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METHODOLOGICAL PLURALISM, NORMATIVE NATURALISM AND THE REALIST AIM OF SCIENCE*

1. INTRODUCTION

There are two chief tasks which confront the philosophy of scientific method. The first task is to specify the methodology which serves as the objective ground for scientific theory appraisal and acceptance. The second task is to explain how application of this methodology leads to advance toward the aim(s) of science. In other words, the goal of the theory of method is to provide an integrated explanation of both rational scientific theory choice and scientific progress.[1]

Theorists of scientific method may be broadly divided into two main camps: monists and pluralists.[2] Traditional methodologists tend to fall into the monist camp. They see science as characterised by a single, universally applicable method, invariant throughout the history of science and the various fields of scientific study. The two leading versions of monism are inductivism, which takes scientific theories to be grounded in inductive inference from observed data, and Popperian falsificationism, which treats the method of science as the ruthless attempt to refute conjectural hypotheses which scientists propose to explain observed phenomena.

By contrast, recent methodological pluralists argue, against the idea of a fixed method, in favour of a plurality of methodological rules governing theory evaluation.[3] Such methodological rules may vary from time to time, as well as field to field, within science. New rules may be introduced and old ones discarded. Rules may be modified, as they undergo refinement in the course of scientific practice. They may be applied in different ways in different fields of science, and different scientists may interpret the same rules in different ways. Moreover, as there is always a plurality of rules, different scientists may choose to emphasise different rules in the evaluation of alternative theories. On the resulting pluralist conception of methodology, science is not characterised by a single invariant method, but by a set of evaluative rules to which scientists appeal in the context of theory appraisal.[4]

As for the aim of science, a number of alternative approaches may be distinguished here as well. According to scientific realism, the aim of science is to arrive at true, explanatory theories of both observable and unobservable aspects of the world, and the best explanation of the success of science is that considerable

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headway has been made toward that aim. For the empiricist, by contrast, the aim of science is restricted to producing predictively accurate theories which are empirically well-supported by the observed phenomena. Conventionalist philosophers of science, who regard theories as classificatory schemes which impose order on experience, take the main aim of science to be to produce an economical ordering of experience. Philosophers of a pragmatist bent emphasise prediction and control of the environment, in the service of successful achievement of practical goals.[5]

In this paper, I will focus on the relationship between methodological pluralism and scientific realism. In particular, I will consider the question of whether sustained application of a plurality of methodological rules conduces to realisation of the scientific realist aim of truth. This question, which raises issues of both an epistemological and a metaphysical nature, is a special instance of the more general demand for an integrated account of rational theory choice and scientific progress. It is, in my view, *the* most urgent question facing the scientific realist who seeks to derive insights about scientific methodology from the pluralist approach found in the work of T.S. Kuhn and P.K. Feyerabend.

The paper is organised as follows. In section 2, I discuss the threat of relativism which is raised by methodological pluralism. In section 3, I show that Laudan's normative naturalist metamethodology removes the threat of relativism. In section 4, I propose that normative naturalism be incorporated within the framework of scientific realism. Section 5 presents objections due to Laudan against the realist aim of truth, which threaten the incorporation of normative naturalism within a realist framework. In sections 6 and 7, I defend the realist aim of truth against these objections. Finally, I argue in section 8 that use of a plurality of methodological rules promotes the realist aim of science.[6]

2. PLURALISM AND RELATIVISM

The main impetus for a pluralist conception of method derives from the historical philosophy of science notably championed by Kuhn and Feyerabend. By contrast with earlier monist orthodoxy, advocates of the historical approach argued that science should be conceived as a developmental process, which takes place in a variety of historical circumstances using a variety of methods, rather than the implementation of an invariant, universal method. Kuhn, who initially argued that standards of theory appraisal vary with scientific paradigm, later came to argue that science is governed by a set of cognitive values (e.g., accuracy, breadth, simplicity, coherence, fertility) which guide theory choice. Feyerabend, for his part, argued not only that all methodological rules are routinely violated in the course of scientific practice, but that there are often good grounds for the violation of such rules.

Some writers suppose that the historical approach of Kuhn and Feyerabend entails wholesale rejection of scientific method. However, I prefer to draw a more positive moral. What is to be rejected, if one adopts the historical approach, is not method as such, but a monistic *theory* of method. Ample scope remains to develop a more adequate account of method within the framework of the

historical approach. In particular, what emerges from the historical approach is a pluralist conception of method, on which the principles of method are not unique and invariant, but multiple and subject to variation in the history of science.

I have elsewhere attempted to sketch the main outlines of a pluralist theory of method (1997a, chapter 7). For present purposes, it suffices to characterise the pluralist account by means of the following five theses, which represent central themes of the historical school:

- *Multiple rules*: scientists utilise a variety of methodological rules in the evaluation of theories and in rational choice between alternative theories.[7]
- *Methodological variation*: the methodological rules utilised by scientists undergo change and revision in the advance of science.[8]
- *Conflict of rules*: there may be conflict between different methodological rules in application to particular theories.[9]
- *Defeasibility*: the methodological rules, taken individually rather than as a whole, are defeasible.[10]
- *Non-algorithmic rationality*: rational choice between theories is not governed by an algorithmic decision procedure which selects a unique theory from among a pool of competing theories.[11]

These five theses constitute the basic elements of a pluralist conception of methodology, according to which scientific theory appraisal is governed by an evolving set of methodological rules. Because the rules may conflict in practice, and are individually defeasible, appeal to the system of rules need not uniquely determine the outcome of theory choice. Accordingly, scientists who place differential weight on various rules may come thereby to decide in favour of opposing theories.

It is precisely the scope that methodological pluralism affords for rationally grounded disagreement between scientists that makes it controversial. For it brings it into tension with methodological monist accounts which restrict rational divergence of opinion to that allowed by compliance with a single method. The opposition between monist and pluralist accounts of method is at the root of much recent concern with epistemological relativism in the philosophy of science. For, on the one hand, it is widely held that a monistic theory of method avoids relativism by grounding theory choice in a shared, invariant method.[12] On the other hand, it is also widely assumed that pluralism entails relativism, since the existence of a plurality of methods would provide scientists with rational justification for the acceptance of opposing theories on the basis of alternative sets of rules.

However, it is a mistake to suppose that rational disagreement due to variation of methodological rules necessarily leads to relativism. For that would be to suppose that mere difference in the rules employed by scientists entails relativism. And that in turn would be to suppose that mere compliance with operative rules suffices for rational justification. Yet the latter assumption is surely mistaken. It overlooks the crucial distinction between rules which provide rational justification and those which do not. Not all methodological rules that may be proposed or employed are capable of providing rational justification. Some provide no

justification at all. Given the distinction between rules which provide justification and those which fail to do so, relativism is not entailed by pluralism, since mere satisfaction of a methodological rule does not suffice for rational justification.

Yet, while a plurality of methodological rules may not entail relativism, the challenge of relativism now arises in a novel form. For the distinction between rules which provide justification and those which do not is a distinction that is itself in need of defence. After all, how can one rule be shown to provide greater rational justification than another? The relativist challenge, therefore, is to show how one methodological rule may be epistemically superior to another.

3. NATURALISM AND RELATIVISM

The question of how to assess the epistemic merits of a methodological rule is a metamethodological question about the nature of epistemic normativity. One of the most promising approaches to this issue is a form of epistemic naturalism which grounds normativity in the facts of inquiry.[13] This approach involves two key elements. On the one hand, it treats methodological rules as empirical hypotheses about how to pursue inquiry, which may be evaluated in light of empirical evidence. On the other hand, such rules are conceived as instruments or tools of inquiry, the epistemic function of which is to advance cognitive ends. The two elements are combined by grounding evaluation of methodological rules in empirical evidence about performance of epistemic function.

As a special case of this naturalist approach to epistemic normativity, I turn to the *normative naturalist* metamethodology of Larry Laudan.[14] Laudan is critical of the scientific realist view defended here that the aim of science is advance on truth. In the sections to follow I will explore the possibility of incorporating Laudan's normative naturalism within a scientific realist framework. However, in this section my concern is with the normative naturalist account of epistemic normativity as a response to relativism.

As a naturalist, Laudan treats metamethodology as an empirical discipline continuous with natural science. In order to ground methodology empirically, it must be possible to treat methodological rules as normative claims about the conduct of inquiry which are capable of empirical evaluation. Accordingly, Laudan proposes that methodological rules be construed in instrumental fashion as recommendations of means of realising desired cognitive ends. This enables such rules to be formulated as conditional claims with the following hypothetical imperative form:

If one wishes to attain aim A, then one ought to employ method M.

As an example of how a methodological rule may be cast in hypothetical form, Laudan offers the following formulation of Popper's rule against *ad hoc* hypotheses:

> [I]f one wants to develop theories which are very risky, then one ought to avoid *ad hoc* hypotheses. (Laudan 1996, p. 133)

Such an analysis permits the recommendation of a methodological rule to be based on historical evidence. For it reveals how such rules may be supported by

claims of statistical covariance between past use of method and achievement of results. Where use of a method has historically proven to be a reliable means of achieving a given end, the method may be recommended on the basis of past performance as means to that end. In this manner, empirical evidence from the history of science may serve as the normative ground of a methodological rule.[15]

The normative naturalist analysis of the justificatory basis of methodological rules enables the distinction to be sustained between rules which provide epistemic support and ones that do not. For if use of one rule reliably conduces to a given aim and use of another fails to, then it provides greater epistemic support than the other. But if one rule may have greater epistemic merit than another, the challenge of relativism may be met. For where there may be variation in the epistemic credentials of rules, rational justification does not reduce to mere compliance with operative methodological rules. Hence, one theory may enjoy a higher degree of support than another, despite a plurality of methodological rules.

4. SCIENTIFIC REALISM AND NORMATIVE NATURALISM

Laudan is a well-known critic of the realist view that truth is the aim of science. Accordingly, Laudan develops normative naturalism within the context of an axiology that allows a multiplicity of scientific aims, rather than being limited to the realist aim of truth. However, in contrast with Laudan, I seek to combine methodological pluralism with scientific realism precisely by incorporating normative naturalism into a realist framework. In so doing, I wish to preserve the normative naturalist response to epistemic relativism while providing an integrated account of both the methodology of science and its progress.

The core of the normative naturalist analysis of methodological rules is that rules may be construed as hypothetical imperatives linking epistemic means and ends. This enables such rules to be treated instrumentally as cognitive tools, which may be utilised to advance the aims of science. Such an instrumental analysis of methodological rules leaves the nature of the epistemic or cognitive aims unspecified. As a critic of realism, Laudan rejects the realist aim of truth, for reasons to be considered in the next section. However, Laudan does not offer any one, unique alternative to truth as the correct analysis of the constitutive aims of science. Rather, he argues that scientists' cognitive aims vary historically as part of the continual process of adjustment and correction of theories, methods and aims which characterises scientific inquiry.[16]

Because the instrumental analysis of rules is neutral with respect to the nature and number of aims that scientists may pursue, I hold it to be possible to set the analysis within a realist framework. In particular, if we treat truth as the paramount aim of science, we may then suppose that the cognitive aim that is to be fulfilled by a proposed rule is advance on the truth about the world.[17] On such a realist construal of normative naturalism, a methodological rule conveys epistemic warrant to the extent that fulfilment of the rule conduces to the aim of truth. As such, normative naturalism emerges as a species of reliabilist epistemology once it is placed within the context of scientific realism. For it is reliability in

leading to the truth which is then the basis of the epistemic warrant of methodological rules.[18]

Where the realist sees truth as the aim of science, Laudan allows that a multiplicity of aims may be pursued by scientists. However, in speaking of truth as the aim of science, the realist need not deny that scientists pursue multiple aims. Instead, the realist need only conceive truth as the paramount aim that constitutes the ultimate goal of science. The various other cognitive aims which may be pursued by scientists may be understood as subordinate aims which subserve the overriding realist aim of truth. This permits the realist to preserve an additional aspect of Laudan's analysis of the epistemic warrant of methodological rules. Where Laudan holds that the warrant of a rule consists in reliable promotion of cognitive ends, the realist need not insist that the specified aim of the rule be truth. Rather, provided that the specified aim subserves the overriding goal of truth, a rule which immediately conduces to a lower level aim may still convey epistemic warrant.[19]

On the assumption that employment of methodological rules conduces to truth, or to aims that subserve truth, the present proposal offers an integrated account of both the methodology and progress of science. However, as I now turn to Laudan's objections to realism, we are about to see that this assumption is in need of defence.

5. LAUDAN AND THE AIM OF TRUTH

Laudan has argued against scientific realism on a number of occasions. Perhaps most notable is his attack on convergent epistemological realism, in which he attempts to sever the explanatory connections drawn by realists between reference, truth and the success of science.[20] Here, however, I focus on two specific objections raised by Laudan against the realist aim of truth. These objections pose a serious threat to my proposal to set the normative naturalist account of epistemic warrant within the context of a realist account of the aim of science.

Laudan's objections turn crucially on what he takes to be the transcendental nature of truth. He assumes that we can tell neither that a theory is true nor that progress toward truth has occurred. Given this initial assumption, Laudan develops two separate arguments that truth cannot serve as a suitable aim for science. He argues, first, that it is not rational to pursue a goal which cannot recognisably be attained or even approached. Second, he rejects transcendental aims such as truth as unsuited to a naturalistic treatment of the methodology of science. Before presenting these two objections, I will examine Laudan's view of the transcendence of truth.

For Laudan, a transcendental aim or property is one to which we have no epistemic access. He describes truth as a 'transcendental property', and contrasts it with an 'immanent' goal such as 'problem-solving effectiveness', which '(unlike truth) is not intrinsically transcendent and hence closed to epistemic access' (Laudan 1996, p. 78). The distinction between immanent and transcendent states corresponds more or less to that between what can be empirically shown to be the case and what cannot. Laudan's grounds for taking truth as transcendental appear to be twofold. On the one hand, he contrasts transcendental aims with the

'detectable or observable properties' (*ibid.*, 1996, p. 261, fn. 19) that provide evidence of methodological means/ends relationships, implying thereby that a transcendental state is one that cannot be directly observed to obtain. On the other hand, he claims that 'knowledge of a theory's truth is radically transcendent', since 'the most we can hope to "know" about [a theory . . .] is that [it is] false' and 'we are never in a position to be reasonably confident that a theory is true' (*ibid.*, 1996, pp. 194, 195).[21] The epistemically transcendent therefore emerges as that which transcends the empirical either by being unobservable or by being based on an ampliative inference that extends beyond the observed data. Accordingly, that is what I shall mean when I speak in what follows of the transcendence of truth or theoretical truth.

Laudan accords truthlikeness a status similar to truth. Since the truth of a theory transcends our capacity for knowledge, we can be in no position to judge how closely an actual theory approximates the truth (*ibid.*, 1996, p. 78). The problem is aggravated by lack of a clear conception of approximate truth. On the Popperian account of verisimilitude, for example, a theory may have high verisimilitude and yet display little or no empirical success (Laudan 1984, p. 118). More generally, Laudan claims that there is no known means to measure or estimate how close a theory is to the truth. Consequently, truthlikeness transcends our capacity to know it every bit as much as does truth.

Given the transcendence of truth and truthlikeness, Laudan objects to the role accorded to such notions within realist accounts of scientific progress. He develops his first objection in the context of a discussion of the rational evaluation of cognitive goals in his (1984, pp. 50–55). According to Laudan, a crucial consideration in evaluating a goal is whether it may be realised. He takes it as a requirement of rationality that there be grounds to suppose it possible to achieve the goals one pursues (1984, p. 51). Goals which are unable to be achieved may be rejected as 'utopian'. Laudan distinguishes three ways in which goals may be utopian: goals that can be shown to be unrealisable are 'demonstrably utopian'; ones that are overly vague or imprecise are 'semantically utopian'; and goals which cannot be shown to obtain are 'epistemically utopian'. Laudan's objection to truth as a cognitive goal is that it is epistemically utopian.

As a prime instance of an epistemically utopian goal, Laudan takes the 'goal of building up a body of true theories' (1984, p. 53). He allows that such a goal may not be demonstrably utopian, and that the concept of truth admits of clear analysis. However, he asks us to consider the case in which one 'has no idea whatever how to determine whether any theory actually has the property of being true' (1984, p. 51). (Of course, as we have just seen, Laudan takes this to be our actual epistemic situation, given the transcendence of truth.) In such a case, where value is placed on an unrecognisable property, Laudan says that 'such a value could evidently not be operationalized' (1984, p. 53), meaning by the latter that no procedure is known which would lead to its attainment (cf. 1984, p. 51). He then concludes that:

> if we cannot ascertain when a proposed goal state has been achieved and when it has not, then we cannot possibly embark on a rationally grounded set of actions to achieve or promote that goal. In the absence of a criterion for detecting when a goal has been realized, or is coming closer to

> realization, the goal cannot be rationally propounded even if the goal itself is both clearly defined and otherwise highly desirable. (1984, p. 53)

Given that Laudan takes truth and truthlikeness to be transcendental, I suggest he is to be understood here as proposing the following argument against the realist aim of truth: (a) it is not rational to pursue an aim which may neither be recognised to obtain nor to be close to obtaining; (b) the goal of true theories may neither be recognised to obtain nor to be close to obtaining; therefore (c) it is not rational to pursue the goal of true theories.[22]

While Laudan's first objection concerns rational pursuit of truth, his second objection derives from his naturalistic view of method. In particular, Laudan argues that transcendental goals such as truth are shown to be illegitimate by the normative naturalist analysis of methodological rules. As we saw in section 3, the normative naturalist construes methodological rules in instrumental fashion as hypothetical imperatives which relate cognitive means and ends. Such an analysis enables methodological rules to be evaluated empirically with regard to their effectiveness in promoting specified aims. According to Laudan, the instrumental conception of method places a premium on the realisability of aims. Aims which cannot be achieved (i.e., utopian aims) are unsustainable, given the goal-directed nature of methodology.

More specifically, Laudan claims that the instrumental conception of method leads to rigorous constraints on the legitimate aims of science:

> any proposed aims for science [must] be such that we have good reasons to believe them to be realisable; for absent that realisability there will be no means to their realization and thus no prescriptive epistemology that they can sustain ... (Laudan 1996, pp. 157–158)

Such constraints have direct bearing on the realist aim of truth:

> one of the corollaries of the instrumental analysis is that those ends that lack appropriate means for their realization become highly suspect. Traditional epistemologists who ... hanker after true or highly probable theories as the aim of science find themselves more than a little hard pressed to identify methods that conduce to those ends. Accordingly, normative naturalism suggests that unabashedly realist aims for scientific inquiry are less than optimal. (*ibid.*, 1996, p. 179)

Thus, the demand of realisability entails the rejection of realist aims as unacceptable for science. The reason, as with the previous objection, turns on the transcendental nature of truth:

> if one has adopted a transcendental aim, or one which otherwise has the character that one can never tell when the aim has been realized and when it has not, then we would no longer be able to say that [a] methodological rule asserts connections between detectable or observable properties. I believe that such aims are entirely inappropriate for science, since there can never be evidence that such aims are being realized, and thus we can never be warrantedly in a position to certify that science is making progress with respect to them. (*ibid.*, 1996, p. 261, fn. 19)

In short, because methodological rules derive their epistemic support from underlying empirical means/end connections, there may be no evidence capable of showing that a rule promotes a transcendental aim, since no empirical evidence may show that a transcendental aim has been reached or is close to being reached.

Based on the lack of possible evidence for advance on truth, Laudan concludes that the realist aim of truth fails to be a legitimate goal for science. While it is not

entirely clear how the various strands of Laudan's thoughts on this topic fit together, I propose the following reconstruction of his argument: (a) the methods of science are instruments for the realisation of the aims of science; (b) given this, a legitimate aim of science must be such that it may be realised *and* there may be evidence of its realisation; (c) because truth is transcendental there may be no evidence that the end of truth is realised; hence (d) truth is not a legitimate aim of science.

In sum, Laudan rejects the realist aim of truth on the grounds that it is neither rational to pursue the truth nor is the truth a legitimate aim of science. Both of these objections turn on the basic assumption that truth is transcendental. Let us now see if these objections may be met.

6. IS TRUTH TRANSCENDENT?

The two objections canvassed in the preceding section stem from the common premise that theoretical truth is transcendent. In this section I will challenge this premise by arguing that it is possible to have theoretical knowledge. In the next section, I will address the negative consequences which Laudan derives from the premise about the rationality and legitimacy of pursuit of truth.

As we have seen, Laudan regards theoretical truth as transcendent in the sense that such truth transcends our capacity to know it. However, it is by no means evident that theoretical truth is unknowable, as Laudan claims it to be. That this is so may be readily shown on the basis of the standard analysis of knowledge as justified true belief. On such an analysis, a knowing subject S knows a theoretical proposition P *iff* three conditions are fulfilled:

1. S believes that P is true,
2. S's belief that P is true is rationally justified,
3. P is true.

Given such an analysis of knowledge, there is no apparent reason in principle why a theoretical proposition may not be known to be true. For in order to know that P is true, it suffices that there be good grounds for the belief that P and that P in fact be true.

More specifically, let us suppose that a scientist believes a theoretical proposition P (e.g., 'Electrons have negative charge') to be true. On the assumption that it is possible for a theoretical proposition to correctly report an actually existing state of affairs (e.g., that electrons in fact have negative charge), then it is possible for P to be true. Provided, moreover, that P satisfies appropriate methodological standards, there may be good rational grounds for the belief that P is true. Given both these assumptions, and the standard analysis of knowledge, it follows that P may be known to be true, for one may rationally believe P and P may be true. Hence, theoretical knowledge is possible.

Against this, it might be objected that one may have a justified true belief that P and yet be unable to tell that P is true. The objection arises because P is a theoretical proposition whose truth is not directly evident. For, while P may well be true, there is no direct means of knowing that this is so. At most, one may

have access to the evidence which justifies the belief that *P*. But there is no access to the truth of *P* that is independent of the evidence for *P*. Thus, even if *P* is true, and justifiably believed to be so, one may fail to be in a position to know that it is true. Given this, the fact that the conditions specified for knowledge may be fulfilled in the case of a theoretical proposition does not show that theoretical knowledge is possible.[23]

This objection rests on a confusion between conditions for the possession of knowledge and criteria for the recognition of knowledge. The justified true belief analysis of knowledge provides a set of conditions, satisfaction of which qualifies a subject as having knowledge. It does not provide criteria which enable a subject to recognise that those conditions obtain, and is thereby in possession of knowledge. Thus, it is possible for one to know that *P* without being able to recognise that one knows that *P* or that *P* is true. In short, one may have theoretical knowledge even in the absence of direct epistemic access to the truth of the theoretical proposition that is known.[24]

Such absence of direct access leads to a further potential objection to theoretical knowledge. For if there are no criteria which enable recognition of theoretical truth, then such truth may not be shown with certainty to obtain. One might then object that theoretical knowledge is not certain knowledge, and so not strictly knowledge at all. Such an objection is suggested by Laudan's previously quoted discussion of the 'epistemically utopian' character of truth, where he says that the value of truth cannot be 'operationalized' and that there is no 'criterion for detecting when a goal [e.g., truth] has been realized' (1984, p. 53). However, I am loath to attribute this objection to Laudan, since he is on record as supporting fallibilism (e.g., 1984, pp. 51, 52; 1996, p. 213), and indeed dismisses 'apodictic certainty' as a transcendental property on par with truth (1996, p. 78).[25] In any event, it is a commonplace of the philosophy of science that scientific theories are constantly subject to revision with the advance of science, so that any adequate conception of scientific knowledge must allow that one may have knowledge without certainty.

There remains an additional basis on which to object to the possibility of theoretical knowledge. Laudan might object to the present use of the justified true belief analysis of knowledge on the basis that there may be no grounds which could rationally justify a scientist in believing that a theoretical proposition is true.[26] In other words, he might deny that the grounds which provide rational support for a theoretical proposition provide support for the truth of the proposition. At first blush, this may seem an implausible objection, since, as has been noted by a number of authors, rational grounds for belief that *P* are *ipso facto* rational grounds for the belief that *P* is true.[27] For if one has grounds for the belief that *P*, then, by semantic ascent, one has grounds for the belief that *P* is true. Hence, one cannot sever rational belief from rational belief in truth in the manner that the objection requires.

There is, however, a consistent line of argument available to Laudan here. On the instrumental analysis of rules, the warrant of a methodological rule relates to the end served by the rule. Hence, since there may be no evidence that a rule conduces to theoretical truth, satisfaction of a rule may provide no warrant for

belief in such truth. Rather, satisfaction of a rule provides warrant only with respect to the end served by the rule. Thus, when the aim served by a rule is that of predictive reliability, for example, satisfaction of the rule by a theory licenses belief that the theory is predictively reliable, not that it is true. Given that justification always relates to the end served by a rule, it is therefore consistent for Laudan to hold that there may be rational grounds for a theory that are not grounds for believing that the theory is true.

However, while it may be consistently denied that a warrant need be a warrant for truth, the resulting position is unsustainable for several reasons. For one thing, it leads to an implausible restriction on the epistemic states of scientists. For if there may be no warrant for belief in theoretical truth, no scientist who accepts any theory as true may do so rationally, no matter how weighty the evidence or how well-established the theory. For another thing, it rests on an unduly narrow empiricist epistemology.[28] For if there may be no warrant for belief in the truth of any proposition that transcends empirical evidence, then all inferential or indirect knowledge is precluded due to lack of rationally justified belief. Finally, denial that methodological criteria provide warrant for truth removes the rationale for scientists' use of a plurality of such criteria in the evaluation of theories. Scientists who accept a theory which satisfies multiple criteria (e.g., predictive accuracy, explanatory breadth, simplicity, coherence) may do so because they interpret such joint satisfaction of criteria as indicating the likely truth of the theory. But in the absence of such a unifying aim served by criteria, scientists are deprived of a rationale for conjoint use of multiple criteria.

I conclude that there is every reason to suppose that theoretical knowledge is possible. Neither our lack of direct or infallible epistemic access to theoretical truth, nor the possibility of a warrant that is not a warrant for truth, entails that we are unable to have theoretical knowledge. It may not be possible to prove beyond a shadow of a doubt that a theoretical proposition is true. But that does not mean that such truth radically transcends our epistemic capacities, as Laudan suggests.

7. THE PURSUIT OF TRUTH

In this section I will consider Laudan's two objections to truth as the aim of science. As we saw in section 5, Laudan argues that truth is epistemically utopian, hence unable to serve as an object of rational pursuit. Nor is truth admissible as an aim of science, since there may be no evidence of its realisation. Since both objections depend on the transcendence of theoretical truth, they are in large part undermined by the possibility of theoretical knowledge for which I argued in the previous section. However, it remains to show this in detail.

If theoretical knowledge may be acquired by methods employed by scientists, it would seem natural to suppose that acquisition of such knowledge is a legitimate goal for science. Before further scrutinising this assumption, however, I will briefly consider the consequences of denying that theoretical knowledge is possible. One might think that if theoretical truth or knowledge were wholly unattainable, there could be no rationale for their pursuit. For it is futile to attempt the impossible.

However, as Rescher notes against Laudan, there are circumstances in which it is rational to pursue an unattainable ideal (Rescher 1982, p. 227). Moral perfection may be beyond our reach, for example, but striving for such perfection may make one a better person. Similarly, truth may function in the manner of a 'regulative ideal' for science. For, while it may be impossible for science to achieve perfection, the idea of a perfectly true theory may serve to maintain the self-corrective, evolutionary character of the scientific enterprise. In addition, the pursuit of an unattainable ideal may yield indirect benefits which are themselves otherwise unattainable. For example, it is arguably the case that the ideal of a comprehensive, true theory of the world exerts pressure on science to develop systematic theories with real explanatory breadth. Indeed, such lower level values as explanatory breadth would seem to have little independent rationale in the absence of a demand for a comprehensive, true theory.

The possibility of a regulative role and indirect benefits secures for truth a legitimate place in science even if it is unattainable by scientific means. However, if, as argued in the previous section, theoretical knowledge is possible, then truth is in fact an attainable end that lies within the reach of science. This would seem to vindicate theoretical truth as a legitimate goal of rational scientific inquiry. For, on the one hand, if truth is a realisable aim of science, it is possible for an agent to rationally pursue truth as a goal. On the other hand, the attainability of truth means that it satisfies the requirement of the instrumental conception of method that only achievable aims be allowed into science.

But Laudan's principal objection is not that theoretical truth is inappropriate as an aim because it cannot be attained. His main point is that we would be unable to recognise truth even if we were to attain it. Given this, it is not rational for an agent to pursue truth, since there are no criteria which would enable one to recognise attainment of the aim or that it is close to attainment. Similarly, it is because there may be no evidence indicating that a method yields truth that truth is excluded as an admissible aim of science.

Laudan's emphasis on the absence of criteria for the recognition of truth may suggest that he endorses the requirement, rejected in the previous section, that one must be able to recognise that one satisfies the conditions of knowledge in order to possess knowledge. But, in fact, Laudan's claim is not that ability to recognise truth is a requirement of knowledge. Rather, his claim is that it must be possible for one to recognise the fulfilment of an aim in order to rationally pursue that aim. Thus, his objection to the rational pursuit of truth is not that we are unable to possess theoretical knowledge because we cannot recognise truth. It is that we are unable to recognise whether an action furthers an aim, where the aim happens to be truth. Laudan therefore takes ability to recognise achievement of an aim as a requirement for the rational pursuit of that aim, not as a requirement for knowledge.

But, while Laudan may only require recognition criteria for rational pursuit rather than knowledge, similar considerations apply in either case. For Laudan's denial that there are criteria for the recognition of truth is only plausible on the assumption that such criteria must provide an infallible indication of truth. It may readily be conceded that there are no infallible criteria of truth. But it by

no means follows that there are no fallible criteria for the recognition of truth. While satisfaction of methodological criteria cannot decisively prove a theory to be true, it may provide good grounds for believing a theory to be true or close to truth. There may well be no criteria which enable a rational agent to know with certainty that they are advancing on truth or have attained it. Nevertheless, such an agent may justifiably believe that a theory which better satisfies the criteria than a rival theory is likelier to be true, or closer to truth, than the alternative theory. Given this, it is entirely possible for an agent to rationally pursue the goal of truth, since satisfaction of methodological criteria may provide a fallible indication of advance on that aim.

Similar remarks apply to Laudan's objection that truth is an inadmissible aim for science, since there may be no evidence that truth is realised by any method. As we saw in section 5, the objection derives from Laudan's instrumental conception of method. What motivates the objection is the thought that if a method functions in the manner of an instrument, then it is to be assessed by how well it brings about the end for which it is proposed. If there is no evidence that it performs its function, then it may not be proposed as a means to that end. The question is whether it is fair to suppose that there may be no evidence that a method leads to truth. It is perhaps true that there may be no direct empirical evidence that use of a method leads to theoretical truth. But there may surely be indirect evidence that a method conduces to such truth. For where the lower level ends served by a method are ends which themselves may be taken to subserve the aim of truth, the success of the method in conducing to such lower level ends may be taken as evidence that the methods conduce to truth. Just as there may be no infallible criteria for the recognition of truth, there may be no infallible evidence that use of a method serves truth. But that is only to say that there is no certain knowledge in theoretical matters.

Finally, a brief remark is in order regarding the basis of the objection. The objection is based entirely on the instrumental conception of method, which entails the demand for realisability. But no independent argument is given for the instrumental conception, other than that it permits the empirical evaluation of methodological rules within a naturalist framework. This is admittedly a powerful point in its favour. But, if the instrumental conception really does entail that truth is an unacceptable aim for science, this may equally well be regarded as a mark against the instrumental conception. In other words, the fact that the instrumental conception excludes truth as an allowable aim may be taken to count against the instrumental conception rather than against the aim of truth. However, since I remain unconvinced that the prospects of finding a place for truth within normative naturalism are as dim as Laudan claims, I see no need at this juncture to put the instrumental conception in question.

8. CONCLUSION

In this paper I have sought to show that a normative naturalist account of epistemic warrant may be combined with a scientific realist conception of the aim of science. On the general picture which emerges, the naturalistic basis of a non-relativist methodological pluralism may be sustained within a scientific

realist framework. As such, the present approach affords a unified account of the method of science and its progress. However, since methods may cohere with aims without promoting them, it remains to show that use of a plurality of methodological criteria advances the realist aim of truth.

Some philosophers deny there is a problem relating method to truth. Internal realists such as Ellis (1990) and Putnam (1981) define truth as maximal (or ideal) satisfaction of methodological criteria. For internalists, advance on truth is the inevitable result of the use of criteria. Truth is not something separate from method to which its use may or may not give rise. Rather, for internalists, continued application of methodological criteria produces theories which increasingly satisfy the criteria. The result is advance on truth, since truth simply is maximal satisfaction of the criteria.

As a realist, I hold that the objective world in no way depends on thought. Therefore I do not equate truth with satisfaction of criteria.[29] The relation between method and truth is not an internal or conceptual relation, but an external or synthetic one. The sole question is whether the relation is necessary or contingent. I have elsewhere defended the view that the epistemic warrant of certain enumerative inductions rests on the essential properties of natural kinds of things.[30] But while I hold that metaphysical necessity grounds the reliability of certain basic kinds of inductive inference, I do not see an analogous role for metaphysical necessity in the case of theory appraisal since the latter involves factors beyond those involved in basic induction. I take the relation between method and truth to be a contingent relation between epistemic means and ends, which may be known in the *a posteriori* manner suggested by Laudan's naturalist metamethodology.

However, as Laudan notes, no direct empirical evidence may show that use of a methodological rule yields theoretical truth. This raises the question why use of criteria of theory appraisal should be taken to promote the goal of truth. In the absence of direct evidence linking method to truth, the grounds for such a link may be at best abductive ones. More specifically, the realist claim that application of a plurality of methodological criteria leads to progress toward truth rests on an inference to the best explanation of scientific success. What best explains why scientific theories increasingly exhibit the epistemic virtues highlighted by methodological criteria is that such theories are increasingly close approximations to the truth.

In arguing this way, I seek to extend the argument of McMullin (1987) that we are warranted in taking a theory to be 'approximately true' if it exhibits 'a high degree of explanatory success' (1987, p. 59). McMullin takes the explanatory success of a theory to be determined by how well it satisfies the various methodological criteria of theory appraisal (1987, p. 54). Where a theory exhibits a high degree of explanatory success, as indicated by satisfaction of the criteria, there are good grounds to take the general kinds of entities postulated by the theory to really exist, as well as what the theory says about such entities to be broadly correct, though open to further development (1987, pp. 59, 60).

I wish to amplify McMullin's argument in two minor respects. First, I do not wish to say simply that the high degree of explanatory success of a theory, as measured by methodological criteria, permits us to infer abductively to the

approximate truth of the theory. I wish, in addition, to say that where a theory possesses an impressive range of theoretical virtues (e.g., accuracy, breadth, simplicity), the best explanation of why the theory possesses such an impressive range of virtues is that it is approximately true. Second, I wish to extend McMullin's argument by explicitly applying it to the advance of science. For where a sequence of theories increasingly satisfies the methodological criteria, the best explanation is that the sequence of theories is advancing on truth. In both these ways, then, the reason for taking continued use of methodological criteria to yield advance on truth is that this best explains why our theories increasingly satisfy such criteria. It is in this sense that what is needed to bridge the gap between method and truth is an abductive argument about how best to explain scientific success. Echoing Lakatos on Popper, one might call this 'a plea for a whiff of abduction'.

Such a whiff of abduction may seem to beg the question against Laudan's critique of the realist's success argument (Laudan 1984, chapter 5). Rebuttal of that critique is, of course, beyond the scope of this paper, but I will briefly indicate why no question is begged by the current proposal. In the first place, Laudan's critique does not impugn all use of the success argument, but only the ambitious attempt to forge a wholesale link between reference, truth and the success of science. Application on a case-by-case basis, restricted for example to entities postulated to fill specific causal roles, may escape Laudan's strictures on the success argument. In the second place, the current abduction does not proceed at the object-level from the widespread success of science to a general realist attitude toward theories, but is a metamethodological inference to an explanation of why a theory manifests a range of methodologically desirable features.

In sum, on the view I propose the realist aim of science is added to normative naturalism by an inference to the best explanation which augments lower level cognitive ends with the aim of truth. It is a fair question, of course, why truth is the best explanation. But consider the alternative. Suppose there is a scientific theory which possesses a variety of methodological virtues to an impressive degree. The theory is accurate, reliable, predicts novel facts, unifies diverse domains, and is simple and coherent. But let us also suppose that the theory is completely false. None of the entities or mechanisms it postulates exist, and it erroneously imposes unity on domains which in fact have nothing in common.

In such a situation, it is sheer luck that the theory has any success at all. This is especially the case with respect to predictive reliability. Either such success is sheer luck, or else a benevolent force makes the theory's predictions come out true despite the theory being false. Of course, there may be worlds which reward luck with predictive reliability. But our world is not a world like that. We are lucky some of the time. But if a theory is predictively reliable, the likeliest explanation is not that our world is one that rewards luck but that we have cottoned on to the way the world really is. For this reason, I claim that satisfaction of methodological criteria provides a sound but fallible indication that a theory is on the road to truth, and may even be there already.

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NOTES

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[1] This view of the task of the theory of method accords, for example, with the two ingredients of a 'rational model of scientific change' identified by Newton-Smith, viz., specification of the goal of science and of principles of theory comparison (Newton-Smith 1981, p. 4). The demand for an integrated response to both tasks is well-exemplified by Lakatos' plea for a 'whiff of induction' in Popper's treatment of the relation between corroboration and verisimilitude (Lakatos 1978, p. 159).

[2] The distinction between monist and pluralist theories of method is somewhat crude, since there are also mixed positions. John Worrall, for instance, holds that there is an invariant core of methodological principles, which remains fixed throughout change of lower level principles (Worrall 1988). The issue of methodological variance masks further complexity, as well. For, in principle, one might argue that at any one time science is governed by a single method, though this method may undergo historical variation. Conversely, one might argue that there is a plurality of methods which are historically invariant. Hence, a full taxonomy of methodological views would include variationist and invariationist versions of both pluralism and monism, in addition to mixed positions.

[3] The best-known pluralists are Feyerabend (1975), Kuhn (1977) and Laudan (1984). Elements of a pluralist methodology may be found in the work of such authors as Chalmers (1982), Ellis (1990), Lacey (1997), Lycan (1988), McMullin (1987), Newton-Smith (1981), Quine and Ullian (1970) and Thagard (1978). I defend a pluralist stance in the later chapters of my (1997a).

[4] *Terminological note:* Some comment is necessary regarding my use of the term 'methodological rule' and related expressions. A variety of terms (e.g., 'criteria', 'norm', 'principle', 'rule', 'standard', 'value') is found in the methodological literature. While there are slight differences of meaning and usage, there is no substantive difference between such terms of relevance to the issues dealt with in this paper. All such terms denote methodologically relevant factors to which appeal is made in theory appraisal and justification of theory choice. The terms might therefore be used interchangeably. However, to reduce scope for confusion I will tend instead to speak either of criteria or of rules, restricting use of related terms to contexts in which another term seems especially apt. I will understand the relation between criteria and rules to be roughly as follows: a criterion is a methodologically desirable feature of a theory (e.g., accuracy, coherence, simplicity); rules are prescriptions typically (but not necessarily) stated in linguistic form (e.g., 'avoid *ad hoc* hypotheses', 'employ double blind tests'). In general, criteria (e.g., simplicity) may be stated in an analogous form as rules (e.g., 'prefer simple hypotheses'). It is also worth noting that for present purposes no decision need be made as to whether rules or criteria are best construed as necessary and/or sufficient conditions of theory acceptance, or merely as factors of relevance to theory appraisal. Hence, I ignore as irrelevant in the present context the otherwise important distinction between rules which dictate theory choice and values which merely guide such choice (cf. Kuhn 1977, p. 331).

[5] The relationship between aims and methods is not straightforward. There is scope for a variety of different accounts of such relationships. For example, in contrast with other theories of method, the conventionalist elevates the aim of overall theoretical simplicity into the paramount methodological principle of science. On the other hand, realists and empiricists may agree on the nature of method but disagree on the aims served by the method.

[6] While the niceties of the doctrine of scientific realism are inessential to the discussion that follows, there is sufficient variation among realist authors to warrant an indication of what I take to be involved in the doctrine. I take scientific realism to involve four main tenets: (a) *axiological realism*: the aim of science is truth, and scientific progress consists in advance on that aim; (b) *anti-instrumentalism*: the unobservable entities postulated by scientific theories are conceived as real entities rather than mere predictive devices; (c) *correspondence truth*: truth consists in correspondence between what a statement says about the world and the way the world in fact is; (d) *metaphysical realism*: the world investigated by scientists is an objective reality, the existence and nature of which are independent of human mental activity.

[7] See, e.g., Ellis (1990, pp. 244–259), Kuhn (1977, pp. 321, 322), Lacey (1997, pp. 31–33), Laudan (1984, pp. 33 ff; 1996, p. 18), Lycan (1988, pp. 129, 130), McMullin (1987, pp. 53, 54) and Newton-Smith (1981, pp. 226–232).

[8] See, e.g., Feyerabend (1978, pp. 33–39, 98), Kuhn (1970, pp. 103–110, 148; 1977, pp. 335, 336), Chalmers (1990, p. 20) and Laudan (1984, pp. 39, 40, 57–59, 81, 82; 1996, p. 17).

[9] E.g., simplicity may favour one theory, coherence or breadth another (cf. Kuhn 1977, pp. 323, 324; Thagard 1978, p. 92). For qualification of the view that there may be conflict between rules, see Laudan (1996, pp. 93, 94).

[10] That methodological rules are defeasible is, of course, the main thrust of Feyerabend's opening argument in his (1975). However, the defeasibility of all rules, taken singly, does not entail that all such rules may be concurrently violated. Hence, while any particular rule may be violated in appropriate circumstances, it is rationally unacceptable to transgress the entire system of methodological rules. While perhaps not entirely explicit in Kuhn, the inviolability in general of the set of rules is in the spirit of Kuhn (1977). For related discussion, see Laudan (1996, pp. 101–105.)

[11] Explicit rejection of an algorithm of theory choice occurs in Kuhn (1970, p. 200; 1977, p. 326), and Laudan (1984, pp. 5, 6; 1996, pp. 17–19). Chalmers tacitly denies an algorithm of theory choice in his discussion of Feyerabend's critique of universal methodological rules (1982, p. 135). Brown develops a non-algorithmic conception of rationality in his (1988). Explicit formulations aside, however, rejection of an algorithm of theory choice is virtually the defining thesis of the historical school.

[12] As such, however, monism need not be immune to the challenge of relativism, since the question may always be raised of the justification of the monist's purportedly invariant method, as against another possible method. For relevant discussion, see the exchange between Laudan (1989) and Worrall (1988; 1989), as well as my (1997a, chapter 10).

[13] Epistemic naturalism is not, of course, the only approach to epistemic normativity. Among the main alternatives to naturalism in metamethodology, it is worth noting the conventionalism of Popper (1959), the intuitionism of Lakatos (1978) and early Laudan (1977), and reflective equilibrium models which trace back to Goodman (1955). For further analysis of the range of metamethodological approaches, see Nola (1987, 1999) and Nola and Sankey (this volume).

[14] See Laudan (1996, chapter 7). While Laudan's normative naturalism is well-suited for the present purpose of defeating the relativist, it is but one instance of a widespread form of epistemic naturalism. Similar views of both the nature and evaluation of methodological rules may be found in Rescher (1977) and Stich (1990). The idea that methodological rules are tools of inquiry has deep pragmatist roots, which may be traced back, for example, to Dewey's comparison of methods of inquiry with methods of farming (Dewey 1986, pp. 107, 108). Closely related views occur as well in Giere (1989) and Kornblith (1993).

[15] The role here attributed to cognitive ends by Laudan raises the spectre of a relativism due to variation of ends (cf. Psillos 1997, p. 707). However, Laudan's hypothetical imperative account of rules needs to be understood in the context of his remarks on rational adjudication of cognitive goals in his (1984, pp. 50 ff). Laudan there adumbrates a number of means of evaluating cognitive aims, e.g. by showing an aim to be utopian, or in conflict with practice. It should be allowed, therefore, that Laudan seeks to avoid relativism due to variation of cognitive aims. Whether he succeeds is another matter.

[16] As examples of cognitive aims that have been pursued by scientists, Laudan mentions infallible knowledge, high probability, simplicity, elegance, as well as Newton's attempt to reveal divine agency at work within the physical world (cf. Laudan 1984, 51 ff; 1996, p. 129).

[17] To say that science aims for truth is not to be distinguished from saying that it aims for truth about the world. Nor would I distinguish it from saying that the aim of science is knowledge (cf. Rosenberg 1990), since knowledge implies truth. Nor either would I demur if a realist were to argue that the aim of science is explanation, as Ellis (1985) does, since seeking true explanations is part of seeking the truth. (However, I would demur at Ellis' suggestion that we renounce the correspondence theory of truth in favour of a pragmatist concept thereof.)

[18] More specifically, combining the instrumental analysis of rules with the aim of truth yields a form of *method*, rather than *process*, reliabilism (cf. Goldman, 1986, pp. 93–95). However, I do not wish to endorse a pure reliabilism on which warrant is strictly identified with truth conduciveness. Such an account is subject to counterexamples, such as Lehrer's case of Mr. Truetemp, who reliably forms true beliefs about the temperature due to a device implanted in his brain, but is ignorant of both the reliability of his belief and of their cause (Lehrer 1990, p. 163). My view is roughly that reliability is a crucial part of the warrant of methodological rules, but that *use* of rules must meet additional constraints, such as being deliberately employed by a scientist on the basis of awareness of such rules.

[19] As an example of a methodological rule which immediately advances a lower-order aim, and indirectly advances the aim of truth, consider Popper's rule against *ad hoc* hypotheses. Avoidance of *ad hoc* hypotheses serves to increase the falsifiability of theories, which thereby subserves the aim of truth, since the ruthless testing of falsifiable theories is held by Popper to conduce, fallibly, to truth, or at any rate to greater verisimilitude.

[20] See Laudan (1981), reprinted as chapter 5 of his (1984).

[21] Laudan credits the point that we cannot know a theory to be true to Hume and Popper (1996 p. 194). However, he also notes (personal communication) that his point is intended to be stronger than simply saying that theories cannot be shown to be true. He refers to the latter as 'Humean underdetermination' (1996, p. 31). By contrast, his point about the transcendence of truth appears to be a strong version of what he describes as 'ampliative underdetermination' (1996, p. 43 ff). For while Laudan denies that ampliative rules of inference underdetermine rational theory choice, his claim that theories cannot be reasonably held true seems to imply that such rules underdetermine rational belief *in the truth of theory*. The grounds for this thesis would appear to be either a version of the 'pessimistic meta-induction' (cf. 1977, p. 126) or his related critique of the explanatory connections drawn by realists between scientific success and truth (1984, chapter 5).

[22] It might be objected that Laudan states the argument in conditional form, e.g. 'if we cannot ascertain when a proposed goal state has been achieved'. Hence, it is not to be interpreted as an argument against realism, but merely as an example of a possible epistemically utopian aim. However, since, as we have seen, Laudan holds truth to be transcendental, he is committed to dismissing it as an epistemically utopian aim, which cannot be rationally pursued.

[23] The present objection to the standard analysis differs from Gettier-style objections. Gettier cases show that the standard analysis fails to provide a set of jointly sufficient conditions for knowledge. By contrast, the present objection turns on lack of direct epistemic access to the truth of theoretical propositions. Incidentally, while Gettier cases show that further conditions are needed to obtain sufficient conditions for knowledge, the conditions specified by the standard analysis remain individually necessary and thereby constitute an approximately correct analysis of the concept of knowledge. Given this, it is unproblematic to treat the standard analysis as an adequate working definition of knowledge.

[24] This implies the falsity of the KK-thesis, i.e., the thesis that in order to know one must know that one knows. I take the KK-thesis to be false, since one may know without being aware that one knows, or even knowing what it is to know.

[25] However, it is not completely clear what Laudan takes to follow from fallibilism with respect to the concept of knowledge. He writes at one point that 'the unambiguous implication of fallibilism is that there is no difference between knowledge and opinion: within a fallibilist framework, scientific belief turns out to be just a species of the genus opinion' (1996, p. 213). This might be taken to suggest that knowledge has no greater warrant than any other form of belief. However, since, in the context in question, certainty is the crucial factor which distinguishes opinion from knowledge, knowledge might still be justified true belief and yet belong to the genus opinion.

[26] That this is indeed Laudan's likely objection is suggested by footnote 21 (above).

[27] The point is made specifically with regard to Laudan by Psillos (1997, p. 712). Lycan makes the point in a more general context in response to the claim that one may have evidence for P but not evidence for the truth of P (Lycan, 1988, p. 137).

[28] The point that Laudan's epistemology is unduly empiricist has been made by a number of authors, including most relevantly (Nola, 1999). It should be noted that Laudan explicitly denies the charge (1996, p. 160). But his denial is difficult to reconcile with his dismissal of theoretical truth as a 'transcendental' aim.

[29] Put simply, my reason is that epistemic theories of truth such as internal realism entail the mind-dependence of reality. For discussion, see Devitt and Sterelny (1987, pp. 195, 196), and Musgrave (1997).

[30] Roughly, the reliability and hence rationale of induction is explained by the fact that members of a natural kind possess their essential properties necessarily. The reason why we are right when we predict that an unobserved member of a kind bears the same essential property as previously observed members is that, being a member of the same natural kind as previous members, the unobserved member necessarily possesses that property. For discussion, see my (1997b), which combines Brian Ellis' recent scientific essentialism with Kornblith's account of the ground of induction.

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