Puzzling Out Science

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Science embodies the epitome of man's intellectual development.
—C.S. Peirce[1]

To say that man is made up of strength and weakness, of insight and blindness, of pettiness and grandeur, is not to draw up an indictment against him: it is to define him.
—Diderot[2]

What do the natural sciences know, and how do they know it? I approach our topic with a little diffidence, since I am neither a scientist nor a historian of science, not even a specialist in the philosophy of science, but a plain epistemologist. Indeed, recalling Sheldon Glashow's reaction to an invitation to discuss what prophets of the end of science present as Grave Epistemological Issues: "forgive me if I thrash about a bit—it's not easy to beat a dead oxymoron,"[3] I feel more than a *little* diffidence! I, too, shall thrash about a bit; in my case, however, not out of pure exasperation, but in hopes of separating the chaff, the absurdities of radical critics who profess to believe that we can no longer imagine that science is an objective endeavor, but must acknowledge it to be a "subjective and relativistic project, operating out of social attitudes and ideologies,"[4] from the serious questions to which an honest epistemologist might be able to contribute something.[5]

Once upon a time (the phrase is a warning that what follows will be cartoon history) it was taken for granted that science enjoys a peculiar epistemic authority because of its uniquely objective and rational method of inquiry. Successive efforts to articulate what that uniquely rational method might be gave rise to umpteen competing versions of what I shall call the Old Deferentialist position:[6] science progresses inductively by accumulating true theories confirmed by empirical evidence, by observed facts; or deductively, by testing conjectures against basic statements and, as falsified conjectures are replaced by corroborated ones, improving the verisimilitude of its theories; or instrumentally, by developing theories which, though not themselves capable of truth or falsity, are efficient instruments of prediction; or, etc., etc. Such obstacles as Quine's thesis of the underdetermination of theories even by all possible observational evidence,[7] Russell Hanson's and others' of the theory-dependence of observation,[8] Goodman's "new riddle of induction,"[9] though acknowledged as tough, were assumed to be superable, or avoidable.

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It is tempting to describe these problems in Kuhnian terms, as anomalies facing the Old Deferentialist paradigm just as a rival was beginning to stir. Kuhn himself, evidently, did not intend radically to undermine the pretensions of science to be a rational enterprise.[10] But most readers of *The Structure of Scientific Revolutions*, missing many subtleties and many ambiguities, heard only: science progresses, or "progresses," not by the accumulation of well-confirmed truths, but by revolutionary upheavals; there are no paradigm-neutral standards of evidence, only the different standards of incommensurable paradigms; a scientific revolution, like a political revolution, depends on propaganda and control of resources; a scientist's shift in allegiance to a new paradigm is like a religious conversion, a conversion after which things look so different to him that he may be said to "live in a different world."

Even so, when twenty years ago Feyerabend proclaimed that there is no scientific method, that appeals to "rationality" and "evidence" are nothing more than rhetorical bullying, he was widely regarded—indeed, he described himself—as the "court jester" of the philosophy of science.[11] Mainstream philosophers of science mostly continued (and continue) to be convinced that the Old Deferentialism is correct in essentials, though conceding that much more work is needed on the details.

Of late, however, radical sociologists, radical feminists and Afrocentrists, and radical followers of the latest Paris fashions in rhetoric and semiotics, have turned their attention to science. Now it is commonplace to hear that science is largely or even wholly a matter of social interests, negotiation, myth-making, the production of inscriptions; that "objectivity" and "rationality" are nothing but ideological constructs disguising the exclusion of the perspective of this or that oppressed group. "[T]he validity of theoretical propositions in the sciences is in no way affected by factual evidence," Kenneth Gergen tells us; "the natural world," Harry Collins writes, "has a small or non-existent role in the construction of scientific knowledge."[12] According to this new orthodoxy, not only does science have no peculiar epistemic authority and no uniquely rational method, it is really, like all purported "inquiry," just politics. "While the dangers are real," Lynn Nelson observes, "the 'noble lie' [that politics can be kept out of science] is far more dangerous"; "I don't see any difference," Steve Fuller boasts, "between 'good scholarship' and 'political relevance.' Both will vary, depending on who you are trying to court in your work."[13]

What the New Cynics offer in place of argument or evidence for their startling epistemological claims is an incoherent farrago of confusion, *non sequitur*, and rhetoric—the last accompanied by an astonishing outbreak of sneer quotes. It is beyond my power, and your tolerance, to deal comprehensively with all this; but not, I hope, to discern some main strategies, and a key fallacy.

The basic strategy is to shift attention from the normative notion of *warrant* (of how good the evidence is for this or that scientific claim) onto the descriptive notion of *acceptance* (the standing of a claim in the eyes of the relevant community). Some play down warrant and accentuate acceptance, insisting that social values are inseparable from evidential ones; some ignore warrant altogether and acknowledge only acceptance, conceiving of scientific knowledge as nothing more than the upshot of processes of social negotiation; some engage in a kind of conceptual kidnapping, replacing the notion of warrant by a socio-political ersatz—thus, for example, "democratic epistemology."

Why the shift? In part, perhaps, because, coming from sociology, anthropology, literary theory or feminist philosophy, many New Cynics are predisposed to focus on the social or the rhetorical, and not well-equipped to attend to the often formidable technicalities involved in questions about the warrant of scientific claims. In part, again, perhaps, because, inadvertently or otherwise, some New Cynics have run together legitimate social and political questions about, e.g., the uses of technology or government funding of scientific projects, with epistemological issues.[14]

More interestingly, some may have been led to the conclusion that scientific knowledge is nothing but a social construction by a faulty inference from the true premise that the scientific knowledge we now possess has been the work of a whole, intergenerational, community of inquirers. Some, again, may have been led to the conclusion that reality is socially constructed by a faulty inference from the true premise that (because of the ever-increasing interpenetration of theory and instrumentation), the objects of scientists' observations are often what one might call "laboratory phenomena"; or by the same faulty inference from the true premise that social institutions and categories (the objects of sociological theories) would not exist were it not for human activities. It obviously does not follow, and neither is it true, that reality, even that social or laboratory reality, is created by scientists' theorizing.

Most interestingly, many New Cynics disdain the notions of evidence and warrant because, as they are at pains to point out, what has passed for warranted theory, relevant evidence, known fact, objective truth, has often enough turned out to be no such thing. This is true; but the conclusion that the notions of warrant, evidence, truth, fact, reality, knowledge, are ideological humbug, manifestly does not follow. So ubiquitous has this manifestly invalid form of inference become of late, however, that it deserves a name; I call it "the 'passes for' fallacy."

Applied, as the New Cynics apply it, to the concepts of evidence, warrant, etc., the "passes for" fallacy is not only fallacious, but self-undermining. If the notions of evidence, fact, truth, etc., *were* ideological humbug, the "investigations," which supposedly established this could not, as advertised, have found "evidence" indicating the "truth" of the social constructivist thesis, but could themselves be no more than socially negotiated myth-making. If you think this point too obvious to be worth making, listen to Stephen Cole: "Given that facts can easily become errors, what sense does it make to see what is at Time 1 a 'fact' and at Time 2 an 'error' as being *determined* by nature?"; and then, a few pages later, "the most important evidence in support of my position is the fact that."[15] Feminists who maintain that the underdetermination of theories by data means that political values may legitimately take up the slack in deciding what theories to accept, and yet assume that we can know what theories would conduce to women's interests, likewise, are sawing through the branch on which they sit.[16]

The shallow rhetoric and the self-defeating arguments of contemporary cynics deserve, indeed, the treatment appropriate to dead oxymorons; and so, holding my nose and stepping delicately around these obstacles, I shall try, in what follows, to sketch an approach that might avoid the real difficulties of the Old Deferentialism without surrendering to the "factitious despair"[17] of the New Cynics.

From the fact that what has passed in science for objective evidence, established truth, etc., has sometimes been nothing of the kind, it does not follow that there are no objective epistemic standards, nor that there is nothing epistemologically special about science. And these cynical ideas are not only unproven, they are false: there *are* objective epistemic standards, and there *is* something epistemologically special about science. The Old Deferentialist picture rightly acknowledges this, but in the wrong way—indeed, by conceiving of science as the source of our epistemic standards, in a way that gives aid and comfort to the "passes for" fallacy. A better way sees science, not as *privileged*, but as *distinguished* epistemically; as deserving, if you will, *respect* rather than *deference*. Science is neither sacred nor a confidence trick.

Our standards of what constitutes good, honest, thorough inquiry and what constitutes good, strong, supportive evidence, are not internal to science. In judging where science has succeeded and where it has failed, in what areas and at what times it has done better and in what worse, we are appealing to the standards by which we judge the solidity of empirical beliefs, or the rigor and thoroughness of empirical inquiry, generally. (Nor, as Old Deferentialists sometimes suggest, is science the *only* source of knowledge.)[18]

It is important to distinguish the question, how to assess the worth of evidence for a proposition, from the question, how to conduct inquiry. The two are run together in much contemporary epistemology and philosophy of science; but, though related, they are as different as criteria for judging roses are from instructions for growing them. The former question, though hard enough, is a bit more tractable than the latter. The goal of inquiry is to discover significant, substantial truths; and, since there is a certain tension between the two aspects of the goal—it is a lot easier to get truths if one doesn't mind the truths one gets being trivial—there can be, at best, guidelines, not rules, for the conduct of inquiry. Criteria for appraisal of the worth of evidence, on the other hand, are focused on only one aspect of the goal, on truth-indicativeness.

Not all scientific theories are well-confirmed by objective evidence; most are, at some stages at least, no more than tenuously supported speculations.

Some may get accepted, even entrenched, on flimsy evidence. Nevertheless, science has succeeded extraordinarily well, by and large, by our standards of empirical evidence.

The best model of those standards is not, as much recent epistemology has assumed, a mathematical proof, but a crossword puzzle. The clues are the analogue of experiential evidence, already-completed entries the analogue of background information. How reasonable an entry in a crossword is depends upon how well it is supported by the clue and any other already completed intersecting entries; on how reasonable, independently of the entry in question, those other entries are; and on how much of the crossword has been completed. An empirical proposition is more or less warranted depending on how well it is supported by experiential evidence and background beliefs; how secure the relevant background beliefs are, independently of the proposition in question; and how much of the relevant evidence the evidence includes. How well evidence supports a proposition depends on how much the addition of the proposition in question improves its explanatory integration. There is such a thing as supportive-but-less-than-conclusive evidence, even if there is no formalizable inductive logic.[19]

Science has come up with deep, broad, and explanatory theories, which are well anchored in experiential evidence and which interlock surprisingly with each other. And nothing succeeds like success; having, as it were, plausibly filled in long, central entries greatly improves the prospects for completing other parts of the puzzle.

The crossword analogy suggests a way to come to terms with some Kuhnian themes sometimes perceived as a threat to the idea of objective evidential support. Normal science can be thought of on the analogy of working on smaller, non-central entries while taking the correctness of intersecting, already-completed, long, central entries for granted. A crisis situation in which anomalies start to pile up for a paradigm can be thought of on the analogy of the smaller, non-central entries becoming more and more strained, as the constraints imposed by the already-completed central entries make it harder and harder to fill them in a way that really fits the clues. A scientific revolution can be thought of on the analogy of finding oneself obliged to revise several long, central entries and, as a result, having to rub out a whole slew of other entries, since they are no longer compatible with the revised ones. (I can't resist adding that fallibilism can be construed as the analogue of the principle: if you must use ink, make sure it's washable!)

The sting is taken out of the supposed paradigm-dependence of observations, things looking different after a paradigm-shift, when it is thought of on the analogy of the way each entry depends, not only on the clue, but also on intersecting entries. The thing observed doesn't change, nor the observer's perceptual state, but the judgment he makes of what he sees changes, because of his changed background beliefs.[20]

The sting is also taken out of the supposed paradigm-relativity of epistemic standards, which is not real incommensurability, but deep-seated disagreement in background beliefs. Suppose you and I are working on the same crossword. You think, given your solution to 7 across, that the fact that a solution to 2 down ends in an "E" is evidence in its favor; I, given my solution to 7 across, that the fact that it ends in an "S" is evidence in its favor. In a weak sense, we might be said to differ about "what counts as evidence," but we are both seeking to fit the entry to its clue and to other relevant entries; in the sense that matters, there is no relativity of standards. Compare the case where you and I are both on an appointments committee; you argue that this candidate should be ruled out on the grounds that his handwriting indicates that he is not to be trusted, I think graphology is bunk and scoff at your supposed evidence. Once again, what we have is disagreement in background beliefs, not real incommensurability of epistemic standards; and the same goes, though on a much larger scale, for disagreements between proponents of rival paradigms.[21]

Epistemic distinction, not privilege: when one turns to the question of method, of the conduct of inquiry, the same kind of curtailment of the Old Deferentialists' conception is called for. In the narrow sense in which the phrase supposedly refers to a set of rules which can be followed mechanically and which are guaranteed to produce true, or probably true, results, there is no "scientific method." No mechanical procedure can avoid the need for discretion—as is revealed by the Popperian shift from: make a bold conjecture, test it as severely as possible, and, as soon as counter-evidence is found, abandon it and start again, to: make a bold conjecture, test it as severely as possible, and, if counter-evidence is found, don't give up too easily, but don't hang on to it too long. In a broader, vaguer, sense, in which the phrase "scientific method" refers to making conjectures, developing them, testing them, assessing the likelihood that they are true, of course there is such a thing; but not only scientists, but also historians, detectives, investigative journalists, and the rest of us, use "the method of science" in this sense.[22]

Nevertheless, there is something distinctive about inquiry in the sciences—or rather, several things: systematic effort to isolate one variable at a time; systematic commitment to criticism and testing; experimental contrivance of every kind; instruments of observation from the microscope to the questionnaire; all the complex apparatus of statistical evaluation and mathematical modelling; and the engagement, co-operative and competitive, of many persons, within and across generations, in the enterprise of scientific inquiry.[23] All of us, in figuring out how things are, use, in Peirce's phrase, "the method of experience and reasoning"; but science has, by all the means just listed, enormously deepened and extended the range of experience and the sophistication of reasoning of which it avails itself. Once again, as one considers, for example, how theoretical advances enable new instrumentation, the phrase that comes to mind is, "nothing succeeds like success."

An adequate epistemology of science will not, as some Old Deferentialists expected, be exclusively logical, but will have a social dimension. Unlike the New Cynicism, however, it will not see the fact that science is a social enterprise as illegitimating its epistemic pretensions, but as an important factor contributing to its epistemic distinction; not as a reason for favoring the notion of acceptance and neglecting warrant, but as an important factor helping to keep warrant and acceptance appropriately correlated.

The ideal would be a scientific community of creative and careful inquirers, with adequate resources of equipment and time, all sincerely seeking the truth and unaffected by prejudice, and each making his or her work freely available to the scrutiny of others, who would thereby be enabled to build on what is solid and to correct what is not.[24] For that would be a community in which creativity in hypothesis and care in seeking out and assessing the worth of evidence—the twin desiderata imposed by the dual goal of inquiry—were maximized.

Idealistic as this is, it already indicates why freedom of thought and information is vital to the scientific enterprise, and thus why pressure for "politically adequate research and scholarship," whether made by totalitarian governments or by feminist philosophers of science like Sandra Harding (whose no-doubt-unintentionally chilling words I just quoted),[25] is epistemologically as well as politically objectionable.

Making the model more realistic by acknowledging that any actual scientific community consists of real, imperfect human beings, reveals how individual idiosyncrasies, or weaknesses, may compensate for each other. I doubt that criteria of better and worse evidence will yield a linear ordering, and I am sure that no mechanical decision-procedure for theory-choice is to be expected. But a community of inquirers will usually, and usefully, include some who are quick to start looking for a new theory when the evidence begins to disfavor the old one, and others who are more inclined patiently to try modifying the old.[26] I doubt that real scientists are ever quite single-mindedly devoted to the truth; all, I expect, are motivated to some extent by the hope of fame or fortune, or to some degree in the grip of prejudice or partisanship. But to the extent that science is organized so as to maximize the likelihood that fame and fortune come to those who make real discoveries, or that partisans of one approach seek out the weaknesses which partisans of the other are motivated to neglect, a real community of imperfect inquirers can be a tolerable ersatz of an ideal community.[27]

Everything I have said so far presupposes that talk of "science" is shorthand for referring to a complex congeries of disciplines and sub-disciplines, the boundaries of which are more than a little fuzzy, and the different parts and stages of which are more than a little uneven in theoretical stability, methodological sophistication, and, yes, reliability. It is to be expected that the influence of prejudice and partisanship will be more of a hurdle in those areas of science where research bears most directly on politically contested issues, i.e., primarily, in the social and human sciences.

This prompts the further thought that the environment in which scientific work is done may be more *or less* conducive to good, honest, thorough, scrupulous inquiry. Among potential hindrances are: the necessity to spend large amounts of time and energy on obtaining resources, and to impress whatever body provides the funds, in due course, with one's success; dependence for resources on bodies with an interest in the research coming out this way or that, or in rivals being denied access to the results;[28] pressure to solve problems which are perceived as socially urgent, rather than freedom to pursue those most susceptible of solution in the present state of knowledge. It is no longer possible to do important scientific work with a candle and a piece of string—ever more sophisticated equipment is needed to obtain ever more *recherché* observations; so it is not surprising that my list of potential hindrances focuses on issues about resources. But other issues are also apropos: among them, a volume of publications so large as to impede rather than to assist communication; and, not least, the influence of the New Cynicism in discouraging some who might otherwise become, if not practicing scientists, members of that vital concomitant of a healthy scientific community, an educated public sufficiently literate scientifically "to distinguish genuine science from fantasy and superstition."[29] It would be less than candid not to acknowledge that this list of potential hindrances by no means encourages complacency.

The question of the progress of science also needs a nuanced answer somewhere between the optimism of the Old Deferentialism and the skepticism of the New Cynicism. At any time, some parts of science may be advancing, some stagnating, and others, quite possibly, regressing. Where there is progress, this may be a matter of accumulation of truths, or of replacement of discredited theories by better ones; and in the latter case, the new theory may entail that the old was correct in a limited domain, or may be incompatible with it, or may partially overlap it, and/or may introduce a new scheme of categories and concepts that can be translated into the older vocabulary only by clumsy circumlocution.

I have sketched an epistemology of science which is realistic in the ordinary sense, neither too optimistic nor too pessimistic, and realistic also in some of the technical senses most directly tied to questions about objectivity. A scientific claim is either true or else false; true or false objectively, i.e., independently of what anybody believes. The evidence for a scientific claim is strong or weak; strong or weak objectively, i.e., independently of how anybody judges it. There is no guarantee that every scientist is fully objective, i.e., is an absolutely disinterested truth-seeker. Nor is there any guarantee that, as science proceeds, it invariably adds to its accumulation of truths, or replaces false theories by true, or gets nearer the truth. But, though there are no grounds for complacency, though, like all human enterprises, science is far from perfect, it has been (if we don't blow it, it can continue to be) the most impressively successful of human cognitive enterprises. And—I conclude as I began, by quoting Charles Peirce, himself a working scientist as well as the greatest of American philosophers—"a man must be downright crazy to doubt that science has made many true discoveries."[30] Or a woman.

Notes

1. *Collected Papers*, ed. C. Hartshorne, P. Weiss, and A. Burks (Cambridge, Mass.: Harvard University Press, 1931–1958), 7.49.
2. *Addition aux Pensées Philosophiques* (c.1762), in *Rameau's Nephew and Other Works*, trans. Jacques Barzun and Ralph H. Bowen (New York: Doubleday, 1956).
3. Sheldon Lee Glashow, "The Death of Science!?," in *The End of Science*, ed. Richard Q. Elvee (Lanham, Md.: University Press of America, 1992), 23–31; the quotation is from p.25.
4. I quote from the letter of invitation to the conference on "The End of Science," held at Gustavus Adolphus College in 1989—the letter about which Glashow is expostulating above. My source is Larry Laudan, who quotes from the letter in the Preface to *Science and Relativism* (Chicago: Chicago University Press, 1990), ix.
5. My concern will here be exclusively with epistemological issues. This does not mean that I deny the legitimacy or the importance of, for example, moral and political issues about priorities in scientific research, or the paucity of female physicists or black biochemists, and so forth. I have had something to say about some of these issues in "Science 'From a Feminist Perspective,'" *Philosophy* (1992), reprinted in P. Halfpenny and P. McMylor's *Positivist Sociology and Its Critics*, Edward Elgar Press, 1994.
6. In this paper I shall be characterizing as the Old Deferentialism versus the New Cynicism what I formerly characterized as the Old Romanticism versus the New Cynicism ("Science 'From a Feminist Perspective,'" and "Epistemological Reflections of an Old Feminist," *Reason Papers*, 18, 1993, reprinted in *Partisan Review*, 1993). The earlier vocabulary, I now realize, was inappropriate because, as Leo Marx puts it, "much of today's criticism of science...may be traced to the...*romantic* reaction of European intellectuals which began in the late eighteenth century" ("Reflections on the Neo-Romantic Critique of Science," in *Limits of Scientific Inquiry*, eds. Gerald Holton and Robert S. Morison (New York: W.W. Norton, 1978), 63, my emphasis).
7. Though this is frequently referred to as "the Duhem-Quine thesis," the attribution to Duhem is not accurate. His thesis, that scientific claims are often not testable in isolation but only in conjunction with a bunch of other claims involved in reliance on instruments, is significantly more modest. Even Quine's commitment to the thesis is not unwavering. In "Empirical Content," *Theories and Things* (London and Cambridge, Mass.: Belknap Press of Harvard University Press, 1981), 24–30, he suggests that what he formerly described as empirically equivalent but incompatible theories would really be verbal variants of one theory (29–30). This reveals that the underdetermination thesis depends implicitly on criteria for the individuation of theories. See Murray G. Murphey, *Philosophical Foundations of Historical Knowledge* (Albany, N.Y.: SUNY Press, 1994), chap. 6, for critical discussion and references.
8. Norwood Russell Hanson, *Patterns of Discovery* (Cambridge: Cambridge University Press, 1958). The thesis that observation *statements* are theory-dependent is already found in Karl Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1959), originally published as *Logik der Forschung; zur Erkenntnistheorie der Modernen Naturwissenschaft* (Vienna: J. Springer, 1935).
9. Nelson Goodman, "The New Riddle of Induction," in *Fact, Fiction and Forecast* (Cambridge, Mass.: Harvard University Press, 1955).

10. See "Postscript—1969" in the second edition of *The Structure of Scientific Revolutions* (Chicago, Ill.: Chicago University Press, 1970); "Reflections on my Critics," in I. Lakatos and A. Musgrave, eds, *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970); "Second Thoughts on Paradigms," in F. Suppe, ed., *The Structure of Scientific Theories* (Urbana: University of Illinois Press, 1970); *The Essential Tension* (Chicago: Chicago University Press, 1977); "Commensurability, Comparability, Communicability," in vol. 2 of *PSA 1982*, ed. Peter D. Asquith and Thomas Nickles (East Lansing, Mich.: Philosophy of Science Association, 1983), 669–88.
11. P.K. Feyerabend, *Against Method* (London: New Left Books, 1975).
12. Kenneth Gergen, "Feminist Critique of Science and the Challenge of Social Epistemology," in *Feminist Thought and the Structure of Knowledge*, ed. Mary M. Gergen (New York: New York University Press, 1988), 37; Harry Collins, "Stages in the Empirical Programme of Relativism," *Social Studies of Science*, 11, 1981, p.3.
    Neither is atypical; compare:

    > How can we account for the fact [sic] that in any one year, approximately one and a half million dollars is spent to enable twenty-five people to produce forty papers?... *A fact is nothing but a statement with no modality... and no trace of authorship.* [Bruno Latour and Steve Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (Beverly Hills, Calif. and London: Sage Library of Social Research, 1979), 70, 82, emphasis in original].

    > [T]he construction of facts is a *collective* process.... I will call [this] our *first principle*.... What is the difference between rhetoric, so much despised, and science, so much admired?...[N]ot that the first makes use of external allies which the second refrains from using; the difference is that the first uses only *a few* of them, and the second *very many*.... [W]e must...come to call scientific the rhetoric able to mobilise on the one spot more resources...[Bruno Latour, *Science in Action* (Cambridge, Mass. and London: Harvard University Press, 1987), 29, 61, emphasis in original].

    > [W]hat the [natural] sciences actually observe is not bare nature but always only nature-as-an-object of knowledge—which is always already fully encultured.... Consequently, the natural sciences are usefully conceptualized as a subfield of social research. [Sandra Harding, "After the Neutrality Ideal: Science, Politics and 'Strong Objectivity'," *Social Research*, 59.3, p. 575 and note 12].

13. The first quotation is from Lynn Hankinson Nelson, *Who Knows? From Quine to a Feminist Empiricism* (Philadelphia: Temple University Press, 1990), 102; the second from an E-mail message, charmingly signed "yours in discourse, Steve Fuller." Fuller continues:

    > Standards of "accuracy" might seem pretty stable over time if the people you're primarily trying to please are those who inherited the jobs of the previous generation's dynasts.... However, if you are trying to court other constituencies, be they academic feminists or popular movements, then different standards will apply.... I am quite happy to talk to anyone about standards of scholarship, once you tell me who you are trying to impress.

    And elsewhere he writes:

    > [T]he cognitive value of a [socially] reproduced perspective cannot be clearly distinguished from its social or "survival" value. [Steve Fuller, "Provocation on Reproducing Perspectives," *Social Epistemology* 2, 1988, 100].

    A few more examples from my collection:

    > [K]nowledge is shaped by the assumptions, values and interests of a culture.... We can continue to do establishment science, comfortably wrapped in the myths of scientific rhetoric, or we can alter our intellectual allegiances. [Helen Longino, *Science as Social Knowledge* (Princeton, N.J.: Princeton University Press, 1990), 191].

> [T]he conditions for the production of epistemologies are political in the sense that these conditions reflect social hierarchies of power and privilege to determine who can participate in epistemological discussions.... [S]pecific theories of knowledge...reflect the social locatedness of the particular theorists.... [E]pistemologies have political effects insofar as they are discursive interventions in specific discursive and political spaces. [Linda Alcoff, "How Is Epistemology Political?," in Roger S. Gottlieb, ed., *Radical Philosophy: Tradition, Counter-Tradition, Politics* (Philadelphia: Temple University Press, 1993), 66].

> [W]e need to uncover masculinist bias and to construct accounts of epistemic practices that are free of such bias (as of other, interconnected biases) and adequate to the task of challenging it both in epistemology and, more importantly, in knowledge practices generally—that is, adequate to a feminist politics. [Naomi Scheman, "Feminist Epistemology," *Proceedings of the American Philosophical Association*, 68.1, 1994, 80].

14. Perhaps, also, some Old Deferentialists are not quite innocent of the same confusion.
15. *Making Science: Between Nature and Society* (Cambridge, Mass. and London: Harvard University Press, 1992), 12, 21. Cole is, by the way, regarded as a dangerous moderate among hard-line social constructivists, since he allows that the world *does*, after all, play some role in science; see the review of his book by the egregious Steve Fuller, *American Scientist*, June 1994, 295–96.
16. For example:

> [I criticized various studies published in *Science*] for their sloppy methods, inconclusive findings, and unwarranted interpretations.... Scientists are human, and each has a...specific location with respect to gender, class, race and ethnicity, and consequently, a set of values, beliefs, viewpoints.... [T]here must be...an irreducible level of distortion or biasing of knowledge production simply because science is a social activity performed by human beings in a specific cultural and temporal context. [Ruth Bleier, "Science and the Construction of Meanings in the Neurosciences," in Sue V. Rosser, ed., *Feminism Within the Science and Health Care Professions: Overcoming Resistance* (Oxford and New York: Pergamon Press, 1988), 92, 100, 101].

> Nineteenth-century biologists and chemists claimed that women's brains were smaller than men's and women's ovaries and uteruses required much energy and rest in order to function properly.... [F]eminist scholars have analyzed these self-serving theories and documented the absurdity of the claims.... Feminist science...must insist on the political nature and content of scientific work.... [Ruth Hubbard, "Some thoughts about the Masculinity of the Natural Sciences," in Mary .M. Gergen, ed., *Feminist Thought and the Structure of Knowledge*, [see note 12], 7, 13].

17. "The philosophy which is now in vogue...cherishes certain tenets...which tend to a deliberate and factitious despair, which...cuts the sinews and spur of industry.... And all for...the miserable vainglory of having it believed that whatever has not yet been discovered and comprehended can never be discovered or comprehended hereafter"—Francis Bacon, *The New Organon* (1620), Book One, Aphorism LXXXVIII.
18. It may be prudent to add that all I mean by this remark is that historians, detectives, investigative journalists and the rest of us all have perfectly good empirical knowledge; not that there are mysterious "ways of knowing" beyond experience and reasoning.

    It may be prudent to add, also, that I do not see literature as a competitor of science; briefly and roughly, science is a kind of inquiry, literature a kind of presentation—they are not properly conceived as rivals in the same domain.

    I discuss the nature of philosophical knowledge, and its relation to the scientific, in "Between the Scylla of Scientism and the Charybdis of Apriorism," forthcoming in *The Philosophy of Sir Peter Strawson*, ed. Lewis Hahn, Open Court.
19. The account sketched here is developed in detail in my *Evidence and Inquiry: Towards Reconstruction in Epistemology* (Oxford: Blackwell, 1993) chapter 4. In the interests of

intelligibility and brevity I have deliberately simplified certain issues. I have written of a theory's being more or less warranted as shorthand for the relevant scientific subcommunity's being, at a given time, more or less justified in accepting the theory. I have not spelled out (as I have in the book) how the degree of justification of a group of people depends on the degree to which each member is justified in his confidence in the reliability of the others. And I have written of "experiential evidence," without here taking into account that it is, of course, individuals, not communities, that have perceptual experience.

20. It is clear that Kuhn's view is that the subject's perceptual experience changes; see, particularly, "Second Thoughts," in Suppe, *The Structure of Scientific Theories*. Mine, by contrast, is that the perceptual judgment, always dependent on background beliefs as well as one's perceptual experience, changes as the background beliefs change, but the experience itself does not.
21. If the analogy strikes you as underestimating the difficulty of the problem, think of "diagramless" crosswords, where one doesn't know the number of letters an entry is to have!
22. It seems possible that Feyerabend's radical-sounding claims, that there is no scientific method, that "anything goes," is based in part on the correct perception that there are many scientific *techniques*, but no exclusive *method*. But this correct perception is no encouragement to epistemological anarchism.
23. "Co-operative and competitive" is intended as shorthand to indicate that, unlike some fuzzy feminists, I conceive of the relevant interactions not simply as manifestations of "trust," but as involving expertise, authority, institutionalized mutual criticism, and so on. David Hull, *Science as Process* (Chicago, Ill. and London: Chicago University Press, 1988), is a good source on these matters.
24. "[T]he...causes of the triumph of modern science, the considerable numbers of workers and the singleness of heart with which–(we may forget that there are a few selfseekers...; they are so few)–they cast their whole being into the service of science lead, of course, to their unreserved discussion with one another, to each being fully informed about the work of his neighbor, and availing himself of that neighbor's results; and thus in storming the stronghold of truth one mounts upon the shoulders of another who has to ordinary apprehension failed, but has in truth succeeded in virtue of his failure"–C.S. Peirce, *Collected Papers* [see note 1], 7.51.
25. Sandra Harding, *Whose Science? Whose Knowledge?* (Ithaca, N.Y.: Cornell University Press, 1991), 280.
26. Kuhn says something not dissimilar in "Postscript–1969" to the second edition of *The Structure of Scientific Revolutions*, 199.
27. Cf. Michael Polanyi, "The Republic of Science," in *Knowing and Being*, ed. Marjorie Greene (Chicago: University of Chicago Press, 1969), 49–62; and Susan Haack, "The First Rule of Reason," forthcoming in *New Topics in the Philosophy of Charles S. Peirce*, eds. J. Brunning and P. Forster (Toronto: Toronto University Press).
28. Cynthia Crossen, *Tainted Truth* (New York: Simon and Schuster, 1994), illustrates the kinds of danger I have in mind.
29. The phrase comes from Paul R. Gross and Norman Levitt, "The Natural Sciences: Trouble Ahead? Yes," *Academic Questions* (Spring 1994), 27, where the point I have mentioned is cogently argued.
30. *Collected Papers*, 5.172.