

Handbook of the Philosophy of
Science

Volume 15:
Philosophy of Anthropology and
Sociology

edited by Stephen Turner and Mark
Risjord

Handbook Editors:
Dov Gabbay, Paul Thagard and John Woods

NATURALISM WITHOUT FEARS

Paul Roth

Reflection on the method of science has become increasingly thinner since Kant. If there's any upshot of that part of modern philosophy, it's that the scientists didn't have a secret. There isn't something there that's either effable or ineffable. To understand how they do what they do is pretty much like understanding how any other bunch of skilled craftsmen do what they do. Kuhn's reduction of philosophy of science to sociology doesn't point to an *ineffable* secret of success; it leaves us without the notion of the secret of success. (Richard Rorty¹)

A concern to understand why the sciences succeed where they do and as well as they do has typically prompted the *philosophical* study of the sciences. What makes (or was thought to make) the study philosophical involves the level of generality at which the presumed secrets of scientific success lay. Philosophical naturalists, however, study science not because they imagine that the sciences possess insight other putative sources of knowledge do not. Rather, naturalists hypothesize, no forms of inquiry apart from those which the sciences provide hold any comparable promise of being successful guides to acquiring any knowledge worthy of the name. Philosophers become naturalists once convinced that the explanation of scientific success does not lie in some set of factors which themselves cannot be accessed, studied, and explained by these sciences.

Since the standards of science themselves fall within the purview of what the sciences examine, philosophical naturalism locates all putatively distinctive philosophical (e.g., normative) issues as continuous with and part of what the sciences study. The sciences in turn have no further justification for their ways of proceeding other than what account they provide of their sources and methods.²

¹Richard Rorty, "Reply to Dreyfus and Taylor", *Review of Metaphysics*, 34, September 1980, 55.

²What counts as a "recognized science" proves to be historically contested and contingent. But that creates no special problem for naturalism as conceived and elaborated in this essay. Since the various sciences critique and monitor their own normative commitments, one result has to be that the disciplines of which the term 'science' may be properly predicated will alter as theoretical and related justificatory commitments do. As I indicate below, one must view the suggestion that the sciences can only be descriptive and not prescriptive as disingenuous, since it presupposes a notion of science to which no one, once asked to make fully explicit what this notion implies, actually subscribes.

Such is Quine's "mutual containment" of epistemology within empirical psychology and empirical psychology within epistemology.³ Insofar as science can provide an account of how it came to be,⁴ it functions as an epistemology. Insofar as epistemology invokes no standards or procedures alien to scientific inquiry, it resides within science. Moreover, naturalism bases this refusal to honor any appeals to extra-scientific justification for the sciences on studies of the history and philosophy of science, albeit recognizing full well that standards change, and not always for reasons current science can explain. That what goes by the title of 'science' shifts need not trouble a naturalist just so long as what the title includes proves the best guide to success in explaining experience.⁵

Philosophy as a naturalist conceives of it shares with more conventional philosophical approaches a concern to conduct a type of meta-level examination of particular sciences. That is, a philosopher *qua* naturalist examines, systematizes, and generally seeks to make explicit the rules by which the first order endeavor proceeds, including those circumstances under which the rules of inquiry themselves might be modified. But a key difference between naturalists and others in formulating and articulating such matters arises from naturalism's commitment to the view that in doing this, philosophy has no special methods or resources other than those which belong to the sciences collectively examined. Normative recommendations regarding, e.g., justification, can only draw from studies of scientific practice.⁶ Moreover, each metascience can in its turn be made an object of study

³One can only regard as deeply if unintentionally ironic those who attribute the rise in interest in naturalism to Quine's landmark essay, "Epistemology Naturalized", (in *Ontological Relativity and Other Essays*, New York: Columbia University Press, 1969) inasmuch as the main morals urged there by Quine are almost universally rejected, including those who profess to be naturalists. For a discussion of this and a defense of Quine's position along lines outlined here, see my "The Epistemology of 'Epistemology Naturalized'", *Dialectica*, 53: 87-109, 1999.

⁴The phrase 'came to be' means to capture both how a theory evolves from some earlier stage, and how it comes to be accepted as correct.

⁵I will not trouble here to try to delineate exactly how to distinguish what separate naturalism and pragmatism. My colleague Ellen Suckiel pointed out to me that, with respect to science, pragmatists tend to be naturalists, and vice versa. However, the two might also diverge; her apposite example involved religious belief. A pragmatist could well find a justification for religious belief; a naturalist would be less likely to do so, barring some at present unknown scientific advantage to, e.g., appeals to intelligent design. Quine suggests that a naturalist, but not necessarily a pragmatist, could take an interest in questions regarding the unity of science. A pragmatist would not have any clear reason to trouble about this question, but a naturalist could find reason to pursue questions of unity (methodological or ontological) as questions within science. See Quine's remarks on this point in, "Naturalism, or Living Within One's Means", *Dialectica*, 49: 251-61, 1995. This suggests that one distinguishing feature would be that naturalists use scientific standards (however broadly the term 'science' might be understood) as their most general framework for determining the relevance of and the means for answering *all* questions, while pragmatists do not endorse scientific standards as those holding final or most general relevance.

⁶I will comment more on this below. However, *qua* naturalist, norms must be tied to the practices of the sciences, broadly understood. Norms, of course, may be thought to have other sources — divine texts, revelation, or the *a priori* (among many others). But for purposes of this essay, the pronouncements of seers with regard to such realms will be kept separate from and not considered under the rubric of naturalism. The suggestion that scientific activity somehow proscribes or precludes consideration of ends just is obviously false. For the doing of science

and analysis, so there exists no final resting place, no *summa scientia*.

The term 'nature' here connotes the world as our sciences collectively picture it. Naturalism situates the study of humans, in all their aspects, as of a piece with those methods and theories used to investigate other objects in nature.⁷ This naturalizing approach was considered less plausible when what counted as science seemed inadequate to the task of fully accounting for creatures like us, enculturated beings capable, *inter alia*, of creating both systems of meaning as well as complex theories of the world. The understanding-explanation divide receives its basic motivation from the thought that explanation requires laws — causal or at least correlational regularities—while social life is marked by localities of reasoning and meaning which do not generalize cross-culturally over time in the requisite ways (for a genuine scientific explanation).⁸ Naturalism so construed denies that human beings *qua* knowledge producing creatures constitute a *sui generis* phenomenon, studiable only by methods uniquely suited to and tailored for conceptualizing creatures.⁹

Thus, the demise of positivism as a philosophy of science, and so by implication as a philosophy of social science, does not preordain the rise of naturalism to philosophical prominence in its stead. For the demise of positivism can just as easily be read as the vindication of interpretivism construed as a form of meaning realism. In this respect, naturalists stand accused of ignoring just those aspects

requires acceptance of the norms of inquiry. Surely some argument is owed if the position of the opponents of naturalism rests on the assertion that reflection on these norms constitutes, *ipso facto*, a non-scientific activity. Debates about theory choice, proof, and evidence occur within science, concern normative issues, and belong to the ongoing discussion of the nature and practice of science. Those who maintain that the sciences provide only "descriptions" owe a characterization of science legitimating this view.

⁷To cite but two examples which manifest a concern for this linkage, and its overwhelming importance for an understanding of what naturalism may be, see David Thomas's *Naturalism and Social Science* (Cambridge UP, 1979) and Harold Kincaid's *Philosophical Foundations of the Social Sciences* (Cambridge University Press, 1996). Although both claim to defend an approach to the social sciences informed by philosophical naturalism, each devotes almost no space to explicating the notion of naturalism. Rather, the efforts lie in providing an account of science, for both reasonably seem to fear, without a well-delineated notion of science, there exists nothing that marks off naturalism from any other approach. But this ties naturalism too closely to the vicissitudes of efforts to delineate what to count as science. Better, I suggest, to cease to worry about a positive characterization of science and indicate what sort of explanatory moves naturalism excludes however science comes to be defined or constituted.

⁸For a survey of some history of this divide, and reasons for now considering it passé, see my "Beyond Understanding", in *The Blackwell Guide to the Philosophy of the Social Sciences*, ed. Stephen P. Turner and Paul A. Roth, Blackwell Publishing Ltd., 2003, 311–33. My argument for the current irrelevance of the explanation-understanding divide turns on the claim that inasmuch as disciplines such as history which countenance reasons as causes no longer need be excluded from what counts as science, the rationale for the distinction disappears. The dualism no longer serves any purpose, e.g., the need to account for people as creatures who act for reasons.

⁹Indeed, as Ian Hacking argues, a particular case for slighting the explanation-understanding divide from a Foucauldian perspective would insist that people both create and come to inhabit categories which allow for their manipulation, medication, and modeling of their behavior. See in particular his, "The looping effects of human kinds", in *Causal Cognition*, ed. D. Sperber, D. Premack, and A.J. Premack, Oxford: Clarendon Press, 1995, 351–383.

of the social which undid the positivists' efforts to provide general templates for explanation and demarcation criteria for determining which disciplines offered legitimate modes of scientific understanding. Brian Fay put the issue succinctly some time back.

many philosophers of the social sciences who have rejected naturalism have not done so because they saw the natural sciences through positivist lenses. (Think of Schutz, Winch, Taylor, von Wright, Gadamer, Habermas, MacIntyre, Harré and Secord, Lévi-Strauss, and Putnam, to name just a few anti-naturalists: none of them are positivists.) Instead, they have rejected naturalism because there is not enough in the natural sciences that is helpful in dealing with the essentially historical, culturally defined, meaningful, mental, and rational character of human phenomena.¹⁰

However, Fay's characterization presumes that 'science' must mean just and only 'natural science', i.e., inquiry which much exclude the 'meaningful' for some reason. But unless history and related disciplines have been denied membership in the club of science for some now unspecified reason, no *a priori* argument excludes the investigation of meaningful behavior from the realm of what can count as science. As argued below, naturalism need make no distinction between sciences hard and soft, or even demarcate timelessly what science is. What science is is something which naturalists study.

In this respect, naturalism can best be delineated by contrasting it with what it presently *excludes* for purposes of explanation (e.g., supernaturalism — views that the natural world requires for its explanation something not found among its objects and the processes governing their interaction (God, the synthetic *a priori*, etc.), or foundationalism — views that the world must be explained by certainties, including certainties about the nature of ordinary experience, not explicable in turn by the sciences themselves). The lack of demarcation criteria proves to be a strength, not a weakness of the position. For it relieves the naturalist of the futile attempt to specify, in advance of what experience reveals, what must be, must remain, or cannot become a science. Any demand for a prior specification of normative framework proves to be no more than a demand that a naturalist not be a naturalist. But why accept that?

Yet the liberation of naturalism from any need to specify what science, timelessly imagined, is brings with it a threat to the doctrine as well. For naturalism has enjoyed increased philosophical favor just at the historical point where the prospects of a purely philosophical delineation of science appear highly unlikely. Understanding this seemingly ironic outcome proves critical, or so I shall maintain, to appreciating what naturalism offers and why so few who profess to be naturalists practice in accord with it.

¹⁰Brian Fay, review of *Naturalism as a Philosophy of Social Science*, in *Philosophy of the Social Sciences*, 14: 542, 1984.

By way of approaching and unpacking the vexed relation between philosophical naturalism and the notion of science, I begin with the question of how philosophical naturalism can be distinguished from more conventional or traditional philosophical approaches to the sciences. Survey the usual suspects collected and arraigned in a philosophical lineup for purposes of providing extra-scientific explanation — analytic or necessary truths, norms, self-evident truths, etc.¹¹ A point to note involves the fact that there exists no physics of the right or the good, the logic of (Goodmanian) projection remains a riddle, and consequently, widespread philosophical practice notwithstanding, there exists no received account of how the notions on offer must interact as elements of explanation.¹² Authors defending antinaturalist approaches appear much more confident in particular alleged certainties than in any theory by which to account for them. In this regard, the issue which separates naturalists and those who would oppose them concerns less, e.g., the naturalization of norms than the question of how to specify just what supposedly needs incorporation into a theory of the world, i.e., a “naturalizing” of this or that.¹³ One cannot say that this or that — norms or whatnot — can (or cannot) be naturalized until given some reasonable specification of what supposedly needs “naturalizing.”

In what follows, I sketch a working notion of philosophical naturalism and offer some justification for it. This includes a brief historical characterization of naturalism, including some (unapologetically whiggish) historical speculations on how it came to be. I then turn to questions of how this serves to distinguish naturalism in the social sciences from two possible anti-naturalist alternatives, a formalist account (of which the best known variant is positivism) and a meaning realist account (some forms of which I term below ‘interpretivism’). A review the reasons thought to support one or the other of these anti-naturalist accounts will remind

¹¹A clear and philosophically unapologetic approach to thinking about epistemological issues which is fundamentally at odds with the approach advocated in this essay can be found in Richard Feldman, *Epistemology*, Upper Saddle River, NJ: Prentice-Hall, 2003.

¹²See, in this regard, essays in *Rethinking Intuition*, ed. Michael R. DePaul and William Ramsey, New York: Rowman & Littlefield, 1998.

¹³For example, Kim’s widely cited essay relies on just the distinction that whatever science is (and Kim provides no such characterization), it can countenance only causal explanations. He is not alone in this. Jaegwon Kim, “What is ‘Naturalized Epistemology’?”, in *Philosophical Perspectives*, ed. J. Tomberlin, Atascadero, CA: Ridgeview, 1988. Kim imagines that whatever norms are, some “special” justification for them must be forthcoming, though no argument hints at a reason for giving norms a non-natural status, or even to say in what forms the imagined norms exercise their magic. For a development of this criticism, see Roth, “The Epistemology of ‘Epistemology Naturalized’”, op. cit. Relatedly, Barry Stroud asserts that naturalism remain caught in a “basic dilemma” because it cannot deny that there exist “psychological facts” regarding beliefs, intentions, and the like, on the one hand, but on the hand, Stroud finds it unclear how these “psychological facts” readily fit into the “restricted conception” (47) of the world to which naturalists subscribe. Many problems suggest themselves here, not the least of which concerns how, on any theory whatsoever, one knows what to count as an “adequate” account of “psychological facts”. It is not impossible that Stroud is right. But one would first need to know exactly what things he finds missing before rushing to conclude that some view or other does or does not account for them. Barry Stroud, “The Charms of Naturalism”, *Proceedings and Addresses of the American Philosophical Association*, 70: 43–55, November 1996.

readers of why these positions were found wanting. Finally, I sketch some varieties of naturalism on the current social scientific scene and rehearse what they imply for the philosophy and the practice of social science.

Nothing in this essay will serve as direct argument to convince someone not now inclined to naturalism to adopt it. Among other reasons for this, some will doubtless judge my negative conclusions about the feasibility or potential fruitfulness of pursuing more traditional philosophical methods and agendas at best premature. *De gustibus non est disputandum*. My approach also implies that while many might claim to be naturalists, few philosophers actually qualify as such in practice.

1 WHAT NATURALISM IS

A working characterization of naturalism needs to be formulated. Despite the trumpeted "naturalists return",¹⁴ the very pervasiveness of the term on the current philosophical scene gives rise to fears that the term has become too polyvocal to be useful. Indeed, its pervasive use only lends credence to the suspicion that the term may be vacuous.

Worries about vacuity tie in part, I suspect, to an absence of a canonical account of philosophical naturalism. Historically, the term connotes more to a loose school than to a specific doctrine.¹⁵ A more substantive and genuine but much

¹⁴Philip Kitcher, "The Naturalists Return", *The Philosophical Review*, 101: 53-114, January 1992. Kitcher identifies anti-naturalism with the animus of Frege and those who followed him towards any appeal to psychological or contingent scientific factors. Frege, focused on mathematics and structures presumed to be universal and shared, could envision no role for the empirical in this account. However, as challenges mounted to attempts to stipulate a principled divide between what requires appeals to experience for verification and beliefs that can be held true come what may, Fregean reasons for precluding the relevance of naturalism appeared less compelling. Michael Friedman's case for a return to anti-naturalism reverts to Fregean themes, but at yet higher levels of mathematical abstraction; naturalism cannot be what philosophy should become, because pure mathematics not only "floats free" of the tribunal of experience, but actually serves as a constitutive condition for constituting any such tribunal. See, for example, Michael Friedman, "Philosophical Naturalism", *Proceedings and Addresses of the American Philosophical Association*, 71: 7-21, 1997, especially p. 14. However, it does seem to be a consequence of Friedman's position that all philosophy comes to are principles at such a level of abstraction, and the only ones that appear to fill that bill belong to extremely abstract and abstruse areas of mathematics. Moreover, a key move in this essay by Friedman in criticism of Quine and defense of Carnap, viz., that Quine's contention in "Two Dogmas" that the dogmas are, at root, identical, is to claim that this rests on Quine's holism. He asserts this without argument, and in this he is wrong. Reductionism would provide just another species analytic statements, inasmuch as the 'reduced' term and the reducing ones are fully equivalent. Holism enters Quine's discussion as a plausible explanation for why it proves so hard to specify which statements are analytic. Holism is *not* given as an argument against analyticity.

¹⁵In his Presidential Address to the APA a half century ago, Ernest Nagel articulates a general framework for conceiving of naturalism to which the present essay chimes. Ernest Nagel, "Naturalism Reconsidered", *Proceedings and Addresses of the American Philosophical Association*, 28: 5-17, 1954-55. Nagel remarks that he uses the term "partly because of its historical associations, and partly because it is a reminder that the doctrines for which it is a name are neither new nor untried". (7) He goes on to add, as I also emphasize, the "if naturalism is true, irreducible variety and logical contingency are fundamental traits of the world we actually

less acknowledged (or at least discussed) worry here arises from the fact that any characterization of naturalism proves unilluminating because it requires a specification of the notion of science invoked when stipulating the doctrine. But, post-positivism, what principled characterization of the notion of science can be provided?

In short, the dividing line between what naturalists embrace and what they exclude seemed clearer when thinkers had confidence that the "real" sciences and their related methods could be formally demarcated from the proposals of pretenders. Insuperable appearing challenges to demarcation stem both from the failure to discern historically any necessary differentia between the true sciences and mere pretenders, on the one hand, and, on the other hand, work by e.g., feminists and sociologists of science which challenges any proposed separation between scientific theorizing and social convention.¹⁶ Failing this ability to demarcate, what does naturalism then connote? In this regard, some attention must be paid to how the notion of science has itself evolved post-positivism in order to appreciate what one endorses if one declares for naturalism.

How then to say what science is? This question underlines the concern that naturalism fails to mark out any special ontological or methodological realm because no philosophically principled lines can be drawn between scientific approaches and others, and so no ontological line between the objects of science and others. Erstwhile naturalists might well fear that their doctrine proves empty either because the social has so expanded as to include "the realm of science" within its ambit of explanation, and not vice versa. If the sciences form only a motley, so much the worse for any doctrine which seemingly relies by definition on the sort of sharp delineation of science which current accounts fail to provide.

Ironically, this very lack of a philosophically principled demarcation of science from other forms of inquiry does *not* mark the passing of or threaten vacuity

inhabit". (10)

¹⁶Examples here are legion. Representative would be, e.g., Helen Longino's work and work in the sociology of science by thinkers such as Barry Barnes, David Bloor, and Steve Woolgar. An important difference in this group concerns the fact that most alternatives to traditional philosophical accounts of scientific rationality reject what they see as 'traditional' about such views or what they view as excessively 'philosophical' about such views. In both cases, critics emphasize the implausibility of excluding social and cultural factors from any explanation why one theory prevails over another. These critics remain well within the tradition insofar as they hold that broadened account of influencing factors suffices to explain theory preference among scientists. For discussions of how the traditional view lives on in the work of its erstwhile critics, see my "Feminism and Naturalism: If Asked for Theories, Just Say 'No'" in *Feminist Interpretations of W.V. Quine*, ed. L. Hankinson Nelson and J. Nelson, University Park, PA: The Pennsylvania State University Press, 269–305, 2003, and "Will the Real Scientists Please Stand Up? Dead Ends and Live Issues in the Explanation of Scientific Knowledge", *Studies in History and Philosophy of Science*, 27: 813–838, 1996. The exception here would be Woolgar's work, which recognizes clearly the irony involved in sociological claims to provide a scientific explanation of scientific practice which relies on a "debunking" of this practice as anchored in norms of rationality. An adumbrated discussion of these issues can be found in Steve Woolgar, *Science: the very idea*, London: Tavistock, 1988. See especially references therein, especially to Woolgar's own earlier work. For a review of this debate, see John Zammito, *A Nice Derangement of Epistemes* (Chicago: University of Chicago Press, 2004).

to naturalism but rather has made possible its resurgence and re-established its relevance. For the wresting free of the concept of science from formalist shackles to which it had become, though much of the 20th century, bound makes possible a “naturalists return” by allowing the notion of science to range over the variety of ways humans systematically explore and account for the world as they find it — from physics to history. Liberalizing what to count as science in this way removes the need for invidious distinctions between the natural and the social sciences. Relatedly, as I detail below, criticisms of the notion of “objective meaning” and the consequences of appreciating the implications of the indeterminacy of translation dissipate fears that a naturalistic social science cannot avail itself of the notion of meaningful behavior. Granted, the account offered in this essay leaves open to change what exactly to count as science. But better to acknowledge that this notion appears fated to remain contested than to pretend to more determinate knowledge than, in fact, we do or can expect to possess.¹⁷

Naturalism, I have claimed, can be best clarified by contrasting it with possible alternatives. For purposes of exposition, Russell’s theory of descriptions may be taken as paradigmatic of a non-naturalistic theory, although I would also echo Frank Ramsey famous *mot* and take it also as a paradigm of (non-naturalistic) philosophy. In this regard, Russell’s analysis of “The present King of France is bald” or “George IV wished to know whether Scott was the author of *Waverley*” proved paradigmatic in two distinct senses. One is as problem-solving model. It greatly simplified the assumptions needed to analyze some standard “hard cases”.

Yet, and more importantly for present purposes, Russell’s analysis belongs to a philosophical theory of meaning of which the theory of descriptions served as but a part. Meaning here becomes a function of a language possessing a particular logical structure, a structure proper analysis reveals. This theory of meaning itself belongs to *no* natural science, but rather presupposes problematic philosophical views regarding “knowledge by acquaintance” and “knowledge by description”. The Russellian paradigm reminds us of how a wrong-headed philosophical theory may lurk just below the surface of elegant and seemingly metaphysically pristine formal analyses.¹⁸

My aim here is not to offer any general criteria for designating theories as philosophical, but only to note some features that can make them ones. Failure to solve conceptual puzzles represents a difficulty, not disconfirmation. In addition, as is notoriously the case in the various formulations of the principle of verifiability, the demarcation criteria kept coming out wrong. Various schemas invariably ex-

¹⁷Those who worry that this approach opens the door to creationism, alchemy, and other questionable claimants to the title of science do so needlessly. By emphasizing more the pragmatic outcomes of theories, one can make a stronger argument than those offered by formalist criteria for favoring one theory over another. Scientific methods can be mimicked; research outcomes are there or not for the world to see.

¹⁸Another important instance of how a logical analysis may overlie a contentious philosophical theory is the verifiability theory of meaning. It merits the title of a “philosophical theory” just because it too purported to explain, without the aid of science, why science works well when it works well.

cluded from the realm of meaningful statements in science some sentences even positivists wanted to keep. But the sundry shortcomings only whetted their philosophical appetite, inasmuch as the theories were held for reasons experiments could not touch, e.g., assumptions about the possible sources of human knowledge, the deep structure of natural language, and the requirements of cognitive significance.

It is important to remember what separates the approach of a Russell and especially a Carnap from those of their empiricist and rationalist predecessors. It is their positive proposal to actually reconstruct the link between existing scientific theories and their empirical base. Somewhat ironically, what we owe to the decades of intensive work especially by Carnap is a deep appreciation of how resistant scientific theorizing is to this specification of its inferential relation to evidence.

This explicitly constructive aspect of the logical positivist project comes finally to define what empiricism is in the 20th century.¹⁹ Rational reconstruction would have established the objectivity and rationality of scientific knowledge to anyone's satisfaction. "Rational reconstruction", Carnap says, attempts "for the first time, the actual formulation of a conceptual system of the indicated sort", (ibid.) i.e., our system of knowledge. Reconstruction would be proof positive of long-standing empiricist claims regarding what the "deconstruction" of empirical knowledge *must* yield. The hallmark of the positivist philosophical theories of knowledge is just this reconstructive claim, of tracing a logical path from "protocol sentences" to those of theory.

This defining characteristic of logical positivist epistemological theories is doubly philosophical. On the one hand, it is not tested via experiment; reconstruction is just an exercise in logical imagination. On the other hand, reconstruction provides the justificatory basis, in the best understood sense of that term — a formal logical derivation — of theoretical claims. Logical reconstruction is then a paradigm *philosophical* claim, a "first philosophy" that is prior to scientific knowledge.

In the above scouted sense of "philosophical theorizing", naturalism is not a philosophical theory of knowledge. Some, to be sure, have tried to make it so. Naturalism asserts a normative and methodological continuity between epistemological and scientific inquiry. That is, the techniques endemic to the former are only a subset of the historically received and contingently held norms and methods of the latter.

What counts as a scientific method for naturalists is not itself limited to or defined by one particular science, or driven by a prior philosophical characterization of such. For Quine, as for American naturalists historically, the methods of science include the full panoply of procedures employed in fact-driven research programs in any area of inquiry. As John Herman Randall puts it:

¹⁹"I had realized, on the one hand, the fundamental importance of mathematics for the formation of a system of knowledge and, on the other hand, its purely logical, formal character to which it owes its independence from the contingencies of the real world. These insights formed the basis of my book... This orientation is sometimes called "logical empiricism" (or "logical positivism"), in order to indicate the two components". Rudolf Carnap, *The Logical Structure of the World*, Berkeley, CA: University of California Press, p. vi, 1967.

The “new” or “contemporary” naturalism . . . stands in fundamental opposition not only to all forms of supernaturalism, but also to all types of reductionist thinking which up to this generation often arrogated to itself the adjective “naturalistic”, . . . [Naturalists agree] that the richness and variety of natural phenomena and human experience cannot be explained away and “reduced” to something else. The world is not really “nothing but” something other than it appears to be; it is what it is, in all its manifold variety, with all its distinctive kinds of activity.²⁰

Quinean naturalism, in particular, demands no strict demarcation criteria of what to count as science or scientific.²¹ Nor are there any philosophical *cum* ontological requirements regarding the necessary building blocks of knowledge. Epistemology is “contained in” empirical psychology only in the sense, and to the extent, that ontogeny recapitulates phylogeny.

In a like manner, those who perceive naturalism as handicapped by some form of the is/ought distinction likewise mistake what a naturalist’s conception of science involves. Why assume that an account of scientific method excludes or precludes evaluations of standards of scientific justification any more than working within a system of logic precludes or excludes consideration of rules for the system? Systems of science and systems of logic alike are constructions whose operant standards are chosen in light of certain ends and purposes — namely, those of their makers and users.

Science and logic are conceived from the outset as systems that stand in a dynamic relation to their rules, rules which are in turn chosen for and adjusted to certain ends. Reflexive adjustment of means and ends is just part of what it is to have and maintain such a system.

Quine’s naturalism extends to rules of logic. In a 1936 essay which prefigures many of his key philosophical themes, “Truth by Convention”, Quine disparages an important attempt to cash out the view of these truths as analytic by appeal to the notion of mathematical or logical convention. There is a fundamental difference, he argues, between rules codified in light of practices, and practices that follow clear rules. Only in the latter case are conventions explanatory. That is, conformity to a convention explains behavior when the convention is specified in advance of the behavior. But when the convention itself is only formulatable *subsequent* to a particular practice, the convention then does not explain what one observes.

When we first agree to understand ‘Cambridge’ as referring to Cambridge in England, failing a suffix to the contrary, and then discourse accordingly, the role of linguistic convention is intelligible; but when a convention is incapable of being communicated until after its adoption,

²⁰ John Herman Randall, “The Nature of Naturalism”, in *Naturalism and the Human Spirit*, ed. by Y. K. Krikorian, New York: Columbia University Press, 1944, p. 361.

²¹ W.V. Quine, “Naturalism; Or, Living Within One’s Means”, *Dialectica*, 49: 251–61, 1995, p. 252.

its role is not so clear. In dropping the attributes of deliberateness and explicitness from the notion of linguistic convention we risk depriving the latter of any explanatory force and reducing it to an idle label. We may wonder what one adds to the bare statement that the truths of logic and mathematics are a priori, or to the still barer behavioristic statement that they are firmly accepted, when he characterizes them as true by convention in such a sense.²²

Where the convention is beholden to some prior practice, citing it adds nothing to "bare behavioristic statement" for purposes of explanation.

Science and logic are then constructed systems and their makers choose the norms constraining them in light of their purposes. In light of this fact, why grant any measure of credence to claims that working with and within such system precludes assessing, emending, or amending the previously chosen norms?

Over 50 years ago, Abraham Edel mounted a defense of naturalism in ethics germane to this discussion of naturalism as a source of normative insight for the sciences. He there nicely articulates just why naturalism is reflexive regarding its normative commitments. In the quote that follows, imaginatively replace each use of 'ethics' or cognate terms with the appropriate form of the term 'science.'

The whole articulation of a morality within a society under given conditions, the problems of change and adjustment within it, require constant valuational activity. We find our commitments as what we are committed to in the specific lines of choice and directions of striving in which we are engaged. Even the major permanent ends we may thus elicit on analysis . . . do not become the objects of isolated independent selection. Their evaluation rests on the whole network of choices and the kind and quality of life to which they commit us.

. . . Mr. Murphy seems to me to pose the question almost as if an ethical theory must somehow equip a hypothetical man who holds no values to choose between conflicting values. If he means to eliminate all reference to an existent value-pattern of the self as already settling the moral problem, then he poses an impossible task. The question "What values should I choose if I had no values?" is meaningful only if it asks what other who had values would recommend for a person in my position. All justification is in a matrix of existent values. Scientific method is applicable to values in so far as it provides a way of identifying one's existent values, testing them, and refining or revising them in choice.²³

We have ends important to us, and we have systems which, we hope, will abet us in achieving those ends. If the ends seem to require rules we find overly restrictive,

²²W.V. Quine, *Ways of Paradox*, 2nd Ed., rev., Cambridge, MA: Harvard U.P., 1976, p. 99.

²³Abraham Edel, "In Naturalism Arbitrary?", *Journal of Philosophy*, 43: 141-52, 1946, pp. 146-7. Again: "A naturalistic ethics recognizes frankly the primacy of human striving or goal-seeking as the matrix of its inquiry. In every moral choice there is a reference to some values which act as standards of judgment." p. 144.

we can alter or drop the goal; if a rule does not function well relative to the end in view, we can change the rule. This is as true as science as for ethics.²⁴

Questions of what ends we ought to choose, in abstraction from lived experience and human history, are meaningless. For such questions cannot apply to us, or anyone known to us. Barring a satisfactory account of just how norms of justification are somehow summoned from realms beyond time and history, there is then no good reason to believe that a naturalistic perspective impedes epistemology's normative aims.

So-called naturalist positions that promise more by way of normative edification than does Quine, Philip Kitcher or Alvin Goldman come to mind here, invariably turn out to fail to justify such normative claims naturalistically or, I should add, to justify them at all. As Miriam Solomon quite properly notes with regard to Kitcher's pseudo-naturalism — a position she dubs "Legend Naturalism" — his "naturalism does no work — no data or theories from psychology or sociology shape the epistemic account — the naturalism is just window-dressing for a previously and independently developed account of scientific rationality".²⁵ Much the same is true, I have elsewhere argued, regarding Goldman.

In this light I propose to examine two common but in fact incompatible criticisms of naturalism. The first insists that the characterization proves too vague to be of any use.²⁶ Van Fraassen, for example, remarks that "To identify what naturalism is . . . I have found nigh-impossible".²⁷ Second maintains that naturalism proves too narrow or restrictive; its link to scientific methods supposedly precludes naturalism from fulfilling any of the normative roles to which philosophy aspires. This alleged problem may be understood as just a version of the Humean is/ought: science describes, norms prescribe, hence the latter cannot be derived from the former. Since naturalism limits itself what science provides, it cannot

²⁴In this respect, I believe, Quine would echo Goodman's "justification of deduction". "Principles of deductive inference are justified by their conformity with accepted deductive practices. Their validity depends upon accordance with the particular deductive inference we actually make and sanction. If a rule yields unacceptable inferences, we drop it as invalid. Justification of general rules thus derives from judgments rejecting or accepting particular deductive inferences . . . A rule is amended if it yields an inference we are unwilling to accept; an inference is rejected if it violates a rule we are unwilling to amend. This process of justification is the delicate one of making mutual adjustments between rules and accepted inferences; and in the agreement achieved lies the only justification needed for either". Nelson Goodman, "The New Riddle of Induction", in *Fact, Fiction, and Forecast*, 3rd Ed., Indianapolis, IN: Hackett, 1979, pp. 63-4.

²⁵Miriam Solomon, "Legend Naturalism and Scientific Progress", *Studies in History and Philosophy of Science*, 26: 205-18, 1995, p. 207. Solomon thinks this is the case for Quine as well, and here I disagree.

²⁶Alexander Paseau, although content to use the term 'naturalism' in his title, quickly alerts his readers that "in general, the term 'naturalism' is overworked in contemporary philosophy. Vague orientation aside, most philosophical naturalisms have little in common with one another". Alexander Paseau, "Naturalism in Mathematics and the Authority of Philosophy", *British Journal for the Philosophy of Science*, 56: 377-96, 2005, p. 377 fn.1. See also Bas van Fraassen "Against Naturalized Epistemology", in *On Quine*, ed. P. Leonardi and M. Santambrogio, NY: Cambridge U.P., 1995.

²⁷Bas van Fraassen, "Science, Materialism, and False Consciousness", in *Warrant in Contemporary Epistemology*, ed. J. Kvanvig, Latham, MD: Rowman & Littlefield, 1996, p. 172.

derive from this a way to philosophically prescribe.

But just how vague is the notion of naturalism? No more vague, I suggest, than our ability to catalog the methods of science. Naturalism, moreover, does not yoke what counts as science to some philosophical characterization. It is ironic, then, to find philosophers such as van Fraassen making continued references to "science", as if they knew exactly what that means, and yet complaining all the while about the vagueness of naturalism. So long as that proves workable, naturalism is as well. I have elsewhere urge a liberal Quinean line in how to construe the notion of science, but that case need not be rehearsed here.

The second, flowing from naturalism's embrace of science and its rejection of claims to other, non-scientific forms of knowledge, invokes the hoary descriptive-prescriptive distinction. The objection here imagines that sciences, however conceived, can only describe the world. Philosophers, intent on a prescriptive (or, as the current favored term of art has it, the 'normative') investigation — not what is the case, but what (ethically, epistemically) ought to be the case — regard scientific investigations so conceived as incapable of providing philosophic insight, so conceived. While science may tell us what is the case, only philosophy, in its various guises, can pronounce on what (rationally) ought to be the case.

Yet why this repeated injunction of the descriptive-prescriptive distinction, as if someone somewhere had established a clear demarcation of these notions and, in addition, adduced arguments that 'real' science partakes of just one and never the other? Given the relentless blurring of the distinction between theory and observation, itself a species of the descriptive-prescriptive dualism, I profess extreme skepticism that any good case could be made at this point in time either for this distinction in general, or for an account of science which has 'real' science doing one and not the other.²⁸

Moreover, the two criticisms — the charge of vagueness, on the one hand, and the insistence on the descriptive-prescriptive distinction within science, on the other hand — appear incompatible. For insofar as what happens to be science can be specified sufficiently sharply so that only descriptive accounts (under some suitably acceptable notion of 'descriptive') qualify, then naturalism cannot be belabored as vague, since its vagueness could flow only from the account of science to which it links. Yet if the going account of science cannot support the charge that 'real' science must fore swear partaking in prescriptive notions, then the alleged hostility of naturalism to the normative cannot be sustained. For embracing scientific methods would not exclude "by definition" examinations of normative considerations. Put another way, consideration of the methods of science includes, I assume, those standards with which scientists work. Limiting examination to the announced results — the products, not the processes — represents an arbitrary limit not backed by any argument. Yet only by such an arbitrary limit do the norms of science themselves not count as a product of scientific inquiry and so open to scientific explanation and scrutiny.

²⁸In this regard, Richard Rudner's brief piece "The Scientist qua Scientist Makes Value Judgments" remains worth reading, *Philosophy of Science*, 20: 1-6, 1953.

It would be a mistake to construe the cut between naturalism and its philosophical Other as rooted in a metaphysics of objects or primary processes. For that would be to make an assumption a naturalist does not, viz., that one can in principle draw some line between what counts or does not as science. Naturalists need not (and, on my view, ought not) be in the business of prescribing in advance what can or cannot be part of the ontology or causal order. Rather, the critical point turns out to be one of a type of *explanatory unity*. Naturalists seek only explanations which fall within the causal and ontological orders as the sciences, broadly construed and contingently constituted, would have them.

In this regard, to claim that there exists, e.g., a normative realm (in logic, in ethics, or wherever) over and above the world science examines simply fails to add to our knowledge, unless one has some special notion of knowledge on offer. Were there to be had some non-stipulative knowledge of these other supposed realms of being, then all would be well. But absent some "physics of the normative", no one knows what ones know and how ones know it for these other realms of supposed knowledge.²⁹ It is not enough, that is, to stipulate what one takes to be true a priori; one would need to know what makes the *a priori* what it is. For present purposes, explanatory resources available to us now must be sought in the sciences, broadly conceived.

In sum, then, naturalism is not a philosophical theory in the previously specified pejorative sense of that term. It is empirical through and through, from its conception of logic to its conception of methods to what even to count as science. Naturalism so conceived is untainted by prior philosophical commitments to reduction or to a hierarchy of sciences. No area of belief stands aloof from alteration or emendation in light of experience. Even the preference for naturalism itself is evidence driven. Should some approaches other than those the sciences offer prove more efficacious in furthering our goals, the commitment to naturalism itself would then be jettisoned. There is no more vagueness to the notion of what naturalism is than there is to what the methods of the sciences themselves are. There is no more an obstacle to examining, emending, or excluding norms within a naturalistic approach than there is in any self-critical scientific approach. Which is to say, there is none at all.

2 NATURALISM AND SOCIAL SCIENCE

With respect to the social sciences, does naturalism represent just one more guise for rampant scientism or unrepentant reductionism? Scientism assumes that the definition of science limits it to the natural sciences; reductionism assumes that within the natural sciences all acceptable theoretical notions ultimately must find a place in terms provided by physics (or whatever the current candidate happens

²⁹ Consider, in this regard, John Mackie's complaint that the "ethically real", should such exist, would constitute an ontologically strange realm. I find no advance in knowledge here by invoking notions of necessity where some corresponding explanation of why such necessities need obtain proves absent.

to be for the genuine *Über-science*). These conceptions of scientism and reductionism, in turn, generate two lines of resistance to naturalism in the social sciences. One line maintains that naturalism fails in the social sciences because given what science is, sciences of the social simply cannot reach the standards of explanation required for "real" science. Here the criticism assumes that what sciences are can be specified relative to formal criteria, and that given these criteria, the sciences of the social just do not meet the mark.³⁰

A second line of resistance in fact grants to formalist conceptions of science the substance of their claim about the natural sciences, but maintain that the object of social science lies elsewhere. Proponents of a special status for the "human sciences" emphasize the centrality of the meaningfulness of actions — the intentionality of some of our behavior. In this regard, meaning is taken to be a non-naturalistic phenomenon — meaning is not a property of behavior as, e.g., color is of an object. Interpretive social science may, in this regard, appear to be a contrast to naturalistic social science, insofar as the former emphasizes the centrality of meaning, and meaning in turn is taken to be a non-naturalistic notion because tied to an intentional vocabulary which has no naturalistic, i.e., scientific, analogue.³¹

A closely related issue, touching on both formalist and interpretive preconceptions regarding the necessary features demarcating their areas of study from others, involves claims about standards of rationality. In its first incarnation, the debate pits formalist conceptions of scientific rationality against interpretivist claims re-

³⁰For a text which takes basically this view, see Alexander Rosenberg, *Philosophy of Social Science*, Boulder, CO: Westview, 1988, especially Ch. 1. Rosenberg, in Ch. 7, raises key moral issues that might arise in "committing social science", e.g., experiments on human subjects. However, to insist that this then establishes the prescriptive/descriptive divide in social science simply begs the question against naturalism in ethics. A very different path to a related conclusion — that the social sciences must satisfy certain formal constraints in order to count as science — can be found in Daniel Little, *Varieties of Social Explanation*, Boulder, CO: Westview, 1991. But whereas for Rosenberg the key formal element concerns the need for laws or law-like statements for explanation, Little emphasizes a particular analysis of causation as the necessary formal condition. Interestingly, they reach opposed conclusions on the question of whether the social sciences satisfy their respective formal desiderata.

³¹This view has origins in the very creation of the idea of a social science, e.g., the work of Max Weber. Insofar as positivism enshrines a certain formalist conception of science, interpretivists mount their opposition to positivism not because they challenge how positivists characterize natural science, but because they claim that this characterization excludes an essential element needed for social science — reference to meaning understood as a non-natural notion. For good historical surveys of the debate over the status and character of the human sciences vis-à-vis the natural sciences which takes for granted both the legitimacy of a formalist conception of science and the non-natural status of the notion of meaning, see K-O Apel, *Understanding and Explanation*, Cambridge, MA: MIT Press, 1984, especially Chs. 1-4. While I am deeply sympathetic to the issues Apel raises in his Appendix, "Is the Controversy between Explanation and Understanding Obsolete?", my own take on the nature and consequences of this obsolescence remain quite different from the analysis Apel offers. Other worthwhile accounts include J. Habermas, *On the logic of the Social Sciences*, Cambridge, MA: MIT Press, 1988, and G.H. von Wright, *Explanation and Understanding*, Ithaca, NY: Cornell U.P., 1971. An excellent overview of the full sweep of this general debate remains R. Bernstein, *The Restructuring of Social and Political Theory*, Philadelphia: The University of Pennsylvania Press, 1978.

garding what to count as rational or “logical” standards regarding the justification of beliefs.³² The next incarnation of the *Rationalitätstreit* pits sociologists of science against philosophers of science with regard to who better explains theory change in science — sociologists by appealing to factors exogenous to scientific method formally conceived, or philosophers by appealing to some formal canon of scientific rationality.³³ Interestingly enough, the first incarnation turns out to be simply an artifact of the now untenable root beliefs held by the competing accounts — the existence of a single logic of science, on one side, the “idea of the social” as a conceptual reality subscribed to by the other. Once rationality itself becomes “naturalized”, then the disputes between philosopher and sociologists of science become tractable, at least in the sense of having common ground on which to settle the issues.³⁴

I focus below on whether the lines of resistance to naturalism emanating from formalist or interpretivist preconceptions about the “human sciences” remain plausible. None of these issues, I claim — the efforts to provide a formalist demarcation of science and non-science, the conception of meaning as a non-natural yet objective phenomenon, the dispute about competing standards of rationality — constitutes a viable objection or obstacle to philosophical naturalism in (or out) of the social sciences.

In the context of philosophy of social science, the two contrasting positions to naturalism are standardly taken to be positivism and interpretivism. The former contrast turns on a view that what science is can be determined by logical form, and the question of form is not itself a matter of investigation by one or another science. Nor is this form a historically contingent matter, except in the philosophically irrelevant sense that it was over time that the proper form was discovered. ‘Science,’ on this view, presents no moving target; our challenge would simply be to discover what the proper form is.³⁵

³²Canonical collections here include *Rationality*, ed. B. Wilson, Oxford: Blackwell, 1970, which examines the dispute as it arose within primarily the ambit of analytic philosophy (pre- and post-Wittgensteinian, on one reading of Wittgenstein). Peter Winch’s essay, “Understanding a Primitive Society” (and included in the Wilson anthology) sparked this debate and remains a primary focus. I would include in this context the Popper-centered dispute which provides the focus for *The Positivist Dispute in German Sociology*, T. Adorno et al., New York: Harper Torchbooks, 1976. Although the issues differ, the dispute remains centered on how to determine what counts as rational standards for justification. In addition, two collections which examine the rationality dispute in broader context are A. Giddens, *Positivism and Sociology*, London: Heinemann, 1974, and *Understanding and social inquiry*, ed. F. Dallmayr and T. McCarthy, Notre Dame, Ind. : University of Notre Dame Press, 1977.

³³A generally good overview of this debate can be found in *Rationality and Relativism*, ed. M. Hollis and S. Lukes, Cambridge, MA: MIT Press, 1982. The essay by Barry Barnes and David Bloor therein provides a helpful, if typically polemical, overview of how the sociologists configure the debate here.

³⁴See “Will the Real Scientists Please Stand Up?”, op. cit..

³⁵For classic expositions of the philosophy of social science in this mode, see Otto Neurath, *Foundations of the Social Sciences*, Chicago: The University of Chicago Press, 1944, or Richard S. Rudner, *Philosophy of Social Science*, Englewood Cliffs, NJ: Prentice-Hall, 1966. Many of the early anthologies on this subject, e.g. May Brodbeck’s *Readings in the Philosophy of the Social Sciences*, reflect in their division of topics — laws, explanation, ontology — the assumption that

Much of what was outlined above with regard to criticisms of philosophical theories applies directly to positivism. But the failure of positivism as a philosophical theory needs to focus here on the particular criticisms internal to that theory, particular those scouted by Carl Hempel in his classic essay, "Problems and Changes in the Empiricist Criterion of Cognitive Significance" and Carnap's essay "Empiricism Semantics, and Ontology". In both these cases, one witnesses positivism ultimately going 'holistic', by which I mean that the question of what makes a statements verifiable (as Hempel stresses) or what makes a theory rationally preferable (as Carnap discusses) turns away from any simple consideration of the relation of statements and evidence and towards more global considerations regarding how theories constitute mutually supporting sets of statements not individually evaluable or how theory choice can be primarily motivated by pragmatic questions. There are no ultimate frameworks by which to decide a "best" theory.

It helps here to emphasize how these conclusions in support of a holist view of theories and pragmatic forces guiding theory choice emanate from writers such as Hempel and Carnap. For if one only discusses, e.g., Kuhn on incommensurability and revolutions or Quine on underdetermination or the analytic/synthetic distinction, the mistaken impression arises that all might be well with the sentence-by-sentence view were some one objection answered. But the Kuhnian and Quinean criticisms prove to be *symptomatic* of the same fundamental problem, viz., that a certain view of how language relates to the world never cashed out as promised. Analysis of the logical structure of the world was to make good on the longstanding empiricist promise that a chain of justified inference led or could be reconstructed from the evidence available to us *qua* embodied and reasoning beings to the highest reaches of the sciences which constitute part of what humans know. But not only was no such determinate chain of inference revealed, but also close inspection turn up compelling reasons to reconceive the entire theory-evidence/word-world relationship. Quine and Hempel offer no novel criticisms of notions fundamental to positivism. Each, rather, rehearses and details generally known failures within that program. The difference between Quine and Hempel in this regard lies not in their cataloging of fundamental shortfalls in the program, but in their imagined philosophical futures. Hempel enjoins his readers to press forward with the original program. Quine offers a specific counter-suggestion to the going dogmas on how to think about our beliefs and the evidence for them. Neither claims to have shown "in principle" why positivism fails, but only to have indicated the massive problems such a program faces.

Positivism was abandoned, and ironically the "reconsideration of logical positivism" stresses not verificationism but its (allegedly under- or unappreciated) ties to neo-Kantian projects. But whatever the interest or legitimacy in reading at least Carnap in this way, the reading serves only to underline that no serious

philosophy of social science is just a spin-off of the philosophy of science. But this very division of topics supposes precisely what no longer can be assumed to be the case, viz., that something distinctive and general with regard to each of these topics marks out what a science is as opposed to another type of study.

effort appears on the current philosophical front to resuscitate the sort of theory-evidence relation imagined by the verifiability criterion of meaning. This, in turn, underwrote the hope that a purely formal or structural analysis of the science could be provided, an analysis which would provide as a direct result a demarcation criterion for the sciences. Absent this structural demarcation, then, the type of anti-naturalism represented by a neo-Kantian reading of the Carnapian project is not one which concerns me. For it remains unclear how this would intersect with worries about the “human sciences” which flowed from the early construal of the work.

Those interpretivists I have labeled “meaning realists” too often endorse formalist notions of science, and content themselves with denying the relevance of this notion of science to the study of the human. Enthusiasts who promote a special status for the human sciences concern themselves rather with the “special” sense in which such studies lay claim to knowledge and objectivity which distinguish the human sciences from the standards staked out by the friends of demarcation.³⁶

The critical point to appreciate, the primary reason making possible the naturalists return, concerns the double failure — but both formalists and meaning realists — to make good on their respective claims to locate fixed points outside of science (however understood) by which to demarcate or delineate their respective objects of study (science and the world of nature, on the one hand, and shared meaning and rules on the other hand). That is, theorists of the social have proven no more adept than the philosophers of science they seek to displace in their efforts to specify the objects — rules and whatnot — which supposedly constitute the world-making stuff of “the social”.³⁷ What “observation sentences” were to strict verificationists, “objective meanings” are to meaning realists, i.e., one pillar on which to base their claims to objectivity. The related pillar in each case constitutes the structural fixed points of analysis — analytic or logical truths for formalists, notions of rules or transcendental bases of meaning for meaning realists. In this respect, the friends of demarcation and the defenders of the special status of the *Geisteswissenschaften* alike were undone by the “holist turn” challenging alleged distinctions between truths certified by non-natural factors — the *a priori*, the eidetic, etc. — and those which people at a moment can find no reason to question.³⁸

³⁶For an account of the relation which embraces the very distinctions my approach denies, see Joseph Margolis, “Knowledge in the Humanities and Social Sciences”, in *Handbook of Epistemology*, ed. I. Niiniluoto, M. Sintonen, and J. Wolenski Dordrecht: Kluwer Academic, 2004, pp. 607-646.

³⁷See my “Mistakes”, *Synthese*, 136: 389-408, September 2003. I return to this point in the final section of this.

³⁸Richard Bernstein emphasizes a version of just this point as well. Phenomenologists proved no better than positivists at disclosing the determinate structures of the social world, and for a related reason. For just as the positivist notion of rationality could not account for the persistence of seemingly “irrational” behavior, phenomenologists could not on their side separate causal determinants from those dependent on an individual’s understanding. See Bernstein, *op. cit.*, 156-67. Although I do not argue here for the view, I maintain that just as the positivist program required the analytic-synthetic distinction in order to forge a working theory of verification,

Neither of these dual and parallel failings — the inability to identify the uniquely “world stuff” to serve as determinate evidence for theories of nature and correspondingly fixed logical points or some special “social stuff” (meanings, practices) and fixed “mental” points (rules, transcendental grounds of meaning) on which to base accounts of the social world — threatens naturalism. For the very blurring of the concept of science allow investigators of the world around us to move past futile debates regarding demarcation — science versus non-science, natural versus social — and on to substantive debates regarding what difference proposed different theories make for purposes of explanation and social engineering.³⁹

3 NATURALIZED EPISTEMOLOGY AND SOCIAL SCIENCE

In this final section, I wish to briefly explore two versions of naturalized epistemology which appear to hold special relevance for the philosophy of social science. One takes to heart the Darwinian paradigm and an evolutionary model. Major exponents of this view include, Donald Campbell, Alex Rosenberg, and Michael Bradie. The other major naturalistic approach stems from work by Barry Barnes, which he terms “natural rationality”. The person who has pushed the critical edge of this view the hardest and the furthest is not Barnes himself, but Stephen Turner. I explore these views in turn.

Michael Bradie distinguishes evolutionary epistemology from other flavors of naturalized epistemology in the following way:

phenomenologists needed a similar distinction between what is objective and constitutive of meaning and what is not in order to make the phenomenology of the social into a suitably scientific enterprise. But phenomenology fared no better with the notion of objective meaning than did positivists with the notion of analyticity, and with similar results. Without this principled distinction in hand, notions only hold fast because people choose to do so, and no mechanism exists, in any case, describing what it is to which people do hold fast when they favor certain beliefs over others.

³⁹In this regard, what remains of the search for the “unity of science” consists primarily of what Charles Morris long ago presciently termed the “pragmatics” of science. See in particular Morris’s essay “Scientific Empiricism” in the *Encyclopedia and Unified Science*, ed., O. Neurath, N. Bohr, J. Dewey, B. Russell, R. Carnap, and C.W. Morris, Chicago: The University of Chicago Press, 1938, pp. 63–75. This is, in turn, Volume I and Number 1 of the *International Encyclopedia of Unified Science* (a series to which *The Structure of Scientific Revolutions* also belongs). Morris’s remarks on pp. 72–75 prove significant and prophetic, especially in the current scene where the notions of the social and the scientific are often treated as contrastive and inimical. In particular, Morris states, “Further, the confirmation of every proposition always involves some instrument, whether this be simply the scientist himself or in addition such instruments as those involved in experimentation — and methodologically there is no important distinction between the two cases. In this (theoretically the most important) sense, all empirical science involves experimentation, and experimentation is an activity, a practice. . . [S]cience is part of the practice of the community in which it is an institution, ministering — however indirectly — to the needs of the community and being affected — and very directly — in its development by the community of social institutions of which it is a part. It is clear that any adequate account of science must take account of these psychological, methodological, and sociological aspects of scientific practice.” (ibid., 72) Compare with Dewey’s essay in this volume, and note the contrast with the essays therein by Russell and Carnap.

Human beings, as the products of evolutionary development, are natural beings. Their capacities for knowledge and belief are also the products of a natural evolutionary development. As such, there is some reason to suspect that knowing, as a natural activity, could and should be treated and analyzed along lines compatible with its status, i.e., by the methods of natural science. On this view, there is no sharp division of labor between science and epistemology. . . Such approaches, in general, are called naturalistic epistemologies, whether they are directly motivated by evolutionary considerations or not. Those which are directly motivated by evolutionary considerations and which argue that the growth of knowledge follows the pattern of evolution in biology are called "evolutionary epistemologies".⁴⁰

Bradie distinguishes between those who conceive of evolutionary epistemology in terms of mechanisms and those who imagine it in terms of competitor or successive scientific theories. The former comports well, he notes, with the contemporary understanding of biological theory. "There is a sense in which some version of the [evolutionary view of human epistemological/cognitive mechanisms] must be true if our current understanding of evolutionary processes is anywhere near correct. What remains to be seen is what useful insights, if any, will be forthcoming about the evolution of the cognitive mechanisms of organisms".⁴¹ Bradie's characterization of evolutionary epistemology as focused on mechanisms might appear to raise again the question of whether naturalized epistemology somehow precludes inquiry into normative issues. He rightly rejects this implication. "If one construes knowledge along Quinean lines as a holistic product of norms and experience, then just as our knowledge *claims* are conjectural and subject to revision so the norms we employ to validate them can be construed as conjectural and subject to revision as well".⁴²

⁴⁰Michael Bradie, "Naturalism and Evolutionary Epistemologies", in *Handbook of Epistemology*, ed. I. Niiniluoto, M. Sintonen, and J. Wolenski Dordrecht: Kluwer Academic, 2004, 735–46, p. 735. This article contains a helpful and current bibliography of work in this area. The *locus classicus* for providing a formulation of an "evolutionary epistemology" in the philosophy of social science is Donald T. Campbell's "Evolutionary Epistemology" in *The Philosophy of Karl Popper*, ed. Paul A. Schilpp, LaSalle, IL: Open Court, 1974, 413–63. Worth noting here is Popper's uncharacteristically enthusiastic response to Campbell's essay, *ibid.*, 1059–65. Campbell is guarded about the extension of this evolutionary model to a kind of social Darwinism view of theories in science, but he does not rule out a role for an evolutionary account even here. See, e.g., Donald T. Campbell, "Science Policy from a Naturalistic Sociological Epistemology", *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association (1984)* 1984: 14–29. General discussions of the significance of Campbell's work on evolutionary epistemology can be found in *selection theory and social construction: the evolutionary naturalistic epistemology of Donald T. Campbell*, ed. Cecilia Heyes and David L. Hull, Albany, NY: SUNY Press, 2001. An anthology emphasizing more the Popperian roots of this view is *Evolutionary epistemology, rationality, and the sociology of knowledge*, ed. G. Radnitzky and W.W. Bartley III, La Salle, Illinois: Open Court, 1987.

⁴¹*ibid.*, 739.

⁴²*ibid.*, 742. For a detailed defense of the view, see Paul A. Roth, "The Epistemology of 'Epistemology Naturalized'", *op. cit.*

One of the most informed and trenchant commentators of efforts to apply a Darwinian model to issues in the philosophy of science or social science is Alex Rosenberg. However, his own writing appears to move through at least three distinct stages. Each separately is worth noting, and collectively they chart a type of evolution not just of a single thinker, but of a species of thinking about how biological models might (or might not) yield more general epistemological insight.

His 1980 book, *Sociobiology and the Preemption of Social Science*,⁴³ marks what I take to be the first stage in the evolution of Rosenberg's thoughts on this topic. In this work, Rosenberg states forcefully a case for sociobiology as representing the "best bet" (perhaps the only bet) which would allow social scientists to fulfill an ambition to be scientists of the social. The key here, and where change occurs under the pressure of Rosenberg's own thought, is that at this point in time, Rosenberg accepts as given that biology counts as a science if anything does. However, that view changes.

In particular, his view on the nature of biological science alters in two distinct ways, each of interest in its own right. The first alteration primarily concerns what Rosenberg now views as a misplaced enthusiasm for the applicability of a Darwinian model to inquiry generally. While he does not rule out in principle, so to speak, that such a model could be applied, he provides by far the best available critiques in the literature of why proposals actually on the table fail to deliver as promised.⁴⁴

But he also raises questions here regarding the status of biology as a science. Rosenberg's conception of science connects to a certain view of what passes muster as a law, and he professes skepticism with regard to the existence or even the possibility of such laws in biology. Thus a naturalist invested in the Darwinian model faces a double failing: on the one hand, a failure of the Darwinian model to do what the naturalist promises in epistemology, and, on the other hand, the failure of biology, *contra* what a work such as that discussed in the previous paragraph assumes, to be a science in the full-blooded sense of that term.⁴⁵

However, in recent work Rosenberg brings together these seemingly conflicting themes — biology looms as the best model for social science, but no one can make this model apply in a fruitful way and biology does not seem to be a true science anyway — in an unanticipated but suggestive way.⁴⁶ For, Rosenberg argues, what three decades of work in biology reveals is how much more like the social sciences

⁴³(Johns Hopkins University Press, 1980.)

⁴⁴The key essays with respect to this facet of Rosenberg's thought are to be found in his excellent collection, *Darwinism in Philosophy, Social Science and Policy*, Cambridge: Cambridge University Press, 2000. Particularly notable are the first two essays in this collection, "A Field Guide to Recent Species of Naturalism" and "Naturalistic Epistemology for Eliminative Materialists".

⁴⁵See his essay, "Limits to Biological Knowledge", in *Darwinism . . .*, op. cit. This rehearses issues more fully argued in his *Instrumental biology, or, The disunity of science*, Chicago: The University of Chicago Press, 1994.

⁴⁶The essay in question is his "Lessons from Biology for Philosophy of the Human Sciences", *Philosophy of the Social Sciences*, 35: 3–19, March 2005, hereafter just referred to in the text as "Lessons".

than the natural sciences biology is. But he maintains, still in all, social science would do best to move closer to the example biology provides.

In what follow I will not be able to give more than a brief summary of what we have learned about the nature of biological science in the past three decades. My aim will rather be to show how it applies to a budget of problems in the philosophy of social science. I start with a simple argument that all the social and behavioral sciences need to be viewed as biological ones. Then I will try to show that doing so leaves most of them pretty much as they were *ex ante*. In effect my project is one of giving the right reasons (and displacing bad arguments) for viewing the social and behavioral sciences as pretty much on the right track, or at least as doing as well as can be expected in the business of explaining and predicting human affairs. ("Lessons", 4)

So, while remaining in the spirit of his work from 25 years earlier — the only plausible model for social science can be found in biological science, Rosenberg surprisingly concludes that what has transpired in the interim has reinforced this view by revealing a deep connection between biology and history, and so the filiation of biological science lies, in fact, in a social science. As he remarks, "Biology is an *almost* completely historical science." ("Lessons", 4)

As one might suspect, the key to Rosenberg's benign appraisal of the current state of affairs in the social sciences lies in the "almost", if not biology. On the one hand, evolution produces functionally related kinds; but functionally determined kinds make it unlikely that the adaptive traits will be explicable by appeal to perfectly general or universal laws as opposed to local, ecological factors. (See "Lessons", 5-6). But, on the other hand, the locality at work in determining kindness is offset, Rosenberg maintains, but the fact that evolutionary theory provides an account of mechanism which counts as a law even by the standard Rosenberg sets: "There is one law or set of laws that is distinctive of biology: the principle or principles of natural selection, which describe the way in which adaptations come about in a purely mechanistic world bereft of causally efficacious purpose, goals, or ends." ("Lessons", 6-7) So while historical (local) contingencies set the problems that organism must overcome or perish, natural selection provides the mechanism which determines how the story plays out.

This brings together the explanatory burden to which the biological or the human scientist must answer. "The task of the biologist and the human scientist is to identify the design problems faced by creatures so that they can individuate the adaptive traits, explain what they were selected for, reveal the mechanism by which they solve the design problem, and then if possible and interesting [sic] explain and predict the particular occasions on which these solutions are deployed." ("Lessons", 10) Rosenberg boldly speculates that this rubric will prove broad enough to cover what presently appears not as a common thread but rather as a gap between how so-called interpretative social science proceeds and the biological and natural sciences.

Interpretative human science, hermeneutics, qualitative social science, symbolic interaction, these are all names for an approach to human behavior as unavoidable as adaptationalism is in biology. And the reason is simple. Humans are biological creatures and interpretation just is adaptationalism. The only difference between the subject matters of interpretative social science, the mathematical modeling social sciences, and the historical/comparative social sciences is the rate at which selection operates to overtake the generalizations these disciplines could or seek to articulate. ("Lessons", 15)

Rosenberg's account here remains at one and the same time the boldest and also the most specific proposal for adapting the biological model to the social sciences. Reduction now proceeds in terms of the search for a mechanism, and the mechanism in turn is that which the natural selection provides. "For each of the real patterns — transitory or persistent — uncovered in the human sciences, there must be a set of underlying mechanisms put in place by natural selection". ("Lessons", 19) It provides as specific a research program as has been proposed in this area. Whether the proposal finds takers, and its results, remains to be seen.

The other version of a naturalized epistemology which holds clear promise for social science turns on a proposal first articulated by Barry Barnes in a landmark piece published in the mid-1970s.⁴⁷ I shall refer to it as the "natural rationality" view (hereafter NR). In the work of the strong programme in the sociology of knowledge which Barnes helped found and with which he has so long been identified, this view has been encapsulated in what is termed the "symmetry principle", i.e., rational and irrational belief acceptance both require explanation. What makes "good" reasons good, that is, cannot not be assumed to be transculturally transparent.

But, I suggest, at least two different strains of NR apart from that propounded by sociologists of science such as Barnes can be identified. One I term the Weberization of the sociology of science, a view put forward in many works by Steve Fuller.⁴⁸ The other involves Stephen Turner's efforts to naturalize talk of rationality and normativity. Turner, in the spirit if not the letter of Rosenberg, lays

⁴⁷See Barry Barnes, "Natural Rationality: A Neglected Concept in the Social Sciences", *Philosophy of the Social Sciences*, 6: 115-126, June 1976. He returns to this theme and updates it slightly in his "How Not to Do the Sociology of Knowledge", *Annals of Scholarship*, 8: 321-36, 1991. But the core of the position does not alter from that articulated in the earlier essay.

⁴⁸I will not here discuss Fuller's work, or at least that aspect of it relevant to a naturalized epistemology and its applications to a philosophy or sociology of science. But Fuller, it should be noted, has been a relentless critic of how sociologists of science, and many in the science studies field, have unflinchingly adopted a "descriptive only" approach to the study of science. Regarding a more general discussion of norms, in terms of how science ought to proceed, and particularly how science policy ought to be fashioned, he finds the science studies literature to be not just quiet, but quietistic. Unlike Woolgar, who emphasizes the irony of this approach, Fuller straightforwardly castigates those in science studies for this quietism. A good example of his work in this vein remains *Philosophy of Science and its Discontents*, 2nd Edition, New York: Guilford, 1993.

emphasis on understanding the mechanism of the transmission of the social.⁴⁹ While not concerned to emulate accounts which follow natural selection, his efforts have a not dissimilar effect, viz., eliminating from social science talk of anything irreducibly social as a causal factor.

A succinct statement of NR is the following: "the fact that we ourselves accept... knowledge as valid does not mean that its emergence, acceptance, and persistence are not empirical phenomena. Acts of validation and assertions of validity are themselves empirical phenomena, and as such are available for sociological investigation".⁵⁰ Perhaps one could quibble here with the use of 'sociological' to modify investigation, but the quibble would only concern the fact that the adjective might mistakenly imply a limiting kind of inquiry into the nature of the phenomenon in question. For what represents the core of NR resides in the claim that inferential practices constitute a type of empirical phenomena, to be studied and understood along with other empirical phenomena, that is to say, naturalistically.

Insofar as inferential practices fall under the heading of empirical phenomena, all must be regarded as contingent. This might strike some as an endorsement of a form of relativism, but that would be mistaken. The spirit here, rather, is best exemplified by Quine's recurrent use of Neurath's metaphor of rebuilding the ship while afloat on it. One's belief structure must constantly be repaired and modified while in use. The status or epistemic place assigned certain practices only reflects a fact about the practices endorsed by particular groups at particular times.

Barnes's favored example of a work which explores 'natural rationality' in a manner he approves turns out, somewhat surprisingly, to be the work of a philosopher of science, Mary Hesse.⁵¹ What Barnes likes is the use by Hesse of a social science to help understand how successful learning, and so the development of science itself, is possible.

[Hesse attempts] to elucidate the natural proclivities which make learning of any kind possible—including the learning of conventions. She strives to identify the preconditions which enable us to find things intelligible at all. This is why her work must be praised and defended as a valuable essay in speculative psychology. Its subject is man as thinker rather than the logic of the natural sciences; its achievement

⁴⁹Of particular note here is his important book, *The Social Theory of Practices*, Chicago: The University of Chicago Press, 1994. See also his more recent collection of essays, *Brains/Practices/Relativism: Social Theory after Cognitive Science*, op. cit.

⁵⁰Barry Barnes, "How Not To Do the Sociology of Knowledge", op. cit., p. 321.

⁵¹In this regard, much of the work of Ian Hacking reflects an examination of certain aspects of "natural rationality". See in particular his "Making Up People", in *Reconstructing Individualism*, ed. T. Heller, M. Sosna, and D. Wellberry, Stanford: Stanford University Press, 1986, "World Making by Kind Making: Child Abuse for example", in *How Classification Works*, ed. Mary Douglas and David Hull, Edinburgh: Edinburgh U.P., 1992, and "The Looping Effects of Human Kinds", op. cit. For a well-taken caution regarding the notion of "social construction", see Stephen Turner's essay, "The Limits of Social Constructionism", in *Turner's Brains/Practices/Relativism: Social Theory after Cognitive Science*, Chicago: The University of Chicago Press, 2002.

may or may not be epistemology; it is certainly a theory of natural rationality.⁵²

There is here, as Barnes acknowledges in later work, more than a slight echo of themes from philosophers such as Nelson Goodman and W.V. Quine. The use of psychology to understand how humans might “bootstrap” themselves into more sophisticated forms of thought defines the naturalization of rationality.

Stephen Turner’s work extends and deepens the account of natural rationality by challenging proposed explanations of reasoning which neglect to account for how the relevant norms and other “shared stuff” read into the heads of members of a society gets to be there. At the core of Turner’s critique of many contemporary varieties of social theory is the “transmission argument”: either provide an account of what is transmitted and how, or forswear the use of a “shared something” as explanatory of observed uniformities in behavior. The core of the argument stresses that what can be observed by way of inculcating uniformities of behavior cannot account for what social theorists characteristically claim is shared, and so appeals to “sharing” turn out to be explanatorily idle — to add nothing to the noting of behavioral conformity. “There is in general no way to make a distinction between ‘having habits that enable public proficiency’ and ‘possessing some shared thing of the basis of which proficiency is possible.’”⁵³ The problem is worse than Turner’s statement suggests inasmuch as there exists no accounts of the norms, rules etc. which any one individual follows, much less a going account of what sharers share “in the head”. Notions such as practices and norms stand in need of clarification and explication and as such can make have no positive contributions when employed in the *explanans*.

The implications of this view for any theory of natural rationality prove profound. For it forces debates about the nature of the social to deliver on mechanisms which must themselves be found “in the open” and influencing individuals in particular ways. At this point, as Turner argues elsewhere, any meaningful distinction between talk of “social construction” and “ordinary” history collapses. For purposes of explaining the social, only history remains.⁵⁴

“Naturalizing” the social has the consequence, both Rosenberg and Turner suggest, of eliminating the social as itself an explanatory notion. Given the paucity of results in social science, one may well wonder what would be lost in this case. Moreover, in each case, the naturalizing move has the advantage of explaining why results in the social sciences have been so meager and hard to come by. Neither rules out the discovery of or a role for generalization. Indeed, Rosenberg insists on such a role. But both indicate why such generalizations will be few, transitory,

⁵²Barnes, “Natural Rationality”, op. cit., p. 121.

⁵³Turner, *The Social Theory of Practices*, op. cit., p. 111. For an expansion and defense of Turner’s points here, see my essay “Mistakes”, op. cit., and “Why There is Nothing Rather than Something: Quine on Behaviorism, Meaning, and Indeterminacy”, *Philosophy, Psychology, and Psychologism: Critical and Historical Readings on the Psychological Turn in Philosophy*, ed. D. Jacquette, Kluwer Academic 2003, 263-287.

⁵⁴Turner, *Brains...*, op. cit., 119.

and difficult in any case to come by.

Bertrand Russell wrote that "every advance in knowledge robs philosophy of some problems which formerly it had, and... it will follow that a number of problems which had belonged to philosophy will have ceased to belong to philosophy and will belong to science".⁵⁵ The dismal image of philosophy Russell offers here pictures the discipline that can prosper and thrive only by lurking in those shadows when the sun of systematic scientific inquiry has yet to shine. Philosophy so conceived cannot survive coeval with science. (Russell, in fact, goes on to compare anti-scientific philosophers to those who continue to migrate to avoid the encroachments of civilization.)

Naturalism does not name the "better half" of a new dualism, one encompassing and superseding all others. To the contrary, as argued above, naturalists need not even insist that anything special marks science from all the rest. It can rest with discovering (and modifying) the conception of science as inquiry proceeds. Naturalists scoff at those who imagine that disciplinary boundaries carve the world at its joints and that department names name an intellectual essence. Thus, naturalism does not define itself by oppositions, but by placing philosophy within and as part of those disciplines which seek to make the best possible overall systematic sense of ourselves and the world.

⁵⁵Bertrand Russell, "What There is", [1918], reprinted in *Classics of Analytic Philosophy*, ed. R. Ammerman, Indianapolis, IN: Hackett, 1990, p. 34.