Rarely Pure and Never Simple: Talking about Truth

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The less said about truth, the better. That is one sentiment about the subject, and it has many and strong points to recommend it. There are quite enough theories of truth and definitions of truth to be going on with: truth as coherence; truth as correspondence; semantic theories of truth; conventionalist theories of truth; actor-network-stabilization theories of truth; pragmatist, deflationary, and redundancy theories (or antitheories) of truth.[1] The community of academic philosophers shows no signs of settling on any one of them, while both everyday worlds and technical subworlds seem to go on their knowledge-producing and knowledge-assessing ways without the benefit of such theories and definitions. I can think of no activities outside the academic world, and very few within it, that await the outcome of academic theorizing about truth.

I am not a philosopher; I have no professional interest in sorting or evaluating philosophers' theories about truth; I do not have another formal theory of truth to propose in opposition to any of theirs; and my part of the academic world is not in the epistemology business. That is to say, the sorting of my subjects' knowledge-claims into proper-stuff and pseudo-stuff is not my job. I am a historian and a sociologist of science. As such, I try to describe what various peoples in various temporal and cultural settings have counted as natural knowledge—knowledge that corresponds, or coheres, or that

1. For a specimen philosophical survey (and recommendation of a deflationary, minimalist conception of truth), see Paul Horwich, *Truth* (Oxford: Basil Blackwell, 1990).

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is in some other way deemed the right stuff. When I am feeling particularly ambitious, I sometimes venture explanations of why people come to the judgments they do.

Nothing follows from this for any sense I may have of the disciplines' relative virtues. I admit to intermittent irritation at philosophers' apparent hubris in setting themselves up, unsolicited, as judges of the quality of knowledge, and I sometimes wonder to what extent many philosophers really care to familiarize themselves with the knowledge whose quality they evaluate. On the other hand, I do not feel myself wholly unresponsive to charges that the rejection of evaluation is either disingenuous or a failure to take up the burden of intellectual duty. I have my intellectual preferences too, but, so far as possible, I try to keep them separate from my historical and sociological accounting. I suspect that epistemologists and sociologists are pretty much on a par with respect to their intellectual evaluations when they clock off their academic day-jobs: sociologists (of course) going to M.D.'s rather than astrologers when they feel poorly, and philosophers (of course) accepting their plumber's inadequately justified version of what is wrong with the pipes. The professional inquiries of both lots of academics are adapted to special-purpose intellectual tasks—one would have thought that much was obvious—and, as prescriptions for how to conduct everyday assessments, both would look fairly ridiculous. The activity of playing baseball and the activity of explaining baseball—in Stanley Fish's vivid example—are distinct: the baseball commentator can justifiably say that he knows more than the baseball player, so long as no claim is made that the commentator's knowledge can substitute for what the player knows, or that commentators' knowledge is a condition for improving play.[2]

As David Hume noted, the world begins to look very different when you leave the study for the street. Both special-purpose skepticism and special-purpose demands for certainty, clarity, universals, and foundations, which flourish in the schools and in the closet, "vanish like smoke" in everyday conversation and practical action. If you feel a fit of metaphysical or epistemological anxiety coming on, Hume suggested, you should have something nice to eat, a chat with your friends, and a game of backgammon.[3] That should make

2. Stanley Fish, "Dennis Martinez and the Uses of Theory," in idem, *Doing What Comes Naturally: Change, Rhetoric, and the Practice of Theory in Literary and Legal Studies* (Durham, N.C.: Duke University Press, 1989), pp. 372–398, esp. p. 374.

3. David Hume, *A Treatise of Human Nature*, ed. L. A. Selby-Bigge (1739; Oxford: Clarendon Press, 1888), pp. 268–269 (bk. 1, sect. 7); idem, *Enquiries Concerning Human Under-*

you feel a lot better. Refreshed by a healthy draft of street-realism, you can go back to your closet, and have another metaphysical or epistemological anxiety attack, if that is what turns you on.

Yet it is just the acknowledgment of my proper business as historian and sociologist that draws me to a sentiment in apparent direct opposition to that with which this essay started: the *more* said about truth, the better. The second sentiment does not necessarily contradict the first just because different communities of practitioners may have different proper ways of engaging with knowledge and its status. Modes of engagement are matters of intellectual decorum: what is proper to this mode of inquiry may be improper to another.[4] Of course, these different modes may have interesting relations with each other. It is plausible to think that anyone concerned to sort and evaluate bodies of knowledge would be interested in how these bodies come to be and how they are sorted, evaluated, and put to use by the relevant knowing communities. But these modes of engagement may nevertheless rightly be held distinct. As Michael Oakeshott insisted, it is *not* a sign of "decadent thought" to "bother about a *confusion des genres*."[5] And we should always be careful not to confuse a category mistake with a critical argument. We know that you cannot logically get from an "is" to an "ought," while the reference of "ought" statements contains descriptions on which evaluation is done, and which give the evaluations their point and pertinence.

The proper business of the historian and sociologist concerned with knowledge involves saying *a lot* about truth, and saying it in far richer detail and with a denser narrative texture than epistemologists' truth-talk. Yet judgments of relative detail and texture presuppose similarity of *reference*, and it is not at all clear that the proper reference of historians' and sociologists' professional talk overlaps with that of philosophers. For sociologists and historians a proper (innocuous and constructive) maxim of method is to "treat knowledge as what counts as knowledge" in various times and places, to "treat truth as what counts as the products of truth-judgments" in various times and places. Relativism—as it is widely embraced in my part of the academic world—is just an aspect of this maxim of

standing and Concerning the Principles of Morals, ed. L. A. Selby-Bigge, 3rd ed. (1777; Oxford: Clarendon Press, 1975), pp. 158–159 (*Human Understanding*, sect. 12, pt. 2).

4. The notion of "epistemological decorum" is spelled out in Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago: University of Chicago Press, 1994), chap. 5.

5. Michael Oakeshott, *Experience and Its Modes* (1933; Cambridge: Cambridge University Press, 1966), p. 5.

method. It implies nothing at all about epistemological evaluations or ontological judgments.[6]

I do not, and I cannot plausibly, assert that history and sociology *contain* no practical and routine epistemological or ontological judgments. Obviously and inevitably, such judgments are folded into every historical and sociological account, as when I count one historian's reconstruction of past events more accurate than another's, or presume that there was such a man as Thomas Hobbes. That is not problematic, and one could hardly imagine what any kind of academic inquiry or, indeed, everyday referential talk would be like if such judgments were excised.

The consequential distinction involves what I like to call "institutionalized intentions." I want to say that historians correcting or praising their subjects' intellectual judgments *would not be doing history*, in just the same sense that a player cheating at chess *would not be playing chess*.[7] Different communities warrantably hold their members to different institutionalized intentions. They are able, on the whole and for the most part, to recognize—from visible behavior—when a member is or is not acting according to the proper intention. Nothing is implied about the capacity of members fully, or purely, to realize that intention. So, for example, while it is arguably the institutionalized intention of historians to tell stories about the past "as it really was," few historians—I trust—are now so naive as to think that stories can be told about the past that do not reflect present concerns, or that do not employ present categories.[8] It would be

6. Crystal-clear, but routinely misrepresented, statements of methodological relativism are, among many examples, Barry Barnes and David Bloor, "Relativism, Rationalism and the Sociology of Knowledge," in *Rationality and Relativism*, ed. Martin Hollis and Steven Lukes (Oxford: Basil Blackwell, 1982), pp. 21–47; Barry Barnes, "How Not to Do the Sociology of Knowledge," in *Rethinking Objectivity*, ed. Allan Megill (Durham, N.C.: Duke University Press, 1994), pp. 21–35; and David Bloor, *Knowledge and Social Imagery*, 2nd ed. (1976; Chicago: University of Chicago Press, 1991), esp. chaps. 1–2.

7. Following Alasdair MacIntyre's account of a "practice," in *After Virtue: A Study in Moral Theory*, 2nd ed. (Notre Dame: University of Notre Dame Press, 1984), esp. chap. 14. One often enough sees such praise or blame in modern intellectual history, but usually as occasional "asides," thematically marked off from the main descriptive or explanatory lines of inquiry. The epistemological idiom is, however, still vigorous in much of what counts as the history of *philosophy*, where the impulse remains strong, for example, to *argue with* Hobbes or Hume rather than to interpret their works as historically situated productions.

8. That basic point was effectively made long ago in a blunt Anglo-Saxon idiom by Edward Hallett Carr (*What Is History?* [New York: Vintage Books, 1961], esp. chap. 1), and later in the high Germanic philosophical manner by Hans-Georg Gadamer (*Truth and Method*, trans. Joel Weinshimer and Donald G. Marshall, 2nd ed. [New York: Contin-

nice to think that there is some formula that can extricate historians from this predicament, but I know of none.

Since historians' and sociologists' business does not involve their setting themselves up as outside arbiters of the quality of knowledge produced by their subjects, there is no reason for them to engage with truth as some special characteristic externally *added onto* subjects' judgments of "what is the case about the world."[9] "What is the case" judgments seem interesting enough, and complicated enough, for historians and sociologists to be getting on with as topics of inquiry. So there is no necessity at all that the term "truth" be used solely to pick out something over and above subjects' stock of beliefs and statements about the world. Epistemologists are free to use "truth" in that sense if they want, and they may even feel that it is the only usage proper to their line of work.

The recognition of decorum may well mean that different intellectual communities let each other get on with their chosen tasks, acknowledging different goals and different standards appropriate to those trying to attain those goals. Such recognition need imply neither a severance of intellectual relations nor bland mutual indifference. On the one hand, we can seek, if we are forced or if we wish, to defend our particular modes of inquiry in terms external to the routine practices themselves. Who in the world cares about the results of our inquiries into knowledge? How effective have our inquiries been in acquitting standards accepted throughout the academic world, or in some relevant chunk of it? Do we deem ourselves, and are we deemed by pertinent others, to be saying new and interesting things, or going round in circles—to be providing new resources and topics for inquiry, or recycling old ones? Are we finding out stuff that others want to incorporate into their inquiries? The final justification of an activity, Wittgenstein taught us, does not belong to the activity, so asking and responding to questions like these is no violation of disciplinary decorum: it is just another kind of activity. And any arguments we have in the course of this activity can be as sharp as we like.[10]

uum, 1993]). See also C. Wright Mills, "Language, Logic, and Culture," *American Sociological Review* 4 (1939): 670–680; idem, "Situated Actions and Vocabularies of Motive," *American Sociological Review* 5 (1940): 904–913.

9. Of course, if historians or sociologists happened to be studying *epistemologists'* assessments, then a set of special-purpose judgments might well become an object of inquiry, a topic (as the ethnomethodologists say) rather than a resource.

10. See, on this point, Stanley Fish, "Anti-Professionalism," in *Doing What Comes Naturally* (above, n. 2), pp. 242–243.

On the other hand, the historical and sociological modes of engagement with truth (or "the case") can always bid to subsume the epistemological mode, though with no guarantee that epistemologists will either recognize or appreciate the subsumption. That is to say, if some community of epistemologists say that what they *mean* by truth is some special quality that they discern in knowledge, or some special distinction they mark among competing knowledge-claims, the historian or sociologist can always ask how they achieve and sustain those meanings or those distinctions. The reciprocal subsumption relation does not, I think, hold. The epistemologist may happen to have no interest in how what counts as knowledge comes to so count, while epistemologists' reading of naturalistic inquiry as coded evaluation is a paradigmatic—though dishearteningly common—violation of decorum.[11]

There are reflectively programmatic, as well as conventional, injunctions to telling rich and detailed stories about truth. In sociology, the ethnomethodologist Peter McHugh influentially directed analysts' attention to the "behavior of seeking truth . . . the institutional and public character of truth, in contrast to the usual psychological and semantic descriptions that depict private disembodiments of that behavior." For the sociologist the only proper way of engaging with truth is through the study of what people collectively do: "Truth resides in the rule-guided institutional procedures for conceding it." In doing sociology, one should accept that "there are no adequate grounds for establishing criteria of truth except the grounds that are employed to grant or concede it."[12]

11. Cf. Hans Reichenbach, "The Three Tasks of Epistemology," in idem, *Experience and Prediction: An Analysis of the Foundations and Structure of Knowledge* (Chicago: University of Chicago Press, 1938), pp. 3–16. Here Reichenbach fully acknowledged the pertinence of what he called the "sociological" task of epistemology—that is, giving an account of what counts as knowledge—while segregating it from the critical and advisory tasks that are supposed to give epistemology its special identity and that mark it off from history or sociology. Of course, those committed to naturalistic inquiry can also be guilty of misconstruing epistemology's subject-matter, though few epistemologists have bounded that subject-matter as clearly as Reichenbach: "Epistemology," he wrote, "does not regard the processes of thinking in their actual occurrence; this task is entirely left to psychology. . . . Epistemology thus considers a logical substitute rather than real processes" (ibid., p. 5).

12. Peter McHugh, "On the Failure of Positivism," in *Understanding Everyday Life: Toward the Reconstruction of Sociological Knowledge*, ed. Jack D. Douglas (Chicago: Aldine, 1970), pp. 320–335: quotations on pp. 320–321, 333–335. McHugh's formulation was prominently cited by H. M. Collins early in the development of the sociology of scientific knowledge: H. M. Collins, "The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of an Experiment in Physics," *Sociology* 9 (1975): 205; cf. H. M. Collins and Steven Yearley, "Epistemological Chicken," in *Science as Practice and*

Similarly, in a pragmatist, antiepistemological idiom Richard Rorty argues that there is "nothing to be said about either truth or rationality apart from descriptions of familiar procedures of justification which a given society—*ours*—uses in one or another area of enquiry."[13] Rorty, of course, does not practice what he preaches. A theoretician, even such a charmingly cracker-barrel antitheoretical theoretician as Rorty, could hardly practice his preaching without leaving off his usual way of going on, and taking up a line of intellectual work alien to him. But the moral of Rorty's sermon is nevertheless clear enough: if you really want to talk about truth in any sort of interesting naturalistic way, then you had best take a close look at the everyday practices through which different kinds of folk make knowledge-claims and assess some of them to be the case. That is to say, you should get to work and give some detailed historical or sociological descriptions of how different communities produce and justify knowledge on a day-to-day basis. And the processes that these descriptions describe are likely to look pretty complicated and messy. Rorty here sides with Algernon Moncrieff over Jack Worthing: "The truth is rarely pure and never simple."

Rorty gives reflective attention and explicit form to a maxim of method rarely articulated as such in relevant strands of historical and sociological practice: I call it the *mundaneness postulate*. Even the most technical, powerful, and highly valued bodies of knowledge—and the findings of natural science count, of course, as reliable and authoritative in our culture—should be regarded as open to inquiry by attending to mundane processes. The mundaneness postulate means in practice that the historian or sociologist should not prejudge what noticeable features of everyday scenes might or might not be pertinent to stories about the making and justification of knowledge—and, conversely, that one ought to tell such stories and see how they play out before conceding the inadequacy of such mundane and noticeable features. Rich and detailed stories about truth-making and truth-upholding in particular settings might want to come to grips with such mundane features of social scenes as conversing, persuading, cajoling, coercing, manipulating, testifying about experience and receiving the testimony of others, making

Culture, ed. Andrew Pickering (Chicago: University of Chicago Press, 1992), p. 303. Michael Lynch and David Bogen, *The Spectacle of History: Speech, Text, and Memory at the Iran-Contra Hearings* (Durham, N.C.: Duke University Press, 1996), offers detailed studies of how a "truth-finding engine" mundanely works.

13. Richard Rorty, "Science as Solidarity," in *Dismantling Truth: Reality in the Post-Modern World*, ed. Hilary Lawson and Lisa Appignanesi (London: Weidenfeld and Nicolson, 1989), p. 11.

marks on blackboards, inscribing in print and circulating printed objects, gesturing, grimacing, grunting (and witnessing others do so), retrieving routine social knowledge about the standing and identity of those making claims, asserting modified and new identities, and so on and on. The mundaneness postulate encourages us to exhaust the truth-making possibilities of the everyday and the material before we turn to the supernatural, the transcendent, and the disembodied.

My own most recent systematic attempt to put that preaching into practice—more accurately, to link my practice with a set of congenial off-the-shelf sermons—was *A Social History of Truth* (1994), and perhaps I should now switch to speaking for myself rather than on behalf of historians and sociologists, many of whom might prefer someone more judicious to speak for them. I called that book "a social history" partly because I wanted to establish a basic point about historical genres: there is no fundamental or necessary opposition between writing the history of intellectual élites and writing that of the masses. Just as we have well-established genres for writing social histories of the mundane practices of eating, dying, breeding, getting, and spending, so too we can have a social history of the practices of truth-making (or "the case"-making)—every bit as mundane as any other object of social-historical inquiry.

My intention in that work was to use a historical case-study of important passages of early modern science to open up widely distributed aspects of knowledge-making, knowledge-holding, and knowledge-evaluating as in-principle topics of mundane social historical inquiry. The question that I meant to press as far practicable was, in all cases: "How did they do it? Through what mundane processes did my subjects arrive at their accounts and assessments?" If the intellectual item in question was the *fact*, I wanted to ask: "How did they identify and establish facts?" If it was inference from the facts, then: "How did they achieve and recognize legitimate inference?" And, though my work dealt only glancingly with mathematical and logical matters, if it was proof or demonstration at issue: "How did they perform and acknowledge adequate proof or convincing demonstration?" Finally, if my subjects discoursed among themselves about special-purpose judgments of the "truth" of claims about the world —that is to say, if they philosophized (as they only sometimes did)— then: "To what ends, and how, did they render such special-purpose assessments?" Systematic and reflective judgments of this kind were not standard among the scientific practitioners I wrote about—they are probably rare among present-day scientists too— but, where they occur, the historian or sociologist may legitimately apply the mundaneness postulate in inquiring how such judgments are rendered.

A caveat: T. H. Huxley once famously described science as "nothing but *trained and organised common sense*," and a surprising number of eminent scientists through the ages—not just relativist sociologists—have enthusiastically concurred, ascribing the view that some special cognitive processes, or some special formal *method*, lie at the heart of science to the delusions of ignorant laity and enchanted philosophers.[14] If you want to understand how scientific truth is made, these scientists instruct us, do not make the mistake of looking for any special modes of cognition or of assessment. So, while scientists do not ordinarily conduct inquiries into the nature of science, expressions of the mundaneness postulate are common enough among them.[15]

The caveat that needs to be made—whether by metascientists or sociologists—is that asserting the *mundaneness* of cognitive and social processes across a range of activities is not the same as claiming the *identity* of these activities.[16] That should be taken as understood. Louis Latour's Château de Grancey Corton and Gallo's Hearty Burgundy are both fermented grape juice: for all sorts of purposes it may be important to be reminded of this fact, without its being understood as meaning that the two products are therefore the same. (You rarely get a duff bottle of the Gallo.) So drawing attention to the mundane means by which scientific knowledge is made may become part of improved and more focused inquiries into the real nature of differences between strands of intellectual practice—if not between a unitary "science" and a unitary "common sense," then, perhaps, between invertebrate zoology and terpene chemistry, between clinical trials of drugs and accountancy, or between cooking and civil engineering. Following Huxley's suggestion, one might, for example, begin to think seriously about differences in organizational forms across a range of practices, about the extent to which different

14. Thomas Henry Huxley, "On the Educational Value of the Natural History Sciences" (1854), in idem, *Collected Essays*, vol. 3, *Science and Education: Essays* (New York: Appleton, 1900), p. 45. See also, among many other similar expressions, P. W. Bridgman, *Reflections of a Physicist*, 2nd ed. (New York: Philosophical Library, 1955), esp. chaps. 5 and 8; Albert Einstein, "Physics and Reality" (1936), in idem, *Ideas and Opinions* (New York: Crown Publishers, 1954), p. 319; Peter B. Medawar, *The Art of the Soluble* (London: Methuen, 1967), esp. p. 132; Mahlon Hoagland, *Toward the Habit of Truth: A Life in Science* (New York: Norton, 1990), esp. pp. xv–xix.

15. I have surveyed such scientists' metascientific sentiments in Steven Shapin, "How to be Anti-Scientific," in *Science Peace*, ed. H. M. Collins and Jay Labinger (forthcoming). An excerpted German version of this has appeared as "Von der Schwierigkeit, ein Wissenschaftsgegner zu sein," *Frankfurter Rundschau* October 27, 1998, p. 9. A fuller French version is forthcoming (March 1999) in *La recherche*.

16. See Steven Shapin, "Here and Everywhere: Sociology of Scientific Knowledge," *Annual Review of Sociology* 21 (1995): 305.

intellectual communities own (or construct) their objects of inquiry, about the relationships between their objects of inquiry and objects-of-interest in the everyday world, about the relations between such considerations and particular paths of historical development, and so on.[17] There is no reason to think that mundane cognitive processes and mundane forms of social interaction cannot be variously stressed, combined, and configured so as to produce cultural results as diverse and as remarkable as those we have actually to account for.[18]

In *A Social History of Truth* I was particularly interested in pointing out constitutive links between aspects of scientific knowledge-making and those mundane features of social and cultural life widely considered to have least to do with scientific practice. Truth, "the case," and facticity are established importantly (I never said wholly) in the small, the intimate, the personal, the embodied, the emotionally textured, and often in the domains of the familiar and the face-to-face. Programmatic commendations of such sensibilities need to be fleshed out by minute and detailed inquiries about how specific items of knowledge are established and evaluated in local interactional settings.[19] To put it another way: for their sensibilities to be realized and made concrete, Ludwig Wittgenstein, Michael Polanyi, and Richard Rorty require the resources of Norbert Elias and Erving Goffman. Scientific knowledge in the early modern era had a human face, and, despite some modernist and postmodernist presumptions to the contrary, I think it still substantially retains a human face. Assessments of knowledge still importantly implicate judgments of the virtues and capacities of familiar people who rep-

17. See, e.g., Richard D. Whitley, *The Intellectual and Social Organization of the Sciences* (Oxford: Clarendon Press, 1984); Zygmunt Bauman, *Intimations of Postmodernity* (London: Routledge, 1992), esp. pp. 71–73; Steven Shapin, "Cordelia's Love: Credibility and the Social Studies of Science," *Perspectives on Science* 3 (1995): 266–268.

18. When, in *A Social History of Truth* (above, n. 4), I say metascientific things like "trust is constitutive of scientific knowledge," I am doing cognitive science without a professional license. I am drawing attention to cognitive and social processes that I believe to be common to a wide range of scientific and everyday practices. What I am not doing is picking out a unique "essence" of some unitary culture called "science." The question of describing and accounting for such differences between practices as can be shown to exist remains pertinent.

19. Those who stress the significance of the local interactional environment should not be embarrassed by evidence that some knowledge that is locally produced may travel in a relatively robust manner. One need not abandon either the mundaneness postulate or the stress on modes of face-to-face interaction in order to address the problem of how science and technology travel around the world. See, for example, David N. Livingstone, "The Spaces of Knowledge: Contributions towards a Historical Geography of Science," *Society and Space* 13 (1995): 5–34; Steven Shapin, "Placing the View from

resent and convey such knowledge: matter is still inferred from manner, facts from physiognomy, credibility from character.[20]

We are now depressingly familiar with charges that (1) sensibilities such as those represented by the mundaneness postulate are motivated by hostility to science or ignorance of it, or that (2) however motivated, they will nevertheless corrupt the youth of Athens. The first accusation—when directed to the work of many distinguished historians and sociologists of science—now deserves to be called by its proper name: it is malevolently misinformed. Truth is not defended by spreading misrepresentations, and scholarly standards are not effectively upheld by displays of shoddiness.

The second charge is more interesting. It presumes that unless the populace believe in some transcendental story about truth and objectivity—*which* story among the many alternatives is often left unspecified—disastrous cultural, moral, even political consequences will ensue. The people will rise up in left-wing rebellion, rise up in right-wing rebellion, or quietistically fail to rise up when certain intellectuals reckon they ought to rise up. Stripped of transcendental stories about truth and objectivity, the people will not treat any claims as adequately justified. They will consider all claims to knowledge to be equally justified or unjustified. Unconvinced of the truth of the laws of physics, they will jump off tall buildings or walk in front of speeding trains.[21]

Such sentiments are as ludicrous as they are prevalent among the academy's newly self-appointed Tribunes of Truth. Their absurdity consists partly in the notion that academic monographs and articles —unfortunately sometimes incomprehensible to more than a handful of professional adepts—have the potential either to mislead po-

Nowhere: Historical and Sociological Problems in the Location of Science," *Transactions of the Institute of British Geographers*, n.s., 23 (1998): 5–12; Shapin, "Here and Everywhere" (above, n. 16), pp. 304–309.

20. See speculations on the continuing significance of familiarity in scientific judgments in Shapin, *Social History of Truth* (above, n. 4), pp. 413–417; and, for the "physiognomy of truth" in contemporary politics and law, see Lynch and Bogen, *Spectacle of History* (above, n. 12), pp. 43–52. For historical and theoretical studies of the embodied presentation of natural knowledge, see Christopher Lawrence and Steven Shapin, eds., *Science Incarnate: Historical Embodiments of Natural Knowledge* (Chicago: University of Chicago Press, 1998).

21. Cf. Malcolm Ashmore, Derek Edwards, and Jonathan Potter, "The Bottom Line: The Rhetoric of Reality Demonstrations," *Configurations* 2 (1994): 1–14; Derek Edwards, Malcolm Ashmore, and Jonathan Potter, "Death and Furniture: The Rhetoric, Politics and Theology of Bottom Line Arguments against Relativism," *History of the Human Sciences* 8 (1995): 25–49.

litical leaders or to pervert Athens' youth.[22] Were science studies academics hostile to science and intent on its destruction, they would not find the job quite so easy.

But what is genuinely interesting about the charge is the notion that the people's trust in science is based upon their acceptance of certain transcendental and absolutist stories about science. It is interesting because there is practically no solid evidence brought forward to suggest that this is the case. What we have here is for the most part a massively uninformed imputation about how expert claims about the world are credited or rejected in lay society, and about the grounds of lay deference to expert authority. It is an imputation that unjustifiably conflates science and its evidently deserved authority with some fanciful academic (usually, but not exclusively, philosophical) *stories about science* and their undeserved claims to authority.[23] It suggests that an attack on philosophical rationalism, realism, or some other metascientific story is an attack on science. It would be as if anyone who disagreed with an ignorant wine writer's pompous prose was to be vilified for hating wine.

22. The idea that governmental cuts in the funding of science were prompted by academic relativism was given an early airing in T. Theocharis and M. Psimipoulis, "Where Science Has Gone Wrong," *Nature* 329 (1987): 595–598. The picture of Margaret Thatcher or Ronald Reagan tucked up in bed with *Against Method* is priceless.

23. There *are* some fine studies describing modern enterprises aimed at distributing certain metascientific stories to governmental patrons and to sectors of the democratic laity, but research devoted to documenting the circulation and the effects of such stories is either nonexistent, superficial, or insufficiently aware of some knotty problems involved in inferring cultural effect from intention. It is an endemic disposition of preachers to presume the potency of their sermons, while more-detached observers may note how many in the congregation are just sleeping through them. For the attempted projection of metascientific stories, see, for example, David A. Hollinger, *Science, Jews, and Secular Culture: Studies in Mid-Twentieth-Century American Intellectual History* (Princeton: Princeton University Press, 1996), esp. chaps. 5, 6, and 8; Ronald C. Tobey, *The American Ideology of National Science, 1919–1930* (Pittsburgh: University of Pittsburgh Press, 1971). For some attempted assessments of their effects, see Margaret Mead and Rhoda Métraux, "The Image of the Scientist among High-School Students," *Science* 126 (August 20, 1957): 384–390, and David C. Beardslee and Donald D. O'Dowd, "The College-Student Image of the Scientist," *Science* 133 (March 31, 1961): 997–1001. For work that reflectively and systematically addresses the complex conditions of lay deference to scientific expertise, see, for example, Brian Wynne, "Sheep-farming after Chernobyl: A Case Study in Communicating Scientific Information," *Environment* 31 (1989): 10–15, 33–39 (reprinted in *Dirty Words: Writings on the History and Culture of Pollution*, ed. Hannah Bradby [London: Earthscan, 1990], pp. 139–160); idem, "Public Understanding of Science Research: New Horizons or Hall of Mirrors?" *Public Understanding of Science* 1 (1992): 37–43; idem, "Misunderstood Misunderstanding: Social Identities and Public Uptake of Science," ibid., pp. 281–304.

We just do not know very much about the laity's appreciations of science—of its methods, its concepts, the bases of its power—and it would be nice to know more, to replace facile imputation with systematic inquiry. There is, however, something inherently implausible in the notion that people's recognition of deserved intellectual authority has to proceed from their acceptance of any metascientific story, still less of absolutist and transcendental stories about truth and objectivity. For one thing, it is hard to identify any such story-to-be-believed stably circulating in late modern culture, and (as I have noted) even scientists cannot agree upon which of the many candidate-stories deserves their collective endorsement. For another, the fact that methodological relativists, such as myself, do not consult astrologers or deny that DNA is the hereditary substance, is testimony to the formal distinction between the bases of scientific authority and epistemological absolutism. As an everyday actor, my very great confidence in astronomy and in genetics lies in those concrete practices themselves, in my direct and indirect appreciation of their *efficacy*, and in my sense of the reliability of my personal and institutional sources of knowledge about them—not in epistemology, or in any global metanarrative about science or truth, about which I have very little confidence indeed.

The grounds of my personal confidence in what our society widely considers to be scientific truth seem plausibly generalizable. Deference to reliable expert knowledge need not have anything to do with formal truth-stories, or objectivity-stories, or absolutist epistemic stories of any kind. Some of our culture's most prestigious and consequential institutions seem to secure their enormous authority through claims to truth and certainty that are far more modest, provisional, and local.[24] Judges, doctors, and generals decide matters of life and death without—and without being expected to display—the intellectual warrants that would satisfy an epistemologist. The valued institutions of our culture do not fall down for want of such warrants; they would be more likely to collapse if we expected such warrants from them. For good or ill, they and we live in a world of knowledge that is good enough, adequate to the case in hand, as reliable as it can practically be, morally certain. If household gods are the only gods going, they are nevertheless entitled to due deference.

24. This I take to be a central point of Barbara Herrnstein Smith's excellent paper "The Unquiet Judge: Activism without Objectivism in Law and Politics," *Annals of Scholarship* 9 (1992): 111–133; reprinted in idem *Belief and Resistance: Dynamics of Contemporary Intellectual Controversy* (Cambridge, Mass.: Harvard University Press, 1997), pp. 1–22.

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