ism has already been successfully defended.]

Harré's essential and almost disrespectful insight is to have tried to formulate an inductive argument for scientific realism on the basis of empirical facts. My conclusion was that we do need these empirical facts, and we do need an inductive argument, but not the direct one Harré relied on, but the inverse one based on epistemic relevance of the Normal Thing and the Rare Event. As long as this style of arguing is used in every-day life, science and philosophy I do not feel too worried that I have not presented a defence of this style of arguing in this paper.

References

Harré, R. (1961). *Theories and Things*. London/New York: Sheed and Ward.

Harré, R. (1970). *The Principles of Scientific Thinking*. London: Macmillan.

Harré, R. (1985). 'Theories Families, Plausibility, and the Defense of Modest realism'. In: N. Rescher (ed.), *Reason and Rationality in Natural Science*. Lanham, MD: University of America Press, pp. 53-81.

Harré, R. (1986). *Varieties of Realism. A Rationale for the Natural Sciences*. Oxford: Blackwell.

Harré, R. (1988). 'Realism and Ontology'. *Philosophia Naturalis* 25: 386-398.

Harré, R. (1988b). 'Where Models and Analogies Really Count', *International Studies in the Philosophy of Science* 2: 118-133.

Harré, R. (1989). 'Realism, Reference and Theory'. In: A. Philips Griffiths (ed.), *Key Themes in Philosophy* (Royal Institute of Philosopy Lectures Series 24. Supplement to *Philosophy* 1988), Cambridge: Cambridge University Press, pp. 53-68.

Harré, R. (1990). 'Exploring the Human Umwelt'. In: Roy Bhaskar (ed.), *Harrré and his Critics*. Essays in honour of Rom Harré with his commentary on them. Oxford: Blackwell, pp. 297-262.

Harré, R., Aronson, H.L. and Way, E. (1993). *Realism Rescued*. Oxford: Blackwell

Truth, existence, and the best explanation

Peter Lipton

1 Introduction

When Oxford offered me a place to read for the B.Phil. in Philosophy some twenty years ago, the decision to accept seemed obvious. It was only some time after arrival that I realised what a chance I had taken. Unlike the other universities to which I had applied, instruction at Oxford is based almost exclusively on individual supervisions. The tutorial system has obvious advantages, but it is also risky. Unlucky students with supervisors who were uninterested, uncooperative, or just uninspiring faced fortnight-long stints of solitary work, punctuated by unremunerative tutorials. Many of these students taught themselves to be good philosophers, but they must have felt that the main philosophical lesson they were learning at Oxford was a lesson in intellectual solipsism.

I was lucky. My B.Phil. supervisors were Freddie Ayer and Rom Harré, not just talented philosophers, but gifted and inspiring teachers. Ayer had a reputation for ruthless criticism of his colleagues, but he treated his students differently. As a teacher, he had an uncanny knack for finding some sensible kernel hidden in the most ill-presented argument and for showing his students how it might be developed into something worthwhile. Harré's pedagogical gifts are more synthetic. He has a marvelous talent for leading his students up to the research front and then locating their work in the current philosophical scene. This enables them to see just what the issues are and how their own argument can make a contribution. Ayer and Harré have become paradigms for my own teaching.

My good fortune at Oxford consisted not just in having two wonderful supervisors, but in having these two in combination. Ayer, though he had already long abandoned the letter of logical positivism, retained a very strong allegiance to the spirit of empiricism he inherited from Hume and Russell. Science remained for him at base an instrument for the anticipation of experience. Harré provided the perfect foil with his robust realism,

according to which science can only be correctly understood as an enterprise devoted to illuminating a realm of largely unobservable entities and processes. And in spite of deep pull that Humean empiricism continues to exert on me, it was Harré's realism that eventually won my philosophical allegiance.

For all its attractions, however, scientific realism is no easy position to defend. And convinced realists, like devotee's to any philosophical position, often need to remind themselves that there can be bad arguments with true conclusions. I think one of the most popular arguments for realism, an argument from the predictive success of a theory to its truth by means of an over-arching inference to the best explanation, is one such. In this essay I will present a version of this truth inference, discuss some of the objections it faces, and then consider whether the situation improves if we follow Harré in moving from a realism based on the truth of scientific theories to one that focusses instead on the existence of entities those theories describe.

2 Inference to the best explanation

However much evidence scientists have, there will always be many theories incompatible with each other but each compatible with that evidence. The correct theory is, in one sense of the term, underdetermined by the evidence; but this is only to say that the inference from evidence to theory is irremediably inductive. But what principles guide a scientist when she infers one of these many hypotheses rather than any of the others? The account of Inference to the Best Explanation is, in the first instance, an attempt to give a partial answer to this descriptive question. The governing idea of the account is that scientists tend to prefer the hypothesis that would, if correct, provide the best explanation of the relevant evidence. Thus, given the observation that the characteristic spectrum of a star is shifted toward the red end of the spectrum, the scientist infers that the star is receding from us, since this recession, while obviously not the only hypothesis compatible with the evidence, provides the best explanation of the observed red-shift.

As it stands 'Inference to the Best Explanation' is a slogan rather than an articulated account; but I can only provide a very partial articulation here. (For more, see my 1991.) There are three important features of the

account that I will mention. All of them concern the notion of 'best' in 'best explanation'. Firstly, on pain of vacuity, 'best' cannot simply mean likeliest, since judgements of likeliness are just what the account is supposed to analyse. Of course one may wish to give a model of inference that does not analyse likelihood in terms of explanatory power, but then the governing idea of inference to the best explanation is no longer doing any work. To preserve that idea, 'best' must not mean likeliest, but rather something like most explanatory, or 'loveliest'. The point of Inference to the Best Explanation is not that scientists infer the likeliest explanation, a claim which is true but uninformative; it is rather that scientists infer that the explanation which would, if correct, provide the most insight or the greatest understanding is also the one that is likeliest to be correct. This ambitious version of the account posits a strong connection between explanatory and inferential virtues and, while it is certainly not obviously a correct description of scientific practice, at least it is not trivial.

The second important feature of 'best' in the account is that, in spite of the sense of uniqueness that the word suggests, Inference to the Best Explanation must not be taken to limit scientists' inferences to a single explanation of the evidence. The same evidence may warrant inferences to two different but compatible explanations. Thus the observation of a large population of a certain species of bird may provide evidence both for the absence of predators and for the presence of a plentiful food supply. So 'best explanation' should be understood as 'best of competing explanations'. Where the hypotheses are compatible, inferring one of them does not preclude inferring others. Finally, the slogan is again perhaps misleading in so far as it suggests that scientists will always make some inference so long as any hypothetical explanation is available. Yet there are many cases where, even if scientists have thought up several possible explanations of the evidence, none of them yet merits inference. So Inference to the Best Explanation must be understood to sanction inference only when the best explanation is sufficiently good. There is obviously much more that needs to be said to turn the slogan into a respectable philosophical model, but this is enough to enable us to turn to the main subject of this essay, which is a particular inference to the best explanation that is supposed to provide an argument for scientific realism.

3 Truth as the best explanation of predictive success

Taken as a principled description of an aspect of scientific practice, Inference to the Best Explanation is no argument for realism. To say that scientific inferences follow a particular pattern does not in itself tell us whether what is inferred tends to be true. And one does not need to be a realist to endorse a version of Inference to the Best Explanation, descriptively construed. Thus a constructive empiricist, who holds that the aim of science is not true theories, but only theories whose observable consequences are true, could in principle accept that scientists tend to infer that the best explanation is empirically adequate. (Bas Van Fraassen, the best-known constructive empiricist, does not even accept Inference to the Best Explanation as descriptively adequate, but this rejection is not an essential component of constructive empiricism. See his 1981.) Many realist have however held that there is a distinctively philosophical argument, which shares the form of scientific inferences to the best explanation, but which does make the case for realism. In its simplest form, the argument is that we are entitled to claim that the predictively successful theories that scientists have inferred are in fact true, since the truth of those theories would be the best explanation of their predictive success. We may infer truth from predictive success, because truth is the best explanation of predictive success.

We must thus distinguish between Inference to the Best Explanation as a general description of scientific practice and the 'truth explanation', a particular inference of that form made by certain realist philosophers. It is that particular inference that concerns me here. But how does the philosophical inference to the truth explanation differ from the first-order inferences that scientists make? It is supposed to share the same general form, an inference to the best explanation, but not the same content. If the content were the same, so that the truth explanation was just a summary of scientists' explanatory inferences, the argument would seem clearly to beg the question, which is whether there is any good reason to suppose that scientists' inferences tend to be true. Moreover, given the intimate relations between a statement P and the statement '"P" is true', it may be difficult to see how the realist's truth explanation could be different from those of the scientist. The difference between explaining the truth of the predictions in terms of the truth of a theory and explaining the predicted phenomena in terms of the theory seems merely verbal.

Fortunately, the explanations are different, though this is perhaps obscured by familiar models of explanation that take what is explained and what does the explaining to be linguistic objects. To see the difference, notice that while scientific explanations are typically causal, the truth explanation is not. It is 'logical': the truth of the theory entails the truth of its observed logical consequences, but it does not cause it. Thus, while the theory of plate tectonics gives causal explanations of various observed features of the continents, the truth of the theory does not cause the truth of the observations, any more than the correctness of a map of the London Underground causes the stations to fall in the order I observe during my journey.

4 The circularity objection

Since the truth explanation is distinct from scientists' first-order explanations, we may accept that the inferences are different too. The realist is not simply parroting scientific inferences, though she is mounting one of her own that shares their form. Unfortunately, this shared form is enough to present the realist with a serious difficulty. For by helping himself to any inference to the best explanation, however distinctive, is he not still begging the question against his anti-realist opponents? This inference to the truth of a theory on the grounds of its predictive success will only have force for someone who already accepts Inference to the Best Explanation. Moreover, although we have seen that one does not have to be a realist to accept some form of Inference to the Best Explanation, this particular inference only takes us to realism under a realist version of the account. The punch line of the truth inference has to be that the theory really is true, not the pallid claim that 'the theory is true' is itself only empirically adequate. In short, since only realists can accept the applicable version of Inference to the Best Explanation, the use of an argument of this form to argue for realism preaches only to the converted and so seems to be worthlessly and viciously circular.

The circularity of the truth inference as an argument for realism is strikingly similar to the classic circularity of the inductive justification of induction. There the argument is from the past to the future reliability of inductive inferences: we say that induction will work in the future because it has worked in the past. In both cases, the justificatory arguments are

distinct from the first-order inferences they are supposed to underwrite, and in neither case does the argument include the conclusion as a premise. The apparent circularity lies rather in the application of the rule of inference. In the case of the inductive justification of induction, we are using an inductive argument to justify induction; in the case of the truth inference we are using an inference to the best explanation in effect to justify Inference to the Best Explanation. Our realist takes it that scientists make inferences to the best explanation, and she is trying to show that these are reliable by means of an argument that is itself an inference to the best explanation.

The circularity objection to the truth inference is a severe blow to the realist. It means that the argument ought to have no force against her opponents. Does it also mean that it has no force for those of us who are already inclined to realism? It is natural enough to suppose that an argument that will only move those who already accept its conclusion ought not to move even them, but this is a mistake. We realists may, that is, be entitled to give weight to arguments for realism that would not move us if we were not already realists.

One way of seeing how this is possible is to consider a reliabilist perspective on the traditional problem of induction. Hume did not argue that induction is an unreliable form of inference, only that its reliability cannot be shown. Indeed he could not so argue, since the claim that induction will be unreliable is itself a prediction that could only be justified with an inductive argument. The reliabilist suggests that Hume has thus failed to show that induction cannot yield knowledge, since what knowledge requires is that the beliefs in question be reliably produced, not that the believer had an argument to establish this reliability. Consider those of our past predictions whose correctness we have subsequently observed. To say that they were reliably produced is to say that their correctness was not a fluke. Given the way the world worked, the methods that led us to those predictions were like instruments in good working order. The idea is roughly that, given the causal laws then in force, not only did those methods in fact produce correct results but, had the situation been different in certain ways, the same methods would still have produced correct though perhaps different predictions. According to the reliabilist conception of knowledge, the actual predictions we made were not just true but also known, if that condition was satisfied. Similarly, if that condition is satisfied in the future, then the predictions we make now also count as

knowledge.

The reliabilist de-intellectualises the concept of knowledge. What is required is true belief produced by a reliable method; but there is no requirement that the reliability itself be shown. In other words, no general justification of induction is required in order for our ordinary and particular inductive inferences to count as knowledge. Perhaps my belief that there is a table before me is based on an inductive inference, perhaps even an inference to the best explanation of the course of my experience. If the table belief also counts as knowledge, this is not because I have some fancy philosophical argument for the existence of the external world. It is rather because the table is there and my senses are reliable instruments for detecting its presence. According to the reliabilist, the same thing holds for our more sophisticated and articulated inductions. Thus Hume's argument that inductive knowledge is impossible can be defeated without providing a positive justification of induction. To show that inductive knowledge is impossible would require showing that it is impossible that our inductive methods are reliable. Since Hume does not and can not show the impossibility of reliability, he can not show the impossibility of knowledge.

To say that no general justification of induction is required is not however to say that none can be had. For we may also apply the reliabilist account of inductive knowledge to the special case of the inductive justification of induction. Although the argument from past to future reliability will not move an inductive sceptic, since it is an inductive argument, it may nonetheless confer reliability and knowledge on those who follow it (cf. Van Cleve 1984). For it may be that induction will in fact be reliable and, if it wasn't going to be reliable, we already would have noticed this in virtue of a prior breakdown of our inductive practices. In other words, it may be that our causal intercourse with the world is such that we are reliable detectors of the reliability of induction.

The reliabilist thus rejects the traditional picture in which there is something very special, and especially bad, about the inductive justification of induction. In this traditional picture, ordinary inductions about the world, unjustified though they may be, at least do not beg any questions. They obviously would not move anyone who did not already accept inferences of their form, but at least they meet the inductivists' own standards of argument. The inductive justification of inductive methods, by contrast, is traditionally held to be worse than unwarranted: it is viciously circular

and ought to have no force, even for those already accept the first-order arguments. For the reliabilist, however, there is no such sharp distinction between the two kinds of inductive argument. Neither will move a sceptic, but both are instruments for the acquisition of knowledge in the hands of those who are willing to deploy them, just in case they are reliable.

The reliabilist is not here endorsing the obviously untenable view that all circular arguments with true conclusions yield knowledge. An argument of the form 'P therefore P', for example, remains worthless on the reliabilist view, since the use of such an argument clearly could not increase the likelihood that one would notice that P was false, if it were. Inferring a statement from itself cannot be an effective method of gaining reliable belief, since an argument can only confer reliability on its conclusion if its premises are antecedently and reliably held. But the situation is different with the inductive justification of induction, since I may have a reliable belief that induction has worked in the past without already believing that induction is generally reliable. Moreover, while I can only infer from past to general reliability by means of an inductive argument, I need not justify the argument itself before I follow it. If I do follow it, and if induction is in fact reliable, then the argument can yield cognitive gain, a reliable belief I did not have before in the general reliability of induction.

What the reliabilist has done is to distinguish the question of whether an argument could convince a sceptic from the question of whether an argument is viciously circular. To say that an argument is viciously circular is to say that it is cognitively impotent, that it could not be used to gain knowledge or reliable belief in its conclusion, even if its conclusion is true. Conversely, an argument that can be so used is one where the premises can provide a reason or justification for the conclusion. The inductive justification of induction passes this test, even though it has no force against the sceptic. Of course to say that an argument is not viciously circular is not to say that it is sound. If induction is in fact unreliable, then its past successes provide no objective reason for trusting induction, since an unreliable form of inference cannot generate reliable belief. But those successes do provide reasons if induction is reliable, and the argument avoids vicious circularity in either case.

Of course we would like an argument for induction that would defeat the sceptic on his own terms, and the inductive justification of induction can not do this. Nor can the argument give us a reason for choosing our

inductive methods rather than some other means of prediction, since such an argument would have to be one that would have force for us prior to our commitment to induction. The request for an argument that would provide a basis for choosing an inductive policy is tantamount to a request for an argument that would move the sceptic, since an argument for choosing would have to be one that could move someone not already committed to induction. The reliabilist claims that the inductive justification may nevertheless enable those of us who already use induction to know that it is reliable. Someone who does not believe that induction is reliable obviously does not know that it is. Similarly, someone who simply guesses that it is reliable does not know. If, however, induction is in fact reliable, then someone who avails himself of the inductive justification of induction comes to know this. Since the argument may take us from ignorance to knowledge, it is not viciously circular.

The reliabilist can tell a very similar story about Inference to the Best Explanation and the truth inference. The first-order inferences to the best explanation that scientists make are inductive inferences that require no independent justification. They can be knowledge-conferring just in case they are in fact reliable forms of inference. And the higher-order inference to the truth of theories as the best explanation of their predictive success, while not required to underwrite scientific practice, is cogent as well if it is an argument of the same reliable form. No inference to the truth of the best explanation will move someone who abjures that form of inference, and the realist's opponents do abjure it; but the argument may still be deployed in good conscience by those of us who are already realists. The truth inference fails as an argument against the realist's opponents, but it may still provide realists with a reason for a belief they already hold, that many scientific theories are at least approximately true. At least the circularity objection does not rule this out.

Although the reliabilist analysis of knowledge provides us with a natural account of how the truth inference might have force for realists even though it begs their opponents' question, perhaps the central point can also be made independently of that analysis. Consider a situation where a scientist argues from the data to the truth of a particular hypothesis. If the hypothesis concerns unobservable entities or processes, the scientific argument will have no force for an anti-realist. Indeed one might say that the argument presupposes precisely what is at issue for the anti-realist. Nevertheless, this ordinary scientific argument is clearly not circular, unless

every inductive argument is. The argument may beg the anti-realist's question, but it does not beg the scientist's question, since he already accepts inferences to the best explanation. What I have suggested is that the truth inference is in a similar case. It too begs the anti-realist's question, but it does follow that it has no force for the realist. For her, it stands alongside other inferences to the best explanation, as an argument of a form to which she is already committed and which can, unlike viciously circular arguments, yield knowledge and justification if that form of inference is objectively reliable.

5 Not the best explanation?

Unfortunately, even if the realist can provide a partial answer to the circularity objection, the truth inference is not out of the woods. We have conceded that the inference fails as an argument against the sceptic, but perhaps it also fails on its own terms, though not on grounds of circularity. That is, maybe truth is just not the best explanation of predictive success. To give this internal objection some bite, we need to specify some alternative explanations. I will consider three.

The first is the *empiricist* explanation. According to its advocates, the best explanation of the predictive successes of a theory is that the theory is empirically adequate, that all its observable consequences are true. The empiricist explanation is not, however, a serious threat to the truth explanation. As I mentioned above, Inference to the Best Explanation does not require that we infer a unique explanation of the evidence, only that we choose the best of incompatible explanations. And the truth explanation is compatible with the empiricist explanation, since a theory may be both empirically adequate and true. Indeed since empirical adequacy follows from truth (though not conversely) anyone who infers that the theory is true will also infer that it is empirically adequate. This shows that the truth explanation is logically stronger than the empiricist explanation, but Inference to the Best Explanation does not restrict inference to the weaker of two equally good explanations when they are compatible. The empiricist could manufacture incompatibility by modifying his explanation to say that the theory is adequate and *not* true, but this hardly seems to improve the quality of empiricist explanation. Indeed it seems to worsen it, by adding an explantorily idle component (the falsity of the theory), so Infer-

ence to the Best Explanation would lead us to prefer the original empiricist explanation to this artificially contrary variant.

Another reply to the claim that the empiricist explanation of predictive success pre-empts the truth explanation is that the empiricist explanation just isn't very explanatory. To say that a theory is successful because it is empirically adequate sounds suspiciously like saying that a theory is successful because it is successful, an explanation even more feeble that the infamous claim that opium puts people because of its dormative power. In fairness, it must be said that success is not quite the same thing as empirical adequacy. To say that a theory is empirically adequate is to say that all its predictions are true, while success refers only to the much smaller class of predictions that have actually been checked. But this does not much improve matters. How does the correctness of unchecked predictions help to explain the correctness of those that have been checked? The realist's truth explanation looks profound by comparison.

A second would-be competitor to the truth explanation is the *selection* explanation (Van Fraassen 1981, pp. 39-40), according to which the best explanation of the predictive success of a theory is that the theory was selected for precisely that reason. On this evolutionary view, many competing theories are generated, but only the predictively successful survive, so it is hardly surprising that those that do survive are successful. Unlike the empiricist story, the selection account is genuinely explanatory. If you ask me why all the people in this room are red-haired, it is genuinely explanatory to be told that this is a meeting of the red-haired league, where hair colour is a criterion of membership. Similarly, if one asks why a certain group of theories has been predictively successful, then it is explanatory to be told that they were selected from a larger group on those grounds.

But is selection a better explanation of success than truth? If this seems a peculiar question, it may be because we have again failed to produce an account that is incompatible with the truth explanation. The realist is happy to accept that theories are retained because of their predictive successes, but this is precisely because the realist takes this success to be a symptom of truth. So the explanatory power of the selection account does not preclude the inference to the truth explanation. If this were all the realist could say in response to the selectionist's threat, however, it might not be enough. For although the two explanations are logically compatible, one may well feel that the selection explanation removes the epistemic

motivation for inferring the truth explanation. The selection explanation appears to eliminate the sense that, unless these theories were true, their success would by a 'miracle', just as the Darwinian explanation of the matching of organisms to their environments eliminates the sense of miracle that the argument from design exploited to make a case for a benevolent and cosmic designer. The existence of God is compatible with natural selection, but the natural selection explanation nevertheless undercuts our epistemic motivation for inferring His existence from the facts of natural design.

The realist, however, does not need to rest exclusively on the compatibility of the two explanations since, although the selection story is explanatory, it does not explain everything that the truth explanation explains. This is a consequence of a peculiar feature of the logic of selection explanations: they may explain why all members of a group have a certain feature without explaining why each one has that feature (cf. Nozick, 1974, p. 22). The club rules explain why everyone in the club has red hair, but it does not explain why Tim (who is a member) has red hair. He obviously does not have red hair because he is a member of the club. Similarly, while the selection story explains why the theories we now accept have been successful, it does not explain why a particular theory so selected is successful. A particular theory is not successful because it was selected; quite the reverse. By contrast, the truth of that theory would explain its success.

The realist thus has little to fear from either the empiricist or the selection explanations. Both are compatible with the truth explanation, so they do not block the inference to it; moreover, truth is a better explanation of predictive success than is empirical adequacy and explains things unaccounted for by the selection story. There is however a third challenge to the truth explanation that cannot be so easily met. It is the existence of the *underdetermined* truth explanations: the explanation of the predictive success of one theory by the truth of other, incompatible theories. A theory may entail its predictions, but not conversely. Hence the familiar point that there are always indefinitely many other theories, incompatible with the first, which would yet share those predictive successes. Suppose now we have a theory whose truth the realist claims to be the best explanation of its predictive success. Although nobody may have formulated them, there are other, incompatible theories that would share that success by sharing those entailments. And since the explanatory power of the truth explana-

tion comes solely from the fact that a true statement must have true deductive consequences, the truth of these competing theories would provide equally good explanations of the predictive success they share with the original theory. So the realist is stuck in the stalemate of underdetermination and cannot make an inference to the best explanation.

It is surprising that the use of an inference to the best explanation to argue for realism should be blocked by a problem of underdetermination since, as we saw at the beginning of this essay, one of the main attractions of Inference to the Best Explanation is that it seems to provide an answer to that problem. Many theories may entail the data, but they would not explain it equally well, so explanatory considerations appear to give us the additional resolution required to distinguish between competing theories that fit those data. Why does this not then give the realist an answer to the objection from underdetermined explanations? Here the realist has been hoist on her own petard. By insisting that her argument for the truth of theory as the best explanation of its predictive success is distinct from the first-order inferences scientists make from evidence to theory, she has lost the power of discrimination that Inference to the Best Explanation provides in that ordinary context. Not every theory that entails that some phenomenon occurs provides an equally good explanation of that phenomenon, but the *truth* of any of those theories would provide an equally good (or bad) explanation of *truth* of a statement describing the phenomenon. As we have seen, the explanatory work in these cases is done entirely by the deductive relation, and this relation is the same in each case.

To see the force of this unfortunate result, it is useful to return briefly to the reliabilist conception of inductive justification. If scientists rely on inferences to the best explanation, and this form of inference is reliable in fact, then the scientists' data do provide them with good reasons to believe their theories, quite independently of any over-arching argument for realism. Consequently, there is a sense in which the truth inference may also provide an objective reason for realism, insofar as it will be reliable just in case the scientists' inferences are. What I have argued, however, is that the truth inference would nonetheless violate one of the central strictures of Inference to the Best Explanation, which is that one ought not infer an explanation when one knows that there are incompatible accounts that would be just as explanatory. Realists are free to follow the scientists' inferences to the best explanation, but they cannot without incoherence use Inference to the Best Explanation to sanction a distinct truth inference,

since the truth of the theories scientists favour is not the best explanation of their predictive success.

My general conclusion is thus that the prospects are dim for arguing from predictive success to truth of theory by means of an inference to the best explanation, where the truth explanation is taken to be distinct from the ordinary scientific explanations that theories provide. Although the inference to the truth explanation is not pre-empted by either the empiricist or selection explanations, it begs the question for the realists' opponents and also fails on its own terms, since the truth of a theory is no better an explanation of its predictive successes than would be the truth of any other incompatible theory that would enjoy the same successes.

6 From truth to existential adequacy

The hapless truth inference is linked to a form of realism that focusses on the truth-values of the statements that make up a scientific theory. But Harré (e.g. in his 1986) and others have argued for scientific realism of a different stripe, one that focusses on the existence of entities rather than the truth of statements. For these entity realists, the central claim of realism is not truth but 'existential adequacy'. What counts is that we can have good reason to believe in the existence of entities, such as viruses and electrons, cited by scientific theories, even in cases where those entities are unobservable. Given the difficulties the statement realist has in defending her position by appeal to the predictive success of science, I want to consider now whether the entity realist is in any better case.

The comparison is not straightforward, in part because the difference between the two realisms is unclear. To say that a particular kind of entity exists is, after all, to make a statement, and one that is true just in case the entity does indeed exist. So the entity realist can not have a general aversion to the truth of statements. What seems to matter is rather that he defends only the existential statements of the theory, leaving its other commitments to one side. He is, in this sense, a restricted statement realist. Now on one conception of the way the terms of a theory acquire their meaning, this is not a coherent position. According to this conception, the meaning and reference of an entity-term is fixed by the entire theory in which it appears, so that the term refers only if all the statements of the theory are true. On this view, commitment to the truth of existential

claims would thus carry commitment to the truth of the entire theory. In what follows, however, I will assume that this implausibly restrictive semantic conception is false. Thus I will assume that the terms of a false theory may refer and that a theory may be existentially adequate even if some of its central claims are false.

Viewed as a restricted statement realist, the entity realist may claim an advantage over the full statement realist analogous to that claimed by Van Fraassen's constructive empiricist (cf. Van Fraassen 1981). The constructive empiricist holds that we should never believe that a theory is true, only that it is empirically adequate, that its observable consequences are true. Similarly, since truth entails empirical adequacy but not conversely, the constructive empiricist claims that his position is safer and so more defensible than statement realism. Since truth entails existential adequacy but not conversely, the entity realist can claim a parallel epistemic advantage of modesty over the statement realist. Perhaps the root problem of statement realism is excessive epistemic ambition.

The advantage of modesty is that it reduces underdetermination. So far as belief is concerned, the entity realist is not forced to choose between theories which, though incompatible with each other, appeal to the same entities. Similarly, the constructive empiricist is not forced to choose between theories that are empirically equivalent. This sort of epistemic advantage is difficult to assess, however, since neither the entity realist nor the constructive empiricist comes close to eliminating all of the underdetermination of salient belief by available evidence. However much evidence the scientist has, it will be compatible with many different ontologies and with many different theories that are not empirically equivalent to each other. (Recall that the notions of empirical equivalence and empirical adequacy refer to the full class of statements about observable states of affairs, not to the small subset of that class that corresponds to actual observations.) Thus, while it is undeniable that the less you believe the less likely you are to be mistaken, neither the entity realist nor the constructive empiricist avoids the challenge of underdetermination.

Moreover, if we are to be modest, how can we decide whether our retreat from full statement realism should take Harré's path or Van Fraassen's? Both are more modest that the statement realist, but is one of them more modest than the other? It depends on which of two forms entity realism takes. If the entity realist believes the empirical claims of the theory along with the existentials, then he believe everything believed

by the constructive empiricist and more, so the constructive empiricist will try to pull him down by the same general argument from modesty that they both used against the statement realist. This is not to say that the empiricist wins, however, since he too is subject to pull from someone even more modest, who is only willing to believe in those empirical consequences of the theory that are actually drawn or even, at the limit, only in those that correspond to what has actually been observed. It is thus unclear whether the retreat from statement realism passes a stable stopping point short of complete inductive scepticism and, if it does, whether it is at entity realism.

The other form of entity realism refuses full commitment to empirical adequacy. In this case, the comparison with constructive empiricism is harder to draw, since neither contains the other. Proponents of these two positions each believe claims not believed by the other, the constructive empiricist about some observable but unobserved consequences of the theory concerning distant times or places, the entity realist about the existence of unobservable entities. How are we to say which position is more attractive? The entity-realist may say on his behalf that he can avoid the excessive epistemic weight the empiricist places on the distinction between the observable and the unobservable; but the empiricist may reply by questioning the epistemic significance of the distinction between a theory's existential commitments and its other claims.

Let us now consider the place of Inference to the Best Explanation in these two schemes. As a description of first-order scientific inferences, it is available to both the constructive empiricist and the entity realist. Of course they will not say that scientists infer that the best explanation is true, but the empiricist may say that they infer that the best theory is empirically adequate, and the entity realist that it is existentially adequate. What now about that special higher-order inference to the best explanation of predictive success that the statement realist hoped would ground her realism? As we have seen, that argument came to grief over the problems of circularity and of underdetermined rivals. How will our two more modest positions fare here?

Van Fraassen opts for the selectionist explanation of predictive success discussed in the last section. Our current theories are successful because they were selected on that basis. The advantage of this position, from our present perspective, is that it appears to avoid both of the problems of the truth inference. None of the constructive empiricist's opponents are likely

---

to deny that something like this kind of selection is going on or that the selection provides a explanation of sorts for predictive success. But this position has two drawbacks. The first is that, as we have seen, the selection mechanism leaves an aspect of predictive success unexplained. It explains why all the theories we now endorse have been predictively successful, but it does not explain each of those theories, selected for their success, enjoyed that success.

Secondly, the selection explanation provides no argument for constructive empiricism (nor does Van Fraassen claim otherwise). If anything, it seems to argue against the claim that the best theories of the day are empirically adequate. Consider the following analogy. We are given the past predictions of an astrologer, call him 'Clarence', and are struck by their number and accuracy. Perhaps there is something to astrology after all! Later, however, we discover told that Clarence was selected on the basis of his success from among hundreds of others astrologers, all the rest of whom made predictions about the same string of events but were wrong. Once we realise that Clarence's successes are only the tip of a disappointing iceberg, our confidence in his next prognostication tumbles (Cf. Dennett 1984, p. 93, on the 'touting pyramid'). Similarly, once the scientific selection mechanism is brought to our attention, we may lose confidence in the inference from past predictive success to empirical adequacy, the success of all future and indeed all possible predictions. We treat Clarence's success as an artifact of the selection mechanism, rather than as a sign of genuine clairvoyance, and the selection mechanism in the scientific case may push us in a similar direction. Van Fraassen and both sorts of realist will all wish to resist the parallel, since it leads to a position too sceptical for any of them, but the point remains that the selection explanation itself provides no positive argument for constructive empiricism.

How will the entity realist explain the predictive success of science? He too can avail himself of the selection explanation but, for the reasons we have just canvassed, this won't advance his position. Like the statement realist, he will want to explain the success of a particular theory by appeal to some feature of the world, not in this case that the world is as the theory says in all respects, but that the world contains the entities the theory cites. Does the entity realist avoid either the circularity or the underdetermination problems that foiled the statement realist's attempt to defend his position with an inference to the best explanation? The circular-

ity problem remains. To infer that a theory is existentially adequate on the grounds that it is the best explanation of its predictive success would be to assume a strong form of Inference to the Best Explanation that the constructive empiricist, for one, would not accept. The assumption is that we are entitled to infer that the best explanation is existentially adequate, but this is just what the entity realist was supposed to show. Does the entity realist avoid the problem of underdetermined rival explanations? This question is harder to answer, because it is unclear what sort of explanation his entities are supposed to provide. As we saw, the statement realist bases her explanation on the deductive relation: if a theory is true, then the predictions it entails must be true as well. But the existence of the entities cited by a theory obviously does not entail predictive success, since a theory may speak of real things yet tell an observably false story about them.

The existential adequacy of a theory will not provide a deductive explanation of predictive success, yet it does seem explanatory. The existence of electrons would provide at least a partial explanation for the predictive success of our electronic theory. Perhaps we could see the explanation as probabilistic. Although the existence of electrons hardly guarantees the success of our theory, one might claim that the success of that theory is more likely if there are electrons than if there aren't. Does this help with the underdetermination problem? In a sense, it does. Consider the many different and incompatible theories of the electron, such that all of them share both the predictive success and the existential commitments of our electronic theory. This degree of underdetermination is enough to foil the statement realist's attempt to show that the truth of our electronic theory is the best explanation of its success. It is, however, no problem for the entity realist, since the very same thing -- the existence of electrons -- would (partially) explain the success of any of these theories, incompatible though they are. That is the good news. The bad news is that plenty of underdetermination remains, since there are also many theories with different ontologies that would be just as successful. Insofar as the existence of electrons explains the success of our electronic theory, the existence of 'shmelectrons' would explain the ability of a shmelectronic theory to entail exactly the same predictive successes. So there is a sense in which the entity realist has less of an underdetermination problem, but it is still bad enough to be fatal to this application of Inference to the Best Explanation. For on that model, the existence of one equally good but competing

explanation is enough to block inference.

7 Carving beliefs at the epistemic joints

The strategy of an over-arching inference to the best explanation of predictive success thus appears of little use to anyone. It does not provide a cogent argument for statement realism, entity realism, or indeed for constructive empiricism. So far as that strategy goes, there is nothing to choose between the three positions: it fails for all of them. That negative result is the main message of this essay but, by way of conclusion, I would like to indicate why I continue to prefer a modest statement realism to either of the other two positions. In my view, constructive empiricism and entity realism both attempt to draw a fundamental epistemic distinction where none exists. The empiricist says that we are entitled to believe claims about observable but not about unobservables; but the distinction between the observable and the unobservable will not bear this epistemic weight. It is not even plausible to suppose that all claims about observables are better warranted than any claims about unobservables. There is a natural epistemic distinction between the observed and the unobserved, but this is quite different from the constructive empiricist's distinction and, in any case, we could hardly make sense of science if we allowed belief only in the observed. Another plausible place to draw an epistemic line would be between the detectable and the undetectable, where observation is only one form of detection, but this too will cross-cut the empiricist's distinction. Unaided human senses are remarkably useful detection instruments, but they are neither the only ones nor the most reliable.

The entity realist says instead that the line is to be drawn between existential claims and the others, but this too seems an unnatural distinction. We have been given no reason to believe that all existential claims are better warranted than any claims of other forms. Perhaps the claim that electrons exist is better warranted than any other claims about electrons, since all those other claims entail the existential claim but not conversely, but this does not make the point. To do so would require showing that the claim that electrons exist is also better warranted than any non-existential claim *not* about electrons, and it is difficult to see how this could be done.

As we have seen, one of the main advantages that the empiricist and the entity realist can claim over the statement realist is the advantage of modesty: by believing less, they run less risk of believing what is false. But gerrymandered modesty is false modesty. There are also several other points that the statement realist should make in response to the argument from modesty. We should not have a Cartesian obsession with error. Error is an epistemic flaw, but so is ignorance. A sensible epistemic policy must balance security and scope, so modesty is not always an epistemic virtue. Nor need statement realism be less modest than the other views. A theory entails both its existential and its observable claims and not conversely, but this does not settle the issue, because only a rabid statement realist would, when she is being careful, claim that our present theories are strictly and certainly true. Any sensible version of statement realism must emphasise the fallibility of judgement and must also, in my view, appeal to the notion of verisimilitude or approximate truth. Moreover, the sensible statement realist will insist that theories are not monolithic structures and that scientists generally have better reason to believe some parts of them than others. Scientists will, however, distinguish those parts locally, by appeal to the particular claims of each theory and to the particular evidence available, not by appeal to global philosophical distinctions between what is observable and what is not, or between existential claims and the rest. A sensible statement realist thus has plenty of room for selective modesty, without the gerrymandering.

I thus continue to prefer statement realism over either entity realism or constructive empiricism. At the same time, it seems to me that entity realism has an important therapeutic role to play, because it weans us from a misleading conception of the way theories are supported by evidence. This misconception is not entailed by statement realism, but it is one that statement realism may unfortunately encourage. The misconception I have in mind is the logical conception of confirmation. When theories are represented as sets of statements, it can seem extremely natural to represent confirmation as a logical relation. The underlying intuition is that we test a theory by a process of semantic sampling, where the samples are logical consequences of the theory. On this view, the claims of a theory are something like a huge linguistic iceberg, the unobservable bulk of which lies below the observable surface. The best we can do, then, is to derive consequences that correspond to the exposed tip and, if those consequences turn out to be correct, infer that the hidden bulk is correct as well. This

picture leads, of course, to the Hypothetico-Deductive model of confirmation.

The difficulties of hypothetico-deductivism are familiar. The model is at once too permissive and too restrictive. Too restrictive, because hypotheses are often supported by data they do not entail. In an attempt to bring these data under the hypothetico-deductive fold, the model is modified to include as confirmatory not only statements entailed by the hypothesis outright, but also those entailed by the hypothesis along with various auxiliary statements. This, however, pushes the model to over-permissiveness, because it will now treat irrelevant data as confirmatory. Indeed any observation O will count as confirming any hypothesis H, if we allow the material conditional H⊃O as an auxiliary. To block this absurdity, the model must specify which auxiliaries are admissible, but this is no easy thing to do. A restriction to true auxiliaries, for instance, would not avoid the problem, since H⊃O is true if the observation statement is. Nor would solving this problem of admissible auxiliaries solve the general problem of over-permissiveness, since the model runs the risk of counting every observation as evidence for any hypothesis even if we use no auxiliaries whatsoever. One reason is that a hypothesis entails the disjunction of itself and any observation statement whatever, and the disjunction is determined to be true by verifying the observation statement.

Difficulties such as these certainly show that a simple version of the Hypothetico-Deductive model is unacceptable. They suggest first of all that the model is not really capturing the sampling intuition, the idea that we support a hypothesis by verifying a part of its content. Some of the difficulties also suggest that the intuition itself is faulty, that it does not provide a good picture of how hypotheses are actually evaluated. For a better picture, we need to engage in 'semantic descent', to focus not on the statement but on the entities and processes that the statement is about. This is a view that Harré has championed for many years. We should, for example, focus on causal rather than on logical relations. This shift of perspective brings out the importance of intervention, manipulation, controlled experiment and other material aspects of experimental practice that the logical conceptions tend either to ignore or misrepresent. This is also one reason why Inference to the Best Explanation is a promising account of first-order scientific inference, even if it does not provide the pattern for a cogent over-arching defence of realism, since so many explanations are causal. Of course, if this approach is to work, we must not use a

Deductive-Nomological conception of explanation, which would simply return us disappointingly to some version of the Hypothetico-Deductive account of confirmation. Indeed most of the difficulties facing the Deductive-Nomological model of explanation are isomorphic to the sort of difficulties mentioned above facing the Hypothetico-Deductive model of confirmation. This should come as no surprise, since the Deductive-Nomological model makes the same mistake of semantic ascent. To understand explanation, we must also focus on features of the world, especially causal features, not just logical features of statements.

Statement realism may perhaps encourage purely logical models of confirmation and explanation, models that Harré and I agree are misguided. It may also have encouraged the misbegotten truth inference that I have criticised in this essay since, as we saw, that argument hinges on the idea of a logical rather than a causal explanation of predictive success. Conversely, entity realism leads us naturally to focus on physical rather than logical relations and so on a better level at which to analyse explanation and inference. In my view, however, statement realism should not be rejected because it has lead some of its advocates to mount bad arguments on its behalf or because it has seduced them into giving unilluminating accounts of confirmation and explanation. No tool, however well-designed, is proof against misapplication. The view that science aims at a true description of a largely unobservable world and that its history displays fallible but marked progress towards this goal remains compelling and more defensible than the epistemic gerrymandering than the alternative views I have discussed in this essay require. In short, my view is that we ought to hold on to statement realism, but only after we have learned the valuable lessons that the entity realist has to teach us.

References

Van Cleve, J. (1984). 'Reliability, Justification, and the Problem of Induction'. In P.A. French, T.E. Uehling Jr., H.K. Wettstein (eds.), *Midwest Studies in Philosophy IX*. Minneapolis: University of Minnesota Press.

Dennett, D. (1984). *Elbow Room*. Cambridge, MA: The MIT Press.

Van Fraassen, B. (1981). *The Scientific Image*. Oxford: Oxford University Press.

Harré, R. (1986). *Varieties of Realism*. Oxford: Blackwell.

Lipton, P. (1991). *Inference to the Best Explanation*. London: Routledge.

Nozick, R. *Anarchy, State and Utopia*. New York: Basic Books.