. As we are about to see in some detail, scientists do not, normally, believe either that everything a successful theory says is truth-like or conversely that, despite its success, nothing it says is truth-like. Rather, the likes of Lavoisier, Laplace and Carnot – to mention just a few – had a differentiated attitude towards their theories (in this case the caloric theory), in that they believed in the truth-likeness of some theoretical claims while considering some others to have been too speculative, or too little supported by the evidence, to be accepted as truth-like. This differentiated attitude was guided by the manner in which the several constituents of the theory were employed in the derivation of predictions (e.g. Laplace's prediction of the correct law of the propagation of sound in air) and in well-founded explanations of phenomena (e.g. Carnot's explanation of the fact that maximum work is produced in a Carnot-cycle). So, theoretical claims which were not essential for the success of the theory were treated with suspicion, as for instance was the case with the assumption that heat is a material fluid; and those claims which 'fuelled' the successes of the theory were taken to enjoy evidential support and were believed to be truth-like, as for instance was the case with the claims that heat can remain in latent form, or that the propagation of sound in air is an adiabatic – rather than an isothermal – process.

My claim is that it is precisely those theoretical constituents which scientists themselves believed to contribute to the successes of their theories (and hence to be supported by the evidence) that tend to get retained in theory change. Whereas, the constituents that do not 'carry-over' tend to be those that scientists themselves considered too speculative and unsupported to be

taken seriously. If this view is right, then not only is the *divide et impera* move not ad hoc, but it actually gains independent plausibility from the way scientists treat their theories, and from the way they differentiate their commitments to their several constituent theoretical claims. If, therefore, there is a lesson which scientists should teach realists it is that an all-or-nothing realism is not worth fighting for.

In the next chapter, I try to substantiate these general philosophical points by means of two detailed case-studies. They concern the two controversial items on Laudan's list: the caloric theory of heat and the optical ether theories of the nineteenth century. Let me here just summarise the main points that these studies will raise and defend.

The study of the *caloric theory of heat* shows that the caloric representation of the cause of heat as a material fluid was not as central, unquestioned and supported as, for instance, Laudan (1984a: 113) has claimed. Caloric was not a putative entity to which the most eminent scientists had committed themselves as the real causal agent of heat phenomena. More importantly, the empirical success of the caloric theory was not essentially dependent on claims concerning the existence of an imponderable fluid which caused the rise (fall) of temperature by being absorbed (given away) by a body. The laws which scientists considered well supported by the available evidence and the background assumptions they used in their theoretical derivation were *independent* of the hypothesis that the cause of heat was a material substance: no relevant assumption was essentially used in the derivation–prediction of these laws. So, the laws which scientists considered to be well supported by the evidence and to generate the empirical success of the caloric theory did not support, nor did they require, the hypothesis that the cause of heat was a material substance. What this study suggests is that the parts of caloric theory which scientists believed in were well supported by the evidence and were retained in subsequent theories of heat, whereas the hypotheses that were abandoned were those which were ill-supported by the evidence. Hence, the point which the first case-study will highlight is this: when the laws established by a theory turn out to be independent of assumptions associated with allegedly central theoretical entities, it makes perfect sense to talk of the approximate truth of this theory, despite the recognition that not all of its theoretical terms refer.

The second case-study – which discusses the *dynamical optical ether theories* of the nineteenth century – aims to offer a different service to realism. It suggests that the most general theory – in terms of Lagrangian dynamics and the satisfaction of the principle of the conservation of energy – which was the backbone of the research programme around the dynamical behaviour of the carrier of light-waves has been retained in the subsequent framework of electromagnetism. This general theory was employed in the study of the *luminiferous ether* which was taken to be the dynamical structure which underlies light-propagation and which was such that it sustained the light-waves, and stored their energy (*vis viva*), during the time between

their leaving the source and until just before reaching the receiver. Given that the carrier of light-waves was a dynamical structure of unknown constitution, the application of Lagrangian dynamics to study its behaviour enabled the scientific community to investigate its most general properties (e.g. its general laws of motion) leaving out the details of its constitution. The investigation of the possible constitution of the carrier of light-waves was aided by the construction of models (e.g. Green's elastic-solid model of the ether), where this model construction was based on perceived analogies between the carrier of light-waves (e.g. its ability to sustain transversal waves) and other physical systems (e.g. elastic solids). It was mostly these models that were abandoned later on. This case-study will show that a reading of the nineteenth-century theories of optics which suggests that the content of these theories was exhausted by the elastic solid-like models confuses the model and the actual, yet concealed, dynamical system the behaviour of which scientists were trying to understand. The advocates of the pessimistic induction would simply make an illegitimate move, if they appealed to those past failed models which scientists took to be heuristic devices, in order to infer that any current or future physical theory is likely to be false.

One of the points that the second study raises relates to the status of the abandoned theoretical term 'luminiferous ether'. It is hard to deny that the postulation of a medium for the propagation of light – denoted by the term 'ether' – underwrote the development of optical theories during the nineteenth century. Yet, the term 'ether' has been seen as an exemplar of a non-referring scientific term. Does it, then, follow that the whole range of dynamical theories of optics in which ether had a central function cannot possibly be approximately true? Discussion of that issue is postponed until Chapter 12, where attention turns to theories of the reference of theoretical terms. There I motivate a causal–descriptive theory of reference and defend the view that it is plausible to think of 'luminiferous ether' as referring to the electromagnetic field.

6 Historical illustrations

THE CALORIC THEORY OF HEAT

Heat as an imponderable fluid or heat as motion?

The core problems of the theories of heat in the late eighteenth and the early nineteenth century were the following: the cause of the rise and fall in the temperature of bodies; the cause of the expansion of gases when heated; the change of state; and the cause of the release of heat in several chemical interactions, and especially in combustion. It was in this problem-nexus that scientists such as Joseph Black, Antoine Lavoisier and Pierre-Simon Laplace introduced the causal–explanatory model of caloric.

Caloric was taken to be a theoretical entity and 'caloric' was the theoretical term purporting to refer to a material substance, an indestructible fluid of fine particles, which causes the rise in temperature of a body which absorbs it (cf. Lavoisier 1790: 1–2). Heat was taken to be the observable effect of the transportation of caloric from a hot body to a cold one (ibid.: 5). Being a material substance, caloric was taken to be conserved in all thermal processes. In 1780s, Lavoisier used caloric as an important element in his anti-phlogiston system of chemistry (ibid.: Part I; also Lilley 1948). Moreover, the assumption that heat was conserved played an important role in the development and theoretical exploitation of experimental calorimetry (see Laplace and Lavoisier 1780: 156). In dealing with the change in the state of a substance (e.g. the vaporisation of water), where, although a large quantity of heat is needed, this change takes place at constant temperature, Black (1803) assumed that heat can exist in a latent form, too. Lavoisier had already suggested that caloric can exist in two forms: either free (*calorique sensible*) or combined. Combined caloric was thought to be 'fixed in bodies by affinity or electric attraction, so as to form part of the substance of the body, even part of its solidity' (1790: 19). So, the existence of latent heat was explained by means of caloric in combined form.

However, a dynamical conception of heat had been the rival of the caloric theory ever since the latter was put forward. According to the dynamical theory, the cause of heat was not a material fluid. Instead, it was the motion

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**For my parents Maria and Demetris,
without whom not,
and for Athena**