



ESSAY REVIEW

Naturalism Without Truth?

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Larry Laudan *Beyond Positivism and Relativism: Theory, Method and Evidence* (Boulder, CO: Westview Press, 1996), ix+277 pp., ISBN 0-8133-2469-6, Hardback \$59.95; Paperback \$22.95.

1. Introduction

Larry Laudan is undoubtedly one of the most influential philosophers of science in the second half of the century. His new book is, as expected, a gold mine of good arguments, philosophical insight and detailed discussion of a number of important issues, from cumulativism to the problem of demarcation. Not only is it full of high-quality philosophy. It is also an exemplar of how to *do* good philosophy, how to write and explain clearly, how to motivate philosophical thinking. Its main aim is to shape up the prospects of methodology in an age where the rationality of science is deeply disputed, an age where relativism, subjectivism and scepticism seem to gain the upper hand. *Beyond Positivism and Relativism* is an edited collection of Laudan's papers that span a period of two decades. Its main pillars are, I think, two: first, a sustained attempt to undercut the sceptical force of the argument from the underdetermination of theories by evidence (henceforth UTE). Second, an account of how methodology should be conducted in the age of naturalism. Laudan seeks to show that methodological naturalism can underpin the rationality of scientific judgement and preserve the normative component of traditional epistemology of science.

In what follows, I shall focus on the two pillars and shall argue that: (a) Laudan's argument against UTE can warrant even more epistemic optimism than he himself suggests; and (b) his *Normative Naturalism* fails to be normative unless it gets connected with a truth-linked axiology.

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2. Underdetermination Undermined

Scientific realists suggest that theory-acceptance should be identified with the belief that the theory is approximately true. Non-realists, however, counter that the argument from the underdetermination of theories by evidence undermines the realists' optimism. Two theories that are, the argument goes, observationally congruent (they entail exactly the same observational consequences), are epistemically indistinguishable too (they are equally supported by the evidence). Hence, there is no positive reason to believe in one rather than the other. This is what Laudan very aptly calls '*the egalitarian thesis*' (p. 33).¹ Since, moreover, for any theory that entails certain observational consequences one can construct incompatible but empirically indistinguishable rivals, no theory can be reasonably believed to be (approximately) true.

This argument lends credence to scientific agnosticism, the view that belief in the approximate truth of a theory is never warranted by the evidence. Van Fraassen, for instance, suggests that acceptance of a theory involves belief in its empirical adequacy but not in its approximate truth. To be sure, he acknowledges that acceptance involves more than belief in empirical adequacy. It takes into account 'virtues' such as parsimony, explanatory power etc. But these are *pragmatic* virtues: they have nothing to do with the truth of the theory, nor can they be reasons to believe a theory.²

UTE rests on two premises.

(a) The *Empirical Equivalence Thesis* (EET): for any theory *T* and any body of observational evidence *E*, there is another theory *T'* such that *T* and *T'* are empirically equivalent with respect to *E*.

(b) The *Entailment Thesis* (ET): the entailment of the evidence is the only epistemic constraint on the confirmation of a theory.

So, if UTE is to be blocked, at least one of the two premises must be defeated.

Laudan correctly points out that 'we know of no algorithm for generating genuine theoretical competitors to a given theory' (p. 61).³ Yet, there is a general feeling that the so-called Duhem–Quine thesis offers a constructive proof of EET. Put briefly, the Duhem–Quine thesis starts with the undeniable fact that all theories entail observational consequences only with the help of auxiliary assumptions, and asserts that it is always possible that a theory together with suitable auxiliaries can accommodate any recalcitrant evidence. A corollary, then, is that for any evidence and any two rival theories *T* and *T'*, there are suitable auxiliaries such that *T' & Suitable Auxiliary Assumptions* will be empirically equivalent to *T* together with its own auxiliaries *A*.

As the standard rejoinder goes, it's not certain that *non-trivial* auxiliary assumptions can always be found. What Laudan observes, however, is that

¹All page-references to Laudan's book will be given in parentheses in the text.

²Van Fraassen, *The Scientific Image* (Oxford: Clarendon Press, 1980), see pp. 87–88.

³Chapter three of the book, pp. 55–73, is co-authored by Jarrett Leplin.

one can do better than that. One can turn the Duhemian argument on its head: it is precisely the fact that the so-called observational consequences of a theory can only be determined with the aid of auxiliaries which shows that diachronic empirical equivalence cannot be guaranteed (pp. 57–59). Suppose that two rival theories T and T' share the same class of empirical consequences at time t . Given that all theories entail observational consequences with the help of auxiliaries, there is no guarantee that this class will increase monotonically, nor that it will remain the same for both theories in future times. As the two rival theories get conjoined with other—hitherto unavailable—auxiliaries, new empirical consequences may arise that are able to discriminate between the two theories and break the observational tie.

Laudan's move enjoys historical support as, for instance, can be evinced from the case of the once-upon-a-time observational tie between the particle- and wave-theory of light. But in all its generality, it is as conjectural as its opponent: the Duhem-Quine thesis. In fact, advocates of this thesis are bound to argue that Laudan's move doesn't discredit their claim. They will say that just as there may be auxiliaries that will help disentangle observational ties, there may be others that will ensure the re-appearance of a tie. Of course, this is just a mere re-iteration of the initial Duhem-Quine assertion. But it does suggest that Laudan's argument can work only in tandem with the standard claim, viz., that the advocates of the Duhem-Quine thesis have not proved the existence of non-trivial auxiliaries which can accommodate any rival evidence. Who has got the burden of proof here I do not really know. So if we do not want to engage in a mere exercise of philosophical doubt, we should look more carefully at real cases of empirical congruence and examine the prospects of their resolution.

Leaving aside the Duhem-Quine Thesis, the force of the first premise is *local* rather than *global*. Suppose that there are *some* theories that share the same class of observational consequences. This fact would create no serious problem for the rationality of scientific inquiry. All that would follow would be that *some* domains of inquiry are beyond our ken. More generally, the existence of empirically equivalent theories can create a genuine problem only if it is shown to be a global phenomenon. This is what UTE should demonstrate if it is to ditch any hope of discovering the blueprint of the universe. But there is no relevant evidence for this.

Is the second premise, ET, sound, anyway? Laudan suggests that when it comes to testing scientific theories, observing their consequences is neither necessary nor sufficient for empirical support. Not all logical consequences of a hypothesis are potentially supporting, and conversely a hypothesis can be supported by evidence that is not among its logical consequences. *Hypotheses are not necessarily confirmed by their empirical consequences*. Here, the point has been brought home by the literature on the infamous Ravens Paradox. Bayesian solutions have stressed that one can consistently deny that positive

instances of a hypothesis necessarily confirm the hypothesis.⁴ Laudan's own examples are even better (p. 68). Quick recovery from a common cold after the patient has prayed for 3 days is a positive instance of the hypothesis that praying for 3 days makes the cold go away. Yet, we would not be willing to say that it confirms the hypothesis at hand, since the evidence offers *no* support to the hypothesis: the evidence would be what it is, even if the hypothesis was false (as in fact it is); the evidence can be easily explained without any loss by a readily available alternative hypothesis, etc. Conversely, *hypotheses can be confirmed by empirical evidence that does not logically follow from them*. A typical example here is that Einstein's account of the Brownian motion was widely taken to confirm the atomic theory although it was not among its consequences. More generally, suppose that a piece of evidence *E* is entailed by a hypothesis *H* which in turn can be embedded in a more general theory *T*. Suppose also that *T* entails another hypothesis *H'*. *E* can be said to *indirectly* support *H'* although it is not a logical consequence of *H'*. In sum, two theories with exactly the same observational consequences may enjoy differing degrees of evidential support: either because only one of them is indirectly supported by other relevant evidence, or because one of them is not really supported by its positive instances. So, ET is defeated.

Incidentally, it might appear that Laudan is committed to accepting both Hempel's Converse Consequence Condition (that if *e* confirms *H*, and *T* entails *H* then *e* confirms *T*) and the Special Consequence Condition (that if *e* confirms *T*, and *T* entails *H'* then *e* confirms *H'* too). But don't these two conditions take together imply the notorious absurdity that any piece of evidence confirms any hypothesis whatever? It should be clear however that Laudan can easily avoid the foregoing absurdity. In line with his central point that entailment of the evidence is not sufficient for confirmation, he can deny that CCC is sufficient to bestow confirmation on the 'larger' theory *T*: there are cases in which *e* confirms *H*, *T* entails *H*, but *T* is not thereby confirmed. Such are, plausibly, the cases that give rise to the foregoing absurdity. Recall that the absurdity is generated as follows. Take any piece of evidence *e* and any hypothesis *h*. (1) *e* entails *e*, therefore *e* confirms *e* (by Hempel's Entailment Condition). (2) *e* confirms *e*, *h*&*e* entails *e*, therefore *e* confirms *h*&*e* (by CCC). (3) *e* confirms *h*&*e*, *h*&*e* entails *h*, therefore *e* confirms *h* (by SCC). But Laudan can easily deny that in premise (2) *e* confirms the 'larger' theory *h*&*e*, arguing that although *h*&*e* entails *e*, *e* does not *support* *h*&*e*. To be sure, as he himself points out (private communication), Laudan needs to specify precisely when entailed evidence is confirmatory and when it is not. More generally, since he has dissociated

⁴Cf. C. Howson and D. Urbach, *Scientific Reasoning: A Bayesian Approach* (La Salle: Open Court, 1989), pp. 89–91.

himself from the view that relations of evidential support mirror logico-semantic relations between hypotheses and evidence-statements, he needs to offer a rival account of evidential support. But even without such a fully-developed account—an account which Laudan would base on his view that all judgements of evidential support are comparative and hence they should involve the evidence and at least two competing theories—it is plausible to argue that the evidence does not lend support to hypotheses that are constructed so that they entail the evidence.

As is well known, scientific realists suggest that when it comes to assessing the support that scientific theories enjoy, we shouldn't just examine their empirical adequacy. This may be necessary but not enough on its own to make a theory well-supported. We also need to take into account several *theoretical virtues* such as coherence with other established theories, consilience, completeness, unifying power, lack of *ad hoc* features, capacity to generate novel predictions. These virtues, they argue, capture the *explanatory power* of a theory and explanatory power is potentially confirmatory. So, even if two theories are observationally congruent, they may not have equal explanatory power, and hence, they may not enjoy the same support. If these extra virtues are taken into account, it won't be easy at all to find more than one theory that satisfies them to an equal degree.

Non-realists counter this last move by denying that explanatory power has anything to do with confirmation and truth: theoretical virtues are pragmatic, rather than epistemic. Realists typically argue that these theoretical virtues have epistemic force because they are part and parcel of rational scientific judgement. McMullin, for instance, suggests that explanation as well as predictive accuracy are the *constitutive aims of science*: hence, it is only rational to choose the theory with the most explanatory power. He adds that the theoretical virtues are those that scientists use to characterise a 'good theory' and those that have been traditionally 'thought to be symptoms of truth generally'. As he put it: 'The values that constitute a theory as "best explanation" are, it would seem, similar to those that would qualify a statement as "true"'.⁵ In an analogous fashion, Boyd suggests that all these virtues guide the scientists' judgements of theoretical plausibility of competing theories and, as such, they are evidential (if only, indirectly) in that they provide good reasons to believe that theories which satisfy them are approximately true.⁶

I think scientific realists should take some comfort from the fact that Laudan's favourite rebuttal of the Entailment Thesis is very close to their claim

⁵E. McMullin, 'Explanatory Success and the Truth of Theory', in N. Rescher, *Scientific Inquiry in Philosophical Perspective* (Lanham: University Press of America, 1987), pp. 66–67.

⁶R. Boyd, 'Scientific Realism and Naturalistic Epistemology', in P. D. Asquith and T. Nickles, *PSA 1980*, Vol.2 (East Lansing, MI: Philosophy of Science Association, 1981), see p. 622.

that theoretical virtues bear on confirmation.⁷ Arguing that theories can gain support from evidence they do not entail is, in effect, tantamount to saying that potential unifying power—a central theoretical virtue—can enhance the confirmation of a hypothesis. Here are two typical cases where a hypothesis can arguably gain indirect support from evidence it doesn't directly entail. (a) A hypothesis H is embedded in a broader theory T that enjoys strong evidential support. For instance, Lorentz's theory of electron gained support from being embedded into Maxwell's theory, although the latter did not entail the former. (b) A hypothesis H acts as a 'bridge' that connects, but does not entail, other apparently unrelated hypotheses H_1 and H_2 . For instance, the atomic hypothesis acts as such a 'bridge' *vis-à-vis* the kinetic theory of gases and the molecular theory of the chemical elements and gains support from both. Both cases are instances of confirmation-via-unification. Conversely, arguing that entailment of the evidence is not sufficient for empirical support is, in effect, another way of saying that we shouldn't accept a hypothesis merely on the basis that it entails the evidence, if the hypothesis is the product of an *ad hoc* manoeuvre, or if we have available another hypothesis that offers a better explanation of the evidence in the light of other independently acceptable background theories, etc. In both cases, the class of probative evidence for a certain hypothesis differs from the class of its observational consequences because we have to take account of its theoretical virtues in comparison to those of its rivals. These virtues are not just parasitic on the entailment relation between the hypothesis and its observational consequences, but rather a function of the overall explanatory value of the hypothesis and its relation with other background theories we accept.

Laudan is certainly right in arguing that UTE can be blocked. But where does that leave us *vis-à-vis* the scientific realism debate? He and the realists would agree that there is need for a richer account of the ampliative nature of the methodology of science than the one offered by the crude hypothetico-deductive method. In particular, they would agree that, given such a richer account, there are good epistemic grounds to choose between rival theories. But Laudan doesn't endorse scientific realism. To be sure, he uses freely expressions such as 'evidential support', 'warranted assent' (p. 56), 'adequate evidential warrant' (p. 63). He talks of theory-acceptance which is presumably distinct from acceptance-as-empirically-adequate, since the latter stance is the natural one to adopt if someone endorses the sceptical force of UTE. He even distances himself from those pragmatists who 'infer that only nonepistemic dimensions of

⁷To be sure, Laudan notes: 'Between two equally well-tested theories, explanatory scope may well be an important desideratum (although I suspect, with van Fraassen, that its importance is more pragmatic than epistemic)' (pp. 122–123). But this remark is part of one of his earlier essays, going back to 1976, and I am not sure that it hangs well with the considerations about evidential support that appear in the more recent essays that constitute chapters two and three.

appraisal are applicable to theories, and that, accordingly, [theory-appraisal] is not exclusively, nor-necessarily, even preferential' (p. 63). But how should we understand the claim that a theory is well-supported by the evidence, or that some theories enjoy more evidential support than others? It seems natural to think that, if distinct from acceptance-as-empirically-adequate, 'warranted assent' should be equated with belief in the approximate truth of the theory; or at least, with the claim that this theory is our current best candidate for an approximately true description of its domain.

Laudan doesn't engage with these issues. But there is no doubt that he is unwilling to endorse the gloss I suggested. Two things show this. First, he is generally sceptical about truth and science's ability to reach it. I shall return to this issue in the next section. Second, his attempt to undercut UTE doesn't aim to defend scientific realism, but rather to defend the possibility of sound methodological judgements (cf. p. 20). For Laudan, methodology should be able to guide the comparative evaluation of *extant* rival theories and to show how rational *comparative* judgements are possible. If UTE is true, then clearly even this more modest aim cannot be fulfilled.

However, once the more modest aim is shown to be achievable, so, I think, is the grander aim of showing that absolute judgements of approximate truth are rational. Suppose we have a theory which is better supported by the evidence than all its extant rivals. What else would we require in order to claim that this theory is (likely to be) approximately true? To put it in a different way, what else would we require in order to show that it is rational to believe in the theory? The sceptic would fall back on a version of UTE. He would suggest that the probability of a theory can never be high enough to warrant the belief because it is possible that another theory, hitherto unthought of, may be at least as well-supported by the evidence as the current best. Laudan considers this move in what he calls the 'non-uniqueness thesis': the thesis that there may be another theory *T'* which is equally supported with *T* with respect to certain ampliative rules and evidence (pp. 33 and 53). He thinks that 'it is an open question' whether the non-uniqueness thesis is true, but reckons that it is possibly true (pp. 42–43). I don't think this can be shown on *a priori* grounds. In fact, the mere logical possibility of such a scenario should not worry us. Nor should it undermine the rationality of absolute judgements. To think otherwise is to suggest that ampliative reasoning should be infallible. This may well be the demand of an outright sceptic, but if Laudan were to endorse that, he would also have to accept that comparative judgements cannot be rational. For even those are ampliative and fallible.

Isn't there ample empirical evidence for the sceptical scenario? One may claim that Laudan, more than anyone else, has provided historical evidence and has consolidated the sceptical scenario in the form of the well-known argument

from the pessimistic induction.⁸ But the credentials of this argument have been recently contested by many authors.⁹ A claim that now emerges with some force is that theory-change is not as radical and discontinuous as the opponents of scientific realism have suggested. Rather, realists show that there are ways to identify the theoretical constituents of scientific theories that essentially contributed to their successes, separate them from others that were 'idle'—or, as Kitcher has put it, merely 'presuppositional posits'—and demonstrate that those components which made essential contribution were those that were retained in theory-change. I have developed this line in considerable detail elsewhere.¹⁰ But if the realist arguments are sound, then this follows: the fact that our current best theory is likely to be replaced by another one which enjoys broader and better evidential support does not, necessarily, undermine the approximate truth of the superseded theory. All it shows is that (a) we cannot get to the truth all at once; and (b) our judgements from empirical support to approximate truth should be more refined and cautious in that they should only commit us to the theoretical constituents that did enjoy evidential support and contributed to the successes of the superseded theory. To put it differently, amassing empirical evidence for the sceptical scenario doesn't undermine the possibility of absolute judgements of approximate truth of current-best theories insofar as these judgements are focused on the constituents of these theories that do enjoy evidential support and insofar as these constituents are in fact retained in subsequent theories. Hence, absolute—but not crude—judgements of approximate truth can be rational. All the more so for someone who, like Laudan, builds his epistemology on the possibility of rational comparative judgements of evidential support.

Perhaps then, Laudan's demolition work on UTE can warrant more epistemic optimism than he explicitly allows. We are still, however, left with Laudan's general misgivings against truth as the aim of inquiry. These are bound up with Laudan's *Normative Naturalism*, his favoured alternative to relativism and positivism.

3. Methodology in the Age of Naturalism

The central component of Laudan's *Normative Naturalism* is Methodological Naturalism (MN): the view that methodology is an empirical discipline—the

⁸L. Laudan, 'A Confutation of Convergent Realism', *Philosophy of Science* 48 (1981), 19–48.

⁹Cf. J. Worrall, 'Structural Realism: the Best of Both Worlds?' *Dialectica* 43 (1989), 99–124; and 'How to Remain (Reasonably) Optimistic: Scientific Realism and the 'Luminiferous Ether'', in D. Hull and M. Forbes, *PSA 1994*, Vol. 1 (East Lansing, MI: Philosophy of Science Association, 1994), pp. 334–344; P. Kitcher, *The Advancement of Science* (Oxford: Oxford University Press, 1993); S. Psillos, 'A Philosophical Study of the Transition from the Caloric Theory of Heat to Thermodynamics: Resisting the Pessimistic Meta-Induction', *Studies in History and Philosophy of Science* 25 (1994), 159–190; and 'Scientific Realism and the "Pessimistic Induction"', *Philosophy of Science*, 63 (Proceedings) (1990), S306–S314.

¹⁰My *Philosophy of Science* paper, *op.cit.*, note 9

theory of 'the regularities governing inquiry' (p. 110)—and as such, part and parcel of natural science. In particular, MN suggests the following. (1) All normative claims are *instrumental*: methodological rules link up aims with methods which will bring them about, and recommend what action is more likely to achieve one's favoured aim. (2) The soundness of methodological rules depends on whether they lead to *successful* action, and their justification is a function of their effectiveness in bringing about their aims. A sound methodological rule represents our 'best strategy' for reaching a certain desired aim (cf. pp. 103 and 128 ff).

Success and effectiveness in promoting aims cannot be evaluated *a priori*. Since they depend on contingent features of the world, they should be tested empirically 'in precisely the same way we test empirical theories' (p. 133), i.e. by looking for correlations, causal linkages (p. 17) and statistical laws (p. 134) between 'doing *x* and achieving *y*'. After all, we want our methods to be effective in this world, that is to guide us to correct decisions and correct strategies for extracting information from nature. In this simple sense the methods we adopt must be amenable to substantive information about the actual world (cf. p. 171).

However, Laudan's instrumental account of methodology threatens to entail relativism. If methodology is only concerned with means-end relations and leaves the ends unspecified, if methodology is just about how to achieve whatever aims we happen to value most, then epistemic relativism seems to follow. If two communities, or two groups of inquirers, pose different aims of inquiry, is there any sense in which methodology can *rationaly adjudicate* between them rather than merely *describe* the effectiveness of their respective 'best strategies'? And if there is a sense in which we can compare their respective 'best strategies', wouldn't that presuppose a meta-perspective from which we methodologists can judge their comparative effectiveness? Such considerations have made more mainstream epistemologists unsympathetic to the naturalist project. Worrall¹¹ and Doppelt,¹² to name but two, have objected that methodological naturalism falls prey to relativism and fails to underpin the rationality of scientific inquiry.

Laudan counters that his MN doesn't endanger rationality and has definite normative-evaluative consequences. On the one hand, MN can offer 'warranted advice' (p. 133) as to how someone can best achieve their favoured aims: given that method *M* promotes aim *A*, if someone wants to achieve *A* then they *ought* to do *M*. On the other hand, when it comes to evaluative comparison of rival methodologies, we can fall back on some principle 'which all the disputing

¹¹'The Value of Fixed Methodology', *British Journal for the Philosophy of Science* 39 (1988), 263–275; and 'Fix it and Be Damned: A Reply to Laudan', *British Journal for the Philosophy of Science* 40 (1989), 376–388.

¹²'The Naturalist Conception of Methodological Standards in Science', *Philosophy of Science* 57 (1990), 1–19.

theories of methodology share in common' (p. 135). Laudan thinks that a suitable version of enumerative induction can be such a shared principle of evidential support: (R_1) if method M has consistently promoted aim A in the past, and method N has failed to do so, then future actions based on the rule 'if you want A, then you ought to do M' are more likely to succeed than actions based on the rival rule 'if you want A, then you ought to do N'.

This very move, however, entails that normative judgements cannot be purely instrumental. R_1 offers the required 'quasi-Archimedean standpoint' (p. 135) only if it is seen as part and parcel of a normative meta-perspective on which all methodological appraisal ultimately rests. One *central* question is this: how is R_1 itself to be evaluated? In particular, can it be evaluated instrumentally? Is it itself warranted because it has been shown to be successful in promoting certain aims—whatever those may be? Or is it warranted because it is a sound rule of ampliative reasoning? I don't want to argue that we cannot evaluate the past performance of R_1 empirically. But two things are worth noting: (a) selecting the data pertaining to the evaluation of R_1 , showing that past correlations are not spurious, establishing that the predicates involved are projectible etc. require the use of more sophisticated and more controversial methodological principles. (b) Suppose that we can establish the authenticity of past successes of R_1 ; whether or not these successes warrant projections to the future, and hence whether or not they warrant acceptance of R_1 as a sound principle, rest on the soundness of inductive reasoning in general. But defending the soundness of inductive reasoning is an epistemic issue: it relates to showing that inductive reasoning is warranted in that it has the capacity to generate true conclusions when fed with true premises.

I would generalise this last point as follows. The *rationality of action*, it can be argued, is, to a first approximation, a means-end issue: rational action is following the 'best strategy' that will promote one's aims. But rationality of action cannot exhaust the content of rational judgement. Nor can it fully capture its normative dimension. An account of rational judgement should accommodate both the *rationality of belief* as well as the rationality of action. A purely instrumental story leaves the rationality of belief unaccounted for: when is it rational to hold a belief? Beliefs guide action and support instrumental claims of the sort that the strategy followed to achieve a certain aim is the 'best'. When are *these* beliefs rational? When are they warranted by the available evidence? Judging their rationality and evidential warrant—and offering relevant normative advice—is an epistemic matter. It is a function of the epistemic relation between the evidence and the belief for which it is taken to be evidence for. And, similarly, it is a function of the soundness of the methods that produce and sustain these beliefs.

Laudan's Methodological Naturalism rightly suggests that the principles of sound ampliative reasoning cannot be laid out without using substantive

empirical knowledge. It is equally right to point out that matters of justification are not *a priori* but amenable to empirical investigation and knowledge. This is the invaluable insight of modern naturalism. This claim has also been defended by *reliabilist* epistemological theories à la Goldman and Papineau.¹³ Their central points are that methodological and reasoning strategies should be evaluated by their success, or tendency, to produce and maintain true beliefs and that judging this success, or establishing the tendency, is open to empirical findings and investigation. Reliabilism, however, can easily account for the rationality of belief—as well as for the rationality of action—by arguing that a rational belief is a well-supported belief and those are the beliefs which are produced by reliable, i.e. truth-conducive and truth-maintaining, methods and processes. However, unlike Laudan's MN, reliabilist epistemology accepts that the *aim* of sound ampliative reasoning cannot be a matter of empirical investigation. Reasoning cannot be 'correct' if it consistently leads to false conclusions. Nor can it be correct if it merely achieves the reasoner's aims. If this was the only requirement, then any fallacious mode of reasoning could be deemed 'correct' if it promoted the favoured aims of the relevant community of reasoners. Sound reasoning is intimately bound up with truth and the capacity to generate and maintain true conclusions from true premises. Truth emerges as the *basic* cognitive virtue of sound reasoning. Achieving true beliefs is the aim in light of which methodological and reasoning strategies should be evaluated. Reliabilism supplements methodological naturalism with a normative meta-perspective of truth-linked judgements.

These last considerations clearly separate Laudan's brand of naturalism both from other fellow-naturalists who accept a truth-linked axiology and a broadly reliabilist account of methodology *and* from non-naturalists who rightly think that rationality, epistemic warrant and justification should answer to truth but argue that, on pain of *vicious* circularity, the naturalist project has no bearing on these issues. I think the right position lies somewhere in the middle: the defence of rational methodological choices against relativists and subjectivists—who are Laudan's prime targets—requires a normative truth-linked meta-perspective, but circularity notwithstanding, this defence can be firmly based on empirical considerations and the findings of the sciences. Here, naturalists have a lot to learn from Laudan's thoughts on how empirical considerations can bear on the adjudication of methodological disputes, e.g. those concerning the 'Rule of Predesignation' (cf. p. 178).

To be fair to Laudan, he acknowledges that Methodological Naturalism should be coupled with a naturalised theory of axiology, that is a general theory about the constraints that 'govern rational choice of aims and goals' (p. 17). He

¹³A. Goldman, *Epistemology and Cognition* (Cambridge, MA: Harvard University Press, 1986); D. Papineau, *Philosophical Naturalism* (Oxford: Blackwell, 1993).

admits that no such general theory is now available, but that, nonetheless, 'we now understand several of those valuative mechanisms pretty clearly' (*ibid.*). For instance, the goals should be 'in principle achievable' (p. 78); 'empirically realisable' (p. 145); 'practically workable' (p. 145). But, according to Laudan, aims are not normative requirements for the appropriate cognitive evaluation of science.

Why is he so vehemently opposed to the view that truth (or truth-linked notions) should be the aim of inquiry? Using his axiological theory, he argues that truth is an exemplar of an unrealisable aim. He points out that truth is 'intrinsically transcendent' and 'closed to epistemic access' (p. 78). He even endorses the epistemic version of Barnes and Bloor's 'symmetry thesis'—although he is keen to dismiss its 'rational version'—on the basis that 'knowledge of a theory's truth is radically transcendent', aka 'radically inaccessible'. As he puts it: 'This transcendence entails the epistemic version of the symmetry thesis since we are never in a position to partition theories into the true and false and then proceed to explain beliefs in them differently on account of their truth status' (p. 195). One of his main complaints against his opponents is that 'traditional epistemologists who [...] hanker after true or highly probable theories as the aim of science find themselves more than a little hardpressed to identify methods that conduce to those ends. Accordingly, normative naturalism suggests that unabashedly realist aims for scientific inquiry are less than optimal' (p. 179).

What's so difficult about truth? Is truth epistemically inaccessible and utopian? First of all, note that Laudan's own arguments against the underdetermination of theories by evidence (UTE) don't fit well with his qualms about truth. As I argued in Section 1, his rebuttal of UTE warrants more epistemic optimism than he explicitly allows. Unless the non-uniqueness thesis is proven, and unless underdetermination is a global feature of scientific theorising, there is no reason to think that theoretical truth cannot be achieved. But neither of the above conditions are established by Laudan. Indeed his own arguments do so much to discredit generalised agnosticism about theoretical truth that one is bound to think that the possibility of achieving theoretical truth faces no damning objections.

Some philosophers' misgivings about truth stem from the thought that science should simply aim for a different target, viz. empirical adequacy, or saving the phenomena.¹⁴ This thought cannot, on its own, undermine the view that science aims at the truth. Empirical adequacy is consistent with truth, and in fact, a necessary condition for it. The issue here is not a choice between two inconsistent aims, but rather why, in the absence of damning arguments against truth theoretical, we should opt for less. As far as I can tell, there is no

¹⁴Incidentally, Laudan offers some nice arguments as to why the two notions above should not be conflated (cf. p. 73).

independent argument for empirical adequacy, other than, that is, that theoretical truth is not achievable. But suppose that theoretical truth were indeed utopian as an aim. Is empirical adequacy less utopian? There is certainly a difference between truth and empirical adequacy, but it is one of degree. Claims of theoretical truth exceed those of empirical adequacy because they involve the theoretical assertions made by scientific theories. But unless one thinks that there is a *special* problem with the cognition of unobservable entities and processes, reaching theoretical truth is 'more of the same'. Van Fraassen would dispute this more-of-the-same line, because he thinks that claims which involve reference to unobservable entities are inherently undecidable. But Laudan doesn't seem to agree with this. He acknowledges that 'entities or processes originally introduced by theory frequently achieve observable or "empirical" status as experimental methods and instruments of detection improve' (p. 57). To be sure, Laudan has offered his own arguments against science's capacity to unravel the deep-structure of the world, but as is well known, they are not based on our limitations in direct observation, but rather on more sophisticated considerations concerning the allegedly poor track-record of science. As I noted, however, towards the end of the previous section, these considerations are not compelling (cf. notes 9 and 10). So, I conclude that Laudan's qualms against truth cannot be based on the possibility of an alternative—and more realisable—aim, viz. empirical adequacy.

Well, could truth be 'transcendent' because it involves this mysterious 'correspondence relation' between statements and facts? This line has been very popular among philosophers. Here again, I cannot enter the subtleties of this debate. But the following is worth noting. As Laudan rightly points out, the underlying thought of correspondence accounts of truth is that beliefs should be grounded in the world (cf. p. 79). I don't see how this perfectly sensible claim should make truth 'transcendent'. All it implies is that a belief is true if and only if its truth-conditions obtain. In particular, it is consistent with Laudan's central message of chapter three that truth-conditions should not be confused with evidence-conditions (cf. pp. 69–73). Truth-conditions are those which, when obtain, render the belief true. Evidence-conditions are those which, when obtain, render the belief warranted or rational. The only remaining issue is this: can we be in a position to assert that the truth-conditions for an assertion obtain?

Let me try to make this last point clearer. Let's bear in mind that Laudan also accepts that certainty in decision procedures is a utopian aim and hence that it must be abandoned.¹⁵ If, however, one accepts that scientific assertions are short of certainty, one should have no special problem to decide their truth. In order to see that, let's ask the following question. Grant that truth is a

¹⁵*Science and Values* (Berkeley: California University Press, 1984), see p. 83

non-recursive property. Is it less decidable than other properties studied by science? What follows goes back essentially to Carnap.¹⁶

Envisage a scientific group, who wants to decide whether a certain substance is of a specific chemical constitution. They run some thorough tests and decide that the substance is, say, an acid. No amount of testing can decide with certainty—i.e. without any possibility of error—that the substance under consideration is an acid. Yet, there is a point when the evidence is enough to warrant the relevant belief. Now, let us grant that the assertion S: ‘Substance X is an acid’ is decidable—i.e. confirmable to a high degree—by some scientific procedures. As Carnap rightly observed, if sentence S is confirmed to a degree r , then the sentence S’: ‘“Substance X is an acid” is true’ is confirmed to exactly the same degree, since S and S’ are equivalent, given English and the disquotational property of the truth-predicate. Carnap concluded that ‘is true’, and ‘truth’, are legitimate scientific notions, precisely on the grounds that sentences that state truth-values are confirmable. The truth of an assertion (or a belief) is no less, nor more, confirmable than the assertion (or the belief) itself. There is no need for an extra assurance that a belief is true, over and above the assurance that we get from the fact that this belief is the product of a reliable—but fallible—method. To be sure, it is perfectly possible that a belief issued by a reliable method *might* be false. Reliable methods are fallible methods: the fact that a method is reliable does not logically *guarantee* that a belief issued by this method is true. The link between a reliably produced belief and a true belief is a synthetic, not a conceptual, one. But notice that fallibility does not entail actual falsity. All that it warns off is a conceptual identification of truth with epistemic notions such as warranted assertibility, ideal justification and the like. A warrantably asserted (or ideally justified) statement may be false: the very claim of warranted assertibility (or ideal justification) does *not* entail that the belief is true. However, unless one claims that no belief can be rationally said to be true unless it is impossible for it to be false, it makes perfect sense to say that belief in truth is rational even though there is a logical possibility that the belief be false. In sum, if we are ready to abandon certainty in decision procedures, and to accept the disquotational property of the truth-predicate, there are no grounds for arguing that truth is undecidable, utopian, and the like. The whole problem of epistemic accessibility of the world relates to the reliability of our methods of interaction with it. The only burden that the epistemic optimist has is to show that our methods of investigating the world are reliable. This is no small issue at all. But there is no reason to think that it cannot be dealt with.¹⁷

¹⁶R. Carnap, ‘Remarks on Induction and Truth’, *Philosophy and Phenomenological Research* 6 (1945/46), 590–602, see p. 602

¹⁷Here, I am not claiming that all questions of theoretical truth can be decided. But, remember, the sceptical claim is that none can. If my arguments are sound, then this claim is false.

The picture I have painted so far is ideal. We *know*, from Laudan's work and that of others, that epistemic optimism can extend only as far as the approximate truth of scientific assertions. Does the fact that we do not have an adequate formal understanding of approximate truth spoil the integrity of this concept and make it implausible as an aim of science? Laudan suggests this much (p. 78). If 'approximate truth' proves to be an elusive notion, then Laudan might be right. But two things are worth noting: (a) lack of a formal representation of approximate truth doesn't entail lack of understanding. The latter is still intuitive. As McMullin put it: 'calling a theory "approximately true", (...) would be a way of saying that entities of the general kind postulated by the theory exist. It is "approximate" because the theory is not definitive as an explanation; more has to be said. But it already has a bearing on the truth because we can say that it has allowed us discover that entities of a certain sort exist, entities that we could not (for the moment at least) have known without the aid of the theory'.¹⁸ But the lack of a formal account is not, necessarily, a defect. Here the comparison with the formal Tarskian understanding of truth is not helpful. The need for formalisation à la Tarski arose, at least partly, from the fact that the intuitive notion of truth led to paradoxes, such as the Liar paradox. No similar paradoxes are known *vis-à-vis* the intuitive understanding of approximate truth. (b) More recent work on approximate truth suggests that we may after all be able to formally characterise the semantics of approximate truth.¹⁹ The formal battle is, surely, uphill. But the whole enterprise is by no means hopeless.

In sum, truth and truth-linked notions are not as problematic as Laudan suggests. And, to be honest, I cannot see why one should think so in the first place. But, criticism aside, Laudan's book is a delight to read. An outstanding contribution to current philosophy of science.

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¹⁸*Op cit.*, note 5, pp. 59–60.

¹⁹For some recent relevant work, cf. T. Weston, 'Approximate Truth and Scientific Realism', *Philosophy of Science* 59 (1992), p. 53–74; J. L. Aronson, R. Harre and E. Way, *Realism Rescued* (London: Duckworth, 1994). For a critical discussion of the last approach, cf. my review of 'Realism Rescued', *International Studies in the Philosophy of Science* 9 (1995), 179–183.