

The One Culture?

A CONVERSATION ABOUT SCIENCE

Edited by Jay A. Labinger and Harry Collins

The University of Chicago Press
Chicago and London

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The University of Chicago Press, Chicago 60637
The University of Chicago Press, Ltd., London
© 2001 by The University of Chicago
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Printed in the United States of America
10 09 08 07 06 05 04 03 02 01 1 2 3 4 5

ISBN: 0-226-46722-8 (cloth)
ISBN: 0-226-46723-6 (paper)

Library of Congress Cataloging-in-Publication Data

The one culture? : a conversation about science / edited by Jay A. Labinger and Harry Collins.

p. cm.

Includes bibliographical references and index.

ISBN 0-226-46722-8 (cloth) — ISBN 0-226-46723-6 (paper)

1. Science—Social aspects. 2. Science and state. 3. Science—Philosophy. I. Labinger, Jay A. II. Collins, H. M. (Harry M.), 1943– III. Title.

Q175.55 .O54 2001

303.48'3—dc21

00-013097

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Contents

<i>Preface</i>	ix
1. Introduction	1
<hr/>	
Part One Positions	
PHILOSOPHIES	
2. Does Science Studies Undermine Science? Wittgenstein, Turing, and Polanyi as Precursors for Science Studies and the Science Wars <i>Trevor Pinch</i>	13
3. Science and Sociology of Science: Beyond War and Peace <i>Jean Bricmont and Alan Sokal</i>	27
4. Is a Science Peace Process Necessary? <i>Michael Lynch</i>	48
PERSPECTIVES	
5. Caught in the Crossfire? The Public's Role in the Science Wars <i>Jane Gregory and Steve Miller</i>	61
6. Life inside a Case Study <i>Peter R. Saulson</i>	73
ORIGINS	
7. Conversing Seriously with Sociologists <i>N. David Mermin</i>	83
8. How to be Antiscientific <i>Steven Shapin</i>	99
DIRECTIONS	
9. Physics and History <i>Steven Weinberg</i>	116
10. Science Studies as Epistemography <i>Peter Dear</i>	128
11. From Social Construction to Questions for Research: The Promise of the Sociology of Science <i>Kenneth G. Wilson and Constance K. Barsky</i>	142

individual, while for me it meant the collective knowledge of a large group. Until this discovery each of us found much of what the other had to say about the relation of knowledge to "what I have been told," quite preposterous, and our responses to each other only heightened the sense of absurdity. "Negotiations" is another tricky term. For sociologists it is a morally neutral characterization of the process by which different people come to a mutually acceptable understanding. But for most scientists it has overtones of duplicity and personal self-interest and suggests cynically splitting the difference in a disagreement rather than searching for a deeper understanding.

Rule 3: Do not assume that it is as easy as it may appear for you to penetrate the disciplinary language of others.

This is rule 2 again, viewed from the other direction. For while it may be obvious to you that others are missing your point, it requires a much greater expenditure of creative imagination to contemplate the possibility that you might be missing theirs. In particular, it is necessary to seek for interpretations of what they are saying that are less absurd than the one that first crosses your mind. Collins and Pinch, and Barnes and Bloor, should have thought again before concluding that I was doing anything as silly as reading them to be putting forth unorthodox physics of their own devising. I should have asked myself whether Collins and Pinch could really have been engaged in a quixotic effort to undermine public confidence in relativity. You cannot argue successfully with people without first persuading them that you have more or less understood what they are trying to say.

In short, assume that people in another discipline really mean what they say, take the trouble to ascertain just what that might be, and, when you are confident of this, take care to formulate your criticism, if you still feel criticism is called for, in terms you can be confident will be understood. These rules for fruitful conversation are so obvious that I feel foolish setting them down. But their neglect, on both sides, is responsible for much of the heat of the science wars.

Chapter 8

HOW TO BE ANTISCIENTIFIC

Steven Shapin

I am not a commissioned officer in the so-called science wars. If anything, I am something between a common soldier and an interested witness to the current hostilities. I was trained in genetics, but for many years I have been a historian and sociologist of science, writing mostly about the development of science in the seventeenth century.¹ I have suffered some minor shrapnel wounds from wildly aimed shells, but, in the main, the Defenders of Science have had bigger game to stalk and have left me to get on with my work and to reflect from a somewhat disengaged perspective on what is going on.

The immediate occasion for the science wars seems to be a series of claims *about* science made by some sociologists, cultural historians, and fuzzy-minded philosophers. (In my ordinary academic work, distinctions between these categories—and subdivisions within them—count as crucial, but in this piece for general readers I mainly lump them together.) As a matter of convenience, I refer to propositions *about* science as "metascience," and, because it is very important to be clear about what is at issue, I list here just a few of the more contentious and provocative metascientific claims:

1. There is no such thing as the Scientific Method.
2. Modern science lives only in the day and for the day; it resembles

A French translation of a slightly different version of this essay has already appeared as "Être ou ne pas être antiscientifique," *La recherche* 319 (April 1999), 72–79, and an excerpt has been published in German as "Von der Schwierigkeit, ein Wissenschaftsgegner zu sein," *Frankfurter Rundschau*, 27 October 1998, sec. Humanwissenschaften, 9.

1. Some of my work in this area includes Shapin and Schaffer 1985 and Shapin 1994, 1995a, 1995b, 1999, and forthcoming.

much more a stock-market speculation than a search for the truth about nature.

3. New knowledge is not science until it is made social.
4. An independent reality in the ordinary physical sense can neither be ascribed to the phenomena nor to the agencies of observation.
5. The conceptual basis of physics is a free invention of the human mind.
6. Scientists do not find order in nature, they put it there.
7. Science does not deserve the reputation it has so widely gained . . . of being wholly objective.
8. The picture of the scientist as a man with an open mind, someone who weighs the evidence for and against, is a lot of baloney.
9. Modern physics is based on some intrinsic acts of faith.
10. The scientific community is tolerant of unsubstantiated just-so stories.
11. At any historical moment, what pass as acceptable scientific explanations have both social determinants and social functions.

For many readers, even listing such statements is unnecessary: they will already be thoroughly familiar with sentiments like these associated with the writings of sociologists of science and academic fellow-travelers, as they will be equally familiar with the outraged reactions to them expressed by a number of natural scientists, convinced that such claims are motivated mainly or solely by hostility to science, or that they proceed from ignorance of science, or both. Science and rationality are said to be besieged by barbarians at the gate, and, unless such assertions are exposed for the rubbish they are, the institution of science, and its justified standing in modern culture, will be at risk. It is therefore incumbent on leading scientists themselves to speak out, to say what the real nature of science is, and to take a stand against the ignorance and the malevolence expressed in these claims.²

Nevertheless, I have to tell you—in the spirit of our troubled culture—that you have just become a victim of yet another hoax. None of these claims about the nature of science that I have just quoted, or minimally paraphrased, does in fact come from a sociologist, or a cultural studies academic, or a feminist or Marxist theoretician. Each is taken from the metascientific pronouncements of distinguished twentieth-century scientists, some Nobel Prize winners. (See the end of

2. Some well-known recent tracts by scientists expressing such sentiments are Wolpert 1992, Gross and Levitt 1994, Gross, Levitt, and Lewis 1996, Sokal and Bricmont 1998, and Weinberg 1995 and 1998.

this chapter for a list of the sources.) Their authors include immunologist Peter Medawar, biochemists Erwin Chargaff and Gunther Stent, entomologist E. O. Wilson, mathematician turned scientific administrator Warren Weaver, physicists Niels Bohr, Brian Petley, and Albert Einstein, and evolutionary geneticist Richard C. Lewontin. This is not a mere a party trick—a device to turn the tables or to play intellectual Ping-Pong—though it would seem so if I left it at that. The point I want to make here is substantial, interesting, and potentially constructive: practically all of the claims about the nature of science that have occasioned such violent reaction on the part of some recent Defenders of Science have been intermittently but repeatedly expressed by scientists themselves: by many scientists of many disciplines, over many years, and in many contexts [14].³

Accordingly, we can be clear about one thing: it cannot be the claims themselves that are at issue, or the claims themselves that must proceed from ignorance or hostility. Rather, it is *who has made* such claims, and what motives can be attributed—plausibly, if often inaccurately and unfairly—to the *kinds of people* making the claims. So one of the very few, and very minor, modifications I have made in several of the quotations above is the substitution of the third-person “they” or “scientists” or “physicists” for the original “we.” We are now, it seems, on the familiar terrain of everyday life: members of a family are permitted to say things about family affairs that outsiders are not allowed to say. It is not just a matter of truth or accuracy; it is a matter of decorum. Certain kinds of description will be heard as unwarranted criticism if they come from those thought to lack the moral or intellectual rights to make them.

Since what scientific family members often do when they make metascientific statements is to *prescribe* how members *ought* to behave—criticizing or praising—there is a tendency to assume that outsiders must be about the same business, though without equivalent entitlements. It is sometimes hard for scientists to understand how the description and interpretation of science could be anything other than coded prescription or evaluation: telling scientists what to do, or sorting out good from bad science, or saying that science as a whole is good or bad. It is hard to recognize, that is, what a naturalistic intention would be like in talking about science, since this is not a luxury readily available to members of the scientific family. Scientists have naturalistic inten-

3. After I had written this essay, I came across a broadly similar observation powerfully made by Israeli historian of physics Mara Beller (1998; see also Beller 1997), though she focuses attention exclusively on the views of twentieth-century quantum physicists.

tions with respect to their objects of study but rarely with respect to the practices for studying those objects. So, for example, some sociologists do indeed insist that scientific representations are "social constructions." And when some scientists read this they assume—wrongly, in most cases and in my view—that these sociologists have tacitly prefaced the phrase with the evaluative word "only," or "merely," or "just": science is *only* a social construction. To say that science is socially constructed is then taken as a way of detracting from the value of scientific propositions, denying that they are reliable about the natural world.⁴ Scientists do that all the time: that is, they "deconstruct" particular scientific claims in their fields by identifying them as *mere* wish-fulfillment, *mere* fashion, *mere* social construction. But they do so to *do* science, to sort out truth from falsity about the bits of the natural world with which they are concerned. They rarely do so with what might be called a disciplinary intention of just describing and interpreting the nature of science. That is one major reason why we seem to be misunderstanding each other so badly. There are important differences in recognized disciplinary intentions, in seeing their different possibilities and purposes and values. We do not always adequately recognize these differences, and we ought to.

That is one lesson to take away from this little hoax. But it is neither the most interesting nor the most fundamental. The more fundamental observation is just that metascientific statements by scientists vary enormously. I have picked out some that resonate with descriptions offered by sociologists, but, of course, there are many that do not. When scientists say metascientific things, they commonly conflict with each other as well as conflicting, occasionally, with what sociologists say.

Indeed, some scientists' pronouncements on the nature of science insist that science is a realist enterprise; others stipulate that it is not. Science, these others say, is a phenomenological, instrumental, pragmatic, or conventional practice. Max Planck, for example, identified the endemic tendency "to postulate the existence of a *real world*," in the metaphysical sense, as "constitut[ing] the irrational element which exact

4. Sociologists of science, notably those Edinburgh school writers criticized by Steven Weinberg and others, have repeatedly stressed that the social component of scientific knowledge is *not* to be set against the causal role of un verbalized natural reality: the social component is seen as a condition for having experience of a recognized kind and for representing that experience in linguistic form. See, for example, Bloor 1991: "No consistent sociology could ever present knowledge as a fantasy unconnected with our experience of the material world around us" (33) and Barnes 1977: "[T]here is indeed one world, one reality, 'out there,' the source of all our perceptions" (25–26); see also Barnes 1992. I have no very satisfactory ideas why the Defenders of Science should miss the facts right in front of their eyes.

science can never shake off, and the proud name, 'Exact Science,' must not be permitted to cause anybody to under-estimate the significance of this element of irrationality" (1949, 106). J. Robert Oppenheimer supposed that laypeople were irritated by scientists' *unwillingness* to use words like "real" or "ultimate": the use of such notions would be a form of metaphysics, and science, Oppenheimer insisted, was a "non-metaphysical activity" (1954, 4). These positions are hard to square with such nervously defiant declarations as Steven Weinberg's: "[F]or me as a physicist the laws of nature are real in the same sense (whatever that is) as the rocks on the ground" (1998, 52).⁵ As it happens, physicists disagree on such things.

Moreover, some scientists—when they say that science is a realist enterprise—mean to pick out a special philosophical position by which theoretical entities are understood to refer to real existents in the world; others seem to be alluding to the sort of robust everyday realism that unites a range of sciences with the practices of everyday life, as when I might say in ordinary conversation, "Look at the cat sitting on the mat," directing someone's attention *over there* and not toward my speech organs or my brain. The realism advocated (or rejected) in scientists' metascientific pronouncements is only very occasionally specified in such ways. Some scientists say that science aims at, or arrives at, one universal Truth, others say that the truths of sciences are plural, or that science is just "what works" and that Truth, or even correspondence with the world, is none of their concern—just "what is the case" or "what seems to be the case to the best of our current efforts and beliefs." Some say that science is Coming to an End—about to be completed—but we should understand that this imminent completion has been promised practically as long as there has been science. Other scientists pour scorn on any such idea: science, they say, is an open-ended problem-solving enterprise, where the problems are generated by our own current solutions and will continue to be, time without end.⁶

Some scientists' metascientific pronouncements say that there is no such thing as a special, formalized, and universally applicable Scientific Method; others insist with equal vigor that there is. The latter, how-

5. Only after this piece was drafted did I become aware of Richard Rorty's similar, but more vigorously expressed, puzzlement about Weinberg's claim (Rorty 1997).

6. The controversy among scientists about whether or not science is about to be completed now even claims space in the *New York Times*: see the debate between John Horgan and John Maddox (1998). For pertinent claims, see Weinberg 1992; Horgan 1996; Horgan and Maddox 1998; and Stent 1969. For historical commentary on recurrent announcements of the End of Science, see Schaffer 1991. For my own engagements with what scientists have meant by truth, see Shapin 1999 and forthcoming.

ever, vary greatly when it comes to saying what that method is. Some scientists like Bacon, some like Descartes; some go for inductivism, some go for deductivism; some for hypothetico-deductivism, some for hypothetico-inductivism. Some say—with T. H. Huxley, Max Planck, Albert Einstein, and many others—that scientific thinking is a form of common sense and ordinary inference. "The whole of science," according to Einstein, "is nothing more than a refinement of everyday thinking" (1954, 319).⁷ Others, like the biologist Lewis Wolpert (1992), vehemently repudiate the commonsense nature of science and suggest that any such idea stems from ignorance or hostility. Few—either for or against the commonsense nature of science—display much curiosity about what common sense is or entertain the possibility that it too might be heterogeneous and protean.

You name it, it's been identified as the Scientific Method, or at least as the method of some practice anointed as the Queen of the Sciences, the most authentically scientific of sciences—usually, but not invariably, some particular version of modern physics. Collect textbook statements about the Scientific Method and see for yourself. Or ask your scientist-friends, one by one, to write down on a piece of paper (no collaborating! no peeking at a philosophy of science textbook!) what they take to be either the Scientific Method or even the formal method thought to be at work in their own practices or discipline. Some of your friends will have heard of Karl Popper, or of Thomas Kuhn, or of Paul Feyerabend and will have their preferences among these—though probably not many of them. (Why should they?) In which case, ask them to write down on another piece of paper what they take to be the position about Scientific Method recommended by their favorite philosopher. (You may find little correspondence with sociologists' or philosophers' professional sense of what Popperianism or Kuhnianism is, and, in any case, sociologists and philosophers also vary in their estimation of what Popper and Kuhn were really saying.)⁸

You might also consider the cultural sources of our current repertoires for talking about Scientific Method. Few chemists, biologists, or physicists will have taken courses on Scientific Method (at least in Anglophone settings), but many psychologists or sociologists will have experienced almost total immersion in such material—ironically taken

7. For Huxley, see Huxley 1900: "Science is, I believe, nothing but *trained and organised common sense*" (45); for Planck, see Planck 1949, 88.

8. For an interesting exploration of what scientists' professions of Popperianism might mean, see Mulkay and Gilbert 1981; for psychological assessments of scientists' grasp of formal logic, see Mahoney 1979 and Mahoney and DeMonbreun 1977.

to be modeled on formal natural scientific method. Perhaps no small part of the enormous success of the natural sciences might be ascribed to the relative *weakness* of formal methodological discipline [18]. It is at least a thought worth thinking. This was, for instance, the opinion of the physicist Percy Bridgman: "It seems to me that there is a great deal of ballyhoo about scientific method. I venture to think that the people who talk most about it are the people who do least about it. Scientific method is what working scientists do, not what other people or even they themselves may say about it. No working scientist, when he plans an experiment in the laboratory, asks himself whether he is being properly scientific, nor is he interested in whatever method he may be using *as method*. . . . The working scientist is always too much concerned with getting down to brass tacks to be willing to spend his time on generalities. . . . Scientific method is something talked about by people standing on the outside and wondering how the scientist manages to do it" (1955, 81).

When we consider the *conceptual* identity of science, the situation is much the same. Is science conceptually unified? To those scientists who consider that it is, a preferred idiom is a unifying materialist reductionism, though scientists of a mathematical or structural turn of mind reject both materialism and reductionism, while biologists continue intermittently to ponder whether there is a not a unique biological mode of thinking and unique biological levels of analysis. Just as E. O. Wilson is announcing a new—or rather a revived—plan for the reductionist unification of the sciences, natural and human, other scientists rebel against reductionism, against the claim that "the whole is the sum of its parts," or against its local manifestations in molecular biology, or they say that what had once been a search for understanding has now turned into a reductionist and shallow quest for explanations. Materialistic reductionism is just a sign that a Scientific Age of Iron has followed an intellectual Golden Age.⁹

The conceptual unification of all the sciences on a hard and rigorous base of materialist reductionism is an old aspiration, but it has never

9. For the most aggressive recent affirmation of reductionist unity, see Wilson 1998b, though Wilson now seems to have forgotten the complaints against rampant molecular reductionism he so eloquently expressed in his autobiography *Naturalist* (1995, chap. 12). Violently antireductionist statements by biologists are not, of course, hard to find: see, among many examples, Shulman 1998, Mayr 1997, Chargaff 1963 and 1978, and Lewontin 1993. For what it is worth, Wilson's vision of reductionist unity is devastatingly taken apart by the philosopher Jerry Fodor: "[Wilson] suspects that if we resist consilience, that's because we're suffering from pluralism, nihilism, solipsism, relativism, idealism, deconstructionism and other symptoms of the French disease" (1998, 3, 6).

commanded (and does not now command) the assent of all scientists. In a whole range of natural sciences—though biology is probably the most pertinent case—reductionist unification is rejected, sometimes very violently, and in other parts of science reductionist unification just doesn't figure. It may be somebody's dream, but it's hardly anybody's work.

Recall that I started by picking out claims about the nature of science that I invited you to associate with ignorant or hostile nonscientists. Then I told you that these statements were in fact made by scientists. Taking the argument a step further, I then acknowledged that metascientific statements by scientists were very various—on all subjects, and on all levels—and that many of these conflicted with sentiments in the quoted set, and with each other.

From this circumstance one could draw a number of conclusions. The first would be that a certain set of these statements—say the first set—is hopelessly in error and that their opposites are correct. I don't want to say that. If I did, it would be as much as saying that Medawar, Planck, and Einstein didn't know what they were talking about, nor do the sociologists whose claims resemble theirs so closely. In all honesty, however, I have to admit that when I plow through the range of individual scientists' metascientific statements I often find more internal variability than makes me professionally comfortable. I might even be accused of the sin of quoting isolated remarks out of context, and maybe I have. No one should tendentiously quote out of context, though perhaps quoting Peter Medawar out of context on the Scientific Method is a less serious offense than (I take a randomly chosen example) quoting Steven Shapin out of context on the role of trust in seventeenth-century English science: Medawar's proper business is less damaged by such misleadingly selective quotation than is mine. It is bad to quote out of context, or to quote misleadingly. It is bad for sociologists to do when writing about science or metascience, and it is bad for scientists to do when writing about the sociology of science. No, I want to say that the quoted set contains quite a lot of truth—with some qualifications that I am shortly going to make.

The second conclusion would be that all metascientific statements by practicing scientists are best ignored. For this view—at the risk of introducing a Cretan paradox—I can cite prominent scientists' pronouncements too. It was, after all, Einstein who famously said that we should take little heed of scientists' formal reflections on what they do; we should instead "fix [our] attention on their deeds": "It has often been said, and certainly not without justification, that the man of science

is a poor philosopher" (1954, 296, 318).¹⁰ So, if we follow Einstein and charitably allow the self-contradiction to pass, what one would be tempted to say is something like this: "Plants photosynthesize; plant biochemists are experts in knowing how plants photosynthesize; reflective and informed students of science are experts in knowing how plant biochemists know how plants photosynthesize."¹¹ As Aesop put it, the centipede does marvelously well in coordinating the movements of its hundred legs, less well in giving an account of how it does so. No skin off the centipede's back, and no skin off the scientist's back if it happens that she's not very good at the systematic reflective understanding of her work. That's not her job. And the point, of course, of Aesop's fable is that the centipede pushed to reflective understanding winds up in an uncoordinated heap. Kuhn just follows Aesop in this regard.

That's not really the conclusion I want to press either, though it does have something to recommend it. I see no necessary reason why certain scientists—perhaps not very many, given the pressures on their time and their other interests—shouldn't be just as good at metascience as professional metascientists, nor any necessary reason why professionals in metascience should ignore the pronouncements of amateurs. Nor do professional metascientists—sociologists, historians, and philosophers—globally *have to* concede that practicing scientists "know the science better or best" or "know more science" than they themselves do, though it is very prudent to respect scientists' particular expertise and to make sure, when one is writing about the object of that expertise, to "get it right." They should take great care not to say something about photosynthesis or about the techniques for knowing about photosynthesis that is demonstrably wrong, as judged by the consensus of expert practitioners in that area.

The reason that sociologists, historians, and philosophers do not globally have to concede that "scientists know better about science" is that knowledge about contemporary plant biochemistry, for instance, is not the same thing as "knowledge about science." There are many sciences at time present, and there have been many more sciences, and many versions of plant science, in past times, and who is to say that the historian or sociologist who knows something substantial about these many sciences knows "less science" than the contemporary plant bio-

10. Quoted more fully, Einstein said: "If you want to find out anything from the theoretical physicists about the methods they use, I advise you to stick closely to one principle: don't listen to their words, fix your attention on their deeds" (1954, 296).

11. I believe I owe this formulation to a conversation with Harry Collins many years ago.

chemist who, pronouncing on the nature of science, knows less or even nothing at all?

I see no reason to turn the tables and celebrate as a fact that I know "more science" than my friend who is a plant biochemist. As it happens, I know almost nothing about photosynthesis beyond what I was taught in college courses in plant physiology and cell biology, and I would be morally wrong and intellectually careless if I pronounced on how matters stand in that part of present-day science. On the other hand, I have the right to feel slightly miffed if I am lectured about how matters stood in seventeenth-century pneumatic chemistry by practicing scientists who are even more incompetent in that part of science than I am in contemporary plant biochemistry.

Almost needless to say, it's vital that you get your facts right in the subject you're writing about. That obligation is absolute and it's general: it applies to sociologists and historians writing about the aspects of science in which they are interested, and it applies to scientists writing about the sociology and history of science. At the same time, one would hope that normal human and professional frailties would be recognized and that we would pause a nanosecond before ascribing to each other the basest possible motives and the most egregious degrees of incompetence. There is indeed some shoddy work in sociology and cultural studies, and some natural scientists persuasively say in public there is shoddy work in their parts of science. *There is no excuse for shoddiness wherever it is found.* But we should at the same time cut each other a little bit of slack. To err is human, but it is as likely that we err in appreciating each others' intentions as it is that major blunders have been committed or that disciplinary hostility is at work. Before pointing fingers in the press or on public platforms, we might try conversations in a café or a pub. The likely result would be lower blood pressure and a less poisonous public culture.

Finally, as I suggested a while ago, scientists' metascientific statements often function in the specific context of *doing* science, of criticizing or applauding certain scientific claims or programs or disciplines. That is to say, they may not be pure expressions of institutional intentions to describe and interpret science but tools in saying what *ought* to be believed or done within science as a whole or within a particular discipline or subdiscipline. Viewed in that way, such statements not only can be taken seriously by students of science, they *must* be taken seriously, but *in a different way*—as part of the *topic* that the sociologist or historian means to describe and interpret.

The major conclusions I want to come to concern both the variability

of scientists' metascientific statements and the nature of their relationship to what might loosely be called "science itself." Here I'd like to say—and again I can call on the additional authority of Einstein and Planck to say it—that the relationships between metascientific claims and the range of concrete scientific beliefs and practices are always going to be intensely problematic. "In the temple of science," Einstein said, "are many mansions" (1954, 224).¹² It is a modernist legacy, inherited from the methodological Public Relations Officers of the seventeenth century, that science is one, and, accordingly, that its "essence" can be captured by any one coherent and systematic metascientific statement, methodological or conceptual.¹³ But, while the vision of scientific unification remains compelling to some, no plan for unification, and no account of the essence of science, carries conviction for more than a fraction of scientists. And that is one of my points.

So what happens if we follow the sentiments of many scientists (and incidentally that of increasing numbers of philosophers) that the sciences are many and diverse and that no coherent and systematic talk about a distinctive essence of science can make sense of the diversity or the concreteness of practices and beliefs? One thing that may happen is that we take a different view of the variability of metascientific statements, taken, that is, as statements about the distinctive nature of something called "science." We may want to say that different kinds of metascientific statements may pick out aspects of different kinds, or stages, or circumstances of the practices we happen to call scientific. Or different metascientific statements may contingently belong to the practices they purport to be about: as ideals, or norms, or strategic gestures signaling possible or desirable alliances. They may be true, or accurate, about science, but not globally true about science, just because no coherent and systematic statement could be globally true or accurate about science and could at the same time distinguish science from other forms of culture. Why ever should we expect that metascientific statements of any sort could hold for particle physics (which kind?) *and* for seismology *and* for the study of the reproductive physiology of marine worms? Some metascientific statements *might be* true about a range of scientific practices localized in time, place, and cultural context, but that is for us to find out, not to assume.

Something else follows from the recognition of diversity for current

12. For increasing pluralist sensibilities about science among philosophers, see, for example, Dupré 1993.

13. On this, see the classic essay by Isaiah Berlin 1998.

concern with antiscience. Because scientists' metascientific statements are diverse, and because it is possible that each picks out some real local features of some sciences, when considered from a certain point of view, the relationship between metascience and science is certainly problematic and at most contingent. For that reason alone, one can be allowed to dispute metascientific narratives of any kind without being understood to oppose science. If science is really as distinct from philosophy as some Defenders of Science insist it is, then it is puzzling in the extreme why they should be so upset when their favorite philosophy is criticized.¹⁴ Natural science justifiably possesses enormous cultural authority; philosophy of science possesses rather little. Some tactical mistake is surely being committed when the Defense of Science appears as a celebration of a particular philosophy, still more when it celebrates versions that have been tried and long abandoned as faulty by philosophers themselves.

How to be antiscientific, then? I can now tell you some ways in which you cannot be coherently and effectively antiscientific. You cannot be against science because you dislike its supposedly unique, unifying, and universally effective Method. You cannot be against science because it is essentially materialistic or essentially reductionist. You cannot be against science because it is essentially "instrumental rationality" or, indeed, because it contains irrationality. You cannot be against science because it is a realist enterprise or because it is a phenomenological enterprise. You cannot be against it because it violates common sense or because it is a form of common sense. Nor can you be against it because it is essentially hegemonic, or essentially bourgeois, or essentially masculinist. And, of course, it should go without saying that you cannot be coherently *for* science for any of these reasons either.

A thought experiment, then a qualification, and finally some remarks on a sense in which one *can* be antiscientific in real, substantial, and constructive ways. First, the thought experiment. I, and some of my colleagues in the history and sociology of science, are methodological relativists. That is to say, I maintain, on the basis of empirical and theoretical work, that the standards by which different groups of practitioners assess

14. For example, Steven Weinberg's judgment that much philosophy of science "has nothing to do with science": "The fact that we scientists [which ones, please?] do not know how to state in a way that philosophers [which ones, please?] would approve what it is that we are doing in searching for scientific explanations does not mean that we are not doing something worthwhile. We could use help from professional philosophers in understanding what it is that we are doing, but with or without their help we shall keep at it" (1992, 167; also 29). (I am delighted to hear that. I would be very disturbed, indeed, if I thought that natural scientists were taking marching orders from philosophers!)

knowledge-claims are relative to context and that the appropriate methods to use in studying science should take that relativity into account. So far as the Scientific Method goes, like Peter Medawar and many other scientists, I am a skeptic. Further, this work leads me to believe that the natural world is probably extremely complex and that different cultures can stably and coherently classify and construe it in very different ways, according to their purposes and in light of the cultural legacies they bring to their engagements with the natural world. This position has been identified as antiscientific—motivated by ignorance and hostility—and, it is said, that people having such small faith in science should follow its logical conclusions: they should jump in front of cars or consult witch doctors rather than neurologists when their heads ache.

It is a silly and misguided argument, but nonetheless an interesting one to consider. I do not jump in front of cars and I do consult physicians when I need to do so. What does this prove? Not that I am insincere in my methodological relativism, or that I have contradicted myself, but that my genuine confidence in a range of modern scientific and technical practices and claims proceeds from different sources than my belief in some set of methodological metascientific stories. My confidence in science is very great: that is just to say that I am a typical member of the overall overeducated culture, a culture in which confidence in science is a mark of normalcy and which produces that confidence as we become and continue to be normal members of it.

I have been to the same sorts of schools as Alan Sokal, Steven Weinberg, Paul Gross, and Norman Levitt; we share other important cultural legacies and sensibilities; we probably vote the same way and like the same sorts of movies, though that's just a guess. Apart from our different academic disciplines, our institutional environment is much of a muchness; and if we met each other at a party with our name tags off, there's a decent chance that we'd hit it off pretty well. But, for all that, my professional confidence in a range of metascientific global stories about the Scientific Method, and its warrant for scientific effectiveness, is very low. So *this* is what is proved by my preference for physicians over witch doctors, for astronomers over astrologers: the grounds of my confidence in science have very little to do with metascientific stories, of any kind. And, arguably, the same situation obtains over a broad range of educated, and perhaps of not-so-educated people.

Now the qualification: in my academic work I have made, and I continue to make, claims about science that have an apparently global character, though to be honest I've become a bit more circumspect about making them as time has gone on. And I want to defend their character,

pertinence, and legitimacy. So, for example, I've been known to say that the social dimension of science is constitutive and that trust is a necessary condition for the making and maintenance of scientific knowledge. These *are* metascientific statements, and they *are* meant to apply to all scientific practices that I know of. So am I not hoist on my petard? I don't think so. The reason is that when I say such things about science I am theorizing about the conditions for having knowledge of any kind. I am, so to speak, doing cognitive science without a license. What I am *not* doing is picking out a unique essence of science, meant to hold good for invertebrate zoology and for seismology and for particle physics (all kinds) and not to hold good for phrenology or accountancy or for the empirical and theoretical projects of everyday life. I may be right or wrong in the domain of theorizing-about-knowledge-of-any-kind, but I am not theorizing about a unique scientific essence. And that is the matter at issue.

Again the question: how to be antiscientific? As I said, being against the essence of science and being against one or other metascientific story uniquely about science are not very good ways of being antiscientific, nor do I find that my skepticism about the Scientific Method frees me in any way and to any degree from belief in the existence of electrons or in DNA as the biochemical basis of heredity. Those who are against the methodological or conceptual essence of science are against nothing very much in particular. And those who might be genuinely hostile to what they take to be the essence of science are probably just as ineffective as they are misguided. Who reads this stuff anyway? In order to corrupt the youth of Athens, you first have to get this stuff in their hands, then you have to get them to read it, and understand it, and care about it; then you have to persuade them—against the background of everything else they've been told—that you're right. Not such an easy business, really, as any teacher in my line of work knows.

But being against something in particular about science is both possible and legitimate. How to be against something in particular about science *should one wish to be so*? Here again it is good to listen to what some scientists themselves have to say. And if we listen to scientists (other than those who are taking the lead in the science wars), what we can hear is not a global defense of science, nor, of course, a global criticism of science. Rather, we can hear local criticisms of certain tendencies *within* science, or within parts of it—criticisms that are often substantial and vehemently expressed.

Some scientists are now violently critical of what *they* take to be the shallowness of reductionist programs, the tyrannizing and stultifying

effects of bureaucratization in science, the dedicated following of scientific fashion and the attendant loss of the Big Picture and of imagination, the hegemony of Big Science at the expense of Little, the incompetence of the peer review system, the commercialization of science and the attendant ethical and intellectual erosion, and many other ills *they* diagnose in the contemporary Body Scientific. Some of these internal criticisms happen to look to professional metascience and even to the history of science for aids in understanding how current arrangements came to be and as tools in making things better; many do not.

It is not difficult to find these public internal criticisms: recent issues of biological periodicals are full of them, and memoirs and reflections by eminent scientists—including those by E. O. Wilson, Erwin Chargaff, Gunther Stent, and Richard Lewontin—are another rich seam of such things. The striking thing, given the ultimate vacuity of the science wars, is just how little professional metascientists have concerned themselves with these internal contests, and, indeed, how little sociologists and historians have even noticed them as topics. That is almost certainly a Bad Thing: if being against science is, as I am suggesting, being against nothing very much in particular, being against the current peer review system, or against the hegemony of Big Science, or against the way in which clinical trials are constituted and funded, is being against something substantial and important. Is it sociologists' and historians' role to take sides in such debates? I don't think so (though I know of some sociologists who disagree). But these debates do offer a venue in which we can have interesting and substantial conversations with our scientist-colleagues. It would be mutually beneficial to have these conversations.

Finally, we need to remember that professional metascientists, like professional scientists, are also citizens. We are equal members—many of us—of institutions of higher education, and we all pay our share in the state support of scientific research. So far as the first type of citizenship is concerned, no one, I think, should rule it out of order, or identify it as *lèse majesté*, to take one side or another in university discussions over, for example, how much science should be taught in the curriculum or how scientific subjects should be taught. If one wants to say (as I do *not*) that there's too much science in the required curriculum, or if one wants to say (as I *do*) that the philosophical, historical, or sociological dimensions should have a place in the science curriculum, then one should be free to do so. And, should one want to make such arguments, one should not have to face accusations of being antiscientific.

Similarly, as citizens paying the bill for much scientific research, one should be free to say if one wants—and on an informed basis—that the

Superconducting Supercollider cost too much relative to its advertised benefits, or that too much money is going to a cure for AIDS and too little for an AIDS vaccine, or that governments have got their priorities wrong as between AIDS research and diarrhea research, or that some science supported by the public treasure is trivial or intellectually unimaginative, or that the links between publicly funded science and the commercial world are becoming worrying. And one should be able to say such things—again, if one wants—without being denounced as anti-scientific. Some scientists say such things on a professional basis, and some citizens may want to say such things as responsible members of democratic societies. They must be free to do so, not intimidated into deferential silence.

My fear is that, if we carry on in our present courses, the ultimate and consequential casualties of the science wars will not be the job security of sociologists of science, but free, open, and informed public debate about the health of modern science. And the health of science ultimately depends on that debate.

Here are the sources for the notorious metascientific claims at the beginning of this essay:

1. Many sources, including Peter B. Medawar (immunologist), *The Art of the Soluble* (London: Methuen, 1957), 132; James B. Conant (chemist), *Science and Common Sense* (New Haven: Yale University Press, 1951), 45; Lewis Wolpert (biologist), *A Passion for Science* (Oxford: Oxford University Press, 1988; compiled with Alison Richards), 3; Richard Lewontin, "Billions and Billions of Demons," *New York Review of Books* 44, no. 1 (9 January 1997):28–32 (on page 29: "The case for the scientific method should itself be 'scientific' and not merely rhetorical").
2. Erwin Chargaff (biochemist), *Heraclitean Fire: Sketches from a Life before Nature* (New York: Rockefeller University Press, 1978), 138.
3. Edward O. Wilson (entomologist, sociobiologist), *Naturalist* (New York: Warner Books, 1995), 210.
4. Niels Bohr, quoted in Abraham Pais, *Niels Bohr's Times, in Physics, Philosophy, and Polity* (Oxford: Clarendon Press, 1991), 314.
5. Albert Einstein (physicist), *Out of My Later Years* (New York: Philosophical Library, 1950), 96; also Einstein, *Ideas and Opinions* (New York: Crown Publishers, 1954), 355. I have here slightly paraphrased Einstein's original statement that the bases of physics cannot be inductively secured from experience, but "can only be attained by free invention." Geometrical axioms—the bases of the deductive structure of physics—are, Einstein said, "free creations of the human mind" (1954, 234).
6. Jacob Bronowski (mathematician), "Science is Human," in *The Humanist Frame*, ed. Julian Huxley (New York: Harper and Brothers, 1961), 83–94 (quote, page 88). I have here altered the first-person "we" to the third-person "scientists."

7. Warren Weaver (mathematician and scientific administrator), "Science and People," in Paul C. Obler and Herman A. Estrin, eds., *The New Scientist: Essays on the Methods and Values of Modern Science* (Garden City, NY: Anchor, 1962), 95–111 (quote, page 104).
8. Gunther Stent (biochemist), interviewed in Lewis Wolpert and Alison Richards, *A Passion for Science* (Oxford: Oxford University Press, 1988), 116.
9. Brian Petley (physicist), *The Fundamental Physical Constants* (Bristol: Adam Hilger, 1985), 2: "Modern physics is based on some intrinsic acts of faith, many of which are embodied in the fundamental constants."
10. Richard Lewontin (evolutionary geneticist), "Billions and Billions of Demons" (see reference 1), 31: "[The public] take the side of science *in spite* of the patent absurdity of some of its constructs, *in spite* of its failure to fulfill many of its extravagant promises of health and life, *in spite* of the tolerance of the scientific community for unsubstantiated just-so stories, because we have a prior commitment, a commitment to materialism."
11. Richard Lewontin, Steven Rose (neurobiologist), and Leon J. Kamin (psychologist), *Not in Our Genes: Biology, Ideology, and Human Nature* (New York: Pantheon, 1984), 33; see also "[T]he internalist, positivist tradition of the autonomy of scientific knowledge is itself part of the general objectification of social relations that accompanied the transition from feudal to modern capitalist societies" (33). It would not be easy to find such a sweepingly didactic statement of this kind expressed by present-day historians or sociologists of science!

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