NORMATIVE NATURALISM*

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Normative naturalism is a view about the status of epistemology and philosophy of science; it is a meta-epistemology. It maintains that epistemology can both discharge its traditional normative role *and* nonetheless claim a sensitivity to empirical evidence. The first sections of this essay set out the central tenets of normative naturalism, both in its epistemic and its axiological dimensions; later sections respond to criticisms of that species of naturalism from Gerald Doppelt, Jarrett Leplin and Alex Rosenberg.

1. Introduction. Like all its fellow -isms, naturalism comes in a variety of flavors. There is ethical naturalism and metaphysical naturalism. Then there is that low-down, subversive sort of naturalism which has the temerity to challenge supernaturalism. Naturalism is unique in being the only -ism generally less familiar to philosophers than the fallacy that is named for it. On the intellectual road map, naturalism is to be found roughly equidistant between pragmatism and scientism. Monism and materialism are said to be somewhere in the same vicinity, but some of the natives dispute such claims as geographic nonsense. My own favorite flavor of naturalism is the *epistemic* variety. Epistemic naturalism is not so much an epistemology per se as it is a theory about philosophic knowledge: in very brief compass, it holds that the claims of philosophy are to be adjudicated in the same ways that we adjudicate claims in other walks of life, such as science, common sense and the law. More specifically, epistemic naturalism is a meta-epistemological thesis: it holds that the theory of knowledge is continuous with other sorts of theories about how the natural world is constituted. It claims that philosophy is neither logically prior to these other forms of inquiry nor superior to them as a mode of knowing. Naturalism thereby denies that the theory of knowledge is synthetic a priori (as Chisholm would have it), a set of "useful conventions" (as Popper insisted), "proto-scientific investigations" (in the Lorenzen sense) or the lackluster alternative to "edifying conversation" (in Rorty's phrase).

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The naturalistic epistemologist takes to heart the claim that his discipline is the *theory* of knowledge. He construes epistemic claims as theories or hypotheses about inquiry, subject to precisely the same strategies of adjudication that we bring to bear on the assessment of theories within science or common sense. Beyond these very general points of agreement, epistemic naturalism subdivides along a variety of different paths. That is more or less inevitable since—although naturalists all subscribe to the view that philosophy and science are justificationally similar—they differ mightily among themselves about precisely what are the methods appropriate to the sciences (and willy-nilly therefore about the methods appropriate to philosophy). The best known naturalist of our time, Quine, subscribes to a very austere view about the methodological strategies open to the scientist; as far as Quine is concerned, these amount exclusively to the method of hypothetico-deduction and the principle of simplicity. Others, like myself, who understand science to involve a much broader range of argumentative strategies than Quine ever allowed, have a rather less spartan view of the modes of justification permissible in a naturalistic theory of knowledge (see Laudan 1977).

But all epistemic naturalists, whether strict empiricists like Quine or broad-minded pluralists, face a challenge from virtually all the non-naturalists. The latter point out, and quite rightly too, that the theory of knowledge has traditionally had a normative and prescriptive role; indeed, at the hands of many of our forebears that had effectively exhausted the role of the epistemologist. The likes of Descartes, Leibniz and Kant were keen to say how we *ought* to form our beliefs and how we *should* go about testing our claims about the world. Science, by contrast, does not appear to traffic in such normative injunctions; it describes and explains the world but it does not preach about it.

Critics of naturalism ask rhetorically: "How, given the contrast between the descriptive character of science and the prescriptive character of traditional epistemology, can the naturalist plausibly maintain that scientific claims and philosophical ones are woven of the same cloth?" Whence arises the naturalistic fallacy in its epistemic form: descriptive claims about knowledge (of the sort we find, say, in psychology) and prescriptive claims about knowledge (of the sort one would like to find in epistemology) cannot possibly be subject to the same forms of adjudication. Is's and ought's, on this view, are on opposite sides of a great epistemic divide. Some naturalists give up the candle at this point. Quine, for one, seems to accept that there is little if any place for normative considerations in a suitably naturalized epistemology. I daresay that Quine regards his relegation of epistemology to a sub-branch of "descriptive psychology" as a matter of boldly biting the naturalistic bullet; but in my view, the abandonment of a prescriptive and critical function for epistemology—if that is what Quine's view entails—is more akin to using that bullet to shoot yourself in the foot. (Besides, where's the fun in being a naturalist, if one is not thereby licensed to commit the naturalistic fallacy?)

1.1. Naturalistic Meta-Methodology. In several publications over the last six years (especially 1984, 1987a, 1987b), I have been propounding the idea that epistemology can be thoroughly "naturalized" whilst retaining a prescriptive dimension. Those writings have provoked a variety of responses (see Doppelt 1986 and Worrall 1988), most relevantly the essays (this issue) by Gerald Doppelt, Jarrett Leplin and Alex Rosenberg, which react in various ways to claims I have made about normative naturalism. My object in this essay is to comment on some of the thoughtful criticisms that these writers have raised. Before I turn to that task, however, it might be helpful to summarize without argument the upshot of my earlier analyses. I have argued that:

- normative rules of epistemology are best construed as *hypothetical imperatives*, linking means and ends;
- the soundness of such prudential imperatives depends on certain *empirical* claims about the connections between means and ends;
- accordingly, empirical information about the relative frequencies with which various epistemic means are likely to promote sundry epistemic ends is a crucial desideratum for deciding on the correctness of epistemic rules;
- so construed, epistemic norms or rules are grounded on *theories* about how to conduct inquiry, and those rules behave functionally within the system of knowledge in precisely the same way that other theories (for example, straightforward scientific ones) do;
- by way of underscoring this parallel between epistemic rules and scientific theories, I have argued that the rules guiding theory choice in the natural sciences have changed and evolved in response to new information in the same ways in which scientific theories have shifted in the face of new evidence;
- hence, epistemic doctrines or rules are fallible posits or conjectures, exactly on a par with all the other elements of scientific knowledge.

From which it follows that a thoroughly naturalistic approach to inquiry can, in perfectly good conscience, countenance prescriptive epistemology, provided of course that the prescriptions in question are understood as empirically defeasible.

1.2. Naturalistic Axiology. As will be clear even from this brief summary, my approach to epistemology makes everything hang on the question of relations of epistemic means to epistemic ends. The situation becomes vastly more complicated when we realize that this hierarchical picture is at risk of leaving the selection of epistemic ends unaddressed. Are we to suppose, with Reichenbach and Popper, that the selection of those ends is just a matter of taste or personal preference? Or, with Aristotle and Kant, that they can be read off in a priori fashion from an analysis of cognition? Are they the same for all inquirers and all forms of inquiry, as the positivists' unity-of-method doctrine suggests, or are the historicists right that the basic aims constitutive of science vary from epoch to epoch, from science to science and, within a given science, even from paradigm to paradigm? And if the aims of science differ significantly through the course of science, how can we avoid the thorough-going relativization of epistemology which seems to follow from the acknowledgement that, when the aims of science are concerned, it is a matter of "different strokes for different folks"? What is needed in a comprehensive naturalized epistemology is not only an account of methodology but also a *naturalistic axiology*; the task of formulating the latter has been studiously avoided by naturalists from Hume to Quine, with the honorable exception of pragmatists such as Dewey.

In brief, and again in facile summary form, I have attempted to tackle this range of issues by holding that:

- the historicists are right that the aims (and methods) of science have changed through time, although some of their claims about how these changes occur (especially Kuhn's) are wide of the mark.
- the naturalist, if true to his conviction that science and philosophy are cut from identical cloth, holds that the same mechanisms which guide the change of aims among scientists can guide the epistemologist's selection of epistemic virtues.
- there are strong constraints on the aims of science which a scientist (and thus a naturalist) can permit. For one thing, he will insist that any proposed aims for science be such that we have good reasons to believe them to be *realizable*; for absent that realizability, there will be no means to their realization and thus no prescriptive epistemology that they can sustain (since epistemology is about ways and means).
- the naturalist also insists that any proposals about the aims of science must allow for the retention *as scientific* of much of the exemplary work currently and properly regarded as such. A suggested aim for science which entailed, for instance, that nothing in Newton's *Principia* was really scientific after all would repre-

sent such a distortion of scientific practice that it would be wholly uncompelling.[1]

The issue to be confronted in the remainder of this essay is how such an analysis fares in the face of the worries voiced by my three critics. In preparing this response, I originally began to write it as a one-on-one reply to each of my three critics. Two things quickly became clear however: (1) that, within the available space, I could not possibly reply to all their worries and would have to focus selectively on the most interesting ones and (2) that my critics frequently end up worrying about the same or similar issues (especially Leplin and Rosenberg) and that therefore the most suitable form for my response would be a topical and generic discussion centered around a few key themes.

2. The Axiology of Science: The Nature and Nurture of Aims. Two of my three commentators, Leplin and Rosenberg, generally agree with what I have to say about methodology but go on to voice several worries about my version of naturalistic *axiology*. Doppelt, by contrast, has grave doubts about both my methodology and my axiology. Since the aims of science loom so large in all three critics' comments, I shall examine that cluster of issues first.

2.1. Do Aims Change? Central to my reading of the epistemic enterprize, although not crucial to naturalistic epistemology per se, is the idea that the aims of science in particular and of inquiry in general have exhibited certain significant shifts through time. The thesis of the modifiability and defeasibility of the aims of inquiry, while not essential to naturalism, does provide collateral support for the naturalist approach. For what the naturalist should believe is that, whether aims change or not, they are to be appraised and assessed in the same way that other elements of our knowledge system are; establishing that aims change through time in the same fashion in which everyone agrees that theories do, reinforces the naturalist's claim that these matters are on the same footing. Leplin and Rosenberg deny that the aims of science change; well not quite, since they concede that the "subordinate" and "secondary" aims of science do change. But they are of remarkably like mind in holding that the *central* or *primary* aim of science is knowledge and that this aim has always remained the same.[2]

[1]Indeed, the only appropriate response to such a suggestion would be: "But you're not offering an aim *of science*." I have set out this argument in some considerable detail in my (1989a).

[2]Rosenberg: "The sole intrinsic goal of science is knowledge" (p. 38). Leplin: "Knowledge in one form or another [always has been and] remains [science's] overriding objective" (p. 25).

I would be the last to dispute that scientists and natural philosophers through the ages would probably all have assented to the claim that the aim of science is "knowledge"; but I think that my critics can take little consolation from that fact. For what closer inspection reveals, as Rosenberg and Leplin readily concede, is that the terse formula "science aspires to knowledge" disguises a plethora of fundamentally disparate notions. Is the knowledge science aspires to a knowledge *of causes*? In that case, we see no agreement among either scientists or philosophers. Of essences? Or of appearances? Is science seeking knowledge that is useful and practical or theoretical and esoteric? Is science after knowledge that is certifiably true or knowledge that, while perhaps false, will nonetheless allow us to save the phenomena? These are matters about which science speaks with different voices at different points in its history. Aristotle, and much of the Greek science for which he spoke, was after a form of knowledge which was certain, essential, causal, largely non-quantitative, and quite remote from practical interventions in the world (of which techné was Aristotle's archetype). Modern science, by contrast, arguably aspires to knowledge that is corrigible, eschews essences, is even willing to forego causes, is highly quantitative and confers predictive and manipulative powers on those who have mastered it.

2.2. Axiology versus Epistemology. Rosenberg says that these differences reflect, not divergent aims, but simply divergent "theories" about what knowledge is. That claim strains credulity, not to say the niceties of language. Most of us would surely agree that, if two people each say "I want *X*" and then proceed to define *X* in fundamentally different (indeed, in mutually incompatible) ways, it would be stretching the principle of charity to the breaking point to suppose that they really wanted the *same* thing but "they just had different theories about what their common goal was". Although Rosenberg's way of viewing this matter seems to me distinctly unilluminating, I am willing to grant his point that we could redescribe the great axiological debates in the history of science and epistemology as if they were debates about theories of knowledge. I readily make this concession because the general points I want to make about these debates would be precisely the same in either case. It is, I think, largely a quarrel of words[3] so long as one agrees that—whether we call it cognitive axiology or scientific epistemology—this creature has changed through time and that those changes in it have been wrought by the same sorts of factors that drive ordinary theory change in the sciences. What I believe separates both these versions of naturalism from other views

[3]As Leplin points out: "what one counts as a change of aims, another counts a change of method; and another, a change of substantive, empirical belief" (p. 28).

about the nature of knowledge is their joint insistence that claims about the aims *or* nature of knowledge are not different in kind from ordinary scientific theorizing.

2.3. The Empiricist/Naturalist Confusion. One worry that both Rosenberg and Doppelt voice is that the naturalizing move may lead to a *narrowly empiricized meta-philosophy*. Surely, they say, there is more to evaluating philosophical claims about knowledge than simply looking at the empirical evidence for them. Indeed, Doppelt engages in much hand-wringing because he thinks he sees a fundamental tension between (a) my naturalism about methods—which he supposes to be rigidly empirical—and (b) my axiology—which (on his reading) chiefly utilizes conceptual criteria. Doppelt is fundamentally wrong on both counts; as I shall show in detail shortly, it is not true that (my version of) naturalistic methodology limits the methodologist's resources to narrow questions of empirical evidence nor is it true that my naturalistic axiology relies exclusively on non-empirical considerations. That Doppelt manages to get both these key elements of my analysis wrong unfortunately vitiates most of the other critical points he makes in his essay, since the latter are generally parasitic on this misreading.

(a). Is Naturalistic Meta-Methodology Narrowly Empirical? As I have already noted, the answer to that question depends on one's response to a prior question; to wit, whether the methods of science are narrowly empirical. The naturalist, recall, need be no more an empiricist about philosophical claims than he is about scientific ones since his naturalism amounts only to the assertion that science and philosophy are epistemically of a piece.[4] Those familiar with my views about the nature of theory choice in science (as developed in *Progress and Its Problems*) will know that I have gone to some pains to argue that science is no narrowly empirical sort of undertaking. The analysis and resolution of what I have called "conceptual problems" are every bit as central to scientific progress as the solution of empirical problems is. Lest someone suppose that I had of late abruptly abandoned those earlier views, I tried to make the point as explicitly as I could in a recent essay on naturalism, in which I stated:

> I am not claiming that the theory of methodology is a wholly empirical activity, any more than I would claim that theoretical physics was a wholly empirical activity. Both make extensive use of conceptual analysis as well as empirical results. But I do hold that meth-

[4]When Leplin cautions me not to "forget that much of science is not fully naturalized" (p. 27), I think he must be supposing that naturalism is indistinguishable from empiricism. Those two need not be regarded as identical and I for one refuse to do so.

odology can be and should be as empirical as the natural sciences whose results it draws on. (1987b, p. 231)

It is instructive to compare this passage with Doppelt's charge that "[Laudan's] naturalistic approach to methodological choice ignores the central role of logical and conceptual anomalies . . ." (p. 15). I find it more than a little strange that Doppelt saddles me with the view that methodology is a strictly empirical affair! Of course, I think nothing of the sort. If, in my recent discussions of the status of methodological rules, I have given greater prominence to the fact-sensitivity of those rules, that has been because no one (least of all myself) disputes the relevance of conceptual factors in methodology.

(b). Is Naturalistic Axiology Chiefly Non-Empirical? Doppelt, having supposed that I thought methodology was purely empirical, compounds the interpretive crime by further imagining that I treat the aims of science as subject only to purely conceptual analysis. That indeed is the basis for his charge that "the model of axiological change to which [Laudan] is drawn is not one for which his present naturalistic framework is very appropriate or promising" (p. 5). That is also why he later opines that my naturalism about methods "is largely irrelevant" to my views on axiology (p. 5). What leads Doppelt to suppose that the only constraints I put on aims are those of conceptual coherence? It is true that I stress that inconsistent or incoherent aims ought to be rejected, but so should similarly afficted rules and theories. But, I went to some lengths to argue in *Science and Values* that the discovery of the *non-realizability* of certain aims (a discovery which frequently emerges *from empirical research*) is a powerful instrument driving the change of aims.[5] By the same token, the most straightforward way of exhibiting the realizability of an aim is by showing that it has been realized, and that is a pretty straightforwardly empirical matter. I must confess to finding it more than a little ironic that, at a time when I am being roundly criticized by some (including Leplin, as we shall see shortly) for having suggested that we need empirical evidence for the realizability of our cognitive aims, Doppelt is merrily supposing that I am an a priorist about aims.

[5]I argued in my (1984) that "the rational adoption of a goal or aim requires the prior specification of grounds for belief that the goal state can be achieved" (p. 51). It is beyond my ken to imagine how statements of this sort can be read as giving "much greater prominence to what [Laudan] once called 'conceptual problems' than to 'empirical problems' in the development of science" (Doppelt, p. 5). The use of empirically grounded scientific theories to delimit the class of permissible aims was at the core of "reticulated model" of scientific rationality which that book described.

2.4. The Warranting of Aims. Leplin poses an apparent paradox for my position. He says that (a) my insistence that proposed aims for science must establish their credentials as realizable before they are acceptable is difficult to reconcile with (b) my assertion that the aims of science change through time. Leplin's paradox would seem to arise as follows: if we suppose that past science was conducted to promote certain aims, A, which are different from aims, A', now under consideration, then it seems impossible to get any evidence whether A' is realizable since by hypothesis no one has hitherto attempted to realize A'. The worry is that, by demanding that we judge the realizability of a set of aims before anyone has actually tried to promote them, I am creating a situation in which genuinely new aims can never establish their credentials in the required manner. This constitutes what Leplin calls "the strange conservatism" of my position.

The apparent force of Leplin's argument depends on an unstated premise; to wit, that the only goals achieved by past science were the goals past scientists were actually aspiring to achieve. But there are two salient facts which should warn us off any such silliness. The first point (and it applies equally to all goal-directed behavior) is that *actions always have unintended consequences*. Although obvious to the point of being a cliché, this point is crucial for my purposes. For one of the primary historical engines driving axiological change (in my view) has been the emergence of theories that, on subsequent reflection, are seen to exhibit traits that come to be regarded as genuine epistemic virtues, *even though those traits were not the virtues sought by the initial propounders of the theories in question*. To consider but one of many examples, it is clear that Newton, in developing his mechanics, was *not* seeking a theory that would yield surprising predictions; but of course, his theory nevertheless made such predictions in abundance. To later physicists, the fact that Newtonian mechanics did make such surprising predictions exhibited the realizability of their new axiological program to make such predictions one of the aims of scientific theorizing, even though Newton himself clearly had no such intentions.

Leplin may think that this was just a lucky accident, a historical fluke, and that one ought not make the aims of science hostage to the vicissitudes of previous history of science. But—and this brings me to the second part of my reply to Leplin's paradox—I see it as no accident at all, but rather, as revelatory of something fundamentally conservative about the kinds of axiological change of which science admits. In showing why it is no accident, I shall also explain why I reject Leplin's suggestion that the basic aims of science must be relatively stable lest (as he puts it) "scientists of periods separated by axiological change [could not] recognize one another as engaged in a common enterprise".

As I have argued in detail elsewhere (Laudan 1989a), an empirically successful enterprise (such as natural science has been for the last three centuries) comes to establish for itself a canonical representation of its past. The great historical moments, the triumphant theoretical innovations, and the classical experiments all come to be part of the essential folklore of the discipline. Now, in my view, what allows physicists (or chemists or geologists) to recognize one another as engaging in a common enterprise is not necessarily that they agree about the aims of their science; rather, they see that they share the same genealogy and that they look to the same canonical achievements. They may describe those canonical moments in different ways; indeed, if the scientists in question have different aims, they are apt to do precisely that. (Witness the fact that instrumentalists, realists, positivists and neo-Kantians alike look to Galileo and Newton as two of their own, even though they differ widely about how to characterize what they find virtuous in Galileo and Newton.)

Hence what establishes the communal in the scientific community is the overlapping canon of great science. What that shared canon also does—and here we return to Leplin's worry about how to get inductive evidence for goals that have not yet been explicitly propounded—is to *serve as certifier or de-certifier for new proposals about the aims of science*. One may plausibly propose a new aim for science, even one that has never been explicitly espoused or deliberately sought! But the manner in which the credentials for that aim are established involves showing that the canonical achievements of the science in question can be preserved as achievements *under that description*. In attempting to show that the canon can be preserved under a new axiological regime, one will have to explore whether the existing canon exhibits instances of the realizability of the new aims. But that is just a special case of "getting empirical evidence for the realizability of one's aims". Once we realize that trial aims for science are vetted by this process, we come to see (1) that scientists can be held to have much in common, even when they disagree about fundamental aims, (2) why it is non-paradoxical to insist on having empirical grounds for asserting the realizability of a set of aims and (3) that my "strange conservatism" mirrors the necessity for looking to a common past in the face of axiological disarray.

I suppose that Leplin might respond to such arguments by saying that his central point did not depend upon whether past scientists actually were aiming at a certain goal (that is, he might grant the point about unintended consequences) but nonetheless argue that my position is stultifying by virtue of its limiting the espousal of new goals to those which have already been realized, even if inadvertently. Perhaps this is what he intends by remarking on my "strange conservatism". If that is what he has in mind, then two brief comments would be in order: (1) Science *is* a highly conservative enterprise where aims and methods are concerned: aims are changed only reluctantly and in the face of very persuasive considerations. So, far from being troubled by the corollary that my approach makes science partially change-resistant, I regard my ability to explain the difficulty of changing scientific aims to be one of the strengths of the program. (2) Not to put too fine a point on it, I confess to finding it richly ironic to be labeled a conservative about aims by someone like Leplin who holds that, in the deepest sense, the aims of science have remained fixed since at least the seventeenth century.

3. Priority Dispute: Rules or Theories? On the traditional view of the relation between methodological rules and scientific theories (as developed, say, by Popper and Reichenbach), rules were justificationally prior to theories since rules justified our theory choices and not vice versa. I have argued that such claims for the priority of rules over theories disguise the fact that rules are in their turn justified by pointing to certain presumed facts of the matters, namely, theories. In the "reticulated" model that I described in *Science and Values*, I asserted the *justificational interdependence* of rules and theories and thus rejected the older hierarchical picture that had put rules and standards above theories in the scheme of things. Rosenberg wants to go me one better. Although he accepts my criticism of the older hierarchical model, he wants to replace it, not with a model that puts rules and theories on the same level, as it were, but with a new, *inverted* version of the old hierarchy. Theories, for him, are justificationally prior to rules. His arguments on this score raise several interesting questions about the nature of naturalism, which make the topic worth exploring here even if, as I suspect, he is wrong in seeking to make theories prior to rules. Rosenberg's argument is worth quoting in full. He writes:

> Theories . . . take a priority over rules, for they are a part of the causal explanation of the success and failure of methodologies, why they work when they do, and why they fail when they do. By contrast, a methodological rule cannot causally explain a theory. The rule cannot show *why* it is true. For the success of a methodological rule is not one of the factors that determine the truth of a theory . . . [it] is no part of the truth conditions . . . of the theory. (p. 36)

Let us suppose that Rosenberg is right both that theories can often be used to explain why methodologies work so well and that methodologies are no part of the truth conditions of a theory. Even then, neither of these points establishes the priority that Rosenberg is keen to assert. The confusion arises because, as the quoted passage makes clear, Rosenberg is running together semantic and justificational issues which probably ought to be more cleanly separated. Note that what Rosenberg says that theories do for methodological rules is to explain "why they work" and "why they fail". But, when he turns to look at the converse side of the relation, he charges that methodological rules are no part of the "truth conditions" for theories. Well, unless we intend to stack the deck ruthlessly, *why should we expect methodologies to do more for theories than theories do for methodologies?* Let us ask, in the spirit of Rosenberg's initial pragmatic concern, whether methodological rules explain why theories "work when they do, and why they fail when they do"? Provided there is parity here, Rosenberg's passing reference to theory semantics will be seen to be red herring.[6] If, as I will presently try to show, the answer to that question is affirmative, then it follows that we should regard theories and methodologies as on all fours justificationally rather than as hierarchically ordered, either in the manner of the positivists or the very different manner of Rosenberg.

How do rules explain the success of theories? To put the answer very schematically: if we have a rule, R, which has already demonstrated its credentials at selecting empirically successful theories, then we can explain why any particular theory, T, it has recently selected works well by pointing out that T was picked out by R and that R has shown itself to be the sort of rule which identifies theories likely to stand up successfully to further testing. Similarly, we can often explain why certain theories (for example, creationism) fail by pointing out that they were picked out using a rule of selection ("assert whatever Scripture asserts") which has not proved very impressive.

I daresay that Rosenberg will fault such "explanations" on the grounds that they make no reference to what it is *causally* that makes the theory so successful. It is, he might say, one thing to explain why we have selected a particular theory and quite another to explain why the theory works. But that is to ignore the fact that modes of selection are routinely used to explain the success of selected outcomes. Suppose someone asks: "Why are Olympic runners more successful than the runners in the local high school?" One answer to that question might involve a long disquisition on the respective anatomy and physiology of the two groups. A very different answer would involve pointing out that the procedures for selecting entrants to the Olympics filters out all but world-class runners whereas the local track team is selected by an indifferent coach with little talent to choose among. Similarly, if we were to ask why there are now so many fewer deaths from the side-effects of medicines than there used

[6]In fact, I think that one could also establish semantic parity for rules and theories as well since (in my view) capacity to be selected by a proper rule of inquiry *is* part of the truth conditions of a theory. But showing that here would take me too far afield.

to be, the most salient explanation would not involve a detailed discussion of the toxicity of the chemicals making up specific prescription drugs; it would, rather, require one to discuss current methods used for the clinical trials of drugs and to contrast those methods with earlier drug-vetting techniques. These two models that we use for explaining why theories work (that is, in terms of their mode of selection or in terms of their underlying causal mechanisms) are interestingly different. But one can grant those differences and still maintain that rules of selection can explain the successes of theories every bit as convincingly as theories can be utilized to explain the success of methodological rules.

What hangs on this dispute (which I view as an in-house debate among naturalists) is how one construes the naturalistic project. Rosenberg's desire to make theories more fundamental than methods of inference reflects a hankering to make the normative thoroughly parasitic on the descriptive. I am more inclined to see normative and descriptive concerns interlaced in virtually every form of human inquiry. Neither is eliminable or reducible to its counterpart; yet both behave epistemically in very similar ways, so that we do not require disjoint epistemologies to account for rules and theories. He and I agree that a naturalist has no business propounding different epistemologies for different subject matters. He avoids that problem by a purported derivation of the normative from the descriptive. That is surely a time-honored naturalistic maneuver. But I want to assert that normative and descriptive claims are on the same epistemic footing without holding that either is more fundamental. After all, the theories that Rosenberg will invoke to underwrite his rules are themselves to be justified by showing that sound rules of inference sanction their use. Under such chicken-and-egg circumstances, one is well advised to be leery about asserting the justificational primacy or priority of either member of the pair.

4. Underdetermination and All That. The most extended argument in Doppelt's essay addresses my claim that methodological rules are best understood as *hypothetical imperatives*—asserting contingent, empirically defeasible linkages between means (rules of appraisal) and ends (cognitive values). Doppelt's reduction goes as follows: if (as I claim) the soundness of methodological rules depends on associated empirical hypotheses about the relations between means and ends—if, that is, such questions are in principle empirically decidable—then why is it that scientists and philosophers still disagree about the correctness of certain methodological rules (such as the rule of predesignation)? He takes it to be an indictment of a naturalistic view of such rules that we have yet to bring all debates about methodological rules to closure. He concludes his analysis with the observation that "the pervasive absence of empirical evidence showing that one among these competing methodologies [namely, methodologies which assert or deny the soundness of the rule of predesignation] is a more effective means to shared cognitive ends than others, calls into question Laudan's whole reading of these methodological rules as hypothetical imperatives" (p. 13).

Doppelt's argument is doubly misleading. In the first place, by focusing on one of the most contested principles in recent philosophy of science (the rule of predesignation), his discussion ignores the very large areas of agreement within science and epistemology about methodological matters. It is rather as if one were to argue that, since there are certain matters which *scientists* have not been able to resolve definitively (for example, whether Pandas are raccoons, whether asteroids are composed of material foreign to the solar system, whether the universe is homogeneous in all directions, etc.), science itself must not be an empirical discipline. I daresay that no one, including Doppelt, would attempt to argue that the persistence of controversies of this sort "calls into question whether scientific theories make empirical claims". Yet that is precisely the argument that Doppelt attempts to run vis-à-vis methodological rules. The fact that certain theoretical disputes and certain methodological disputes have yet to be resolved is no argument whatever against the empirical character of those disputes. It is obvious that, especially when the relevant evidence base is small, there is a significant degree of underdetermination of our theoretical claims by the available evidence. The (arguably temporary) non-closure of debate between certain rival claims in both science and methodology tells us nothing whatever about whether those claims are ultimately empirical or not.

The second fact Doppelt overlooks is that, if there is frequently less empirical evidence about methodological rules than there should be, it is because most philosophers—like Doppelt himself—continue to think of methodological rules as being grounded in foundationalist epistemology. Holding that belief, they do not bother to look for any empirical evidence to sustain their claims or to undermine the methodological claims of others. I have no doubt but that, if scientists and philosophers were to seriously investigate the question, "Have theories which subsequently stood up well to empirical test been ones which could have been picked out in their formative stages by the rule of predesignation?", we might well be able to get impressive evidence for or against that rule. But since philosophers have not generally cast the problem in those terms, they have not bothered to collect the relevant information. Under those circumstances, who should we blame for the non-closure of such methodological debates: naturalists who say that they do hinge in part on ascertainable matters of fact, or neo-foundationalists like Doppelt whose view of the status of rules disinclines them to seek out any relevant facts of the matter?[7] What Doppelt sees as an acute embarrassment for my thesis that rules are empirically defeasible hypothetical imperatives (to wit, the existence of prominent, unsettled disputes about methodological rules), I see rather as a telling indictment of the sort of armchair epistemology which has been the prevailing practice in our discipline.[8]

5. Dispensing with Foundations. Crudely put, the normative naturalist holds that the best methods for inquiry are those which produce the most impressive results. He thus uses an ampliative yardstick for judging ampliative rules. This variant of what used to be called the "pragmatic justification of induction" has always troubled foundationalists, who suspect that it is viciously circular or otherwise question-begging. As Leplin rightly points out, such worries—although foundationalist—are without foundation. The naturalist uses the simple method of induction to "bootstrap" his way to more subtle and more demanding rules of evaluation which, in their turn, become the license for subsequent and yet more highly refined rules and standards. The virtue of this way of proceeding, and why it makes the foundationalist's search for deeper underpinnings gratuitous, is that it is capable at any point of revealing its own flaws if any. If the naturalist is led to espouse methods which turn out as a matter of fact to be persistently bad indicators of a theory's future performance, then experience gives us machinery for recognizing the breakdown of those methods and doing something to patch them up. The normative naturalist is unfazed by—if anything welcomes—the much-heralded collapse of foundationalism; for he sees in the capacity of "scientized" philosophy to correct itself the dispensability of other, "higher" forms of grounding.

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[7]Lest my insistence that methodological rules be empirically scrutinized be regarded as little more than a hollow bow in the direction of experience, I should point out that in two large-scale recent projects, I and my collaborators collected massive amounts of empirical evidence to test about half-a-dozen familiar methodological principles, including the predesignationist doctrine that surprising predictions lend more support to a theory than do non-surprising ones. (For details, see A. Donovan, R. Laudan and L. Laudan, (1988) and L. Laudan, et al. (1986).)

[8]Doppelt's argument against naturalistic methodology is rather as if someone had said to Francis Bacon in the 1620s that his plea that science should become more experimental was vitiated by the fact that most scientific issues of his day had not already been experimentally sorted out.

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