

See discussions, stats, and author profiles for this publication at: <https://www.researchgate.net/publication/229418561>

Laudan's Normative Naturalism

Article in *Studies In History and Philosophy of Science Part A* · June 1990

DOI: 10.1016/0039-3681(90)90028-7

CITATIONS

32

READS

413

1 author:



H. Siegel

University of Miami

126 PUBLICATIONS 2,049 CITATIONS

SEE PROFILE

Some of the authors of this publication are also working on these related projects:



Education's Epistemology [View project](#)

DISCUSSION

HARVEY SIEGEL*

LAUDAN'S NORMATIVE NATURALISM

Abstract — Unlike more standard non-normative naturalizations of epistemology and philosophy of science, Larry Laudan's naturalized philosophy of science explicitly maintains a normative dimension. This paper critically assesses Laudan's normative naturalism. After summarizing Laudan's position, the paper examines: (1) Laudan's construal of methodological rules as 'instrumentalities' connecting methodological means and cognitive ends; (2) Laudan's instrumental conception of scientific rationality; (3) Laudan's naturalistic account of the axiology of science; and (4) the extent to which a normative philosophy of science can be naturalized. It is concluded that Laudan's normative naturalism is as problematic as its non-normative naturalist cousins.

Introduction

RECENT epistemology and philosophy of science have been characterized by a turn toward 'naturalism'. Different authors have filled out their proposals for naturalization in different ways, of course, but for all of them naturalization involves the settling of epistemological/philosophical questions by appeal to science: for the naturalized epistemologist, to the scientific study of language learning and belief forming and sustaining; for the naturalized philosopher of science, to the scientific study of science and its practitioners. In both domains what is advocated is the scientific study of the natural processes by which beliefs and theories are acquired, maintained, and altered in the light of experience.

This turn to science has been thought by many naturalists to involve the abandonment of the normative questions concerning rationality and justification which have characterized traditional epistemology and philosophy of science. Quine, for example, is widely (though not uncontroversially) held to think that naturalizing epistemology involves replacing epistemology with psychology, and refraining from engaging in the traditional project of constructing a normative theory of evidence;¹ Giere's naturalized philosophy of

*Department of Philosophy, University of Miami, P.O. Box 248054, Coral Gables, FL 33124, U.S.A.

Received 20 June 1989.

¹The *locus classicus* is, of course, Quine (1969). Kornblith (1985, Introduction) also interprets Quine this way, as do Goldman (1986), Siegel (1980), Kim (1988), and Laudan (1984), pp. 39–40n. Laudan distinguishes his naturalism from Quine's here, and also in Laudan (1987), pp. 24–25, and Laudan (1988), pp. 347, 350.

science banishes inquiry concerning the rationality of science because rationality is a non-natural concept (Giere, 1985, 1988), and treats theory choice descriptively only. The normative dimensions of epistemology and philosophy of science are either ignored or actively denounced by most contemporary naturalists.

Views of naturalism which banish normative concerns face a severe self-reflexive difficulty: they are incapable of justifying themselves without contradiction. Non-normative naturalism cannot itself be justified naturalistically; by its own lights, therefore, it is not justified. Putative naturalistic justifications of naturalism beg the question against the non-naturalist; non-naturalistic justifications of naturalism are self-defeating (Siegel, 1980, 1984, 1989). Thus it appears that any viable naturalism must retain a normative dimension.

Larry Laudan's version of naturalized philosophy of science does retain that normative dimension. Laudan's version seeks to reconcile normativity and naturalism; he argues that naturalized methodological rules 'retain all the[ir] normative force', but 'they derive their warrant from empirical information about how this particular world is constituted. One can thus "naturalize" methodology . . . without being forced (with Quine) to believe that making it empirical and descriptive robs it of its normative force' (Laudan, 1988, p. 350).

In this paper I examine Laudan's normative naturalism. My aim is to determine whether Laudan's version of naturalism, which seemingly avoids the self-reflexive problem that undermines more standard non-normative versions of naturalism, can successfully avoid other difficulties which accrue to normative naturalism. I shall argue that Laudan's naturalism, and the conception of rationality which underlies it, faces difficulties which are at least as serious as those facing non-normative versions of naturalism.

I. Very Brief Overview

Laudan is a strong advocate of philosophy of science retaining a normative character, and his naturalism is developed against that background. '[P]roviding an epistemic warrant for accepting scientific theories', he writes, is 'an unquestionably important philosophical problem' (Laudan, 1980, p. 182).² Basic to Laudan's view is the thesis that naturalization does not preclude normativity: '... a denormativization of methodology is not entailed by its naturalization. Quite the contrary, *one can show that a thoroughly "scientific" and robustly "descriptive" methodology will have normative consequences*' (Laudan, 1987, p. 25, emphasis in original). Indeed, *Science and Values* (Laudan, 1984), in which Laudan introduces his normative naturalism, is devoted to the articulation and defence of a normative theory of the rationality of science. Basic to that theory is what Laudan calls the 'reticulation model' of scientific

²See also Laudan (1983), pp. 321–322.

rationality. According to that model, scientific (dis)agreement can occur at three levels: the factual/theoretical, methodological, and axiological levels. On the 'hierarchical model', which Laudan contrasts with his reticulation model, factual/theoretical disputes are adjudicated by appeal to methodological principles, and methodological disputes are adjudicated by appeal to axiology, i.e. to the aims or goals of science. When scientists disagree about axiology, on the hierarchical view such agreement cannot be rationally resolved, since there is nothing higher than axiology in the hierarchy to appeal to. The hierarchical model, coupled with other assumptions of a Kuhnian flavour, thus paves the way toward the conclusion that science is irrational (Laudan, 1984, chaps. 3–4). The reticulation model provides the avenue through which Laudan presents his new account of science's rationality, according to which both methodological and axiological disputes can be rationally resolved by reticulation, i.e. by mutual adjustment and justification among facts/theories, methodology, and axiology.

II. Normative Naturalism: The Empirical Character of Methodology and Science's Rationality

On Laudan's view, methodological rules and principles are best understood as hypothetical imperatives:

Scientists presumably have the methodological rules they do because they suppose that following the rules in question will bring about ... the realization of their cognitive or doxastic aims. So conceived, methodological rules are nothing but putative instrumentalities for the realization of one's cognitive ends; in a word, the rules of science are designed simply as means to cognitive ends or tools for performing a task (Laudan, 1984, p. 34).

I submit that all methodological rules should be construed not ... as if they were categorical imperatives, but rather as *hypothetical* imperatives. Specifically, I believe that methodological rules, when freed from the elliptical form in which they are often formulated, take the form of hypothetical imperatives whose antecedent is a statement about aims or goals, and whose consequent is the elliptical expression of the mandated action. Put schematically, methodological rules of the form:

(0) 'One ought to do x',

should be understood as having the form:

(1) 'If one's goal is y, then one ought to do x'.

...

Imperatives of the sort schematized by (1) above always assert a relation between means and ends. ... Methodological rules are thus statements about instrumentalities, about effective means for realizing cherished ends (Laudan, 1987, p. 24, emphasis in original. See also Laudan, 1988, pp. 349–350).

Construing methodological rules as 'instrumentalities', as hypothetical imperatives relating desired ends with effective means for realizing those ends,

immediately reveals the sense in which methodology, for Laudan, is naturalized:

Crediting or discrediting a methodological rule requires us to ask ourselves whether the universe we inhabit is one in which our cognitive ends can in fact be furthered by following this rule rather than that. Such questions cannot be answered a priori; they are empirical matters. It follows that scientific methodology is itself an empirical discipline which cannot dispense with the very methods of inquiry whose validity it investigates. Armchair methodology is as ill-conceived as armchair chemistry or physics (Laudan, 1984, pp. 39–40).

I am suggesting that we conceive rules or maxims as resting on claims about the empirical world, claims to be assayed in precisely the same ways in which we test other empirical theories. Methodological rules, on this view, are a part of empirical knowledge, not something wholly different from it (Laudan, 1987, p. 24; see also Laudan, 1988, pp. 349–350; Laudan, 1987a, p. 231).

Our final preliminary is to trace out the connections between Laudan's naturalism and his normative conception of rationality. Laudan's account of rationality is thoroughly instrumentalist:

Whatever else rationality is, it is agent- and context-specific. When we say that an agent acted rationally, we are asserting minimally that he acted in ways which he believed would promote his ends (Laudan, 1987, p. 21).

The conduct of a given inquiry will be *rational* just insofar as we have grounds to believe that that inquiry process will be likely to realize our ends ... (Laudan, 1988, p. 349, emphasis in original).

Whatever else it is, rational behavior consists in selecting actions which we believe are conducive to our ends (Laudan, 1987a, p. 227).

... an empirical approach to epistemology requires attention to precisely those normative linkages between cognitive ends and means which constitute scientific rationality (Laudan, 1984, p. 41).

Establishing that actions are conducive to the realization of our ends is an empirical matter; hence naturalism. Given that some action is in fact so conducive provides reason for engaging in that action; hence normativity (and rationality). Thus we have Laudan's normative naturalism: naturalistic inquiry tells us what we can do to further our ends, and, given that they are our ends, what we ought to do to bring about their realization. The rationality of science involves simply the 'normative linkages between cognitive ends and means'. Methodological rules linking such ends and means

retain all the normative force associated with any prudential rule of conduct, yet they derive their warrant from empirical information about how this particular world is constituted. One can thus "naturalize" methodology ... without being forced ... to believe that making it empirical and descriptive robs it of its normative force (Laudan, 1988, p. 350).

Laudan's normative naturalism centrally involves: (a) the construal of methodological rules as 'instrumentalities' connecting cognitive ends and

means; (b) an instrumentalist conception of (scientific) rationality; (c) a naturalistic, instrumentalist treatment of the axiology of science; and (d) a commitment to a thoroughgoing naturalism in philosophy of science. Each of these aspects of Laudan's position is, I believe, problematic.

III. Methodology and Instrumentality

As we have seen, Laudan treats methodological rules as hypothetical imperatives which relate cognitive ends with means which are efficacious for the realization of those ends. Such methodological rules are justified instrumentally and empirically: a methodological rule is justified to the extent that we have reason to believe that the means it recommends really are the most efficient way we know of bringing about the end;³ and empirical, scientific inquiry provides us with whatever reason to believe this that we have.

Consider the methodological rule (discussed in Laudan, 1984, pp. 38–39) which might ordinarily be stated:

Q': Prefer double-blind to single-blind experiments.

On Laudan's view, this rule is stated elliptically, for the aims which the rule is alleged to further are not mentioned. In full hypothetical form, the rule should be formulated as follows:

Q: If one wants to learn whether a drug or therapy is genuinely effective, prefer double-blind to single-blind experiments.

According to Laudan, *Q* is justified by empirical considerations which demonstrate the efficacy of double-blind experiments in realizing its stated end. If we want to know whether some particular drug or therapy is genuinely effective, we are instrumentally well-advised to conduct double-blind rather than single-blind experiments.

Why are we so well-advised? It is because we have learned that single-blind experiments, in which the researcher but not the subject knows whether the subject is receiving the experimental drug/therapy or a control, are not reliable indicators of drug/therapy efficacy. This is because researchers in single-blind experiments can and do convey to subjects their own therapeutic expectations, which render experimental results unreliable because they fail to control for subject expectation (the placebo effect). So, if we want to know whether the drug/therapy under test is efficacious, and we want to rule out the possibility that positive test results are the result not of the drug's efficacy but rather of the subject's expectations of improvement, we had better rule out subject expectations which result from unconscious transmission of the researcher's expectations to the subject. We do this by utilizing double-blind methodology,

³This is not quite right, for it neglects Laudan's account of the naturalistic justification of ends. We will consider Laudan's discussion of axiology below.

since in a double-blind study the researcher does not know whether a subject is receiving the drug/therapy under test or an inert control substance, and so has no relevant expectations to convey to the subject.

Laudan is of course correct that double-blind experiments are methodologically preferable to single-blind experiments. But it is not the fact that double-blind experimentation is more instrumentally efficacious than single-blind experimentation in achieving our goal (of finding out whether the drug/therapy really works) that justifies *Q*. If it were, then a change in goal could render *Q* *unjustified*. Imagine that we know everything we now know about single- and double-blind methodology, the placebo effect, and so on, except that: (1) subject's expectations have a real but very small effect on experimental results, and (2) the cost of double-blind experimentation greatly exceeds the cost of single-blind experimentation, so that a dogged insistence on double-blind methodology would predictively have the effect of failing to establish (by way of single-blind methodology) the efficacy of large numbers of experimental drugs/therapies. Imagine, that is, that the research community has a choice: (a) utilize double-blind experimental procedures, which control for a real but very small error resulting from the transmission of researcher expectations to subjects, at the cost of significantly restricting the number of drugs/therapies which can be tested; or (b) utilize single-blind procedures, which fail to control for real but small placebo errors, but which also allow for the testing of a much larger number of drugs/therapies. If the research community faced this choice, some members of the community might well reject *Q* for an alternative rule:

R: If one wants to learn, to an acceptable degree of approximation, whether a drug or therapy is genuinely effective, and one wants to learn this of the largest number of drugs/therapies one can, prefer single-blind to double-blind experiments.

If the research community was genuinely divided about the goals of its research as those goals are articulated in *Q* and *R*, then it would divide on the methodological question at issue here. Some would argue that *Q* is the methodological rule which should govern the community's research, since that rule is instrumentally efficacious with respect to the aims of research articulated by *Q*; others would argue that *R* is the rule which the community should embrace, because that rule is instrumentally efficacious with respect to the aims of research articulated by *R*. *Ex Hypothesi*, both *Q* and *R* are instrumentally justified methodological rules, on Laudan's account of methodological rules as 'instrumentalities' which are justified by empirical considerations showing that the rules efficaciously relate methodological means and cognitive ends.

My point in constructing this example is not to show that Laudan's naturalizing of methodology has problems when confronted with methodological rules which are instrumentally efficacious with respect to alternative, incompatible aims. Laudan's treatment of axiology will be considered below.

The point of the example, rather, is that methodological rules are not justified instrumentally. This can be seen by dropping talk of aims and goals altogether, and restoring *Q* and *R* to 'elliptical' form:

Q': Prefer double-blind to single-blind experiments.

R': Prefer single-blind to double-blind experiments.

We all agree that *Q'*, not *R'*, is the justified methodological rule. But its justification has nothing to do with its instrumental efficacy in realizing some cognitive aim. *Q'* is justified, not instrumentally, but *epistemically*: double-blind experimentation provides better *evidence* for a drug's efficacy than single-blind experimentation, because it controls for an additional source of possible error (namely, bias created by researcher expectation transmitted to subjects and manifested in the placebo effect). The fact that double-blind experimentation controls for this possible source of experimental error, which single-blind experimentation does not, establishes that *R'* is less well justified than *Q'*. *R'*'s unjustifiedness is not a function of instrumental efficacy: as the example above illustrates, single-blind experimentation can be plausibly thought to be more instrumentally efficacious than double-blind experimentation, with respect to plausible research goals. However instrumentally efficacious single-blind experimentation might be, however, double-blind experimentation controls for an additional source of possible bias/experimental error, and so affords stronger evidence for a drug's efficacy, than single-blind experimentation; for that reason, *Q'* is the justified methodological rule here. This would be so even if the research community adopted the goals articulated in *R* — that is, even if we adopted those goals, we would be more justified in believing that drug/therapy *D* really works if the evidence for that belief stems from double-blind experimentation than we would be if our belief was based on numerically equivalent data stemming from single-blind experimentation. Our belief about *D* is better justified in the first circumstance because double-blind experimentation provides better evidence for such claims and is more epistemically forceful; methodological rule *Q'* is more justified than *R'* because it affords greater epistemic weight than *R'* to the results of experiments of the sort it urges. The justification of methodological rules is an epistemic, not an instrumental, matter: in general, a rule is justified insofar as it confers warrant to the results of experiments conducted in accordance with its directives.⁴

In some circumstances we might be pragmatically justified in utilizing the less justified *R'*, for example when we have an overriding interest in testing a large number of drugs/therapies and can tolerate a real but small experimental

⁴The justification of methodological rules proceeds in accordance with relevant epistemic principles; in the case under discussion, the principle might be something like 'A methodological rule is justified insofar as it maximizes the probability that experimentation conducted in accordance with it leads to true (or valid) results'. Of course there is much more to be said about such principles and their epistemic status.

error (e.g. in an epidemic). But this would not show that R' is a better justified methodological rule than Q' . Q' is better justified because it controls for a possible source of error for which R' does not control. The greater control afforded by double-blind experimentation provides greater warrant for the results of such experimentation; the fact that double-blind methodology yields more highly warranted research conclusions establishes the methodological superiority and justifiedness of Q' . We should not confuse the (true) claim that we are pragmatically justified in some circumstances in utilizing an epistemically less well justified methodological rule, with the (false) claim that in such circumstances the less well justified rule becomes or is more justified. So long as R' 's epistemic credentials are inferior to Q' 's credentials, Q' will be a better justified methodological rule than R' , whatever our pragmatic circumstances happen to be. This is a familiar point which extends far beyond the context of methodology: as a hostage or prisoner of war I may be pragmatically justified in the extreme in believing that my government will do everything in its power to rescue me; that belief, nevertheless, may be epistemically unjustified in light of the total relevant evidence.

Notice that I am not challenging Laudan's naturalism here. He is correct that it is an empirical matter that double-blind experimentation controls for a source of possible error that single-blind experimentation does not. This should not be a surprise; evidence is typically empirical and 'natural'. The present challenge is rather to Laudan's instrumentalism. It is not the instrumental relationship of means to ends which justifies methodological rules; it is rather the epistemic features of methodological rules which affords them whatever justification they enjoy.

Laudan's instrumentalist account of methodological rules and the rational resolution of disputes concerning them constitutes an instrumentalist view of scientific rationality; it presupposes an instrumentalist view of rationality more generally. This aspect of Laudan's instrumentalism is also problematic.

IV. Instrumentalism and Rationality

Laudan models his instrumentalist account of scientific rationality on that of goal-directed rational action. He claims that a necessary condition of the rationality of purposive action is the actor's belief that her action furthers her aims, and simply applies that model of rational action to the scientist. So applied, scientific rationality is measured by the degree to which it is believed that scientific research, as guided by its methodological rules, realizes its aims:

When we say that an agent acted rationally, we are asserting minimally that he acted in ways which he believed would promote his ends. Determining that an agent acted in a manner that he believed would promote his ends may or may not be sufficient to show the rationality of his actions; philosophers will quarrel about that matter. But

few would deny that it is a *necessary* condition for ascribing rationality to an agent's action that he believed it would promote his ends (Laudan, 1987, p. 21, emphasis in original).

It is true that 'rationality' is regularly used to denote instrumental efficacy. Still, is it clear that the last mentioned belief is a necessary condition for rational action? It can plausibly be thought not — or, more modestly, it can plausibly be thought that, even if necessary, the last mentioned belief is a relatively minor part of a full and adequate account of rational action. Consider a case in which an agent believes that her action will promote her ends, when she is unjustified in believing that it will: e.g., a mother's belief that frequent insistence upon her son's regular attendance at Sunday dinner will promote her end, namely his regular attendance, in the face of massive evidence that her frequent insistence has the effect of making his attendance less frequent. In this case the mother's belief in the instrumental efficacy of her frequent insistence upon her son's attendance is unjustified; she believes that her action will promote her ends, but that belief is mistaken. Is her action rational?⁵

A different kind of problematic case is one in which an agent justifiably believes that her action will further her ends, but has very good reason not to hold those ends. Consider a father who insists that his daughter devote herself to the piano, for he wants her to be a professional pianist and correctly believes that devoted study is necessary for a career as a pianist. In this case the father correctly and justifiably believes that his insistence, and his actions which manifest his insistence, will promote his end of raising his daughter to become a professional pianist. But ought he to have that end? There are plausible reasons for thinking that he should not. For one thing, his insistence fails to promote her autonomy in that it eliminates many life-options that she would have if she weren't so slavishly devoted to the piano, but there are powerful reasons for thinking that parents should foster children's autonomy (Siegel, 1988). For another, there is ample psychological evidence that the life of a professional pianist is troubling in ways that other lifestyles are not, but ordinarily we think that parents should strive to enable their children to lead happy and satisfying, rather than frustrated, lives. In the example the father has not considered these challenges to his ends and defeated them; he has simply not thought carefully about this particular end of his. So he has an end

⁵A analogous scientific case might be this: a scientist has the goal of publishing only accurate calculations concerning her experiments, and believes that the laborious checking and re-checking of her calculations will be instrumentally efficacious in realizing this end. So she sets for herself the rule, 'always re-check calculations at least ten times before submitting work for publication', because she believes that following this rule will be instrumentally efficacious in achieving her end. But in fact she has never discovered an error after the second re-check. Is her re-checking a seventh or tenth time rational?

which is unjustified, and he acts so as to promote that end. Is his action rational?

What these cases, and cases like them, suggest is that actions can be instrumentally efficacious and yet irrational, and that the relationship between instrumental efficacy and rationality is more complex than Laudan acknowledges. If so, then we should be wary of Laudan's claim that belief in the instrumental efficacy of one's actions in achieving one's ends is a necessary condition of rational action. For even if we accept that claim, its contribution to an account of rational action will be minimal. Central to a fuller account will be two further conditions, highlighted by our two examples: for an action to be rational, the agent must not only believe that her action is instrumentally efficacious for the realization of her ends; in addition, (1) that belief must itself be justified, and (2) the ends to which actions putatively conduce must themselves be justified.

These two further conditions on rational action render an adequate account of such action less instrumental than it appears in Laudan's hands, for both conditions must themselves be met epistemically rather than instrumentally. When we ask whether the mother's belief in the instrumental efficacy of her action (in the first example) is justified, we are not asking about that belief's instrumental relationship to some aim she has; we are rather asking if the evidence warrants the conclusion that her action is instrumentally efficacious in realizing her end. Similarly, when we ask if the father's end (in the second example) is itself justified, we are not asking about the instrumental efficacy of that end with respect to other ends he might have; we are rather asking whether, all things considered, the end is one that the father ought to have. Here we are calling for reasons which bear on the justifiedness of the end under examination.⁶

Both these considerations suggest that the condition Laudan seizes upon is not so central to rationality as he supposes. With respect both to science and to action, rationality is not obviously solely an instrumental matter. We can question the rationality of instrumentally efficacious goal-directed action. I do not deny that 'rationality' is used as Laudan uses it, to denote instrumental efficacy. But the term is ambiguous: 'rationality' is also used non-instrumentally to denote an epistemic relation between claims and reasons which support

⁶This discussion suggests that Laudan's condition on rational action, even if necessary, is not central. Other kinds of cases suggest that it is not even necessary: such cases are ones in which one acts rationally not because it furthers one's ends, but because one has good reasons for doing so. Laudan's account presupposes that the only sort of good reason for acting there can be is one which instrumentally relates means and ends. But I can have good reasons for doing something even though doing it furthers no end of mine: e.g., for being kind. Laudan's account presupposes that instrumental rationality is the only sort of rationality there is; but this begs the question against theorists who argue for a more categorical, less instrumental conception of rationality. In the context of the rationality of science, the former conception is defended (for example) by Giere (1987, 1988), the latter by Siegel (1985).

them. It is ironic that it is Laudan who in the work we are considering ignores this epistemic sense of rationality, for in his earlier book he made it central:

At its core, rationality ... consists in doing (or believing) things because we have good reasons for doing so ... if we are going to determine whether a given action or belief is (or was) rational, we must ask whether there are (or were) sound reasons for it (Laudan, 1977, p. 123).⁷

If rationality is a function of good reasons, then instrumentalism cannot be the whole story about rationality, unless the only sort of good reason there can be is an instrumental reason. But we have no reason to think that instrumental reasons are the only sort of reasons there are. Indeed, on Laudan's own instrumentalist view he needs a non-instrumental species of reasons to establish instrumental efficacy:

... inquiry exemplifies the general notion of practical reason and practical action. The conduct of a given inquiry will be *rational* just insofar as we have *grounds* to believe that that inquiry process will be likely to realize our ends ... (Laudan, 1988, p. 349, second emphasis added).

What will such 'grounds' be like? They will not themselves be 'instrumentalities'; rather, they will be *evidence* — evidence that the inquiry process in question is indeed likely to allow us to realize our ends. Consider our earlier methodological rule concerning double-blind experimentation. What counts as grounds for thinking that inquiry conducted according to that rule will help us to realize our end (of establishing that tested drugs/therapies are (not) genuinely effective)? Grounds for this claim are constituted simply by empirical evidence that double-blind experimentation in fact eliminates a sort of experimental error not eliminated by other procedures of inquiry. The grounds in question are nothing but evidence that a proposition relating ends and means is true; that the sought-after relation indeed obtains. Thus to establish the instrumental efficacy of a methodological rule, we need, on Laudan's own account, non-instrumental evidence which provides good reason for thinking that an instrumental relation in fact holds between (acting in accordance with) that rule and some end of inquiry. Thus even on Laudan's own account it is not the case that the only sort of good reasons there are are instrumental ones, or that instrumental rationality is the only sort of rationality science or purposive action can have. In seeing this we see that we have good reason to

⁷As Laudan notes in the text surrounding this passage, the thesis that rationality involves good reasons is not itself a theory of rationality (although it is a constraint which such theories must satisfy); one must provide an account of the constitution of good reasons. Laudan tried to do this in his (1977); he has since repudiated that account, and in the work under discussion here offers an alternative account. I have been arguing that this new account fails; the irony here consists in the fact that Laudan's new account fails to recognize as a constraint on the theory of rationality that, as the cited passage states, such a theory must involve good reasons. If Laudan's earlier theory failed, at least it recognized this constraint. Thanks to Harold I. Brown for challenging correspondence concerning this point.

reject Laudan's instrumentalist conception of rationality, and the instrumentalist conception of methodology built upon it.⁸

V. Axiology and the Rational Resolution of Axiological Disputes

Once the instrumentalist conception of rationality is rejected, Laudan's account of axiology and the rational resolution of controversy concerning scientific ends is jeopardized. For Laudan's proposals concerning the rational evaluation of ends depend on instrumentalism.

Laudan is insistent that an account of scientific rationality include an account of the rational evaluation of putative scientific ends:

We have so far been assuming that all aims were on a par and that a methodology's task was simply to investigate, in an axiologically-neutral fashion, which means promote those aims. On this analysis, the construction of a methodology of science is the development of a set of methodological rules, conceived as hypothetical imperatives, parasitic on a given set of cognitive or epistemic ends. Yet, although this is an attractive conception of methodology, it scarcely addresses the full range of epistemic concerns germane to science. I suspect that we all believe that some cognitive ends are preferable to others. Methodology, narrowly conceived, is in no position to make those judgments, since it is restricted to the study of means and ends. We thus need to supplement methodology with an investigation into the legitimate or permissible ends of inquiry. That is, a theory of scientific progress needs an axiology of inquiry, whose function is to certify or de-certify certain proposed aims as legitimate (Laudan, 1987, p. 29).

Laudan argues that cognitive/scientific goals can be rationally evaluated; that 'there is a wide array of critical tools which we can utilize for the rational assessment of a group of cognitive aims or goals' (Laudan, 1984, p. 50). His discussion (pp. 50–66) concerns two such tools: utopianism, and failure to accord with the values implicit in communal practices and judgments.

A goal is *utopian* if proponents can offer no grounds for thinking that it can be achieved. One can criticize goals for utopianism in three different ways: a goal is *demonstrably* utopian if it can be demonstrated that it is unachievable (e.g. the goal of infallible knowledge of universal claims, given empiricist strictures on evidence and the impossibility of establishing universal claims by (finite) experience); a goal is *semantically* utopian if it cannot be cogently

⁸The account of rationality Laudan offers is unambitious:

... beyond demanding that our cognitive goals must reflect our best beliefs about what is and is not possible, that our methods must stand in appropriate relations to our goals, and that our implicit and explicit values must be synchronized, there is little more that the theory of rationality can demand (1984, p. 64).

But the theory of rationality, understood epistemically, obviously does more. It tells us why certain beliefs are our 'best' ones; it tells us why satisfying these constraints (if Laudan is right about them) constitutes good reason for thinking that that which satisfies them is rational. The theory of rationality is the theory of evidence/good reason. It strives to say what is appropriately regarded as evidence/good reason. Laudan's account of rationality is far more shallow than this.

characterized (e.g. the goal of simplicity); and a goal is *epistemically* utopian if, although not utopian in the first two senses, criteria for determining whether the goal has been achieved cannot be specified (e.g. the goal of truth given a realist, non-epistemic conception of truth) (Laudan, 1984, pp. 52–53). If a charge of utopianism can be successfully made against some putative goal, that goal has been rationally criticized and is irrationally held as a goal of inquiry.

A goal *fails to accord with the values implicit in communal practices and judgments*, or fails to reconcile theory and practice, if its espousal and advocacy is incompatible with actual scientific practice or judgment (e.g. the scientific community's explicit goal of refraining from postulating unobservables while utilizing in practice the so-called 'method of hypothesis') (Laudan, 1984, pp. 55–59). If it can be shown that an explicitly avowed goal conflicts with the goals which seem implicitly to inform judgment and practice, that goal has been rationally criticized and is irrationally held as a goal of inquiry.

Why are goals which are criticizable by these two strategies irrational? Why are these two criteria of axiological critique properly thought of as being epistemically or normatively forceful? Laudan justifies the first, the utopian strategy, in terms of a conceptual analysis of rationality:

... it is at the very core of our conception of the rational and the reasonable that anything judged as satisfying that family of concepts must, in appropriate senses, be thought to be both possible and actionable. To adopt a goal with the feature that we can conceive of no actions that would be apt to promote it, or a goal whose realization we could not recognize even if we had achieved it, is surely a mark of unreasonableness and irrationality (Laudan, 1984, p. 51).

Leaving the status of conceptual analysis aside,⁹ why should the fact that a goal is utopian count as a rational objection to it? Pursuing such a goal is irrational, for Laudan, because one would not be able to realize it, or know it if one had. That is, such a goal could not be *instrumentally* pursued. Here Laudan's presupposition of an instrumentalist conception of rationality is evident. But as we saw earlier, instrumental reasons are not the only sort of reasons there are. If we take seriously Laudan's earlier (1977) pronouncement on rationality, according to which rationality is a function of good reasons rather than instrumentality, then we see that one can pursue a utopian goal if one has good reasons for doing so — even if the goal is indeed utopian, so that one does not have a good instrumental reason for such pursuit. The point can be seen by applying it to Laudan's own view. He urges us to embrace an instrumentalist conception of rationality. Why, on his view, should we do so? Not because it is instrumental for the achievement of some other goal. Rather, Laudan offers us arguments, by conceptual analysis, which are supposed to

⁹Laudan is clear that metamethodology should on his view have both naturalistic and conceptual components. See, e.g., Laudan (1987a), p. 231.

count as good reasons for adopting that view of rationality. Suppose his arguments are sound. Then we have good reasons for adopting the instrumentalist conception of rationality. But those reasons, which justify that conception of rationality, are not instrumental; they do not suggest that adoption of the instrumentalist conception will further our ends.

We can draw two conclusion from this. First, instrumentalism cannot be the whole story about rationality, for it is itself not justified instrumentally.¹⁰ Rather, it presupposes a larger view according to which rationality is a function of good reasons, and in which instrumental reasons are only one kind among many. Second, and more relevant to the present topic, Laudan's defence of the utopian strategy depends upon the instrumentalist conception of rationality, which (as we have seen) is inadequate. A charge of utopianism shows that a goal is irrational only if the only sort of reason for pursuing a goal there can be is instrumentalist. Once we reject the instrumentalist conception of rationality, however, we see that one can have a good non-instrumental reason for pursuing a utopian goal.¹¹

The second sort of axiological challenge — the objection to a goal of inquiry that it fails to conform to the values implicit in communal practice and judgment — is unconvincing, in the absence of a fuller picture of how conflicts between goals and practices are to be resolved.

Laudan is clear that the force behind this sort of axiological challenge derives from scruples concerning (pragmatic) contradiction:

On pain of being charged with inconsistency (not to mention hypocrisy, dishonesty, etc.), the rational person, confronted with a conflict between the goals he professes and the goals that appear to inform his actions, will attempt to bring the two into line with each other (Laudan, 1984, p. 55).

Suppose I am accused of such contradiction. I can escape my difficulty either by changing my professed goals, or by maintaining those goals and changing my actions:

Whenever a case can be made that a group of scientists is not practicing what it preaches, there are *prima facie* grounds for a change of either explicit or implicit values. The change may come, of course, in either area, or in both. One may retain one's professed goals and force them to shape one's practical judgments and actions; or one may adopt a new set of explicit values that accord more nearly with the [sic]

¹⁰This is similar to the self-reflexive difficulty with non-normative versions of naturalism noted earlier.

¹¹An example might be the goal of truth. Even if we grant Laudan his claim that truth is epistemically utopian in that we can never tell whether we've got it, we may still have other reasons for recognizing it as a goal of scientific inquiry: for example, that positing it as a goal provides us with the most compelling and comprehensive overall understanding of scientific practice and theory. Of course, making good on this claim would be very difficult. The point here is simply that, if such a story could be told, we would then have a good reason for recognizing truth as a goal of inquiry, despite the sustained charge of epistemic utopianism.

one's actions and practical judgments. Whichever way it goes, the engine driving axiological change is grounded in a theory of rationality, acting to overcome a state of disequilibrium (Laudan, 1984, p. 55).

But if I can change either my explicit goals or my judgments and practices, then the charge of pragmatic inconsistency is not a challenge to one's axiology. It is a challenge rather to the set of goals and judgments/practices. If one holds that practice is primary,¹² so that in cases of conflict between goals and practices the goals must yield, then the charge would be to one's axiology. But Laudan is clear that one can meet the challenge by maintaining one's avowed goals and altering one's practices to fit. If so, the charge is not rightly understood as an axiological challenge.

In cases of conflict between goals and practices, one must decide which to change. But not any change will do; rather, one must *rationaly* decide whether (and how) to alter one's goals or one's practices. Should one argue that it is the goals which must give way — as in the case of changing goals from that of explanation by observables to that of explanation by unobservables, via the 'method of hypothesis'¹³ — one must offer reasons for thinking that it is the goals that should be changed. The rationality of the change of axiology is secured only by argument concerning the relative merits of alternative changes, including changes which restore equilibrium by changing not axiology but practices and judgments instead. Thus, the charge of pragmatic inconsistency is not rightly regarded as an axiological challenge; moreover, restoring equilibrium is not sufficient for establishing rationality. Finally, the rationality of axiological change must be seen, here as earlier, as fundamentally epistemic rather than instrumental — values and aims are rationally altered when a compelling case can be made for the adoption of alternative aims. Such a case can, but needn't, be made on the basis of instrumental considerations.¹⁴

I quite agree with Laudan that rational axiological critique is both possible and central to philosophy of science. My aim in this section has not been to deny the possibility of such critique. Rather, my aim has been to point out that

¹²As Nelson Goodman sometimes seems to do. For criticism of the view, see Siegel (1984a).

¹³As several commentators have noted, Laudan's sharp separation of methodology and aims or goals is dubious. In Laudan (1984), pp. 55–62, this case is treated as an axiological dispute; but it is clearly a methodological dispute as well. See Doppelt (1986), pp. 232–233; Brown (1986). It should also be noted that Laudan's discussion of the shift to the method of hypothesis alternates between explanation and justification of the change: one may explain the change (in part) by noting the tension between the old axiology/methodology and scientific practice; but one can justify it only by providing reasons for honouring the practice and changing the methodology/axiology to fit rather than vice versa. See Laudan (1984), p. 59, for this alternation between explaining the change and justifying it/establishing its rationality.

¹⁴Indeed, even the case for the undesirability or irrationality of inconsistency between axiology and practice, though it can be made instrumentally, is best made epistemically, by noting the relationships between the notions of consistency and good reasons. Laudan's remarks on the theory of evidence (1984, p. 98), though made in another context, acknowledge the non-instrumental character of at least some good reasons.

the two critique-tools Laudan offers here are of questionable character and force. Utopianism successfully challenges axiology only on the presumption of instrumentalism; pragmatic contradiction challenges axiology only when conjoined with further considerations which point to the axiology in question as the source of the difficulty. Both sorts of criticisms of axiology are misunderstood if taken, as Laudan takes them, as instrumentally based criticisms of cognitive goals. To fully understand the variety and force of axiological critique, one must recognize that such critique is an epistemic, not an instrumental, affair.

VI. Naturalism and Its Limits

What shall we say, then, of Laudan's naturalization of philosophy of science?

The first point to note is that Laudan's interpretation of methodological rules as conditional imperatives, which have normative force only insofar as they assert a naturalistically establishable connection between methodological means and the cognitive ends of inquiry, is problematic. As we have seen, this account of methodological rules fails to capture the fundamental epistemic character of methodology.

Methodological disputes are often not open to empirical resolution, because such disputes involve not the instrumental efficacy of alternative rules, but rather their epistemic standing. Consider the controversy concerning predesignation (Laudan, 1984, p. 36). Popper, Peirce, Whewell and others endorse the predesignation rule: test hypotheses only by new predictions drawn from them, not by post-hoc ability to explain what was previously known. Mill, Keynes, and others reject the rule. In this case, there is no axiological dispute: as Laudan says, 'All parties to the controversy would, I believe, subscribe to substantially the same cognitive aims. They seek theories that are true, general, simple, and explanatory' (1984, p. 36). If so, then on Laudan's view the dispute is an empirical one: is investigation in conformance with the rule instrumentally efficacious in the realization of those aims, or is investigation in violation of the rule more efficacious? Laudan acknowledges that this 150-year-long and on-going dispute has not been resolved by appeal to empirical evidence concerning instrumental efficacy; his explanation of the failure of empirical investigation to resolve the dispute is that the relationships between cognitive aims and methodological rules are complex:

... no one has been able to show whether the rule of predesignation is the best, or even an appropriate, means for reaching those ends. That failure is entirely typical (Laudan, 1984, p. 36).

But how could one show this? On Laudan's view one would show it by showing that following the predesignation rule is more instrumentally effica-

cious in realizing the relevant cognitive aims than rejecting it. But instrumental efficacy has nothing to do with this dispute, which rather concerns the evidential and normative standing of certain procedures. The debate about predesignation is not an empirical debate at all — which explains why it has not been resolved by empirical findings. It is rather a debate about the epistemic status of hypotheses which bear particular relationships to putative evidence. Pro-predesignationists deny that a hypothesis is warranted by evidence antecedently known; anti-predesignationists hold, to the contrary, that a hypothesis does, or at least can, gain in warrant from such evidence. The debate is not about conditional imperatives, as Laudan's instrumentalist approach maintains; it is rather a debate about the epistemic standing of the fruits of alternative methodological practices. The predesignation dispute is not an empirically resolvable dispute because it is not an empirical dispute at all. It is not the case, more generally, that 'methodological norms and rules assert empirically testable relations between ends and means' (Laudan, 1984, p. 40); consequently, construing methodological rules and norms as conditional imperatives badly mischaracterizes such norms and rules. Laudan is correct that methodology is normative. But it is normative not because it instrumentally relates means to ends, but because it is primarily concerned with the epistemic features of science (Doppelt, 1986, pp. 232–333).

Second, granting the naturalistic character of methodology fails to establish the rationality of particular rules, in the absence of an account of the rationality of the ends the rules are to realize. Without such an account, 'showing that a methodological rule is an effective means to realizing a given scientific aim will not establish the rationality of accepting the rule' (Doppelt, 1986, p. 231). Laudan responds that he has provided a naturalistic account of the rationality of aims (Laudan, 1987a, p. 232; 1988, pp. 350–352). But, as argued above, a viable account of the rationality of ends must hold that such rationality is itself established epistemically, not naturalistically. Laudan's arguments that axiology is naturalistic founder on the instrumentalist presuppositions Laudan brings to his analysis of the rational resolution of axiological dispute.

Third, even if we grant Laudan his claim that aims are naturalistically established and criticized, it should not be thought that he has thereby shown that philosophy of science has itself been naturalized. That an aim is utopian (e.g.) may be established naturalistically; that a utopian aim ought not to be pursued is not. Laudan's arguments against the pursuit of utopian aims (a) presuppose, as argued above, a problematic instrumentalism; and (b) are themselves not naturalistic but consist entirely in non-naturalistic conceptual analysis (e.g. Laudan, 1984, p. 51; 1987a, p. 227; 1988, p. 352). Thus, even if we grant Laudan his claims (as I have argued we should not) concerning the naturalistic character of both methodology and axiology, it does not follow,

and is not the case, that the normative dimension of philosophy of science is itself rightly regarded as naturalistic. The *rational* evaluation of both methodological rules and scientific/cognitive aims remains, even in Laudan's hands, a non-natural, epistemic matter. To the extent that philosophy of science retains a normative dimension, as it does in Laudan's account, that dimension will be non-natural in character.

The limits of a viable naturalism are thus quite strict. As noted earlier, evidence is very often empirical; to the extent that philosophy of science is concerned with establishing empirical relationships — for example, that a methodological rule is in fact instrumentally efficacious in achieving some cognitive end; or that an end is realizable and so not utopian — naturalistic inquiry is entirely appropriate. But to the extent that philosophy of science is concerned with normative meta-comment about such empirical relationships — e.g., that methodological rules are rightly regarded as conditional imperatives; that utopianism is rightly regarded as a rational constraint on scientific goals; or that the rationality of science is best understood instrumentally rather than epistemically — naturalism has little if anything to contribute.

Acknowledgements — I am grateful to Harold I. Brown, Edward Erwin, Shelby Hunt and Larry Laudan for helpful comment on an earlier draft.

References

- Brown, H. I. (1986), 'Review of L. Laudan, *Science and Values*', *The Philosophical Review* **95**, 439–441.
- Doppelt, G. (1986), 'Relativism and the Reticulational Model of Scientific Rationality', *Synthese* **69**, 225–252.
- Giere, R. N. (1985), 'Philosophy of Science Naturalized', *Philosophy of Science* **52**, 331–356.
- Giere, R. N. (1987), 'The Cognitive Study of Science', in N. J. Nersessian (ed.), *The Process of Science* (Dordrecht: Martinus Nijhoff Publishers), pp. 139–159.
- Giere, R. N. (1988), *Explaining Science: A Cognitive Approach* (Chicago: University of Chicago Press).
- Goldman, A. I. (1986), *Epistemology and Cognition* (Cambridge: Harvard University Press).
- Kim, J. (1988), 'What Is "Naturalized Epistemology?"', in J. E. Tomberlin (ed.), *Philosophical Perspectives, 2: Epistemology, 1988* (Atascadero, California: Ridgeview Publishing Co.), pp. 381–405.
- Kornblith, H. (ed.) (1985), *Naturalizing Epistemology* (Cambridge, Mass.: MIT Press).
- Laudan, L. (1977), *Progress and Its Problems* (Berkeley: University of California Press).
- Laudan, L. (1980), 'Why Was the Logic of Discovery Abandoned?', in T. Nickles (ed.), *Scientific Discovery, Logic, and Rationality* (Dordrecht: D. Reidel Publishing Co.), pp. 173–183.
- Laudan, L. (1983), 'Invention and Justification', *Philosophy of Science* **50**, 320–322.
- Laudan, L. (1984), *Science and Values* (Berkeley: University of California Press).

- Laudan, L. (1987), 'Progress or Rationality? The Prospects for Normative Naturalism', *American Philosophical Quarterly* 24, 19–31.
- Laudan, L. (1987a), 'Relativism, Naturalism and Reticulation', *Synthese* 71, 221–234.
- Laudan, L. (1988), 'Methodology's Prospects', in A. Fine and P. Machamer (eds), *PSA* 86, Volume 2, pp. 347–354.
- Quine, W. V. (1969), 'Epistemology Naturalized', in Quine, *Ontological Relativity and Other Essays* (New York: Columbia University Press), pp. 69–90.
- Siegel, H. (1980), 'Justification, Discovery, and the Naturalizing of Epistemology', *Philosophy of Science* 47, 297–321.
- Siegel, H. (1984), 'Empirical Psychology, Naturalized Epistemology, and First Philosophy', *Philosophy of Science* 51, 667–676.
- Siegel, H. (1984a), 'Goodmanian Relativism', *The Monist* 67, 359–375.
- Siegel, H. (1985), 'What Is the Question Concerning the Rationality of Science?', *Philosophy of Science* 52, 517–537.
- Siegel, H. (1988), *Educating Reason: Rationality, Critical Thinking, and Education* (London: Routledge).
- Siegel, H. (1989), 'Philosophy of Science Naturalized? Some Problems with Giere's Naturalism', *Studies in History and Philosophy of Science* 20, 365–375.
- Worrall, J. (1988), 'The Value of a Fixed Methodology', *The British Journal for the Philosophy of Science* 39, 263–275.