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Normative naturalism and the challenge of relativism: Laudan versus Worrall on the justification of methodological principles

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Abstract *In* Science and Values, *Larry Laudan argues that rational scientific change is not restricted to scientific theory, but may also affect the methodology and axiology of science. In subsequent debate, John Worrall has raised the question of whether invariant principles of methodology are necessary in order to avoid epistemological relativism. Worrall argues that Laudan's denial of such principles leads straight to relativism. By contrast, Laudan claims that, rather than methodological invariance, what is required to escape relativism is a rational justification of such principles. In this paper, it will be argued that the* normative naturalist *meta-methodology, which Laudan has developed in work subsequent to* Science and Values, *contains the resources needed to mount a satisfactory response to Worrall's charge of relativism.*

1. Introduction

In a recent exchange,[1] John Worrall and Larry Laudan have debated the merits of the model of rational scientific change proposed by Laudan in his book *Science and Values*. On the model advocated by Laudan, rational change may take place at the level of scientific theory and methodology, as well as at the level of the epistemic aims of science. Moreover, the rationality of a change which occurs at any one of these three levels may be dependent on considerations at the remaining levels. Yet, in spite of the avowedly anti-relativistic motivation of Laudan's model, Worrall criticizes Laudan for irrevocably relativizing scientific rationality to historically variant methodological standards.

In Worrall's view, epistemological relativism is inescapable for Laudan, given the latter's rejection of fixed principles of scientific methodology. However, in reply to Worrall, Laudan accuses him of failing to understand the true nature of the challenge presented by epistemic relativism. According to Laudan, the challenge of relativism is not simply to show that methodological standards are historically invariant. Rather, it is to show that such standards may be provided with a sound epistemic justification. And this challenge arises whether or not standards are subject to variation.

As against Laudan, Worrall charges that relativism, so construed, is unavoidable, since no ultimately compelling epistemic justification of any methodological standard may be given, on pain of a sceptical regress of justifications. Laudan would presumably dispute Worrall's claim that there may be no epistemic justification of standards capable of resisting the relativist challenge. For, in a separate series of publications, Laudan has

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recently spelled out the meta-methodological position underlying his model of scientific rationality. This position, which he calls *normative naturalism*, directly addresses the issue of the epistemic justification of methodological standards. Given the apparent relevance of normative naturalism to the dispute between Laudan and Worrall, the question arises whether normative naturalism contains the resources necessary to avoid relativism.

In this paper I will consider the debate between Laudan and Worrall in an attempt to determine whether normative naturalism is able to meet the challenge of relativism which Worrall raises against Laudan's model. The next three sections of the paper are largely devoted to setting the stage. Section 2 presents Laudan's model of rationality, section 3 reviews his debate with Worrall, and section 4 outlines normative naturalism. In subsequent sections I turn to the main purpose of the paper. In section 5, I argue that normative naturalism meets the relativist demand for justification of methodological standards, while at the same time avoiding several other forms of relativism. However, in section 6, I show how a form of epistemic relativism involving a justificatory regress returns to haunt normative naturalism, as Worrall suggests. In sections 7 and 8, I present and evaluate Laudan's likely reply to this challenge.

2. Laudan's *Science and Values*

I will begin with a sketch of the relevant features of the model which Laudan proposes in *Science and Values*. Laudan takes the problem of the formation of consensus in science to be one of the key issues which have divided recent philosophy of science. Roughly stated, empiricist philosophy of science (e.g. falsificationism, logical empiricism) has a ready explanation of consensus formation in terms of shared methodological rules which function as algorithms that determine choice of theory (Laudan, 1984, pp. 5–6). But such an account leaves little room to explain how or why scientists should ever come to disagree in choice of theory. By contrast, post-empiricist philosophers of science (e.g. Kuhn, Lakatos, Feyerabend), who reject the idea of a fixed algorithmic method, have difficulty accounting for consensus formation. For their models of science contain elements (e.g. incommensurability, underdetermination, violation of methodological rules) which suggest, not only that dissensus is widespread, but that there is no rational means of bringing it to an end (*ibid.*, pp. 16–17).

Thus, while empiricists explain consensus but have a hard time with disagreement, post-empiricists emphasize dissensus at the cost of being unable to explain how agreement is arrived at. But, Laudan argues, an adequate philosophical model of scientific rationality must explain both consensus formation and the existence of widespread disagreement. Laudan's own proposal for such a model of rationality is based on his analysis of the source of the trouble facing both empiricist and post-empiricist philosophy of science. On Laudan's analysis, the trouble stems from acceptance by both schools of thought of a common model of the nature of epistemic justification in science, which he refers to as the *hierarchical model of justification* (*ibid.*, p. 23).

According to the hierarchical model, rational consensus formation in science is characterized by a hierarchy of three levels of possible agreement or disagreement (*ibid.*, pp. 23–26). At the base level of the hierarchy are opinions about matters of fact which relate to both observable and unobservable states of affairs. Disagreement arising about such matters of fact may be resolved at the second level of the hierarchy, which is the level of methodological rules. For, where methodological consensus exists, factual disagreement may be resolved by appealing to shared methodological rules. However,

where no methodological consensus exists, first-level factual disputes are unable to be decided by appeal to second-level shared rules. Resolution of such disputes requires that reference be made to the third level of the hierarchy, the axiological level, which involves the aims or goals of science. For, provided that scientists share cognitive aims, agreement may be reached by deciding which methodological rule provides the best means of fulfilling their common aims.

A serious flaw in the hierarchical model emerges in the absence of shared aims (*ibid.*, pp. 42–43). For where scientists disagree about the aims of their enterprise, no appeal can be made to common goals to resolve lower-level disputes about methodological or factual matters. Given that scientific disputes are to be resolved at a higher level, the hierarchical model does not possess the resources to explain resolution of disputes arising at the top of the hierarchy. Thus, the hierarchical model fails because it is unable to provide an account of how dispute at the level of aims may be rationally adjudicated.

To remedy this situation, Laudan proposes an alternative model on which cognitive aims are also brought within the range of rational appraisal (*ibid.*, pp. 62–64). Laudan sketches a *reticulated model of scientific rationality*, on which aims, methods and factual beliefs form a network of shifting and interdependent justifactory relations. On this model, justification runs up and down the hierarchy, rather than being restricted to descent from top to bottom. Thus, not only may aims justify methods and theories, but factual information may be relevant to the appraisal of methods, and theories provide constraints on appropriate cognitive goals. Furthermore, considerations about available methods may shape scientists' views about the attainability of specific cognitive goals. Given the reticulated nature of justificatory relations, changes that take place at one or more levels of the hierarchy may be warranted on the basis of factors obtaining at any other level of the hierarchy.

The main novelty of the reticulated model lies in the rational evaluability of cognitive aims. However, in the context of Worrall's objections, Laudan's views on the rational justification of methodological change are of greater significance. There is, of course, scope for rational methodological change within the hierarchical model, since it may be possible to determine which of competing methods better conduces to the fulfillment of a given cognitive aim. But the hierarchical model is unable to deal with all cases of such change, since it accords no role to first-level factual considerations in the evaluation of methodology. Laudan argues that factual considerations do, however, play a major role in justifying methodological change, since such considerations are often needed in order to determine whether a given method does indeed lead to a particular aim (*ibid.*, pp. 38–39). Given such a role for factual considerations, rational methodological change may occur as the result of empirical discovery (*ibid.*, p. 39) or change in theory (*ibid.*, p. 77). There are, in addition, other possible forms of rational methodological change not available within the hierarchical model; for example, where scientists adopt a novel set of cognitive aims, it may be necessary to develop new methods suited to such aims (*ibid.*, p. 57).

3. Worrall versus Laudan

In his review of *Science and Values,* Worrall objects that Laudan's reticulated model "collapses into relativism" (Worrall 1988, p. 275); thus, while the model "sounds just the ticket ... it is a ticket onto the rocks" (*ibid.*, p. 266). According to Worrall, Laudan's position leads to relativism because it allows wholesale change in the methodology of science.[2] As Worrall says,

> If no principles of evaluation stay fixed, then there is no objective viewpoint from which we can show that progress has occurred and we can say only that progress has occurred *relative to the standards that we happen to accept now.* However this may be dressed up, it is relativism. Without fixed standards, no amount of "mutual adjustment ... among all three levels of scientific commitment" can avoid it. (*ibid.*, p. 274, emphasis in original).
>
> There is an important decision to be made: either there is an invariant core ... of methodological principles or everything is open to change ... *without* such an [invariant core, Laudan's] model collapses into relativism. (*ibid.*, p. 275, emphasis in original).

As these quotes indicate, Worrall insists that "laying down *fixed* principles of scientific theory-appraisal is the only alternative to relativism" (*ibid.*, p. 265, emphasis in original). Worrall does not, however, develop the point in great detail at this point.[3] But his argument appears to turn on the assumption that without a fixed methodology there may be no "objective viewpoint" from which to judge the progressiveness of science. Presumably, the idea is that if there is a fixed methodology, which applies throughout the history of science, then the judgement that a given episode in the history of science is progressive may be based on considerations which are independent of our own particular viewpoint. But if there is no such methodology, then the judgement that a historical episode is progressive amounts at most to the judgement that it is progressive *from our point of view.* A judgement of the latter sort would reflect our local standards, rather than unchanging, universal standards. Thus, members of another community, who consider the same episode from the viewpoint of a different set of standards, might disagree with us about the progressiveness of that episode. Yet in the absence of independent standards, there is no sense in which we are right and they are wrong. Relative to local standards both are right, and there is no further question of rightness or wrongness which can be raised.

In his response to Worrall, Laudan challenges the assumption which lies behind Worrall's objection.[4] Where Worrall assumes that variation of methodology leads straight to relativism, Laudan argues that the issue of methodological variance versus invariance has nothing to do with relativism.

> The central claim of the epistemic relativist, at least where standards and methods are concerned, is not that those standards change but that—whether changing or unchanging—those standards have no independent non-question-begging rationale or foundation. Even if man had been using exactly the same inferential principles ever since the dawn of science, the relativist would doubtless ask, and properly so, "What is their justification?" ... the challenge of relativism is exactly the same whether the methods of science are one or many, constant or evolving. If we can answer that challenge, i.e. if we can show why certain methods are better than others, then we can offer a justification for the current methods of science, even if they are different from the methods of science of three centuries ago. If, on the other hand, we cannot resolve the relativist's meta-philosophical conundrum, then it will be wholly beside the point whether methods are constant or changing. (Laudan, 1989, pp. 369–370)

Laudan's point against Worrall may be summarized in the following terms. The challenge of relativism is precisely *not* to show that there are absolute standards which are invariant throughout the history of science. Rather, the challenge of relativism is to

provide a rational justification for the methodology that science uses. This is because, even if it could be shown that the same methodology has been employed throughout the history of science, the relativist challenge may still be raised against that invariant methodology. For what rationally justifies such a methodology, as opposed to some wildly different one, can hardly be *that* the methodology is historically invariant: *not* having changed throughout history is no justification for a methodology. Thus, the problem of showing that present scientific methods are rational methods arises whether or not present methods are the same as past methods. Given this, Laudan objects against Worrall that: "Sporting bumper stickers proclaiming that 'scientists always do it the same way' is a laughably feeble response to the relativist's demand" (*ibid.*, p. 370); to respond to relativism, it is "to no avail to dig in our heels and say that 'everything's okay as long as the aims and methods of science don't change'" (*ibid.*, p. 371).

In reply to Laudan, Worrall, in effect, denies that Laudan's version of the relativist challenge can be answered.

> Relativism, as *Laudan* defines it, is inevitable. There is a potential infinite regress of justification and this means that *ultimately* the only way to avoid sceptical relativism is to dig in one's heels. (Worrall, 1989, p. 381, emphasis in original)

In other words, Worrall is suggesting, the demand for the rational justification of methodological principles, which Laudan sees as the challenge of relativism, leads directly to an infinite regress, so that relativism deriving from that source is unavoidable. What the regress of justifications shows, according to Worrall, is that rationality is subject to intrinsic logical limitations.[5] These limits must simply be admitted:

> if the sceptic really presses, then the only option is, I believe, the honest admission that *ultimately* we must stop arguing and 'dogmatically' assert certain basic principles of rationality. If Laudan is right that this honest admission entails relativism, then relativism wins. (*ibid.*, p. 383, emphasis in original)

But while Worrall takes such "sceptical relativism" to be unavoidable, he denies that this is the real problem posed by relativism. Instead, he continues to maintain that the real threat of relativism stems from the claim that there are no invariant standards of scientific methodology.

In summary, then, Laudan and Worrall are fundamentally at odds over the nature of the challenge presented by relativism. Worrall maintains that the challenge of relativism is to establish an invariant core of methodological principles, on the basis of which choices of theory throughout the history of science may be objectively justified. By contrast, Laudan sees relativism as leading to a demand for an account of the justification of methodological principles, which must be applicable regardless of whether such principles are subject to variation. As we have just seen, however, Worrall takes the demand for the justification of methodological principles to involve a form of relativism that is unavoidable. It remains to be seen whether Laudan's position contains the resources to meet this form of relativism. We will return to this question after discussion of Laudan's normative naturalist meta-methodology.

4. Normative naturalism

Laudan appears not to have explicitly replied to Worrall's claim that "relativism, as *Laudan* defines it, is inevitable" (Worrall, 1989, p. 381, emphasis in original). However, it seems clear that Laudan would disagree with Worrall about the impossibility

of providing a rational justification of the methods of science which meets the challenge of relativism. This is because Laudan has developed an approach to such meta-methodological matters which is designed precisely to provide a rationale for the methods of science. In this section I will present the outlines of this meta-methodological view, and in the next four sections I will consider whether this view successfully meets the relativist challenge.

In a subsequent series of publications, Laudan has continued to develop the details of the meta-methodological position which underlies the model of scientific rationality proposed in *Science and Values*.[6] Laudan calls this position *normative naturalism*. The position is *normative* because it seeks to illuminate the nature of epistemic justification in science, and because it is prescriptive rather than merely descriptive.[7] It is *naturalistic* because it treats methodology as "continuous with other sorts of theories about how the natural world is constituted" (Laudan, 1990a, p. 44), and "as co-extensive with the sciences" (1990b, p. 315). And it is a *meta*-methodological position because it is a theory about the justification of methodological rules, rather than a mere specification of such rules (cf. Laudan, 1987a, p. 23 and 1990b, p. 315). As a naturalistic meta-methodology, normative naturalism stands in opposition to the conventionalist meta-methodology of Popper (1959, pp. 53–56) and the intuitionism previously espoused by Laudan himself (1977, pp. 158–163).[8]

The key to Laudan's normative naturalism is his analysis of the syntax and semantics of methodological rules (1987a, pp. 23–26). According to Laudan, methodological rules are to be analyzed as hypothetical imperatives stating a relation between cognitive means and ends. For example, Laudan suggests that Popper's rule against *ad hoc* hypotheses be expressed in the form of a conditional: "if one wants to develop theories which are very risky, then one ought to avoid *ad hoc* hypotheses" (*ibid.*, p. 24). On such an analysis, methodological rules constitute claims about how to attain particular goals, which rest on contingent facts about the way the world is. Such rules are therefore to be thought of as elliptical formulations of empirical claims about the world and how to find out about it. Accordingly, the truth of a methodological rule depends on a contingent state of affairs; in particular, it depends on there being a correlation between use of a given method of inquiry and attainment of a specific epistemic result (*ibid.*, p. 25).[9]

On such an analysis of methodological rules, a methodology is to be conceived as, in effect, a broadly empirical theory about how to conduct inquiry (1987b, p. 349). Because of their theoretical status, methodological rules are, like scientific theories, subject to appraisal, revision, and possible replacement, as a result of empirical considerations. Moreover, in order to provide a rational justification for such rules it may be necessary to put forward empirical evidence on their behalf. Because of their hypothetical form, methodological rules presuppose the existence of connections between particular cognitive means and ends. Thus, justification of such a rule requires evidence that the means does indeed reliably conduce to the desired end.[10] In particular, it requires evidence that a correlation obtains between use of a given method and realization of the intended epistemic goal.

At the heart of Laudan's endeavour to naturalize meta-methodology, therefore, lies the thesis that rules possessing normative force may be grounded in factual means–end relations. Moreover, one of the central motivations of his normative naturalism is to provide an account of the rational justification of methodological principles. Thus, one of the chief aims of normative naturalism is evidently to meet the relativist demand for

an epistemologically satisfactory account of methodological justification. In the next section, I will argue, as against Worrall, that there is a clear sense in which Laudan meets the relativist challenge.

5. Normative naturalism and epistemic justification

As we saw in section 3, Laudan and Worrall differ fundamentally on the nature of the relativist challenge. Worrall sees the challenge of relativism as a demand for invariant standards, whereas Laudan takes the challenge to be to show that methodological standards are justified. In this section, I will argue that normative naturalism meets the relativist challenge in the sense that it provides an account of the rational justification of methodological standards. However, as we also saw in section 3, Worrall holds that the demand for justification leads inexorably to a relativism of ultimately indefensible principles. I will consider the ramifications of this problem in the following three sections.

According to Laudan, the relativist is rightly unimpressed by the claim that the principles of scientific methodology are historically invariant. For the relativist may always reply to such a claim. "What is their justification?" (Laudan, 1989, p. 370). The question of how such principles are justified is precisely the question addressed by normative naturalism. The central thesis of normative naturalism in this regard is that the justificational basis of a methodological rule does not differ fundamentally from that of any other broadly empirical claim about the world. Given their hypothetical imperative form, methodological rules are justified by presenting evidence that the means–end relations which they presuppose do in fact obtain. Because such rules are, in effect, low-level empirical claims, providing evidence on their behalf presents no greater obstacle than does establishing any other low-level empirical claim.[11]

Laudan tends to portray methodological justification as a comparative matter.[12] While it is unclear whether methodological justification is necessarily comparative, it seems clear that it must at least in general be so. This can be seen by consideration of the prescriptive force of the rules in question. Since such rules are, in effect, recommendations on how best to achieve a desired end, what prescriptive force they possess must rest on their purportedly being the best available means to that end. Accordingly, evidence for such a rule must be evidence to the effect that it is the most effective method among the available alternatives (Laudan, 1987a, p. 26).

The comparative nature of methodological justification is particularly significant for the issue of relativism. For if one methodological rule can be shown to be better justified than another, then, as Laudan notes (1989, p. 370), it becomes possible to provide a rational justification for presently accepted scientific methods. In particular, if present methods can be shown to better promote our cognitive aims than previously employed methods, then we are justified in using present methods.

This point has immediate relevance to Worrall's initial objection to Laudan that the denial of methodological invariance leads straight to relativism. Given that one method (or set of methods) may have stronger evidential support than another, Worrall's argument that there may be no "objective viewpoint" from which to judge scientific progress breaks down. For, even in the absence of an invariant method, the transition between theories may still be progressive, for example, if a later theory satisfies a rule which has been shown to lead to a given aim more reliably than did the rule satisfied by an earlier theory. There is, moreover, no need to step outside history to make objective judgements of progress: provided only that present methods are better justified

than previous methods, we are perfectly entitled to look back on the history of science and judge that particular episodes were conducive to present cognitive aims. Nor need the variation of methodology land us in a relativity of judgements of progress to operative standards, since some standards are better justified than others.[13]

Normative naturalism also contrasts sharply with forms of relativism which deny a basis for rational choice between alternative methodological standards. One example of such relativism is relativism due to the conventional status of methodology, which Laudan ascribes to Popper.[14] Another example is the form of relativism often attributed to Kuhn, according to which methodological standards vary with paradigm, and there are no "higher" standards on which to base a choice between standards.[15] On either of these views, there is no basis on which to show that one set of standards is rationally better justified than another. Yet, precisely because normative naturalism provides scope for the epistemic justification of methodological standards, normative naturalism fails to render such justification relative in either of these senses.

In sum, normative naturalism provides an account of the justification of methodological standards by means of empirical evidence for cognitive means–end connections. As such, it avoids forms of relativism which relativize judgements of progress to variant standards, or which provide for no rational justification of methodological standards. Given that normative naturalism provides an account of epistemic justification, and that it avoids such forms of relativism, there is a clear sense in which normative naturalism meets the relativist challenge.

6. Normative naturalism and sceptical relativism

Despite having just argued that there is a clear sense in which normative naturalism meets the relativist challenge, I will now argue that there remains a sense in which normative naturalism falls prey to relativism. Specifically, I will argue that normative naturalism is subject to a sceptical regress of justifications which leads to a relativism of indefensible ultimate principles. In other words, I will argue that normative naturalism faces a severe threat of relativism, which is precisely analogous to that highlighted by Worrall's argument that the demand for justification leads to relativism. In the next section, I will consider Laudan's likely reply to this version of the relativist challenge.

Before presenting the argument, it is worth commenting briefly on the relation between scepticism and relativism. As they are usually understood, scepticism and relativism pull in opposite directions. Scepticism denies knowledge, whereas relativism makes knowledge relative to context. However, there is a form of relativism which may be derived from a classical sceptical form of argument. In particular, it may be argued along the lines of the sceptical problem of the criterion that no methodological rule or standard can be provided with an ultimately compelling rational defence. For the attempt to justify any given standard leads to an infinite regress, as the demand for justification continues to be pressed. Alternatively, it may proceed in a circle, or else grind to a halt at a standard for which no justification may be given. Yet if there is no ultimate justification of any standard, then one standard is as rationally well founded as any other. This entails the relativistic thesis that it is just as rational to proceed in accordance with one standard as any other standard that might be proposed.[16] Given the source of this form of relativism, it is not altogether devoid of significance that Worrall should refer to it as "sceptical relativism".

To see how such sceptical relativism arises with respect to normative naturalism, recall that methodological rules receive justification, according to normative naturalism,

by means of empirical evidence of cognitive means–ends relationships. Since methodological rules are to be cast in the form of hypothetical imperatives which recommend performing a given action in order to realize a particular aim, they are to be supported by evidence to the effect that performing such an action reliably leads to the aim in question. Thus, justification of such rules rests on evidence for the existence of correlations between performing a particular kind of action and achieving a particular kind of result.

Suppose, then, that evidence has been put forward on behalf of a given methodological rule to the effect that an appropriate means–end relationship obtains. Such evidence might well provide rational support for the use of the rule in pursuit of the desired aim. Nevertheless, the question immediately arises of whether acceptance of the evidence is itself rationally justified. In response to this question, further evidence might be advanced in support of acceptance of the initial evidence.[17] But, as before, this raises the question of whether acceptance of the further evidence is rational, which leads to an infinite regress. To avoid the regress, appeal might be made to the initial evidence in support of its own acceptability; but this would be to argue in a circle. Alternatively, a halt might be called at some final item of evidence for which no further justification may be given. Since neither a regress of reasons, circular argument nor dogmatic halting-point provides the original evidence with rational support, it follows that the evidence advanced on behalf of the methodological rule must ultimately fail to provide it with such support. Furthermore, since a similar argument can be employed against any rule of which evidence might be proposed, it follows that one rule is as rationally well grounded as any other.

This argument shows that the normative naturalist account of epistemic justification is open to relativist attack by means of a sceptical regress of reasons. It would therefore appear that the normative naturalist account of epistemic justification does not contain the resources to meet the challenge of sceptical relativism raised by Worrall. However, it will be considered in the next section whether there is any basis on which Laudan can respond to the sceptical relativist challenge.

7. Blocking the sceptical relativist regress

As it happens, Laudan explicitly addresses the threat of an infinite regress in the course of his discussion of the evidential basis of methodological rules (1987, pp. 25–26). He argues that the justificatory regress, which would ensue from questioning the evidential basis of a methodological rule, may be brought to an end by appeal to a general inductive principle of evidence. Thus, Laudan, in effect, anticipates the threat of sceptical relativism which Worrall raises against the demand for methodological justification.

According to Laudan, the threat of a regress arises against normative naturalism because of the need to justify the test procedures employed in providing empirical support for methodological rules. As he says,

> we could "test" a methodological rule only by taking for granted the prior establishment of some other methodological rule, which will tell us how to test the former. And that latter rule, in its turn will presumably require for its justification some previously established methodological rule, etc. (*ibid.*, p. 25)

Given the supposed need to empirically justify methods, how is the regress to be avoided?

Laudan proposes that what is needed to "block the regress" is a principle of evidence, common to all methodological theories, which may serve as "a neutral and impartial vehicle for choosing between rival methodologies" (*ibid.*). Such a principle is to be found, Laudan suggests, in our ordinary "inductive convictions about the appraisal of policies and strategies" (*ibid.*). Laudan formulates this principle as follows:

> (R_1) If actions of a particular sort, *m*, have consistently promoted certain cognitive ends, *e*, in the past, and rival actions, *n*, have failed to do so, then assume that future actions following the rule, "if your aim is *e*, you ought to do *m*" are more likely to promote those ends than actions based on the rule "if your aim is *e*, you ought to do *n*". (*ibid.*)

This is an explicitly inductivist meta-methodological principle, which licenses inference from the past performance of a method to the likelihood of its future success. In particular, provided there is empirical evidence that a given method is the most reliable means of achieving a given cognitive aim, it follows deductively from (R_1) that one ought to employ such a method in order to achieve that aim.

Given that (R_1) serves, in conjunction with the statement of a cognitive means–end relation, to entail a methodological rule, the justificatory role of (R_1) seems straightforward. Specifically, (R_1) provides the license for future application of empirically well-founded methodological rules. The question remains, however, of how (R_1) itself is to be justified.

Laudan does present (R_1) as a principle which will "block the regress". This may suggest that appeal to (R_1) would prevent the sceptic from being able to generate the infinite regress. But this seems clearly false. For one may always ask for justification of (R_1). If an argument is presented for (R_1), then the premisses of the argument may be challenged, as well as the premisses of any further argument which may be proposed in support of those premisses, and so on, *ad infinitum*.[18]

Given that scope remains for a regress, I submit that the purpose of (R_1) is not to eliminate altogether the possibility of generating a regress. Rather, (R_1) is presented as a general meta-methodological principle, which we have compelling epistemological grounds to accept. Thus, while it remains in principle possible to generate a sceptical regress on the basis of (R_1), this possibility is not to be granted any particularly great epistemological significance. Assuming that strong grounds may be advanced in favour of (R_1), the possibility of a regress constitutes, at most, the rather abstract possibility that a persistent questioner might repeatedly press the challenge of justifying reasons. But, surely, given the logic of justification, this possibility always exists.[19] Provided that (R_1) is independently well justified, however, it does not follow that a sceptical relativism of ultimate principles is the inevitable result.

8. Scepticism, induction, naturalism

The remarks with which I closed the preceding section give rise to two immediate questions. First, does (R_1) possess a sound epistemic justification? Second, is it plausible to relegate the sceptical regress to the status of a mere abstract possibility? I will briefly address each of these questions in turn.

As for the issue of justification, Laudan presents two key considerations:

> (1), (R_1) is arguably assumed universally among philosophers of science, and thus has promise as a quasi-Archimedean standpoint, and (2), quite indepen-

> dently of the sociology of philosophical consensus, it appears to be a sound rule of learning from experience. Indeed, if (R_1) is not sound, no general rule is. (1987a, p. 26)

In support of point (1), Laudan argues that (R_1) should be acceptable to the major contemporary theories of methodology, viz., inductivism, historical philosophy of science, and Popper's falsificationism.[20] Yet, even if he is right about this, it is unclear how the existence of such a consensus among methodologists of science might serve as a warrant for (R_1): "for after all, 'the whole of [methodology]' might err".[21] Moreover, for Laudan to appeal to consensus as the epistemic basis for (R_1), would seem to commit him to a *meta*-meta-methodological conventionalism not in keeping with his explicit rejection of conventionalism at the meta-methodological level.

Both these objections miss the point, however, as is evident from Laudan's remark that (R_1) "has promise as a quasi-Archimedean standpoint". Rather than philosophical consensus providing a warrant for (R_1), the existence of such consensus suggests that debate amongst philosophers over a given methodological rule is unlikely to proceed beyond (R_1). For the role of (R_1) is precisely to provide a common ground on the basis of which to forge consensus in debate about the merits of a given methodological principle. This means, first, that such debate is unlikely to degenerate into a regress of reasons; and, second, that debate is likely to focus more narrowly on the evidential credentials of the rule in question. Consensus on (R_1), therefore, does not provide the epistemic grounds for (R_1), but rather serves as neutral court of appeal which may adjudicate between rival methodologies.

This shifts the epistemic burden for (R_1) to Laudan's point (2), according to which (R_1) is "a sound rule of learning from experience". What appears to be the ground for this claim is Laudan's remark that "if (R_1) is not sound, no general rule is". And this appears, as Alexander Rosenberg has remarked, to be a form of the pragmatic justification of induction, according to which use of induction is pragmatically justified, since inductive inference will succeed, if any predictive method will succeed.[22] Now, as much ink as has been spilt over the problem of induction, it can hardly be supposed that the pragmatic justification of induction will command universal acceptance among philosophers of science. But Laudan's point is presumably not that this is *the* canonical solution to the problem of induction. Rather, what the pragmatic response establishes is a more minimal point: viz., that without at least assuming that induction works we can make no sense of learning from experience at all. For to learn from experience is precisely to be able to take past experience as a guide to the likely course of future experience.

Of course, this view of the indispensability of induction may seem to beg the question against the inductive sceptic, who demands that induction be given a non-circular justification. This brings us to the second question stated at the opening of this section. There is a parallel between refusal to provide a more substantive response to the inductive sceptic and relegation of the possibility of sceptical regress to the realm of abstract possibility. In particular, I wish to suggest that what lies behind both failures to directly resolve the problem of sceptical demand of justification is a robustly naturalistic approach to matters of epistemic justification.

More specifically, what frames Laudan's apparent refusal to provide an account of epistemic justification which would satisfy the sceptic is a rejection of the sceptic's demand of ultimate (or "higher") justification. A concern for modes of justification over and above those employed in the practice of science is notoriously absent from the

sciences. Hence, a rejection of such a demand is entirely consonant with a naturalistic insistence that the epistemic standards of the sciences provide an appropriate level of rigour for epistemology. Laudan's naturalism is therefore a crucial element in his response to sceptical attacks on epistemic justification: for it is precisely because he takes a naturalistic view of such justification that he rejects the sceptical challenge.

There is, however, more to such naturalism than a swift dismissal of scepticism in the name of science. The appearance of question-begging against the sceptic may be dispelled by reflection upon a further aspect of naturalistic thinking about justification. It has often been argued that the sceptic seeks to impose inappropriate standards on the application of epistemic concepts, which constitutes an illegitimate break with the usual standards governing our ordinary use of such concepts.[23] This claim of the inappropriateness of the sceptic's demands accords well with the naturalistic view that there is no higher form of justification of the kind sought by the sceptic. Thus, in refusing to answer the sceptic's demand for ultimate justification, Laudan's insistence on ordinary modes of justification is of a piece with his naturalism.

Finally, lest it be supposed that the refusal to meet the sceptic's demand signals a significant lowering of justificatory standards, it should be noted that Laudan's naturalist meta-methodology is intrinsically self-corrective. Not only is it possible on his view to refute particular methodological rules, but (R_1) itself rests on the contingent reliability of induction, and is itself therefore defeasible.[24] Given that normative naturalism, like science itself, is open to revision as a result of empirical inquiry, it can hardly be thought to set the standards too low.

9. Conclusion

The aim of this paper has been to evaluate Worrall's charge of relativism against Laudan's model of scientific change in light of the latter's normative naturalist meta-methodology. As we saw in section 3, however, three different senses of the challenge of relativism are at issue in the debate between Laudan and Worrall. How has normative naturalism fared on each of these versions of the relativist challenge?

In the first sense, relativism threatens due to the absence of invariant methodological standards. I argued in section 5 that, because of the comparative nature of the justification of methodological rules, one rule may be better justified than another, so that relativism does not follow from the denial of methodological invariance. The second sense of the relativist challenge, found in Laudan's reply to Worrall, is to provide an epistemic justification of methodological standards. However, as we saw in sections 4 and 5, an account of the epistemological justification of methodological rules is precisely what normative naturalism offers. The third sense of the challenge owes much to scepticism, and also employs a justificatory regress to induce a relativism of undefended ultimate principles. As I argued in sections 6 and 7, sceptical relativism poses a serious threat to normative naturalism. However, as I suggested in section 8, a response to such sceptical challenges is available to Laudan, by stressing the naturalistic rejection of sceptical assumptions about the nature of epistemic justification. Given the considerations that I have advanced in connection with each of these three points, I conclude that, while Worrall's sceptical relativism poses a serious threat, Laudan's normative naturalism does contain the resources to withstand the threat of relativism.

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Notes

1. See Laudan (1989), and Worrall (1988, 1989).
2. One might think that such an objection misses the mark, since Laudan's point is precisely that methodological changes can be justified relative to epistemic aims. But in fact Worrall is unimpressed by this aspect of Laudan's position: he rejects Laudan's hierarchical interpretation of the traditional model of rationality; and he dismisses "discussion of the aims and goals of science" as "quite unsuited to settling methodological disputes" (Worrall, 1989, p.269).
3. But it is a recurring theme in a number of Worrall's papers (e.g. Worrall, 1985), and receives further development in his response to Laudan: "the serious threat" of relativism, he says, comes from one who denies fixed standards and argues that "his own principles therefore, while admittedly different from those presently accepted by science, may even become the principles accepted by the science of the near future. So why should he now give them up?" (Worrall, 1989, p. 383).
4. It is worth remarking that, while Worrall takes Laudan to assert wholesale methodological variation in the history of science, Laudan denies that this is his position. Rather, Laudan espouses a somewhat weaker position: he claims to have shown "that some rather central methodological principles have been abandoned or significantly altered over the course of time"; and he "can see no grounds for holding any particular methodological rule ... to be *in principle* immune from revision" (Laudan, 1989, p. 371, note 6, emphasis in original). While these points clearly raise important issues about methodological variation and relativism, given the substance of Laudan's reply to Worrall, the issue of whether methodological change may be comprehensive represents something of a side issue.
5. In support of the idea that rationality has logical limits, Worrall cites the treatment by Popper (1945, vol. 2) and Bartley (1984) of the possibility of rational justification of a rationalist approach, as well as Lewis Carroll's parable of Achilles and the Tortoise.
6. Laudan (1987a, 1987b, 1987c, 1990a, 1990b). This position also informs the remarks of the pragmatist interlocutor in the dialogue in Laudan's *Science and Relativism* (Laudan, 1990c, see especially Chapter 4).
7. Thus, unlike Lakatos, Laudan takes philosophical theory of methodology to be both a theory of rational justification and a source of prescriptive advice for scientists.
8. See Laudan (1986) for his rejection of intuitionism.
9. Since it might be thought that conceptual considerations reveal that *ad hoc* modifications of theories reduce the risk of falsification, it might appear that Popper's rule against *ad hoc*ness is not the best illustration of the bearing of empirical considerations on meta-methodological issues. However, it should not be assumed that exclusively empirical evidence is required for the justification of methodological rules. While Laudan is primarily concerned to argue against the view that such justification may proceed in an *a priori* fashion, he also insists that non-empirical conceptual considerations are crucial to both science and its methodology (1990a, pp. 50–51).
10. In light of the requirement that the means *reliably* conduce to the desired end, normative naturalism might appear to be a form of reliabilist epistemology. There do, however, appear to be a number of salient differences between normative naturalism and reliabilism, at least as it is classically understood (e.g. Goldman, 1979). First, for Goldman a reliable method is one which leads reliably to truth, whereas for Laudan the cognitive ends in question are typically something other than truth. Second, reliabilism is a theory of the justification of an agent's epistemic states, whereas normative naturalism is a theory of the justification of method. Thus, rather than take a reliabilist view of individual epistemic rationality, Laudan operates with an instrumental account of rationality on which an agent's belief that an action will lead to their aim is required for the act to be rational (cf. Laudan, 1987a, p. 21).
11. As Paulo Abrantes has pointed out to me, Laudan tends to emphasize the history of science as a source of empirical data (e.g. 1987a, pp. 27–28), at the expense of, say, cognitive science or evolutionary biology,

to which other naturalistic epistemologists might be inclined to look for evidence. This is presumably because, while the latter may be perfectly good sources of data regarding perceptual and inferential processes, they are less well suited as sources of data for the performance of methodological rules in selecting successful theories. Indeed, there is a good deal of *prima facie* plausibility in the thought that the history of science should be the primary source of data concerning the track record of scientific methods.

12. For example, in the following previously quoted passage he explicitly identifies an answer to the relativist challenge with the comparative appraisal of methods: "If we can answer that challenge, i.e., *if we can show why certain methods are better than others,* then we can offer a justification for the current methods of science, even if they are different from the methods of science of three centuries ago" (1989, p. 370, emphasis added).
13. As for the point that judgements of progress depend on assumed aims, the threat of relativity to variant aims dissipates when it is recognized that aims too may be adjudicated rationally (cf. Laudan, 1984).
14. Given Popper's lifelong opposition to relativism, this claim of Laudan's may strain credibility. Nevertheless, Laudan has plausibly argued in a number of places that the conventionalist meta-methodology espoused by Popper (1959, Chapter 2) relegates the standards of scientific method to a purely conventional status (see e.g. Laudan, 1984, pp. 48–49; 1989, pp. 370–371; 1995).
15. Such an interpretation of Kuhn, which may be found, for example, in Lakatos (1970, p. 178), is suggested by combining Kuhn's claim that paradigms "are the source of the methods, problem-field, and standards of solution" (Kuhn, 1970, p. 103) with his claim that "as in political revolutions, so in paradigm choice—there is no standard higher than the assent of the relevant community" (*ibid.*, p. 94).
16. Apart from obvious similarities to the problem of the criterion, the argument I have sketched has affinities with what Bartley calls the "dilemma of ultimate commitment", which leads to what he terms "ultimate relativism" (Bartley, 1964, p. 6; 1984, pp. 72–73).
17. Alternatively, appeal might be made to a general principle of evidence, for example, one which supports the use of observation as a source of evidence or justifies the particular manner in which the evidence was collected. But similar considerations to those about to be presented in the text would then apply to such a general principle.
18. For simplicity, I overlook the possibility of circularity or calling a dogmatic halting-point to avoid the regress.
19. By the "logic of justification", I mean simply that justification has the premiss–conclusion format of a logical argument (be it inductive or deductive): what is justified appears as conclusion, and what does the justifying appears as premiss. But, since the premisses of any argument constitute undefended assumptions within the context of the argument, the premisses of any argument may always be questioned.
20. As Laudan notes, it is somewhat controversial to attribute acceptance of an inductive principle such as (R_I) to Popper. However, quite apart form Popper's "whiff of inductivism" (Popper, 1974, pp. 1192–93), it is arguable that (R_I) should be acceptable within a Popperian framework. For it could simply be said that a methodological rule, which is empirically supported by evidence of a strong correlation between cognitive means and ends, has attained a high degree of corroboration.
21. With apologies to Popper (1959, p. 29).
22. See Rosenberg (1990, p. 41).
23. Similarly, it is sometimes argued that scepticism breaks with the usual norms governing challenge to the epistemic justification of empirical claims. While at least *prima facie* grounds against a claim are ordinarily required in order to raise doubts about it, the sceptical challenge arises by pressing gratuitous demands for justification. For related discussion see Rescher (1980, pp. 169–172).
24. For the point that normative naturalism rests, in this respect, on thoroughly contingent matters, see Leplin (1990, pp. 29–30).

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