Following are the unedited manuscripts of my three contributions (essay, commentary, rebuttal) to:
The One Culture? A Conversation about Science. J. A. Labinger and H. Collins, Eds., Chicago: University of Chicago Press, **2001**

1. AWAKENING A SLEEPING GIANT?

The disagreements that constitute what have come to be known as the Science Wars have been vigorously argued, by combatants from all reaches of a broad spectrum of positions. There does not appear to be much dispute, however, on one point: very few scientists are interested, let alone involved. One of the most prominent of those who are, Paul Gross, is aware of fewer than a dozen scientists active on his side.[1] If we include all practicing scientists who have participated from any perspective the numbers might grow by a fewfold, but not much more. Similarly, Alan Sokal places the main locus of the Science Wars outside the realm where scientists live:

> Finally, *within* academia and the left, this affair tapped into a pre-existing pool of consternation and resentment among non-postmodernist academics in the humanities and social sciences (of which I, as a scientist, was largely unaware). It's this latter factor that has kept the affair going — in the form of innumerable forums, colloquia and debates — in academia.[2]

As Sokal's comment suggests, the disconnect goes well beyond non-participation, to non-awareness, on the part of most scientists. Indeed, when scientists I meet ask me what I'm working on, if I mention "the Science Wars" the reaction is almost invariably incomprehension. This is despite the fact that the events and issues at stake do receive at least occasional airing in the "trade" journals that most scientists look at regularly (e.g., Science, Nature, Physics Today, Chemical & Engineering News), not to mention their considerable visibility in popular organs such as the New York Times and Newsweek, especially during the months following the eruption of the Sokal Affair in 1996.

It is doubtful whether this situation is alterable to any great extent. Most professional scientists have neither the inclination nor incentives to divert any appreciable fraction of their time and energies from their main pursuits. But should one try? Some want scientists to become more aware of science studies, so that they can better defend against the dangers posed by the “antiscience brigades.” In contrast, calls for positive engagements have been rather rare even on the part of science studies practitioners, who one might think would have a strong interest in enlisting allies from the scientific ranks. I would like to argue that a defensive stance is not warranted, and then try to present a case for the

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1. Paul R. Gross, "Evidence-Free Forensics and Enemies of Objectivity," in A House Built on Sand, ed. Noretta Koertge, Oxford: Oxford University Press, 1998, p. 114.
2. Alan D. Sokal, posted on an e-mail discussion list, Dec. 12, 1996.

potential benefits that could be realized if (at least some) scientists were to take a more serious interest in science studies.

Manning the Barricades?

A number of writers have proclaimed that science studies pose a threat to science; for example:

> Scientists also should confront the sociologists and philosophers at their institutions who are attacking the foundations of science. Presumably, tenure decisions and promotions at universities are based on scholarship, and academic scientists must take an interest in the academic decisions in other departments on campus. This is not a question of academic freedom, but rather one of competency. We should expose political correctness and fundamentalism that lead to misinformation about science.[3]

This particular statement seems to be imbued with more than a little fundamentalism of its own. Are there in fact inviolable "foundations of science" that may not even be examined without raising issues of hostility and/or incompetence?

The suggestion that science studies, broadly construed, are <u>deliberately</u> hostile to the scientific enterprise — consciously aimed at undermining scientific authority and degrading public enthusiasm for science — seems to me plainly unjustified, not least because of explicit statements to the contrary by many of the more prominent practitioners. This point is considered at some length in several of the pieces in this book, so I will not spend any time on it.

However, a claim might be made for <u>inadvertent</u> hostility; i.e., that this body of work creates a climate in which the above-mentioned undermining and degrading goes forward, notwithstanding its authors' intentions:

> [T]he fatuities propounded in the name of "science studies" are not, in themselves, particularly dangerous. I can't envision a host of postmodernists taking to the streets in the name of epistemological relativism. Rather, the long-term danger of the phenomena...is that they help erode such defenses against credulity (and they are very meager) as already exist within the culture. In other words, Paul Forman, Sandra Harding, and company, whether they know it or not, are essentially running interference for P. E. Johnson, Duane Gish and company.[4]

Such an argument may be defensible on philosophic grounds, I suppose; but in practice? Is there really any likelihood that any significant number among those who believe in creationism (or astrology, or UFOs, or whatever) have the slightest awareness of science studies? Or are more easily influenced by the likes of Johnson and Gish if and when the latter use science studies to support their positions? Or would be the least bit affected if all the scientists in the world spoke out against "harmful" science studies?

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3. Allen J. Bard, "The Antiscience Cancer," <u>Chemical & Engineering News</u>, April 22, 1996, p. 5.
4. Norman Levitt, posted on an e-mail discussion list, Sept. 8, 1998.

A specific arena for concern has been that of science education. Some have argued that increasing influence of science studies will be detrimental;[5] others, that they are already substantially responsible for the dismal state of K-12 science education in the US. In a recent book physicist Alan Cromer places much of the blame for the latter on the prevalence of the constructivist approach to education, which in turn derives much of its appeal, according to him, from the social constructivist turn that science studies has taken over the past couple of decades: "Constructivism is a postmodern antiscience philosophy...."[6] While in no way denying that science education in the US is in a sorry state, I have trouble seeing how this can be legitimately ascribed to the onset of science studies. The examples Cromer offers of abysmal teaching practice are not an obvious consequence of constructivism: one can teach badly under any educational philosophy.

As for his characterization of constructivism as an "antiscience" philosophy, it is intriguing to note his defense of science against a charge of amorality:

> The purpose of a scientific study of human behavior isn't to undercut morality, or to dictate conduct, but to enlarge the scope of discourse about such matters. It should offend only those who are completely comfortable with their sect's, or cult's, or party's peculiar view of the world. (p. 79)

Suppose we were to change a couple of words?

> The purpose of a sociological study of scientific practice isn't to undercut authority, or to dictate conduct, but to enlarge the scope of discourse about such matters. It should offend only those who are completely comfortable with their sect's, or cult's, or party's peculiar view of the world.

Perhaps many scientists would be offended by the suggestion that they constitute a sect or cult, or that the word "peculiar" (in any sense) is applicable to a scientific worldview. But perhaps many would be sympathetic to the notion that they should evaluate apparently foreign programs with the same degree of charity that they would like outsiders to apply to their own.

None of this is to deny that scientists find profound philosophical differences between the conclusions of science studies and their own opinions, nor that they should strongly argue for their viewpoints — I have done so myself.[7] But if these are philosophical arguments, not wars against the heathen, they need to be carried out in appropriate discourse. The far-too-prevalent practice these days seems to be to simply hold up apparently murky and/or outlandish statements for ridicule — a sort of res ipso loquitor argument.

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5. For example, Noretta Koertge, "Postmodernisms and the Problem of Scientific Literacy," in A House Built on Sand, ed. Noretta Koertge, Oxford: Oxford University Press, 1998, pp. 257-271.
6. Alan H. Cromer, Connected Knowledge: Science, Philosophy, and Education. New York : Oxford University Press, 1997, p. 10.
7. Jay A. Labinger, "Science as Culture: A View from the Petri Dish," *Social Studies of Science* 25 (1995): 285-306.

For example, in a recent mail forum on the Science Wars,[8] one contributor wrote: "Let's see if the magazine will print a particular sentence (a whole sentence!) from the work of a leading postmodernist, Sandra Harding. I claim it gives a much fairer picture of the postmodernist attitude toward, and understanding of, science than anything PHYSICS TODAY has yet been willing to discuss." (The sentence, of course, is Harding's (in)famous "Newton's Rape Manual" comment.) The writer clearly believes that it is "fair" both to represent a large and diverse group by a single sentence from a single author, and to offer that sentence, without any attempt to discern or explain what she might have meant by it, as an overwhelming argument. It's hard to decide which of these attitudes is more disheartening.

Of course, it is easy to see why this is an attractive strategy; no less an authority than Voltaire has extolled its rhetorical potency: "Ridicule overcomes almost anything. It is the most powerful of weapons."[9] But it is a corrosive weapon, which may as easily be brought to bear against science as for it. That is what concerns me about interventions like the famous Sokal imposture. Originally that appealed to me, both for its humorous aspects, and as a (self-proclaimed) experimental test of the ability to distinguish whether apparently obscure writing actually conceals deep meaning or is simply obscure. I still believe those are merits. (And, I should note, I don't include Sokal with those who are content to let "ridiculous" statements stand on their own; in his subsequent writing he has tried to engage with the possible meanings thereof.)

More recently, though, something that happened during the last (1998) political campaign has given me second thoughts. A candidate for US Senator repeatedly broadcast ads charging his incumbent opponent with wasting taxpayer money by, among other things, supporting a research project on "cow gas." (The ad was accompanied, naturally, by appropriate background sound effects.) According to some analysts, this and related ads were largely responsible for bringing the challenger from a far-distant second in the early polls to a very narrow loss on election day. But the aspect I found most striking was the fact that the incumbent's campaign managers defended their candidate only on procedural grounds: they pointed out that his "support" for the study consisted merely of a vote against cutting a large appropriation for the Environmental Protection Agency, of which the particular project in question was just a small piece. There was no attempt at all (based on the news releases and other pieces posted on the candidate's Web site) to argue that the study in question (which is concerned with the efficiency of cows' transforming caloric intake into milk production as opposed to wasteful — and environmentally harmful — methane formation, and whether improvements might be made) just might be scientifically valid and even potentially important. Once the study had been presented as ridiculous, with no examination of its content whatsoever, any course of action based on

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8. PHYSICS TODAY, January 1999, pp. 15 *ff*.
9. Quoted in Hal Hellman, Great Feuds in Science: Ten of the Liveliest Disputes Ever. New York: John Wiley and Sons, 1998, p. 63.

actually trying to address the merits of the case was obviously considered much too risky.

I certainly don't want to argue against the use of humor as a rhetorical (or political) device; I would hate to end the Science Wars by making them so boring that everyone loses interest! But episodes such as the above show how the strategy of ridicule can do damage to the overall climate of discourse, and one might hope that it will be wielded judiciously, if at all.

Joining the Party?

If there is no need for scientists to take a defensive interest in science studies, what is the case for their paying <u>any</u> attention? Is benign neglect the right attitude after all? I will argue, for a number of reasons, that it is not. The most straightforward is that some scientists will find science studies interesting intrinsically, just as many scientists are interested in "traditional" history of science. Clearly this alone won't convert anyone who is not of that persuasion, but I believe there are stronger claims.

A number of arenas where the interests and expertises of scientists and science studiers intersect may be readily identified — science policy, political decision making, public understanding of science, to cite a few broad characterizations. Most scientists will readily admit that they cannot claim sole authority over such issues; but they are equally unwilling to cede it to any other group, and mechanisms for integrating the science community with other components of the body politic to reach consensus seem to be lacking. The model most scientists appear implicitly to prefer is: let us do the science, and then the rest of you can make a political judgement based on our firm scientific conclusions.

But surely we don't need the science studiers to point out to us (although they have done so repeatedly) that this model is often (usually? always?) unworkable in practice. For it to succeed, we would need a scientific consensus that surpasses a point where a respectable set of dissenting scientists can still be found to support an opposing course. Experience tells us this is <u>very</u> difficult to achieve. Another problem, perhaps even more serious, is that decisions often must be made before anything close to scientific agreement has been reached.

Global warming may be a good example of the latter. Many experts — probably a majority — are convinced by evidence already in hand; but a very sizable group, including many well-regarded scientists, are not. So shouldn't we take the "obvious" course of holding off on any policy decisions until we have the additional evidence needed to decide the matter? But what if, as many believe, it will then be too late to avert changes of an unacceptable magnitude? If it comes to that, will we then excuse ourselves by saying that yes, in retrospect, things might have worked out better if we had taken action sooner, but as scientists we were acting responsibly by waiting for decisive knowledge? (And will anyone who <u>isn't</u> a scientist be much impressed by that defense?)

We urgently need better mechanisms for dealing with such cases; and if scientists are not in a position to provide them on their own, nor willing to hand over authority to other sectors, the only alternative is to foster active collaborations. And that, in turn, requires that scientists begin to look into what is going on in other fields.

The strongest possible reason for scientists to follow science studies, of course, would be to demonstrate that they could have an impact on how one practices one's science. There is no convincing case for this yet; indeed, some science studies practitioners have explicitly denied it. But I believe at least the possibility exists. Scientists have long been dubious whether philosophy of science has anything to offer them, as in Weinberg's quote (possibly originally attributable to Feynman:

> I've heard the remark (although I forget the source) that philosophy of science is just about as useful to scientists as ornithology is to birds.[10]

But one of the hallmarks of recent science studies is its emphasis on practice as opposed to theory, as noted for example by Pickering:

> In the 1970s, academic science studies took a naturalistic turn. Led by a few sociologists, people started examining what scientists did for a living, and discovered that they experiment. It turned out that scientific experiment was not some trivial adjunct to theory....[11]

I must confess that "scientists experiment" doesn't strike me as much of a discovery: I doubt if many chemists, at least, would have ever dreamed of representing experiment as the tail on theory's dog. But there is a valid point here: there is a growing body of work that at least <u>tries</u> to be more relevant to the day-to-day practice of science, and scientists who are unaware of it (most?) might well find things there that speak to them.

One frequently voiced objection is that these studies are at best incomplete: by focusing on the social to the exclusion of the natural world they produce severely distorted pictures, or even miss the point altogether. I would agree with the part about incompleteness; but how often do we expect a scientific study to be the last word on its subject? In fact, one typical strategy of scientific experimentation is isolation of variables: determining the effect of changing one while holding others constant. We are aware of the limitations of this approach — interactions between variables can well compromise any conclusions we might reach — but we do it anyway, as a useful strategy. The sociologist's practice of "bracketing out" the "correct" results in carrying out his case study can arguably be viewed

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[10]. Steven Weinberg, "Newtonianism, Reductionism and the Art of Congressional Testimony", <u>Nature</u>, Vol. 330 (3 December 1987), 433.
[11]. Andrew Pickering, review of P. Galison's <u>Image and Logic</u>, <u>Times Literary Supplement</u>, 24 July, 1998.

as a close analog of that practice. I take note of McKinney's objections, addressed specifically to Collins' and Pinch's The Golem:[12]

> Collins and Pinch and their colleagues have done science studies a great service in choosing to focus on the broader, less methodological issues that naively scientistic accounts of the scientific process have ignored for far too long. But if Collins and Pinch wish to remain true to their title, The Golem: What Everyone Should Know About Science, they must tell the whole story, not just the parts that have been neglected. Replacing one incomplete story with another serves nobody's interests.[13]

But this complaint seems to me unreasonable: who can tell a complete story? Isn't adding to, rather than replacing, another story a worthwhile goal?

And furthermore, scientists (like McKinney) who think science studies do have something to offer, but have reservations about how they are carried out, may well find that sober and reasoned criticism can stimulate some of the reconsideration they seek. The recent 2nd edition of the aforementioned The Golem includes a substantial appendix, and significant revisions, prompted by scientists' responses to some of the language they found objectionable in the first version. Perhaps in many other cases, a convergence of views (at least partial) might not be too much to expect!)

To illustrate my belief that science studies can offer something to the practicing scientist, here is an example I'm hoping to expand into a full-fledged study: that of so-called "bond-stretch isomerism."[14] This was a case, first reported in the literature in the early 1970s, where a chemical compound was found to exist in two isomeric forms, differing in color. That in itself is common; but here the detailed molecular structure as revealed by X-ray crystallography showed that the two forms differed only in the length of one of the bonds. Everything else — all the other bond lengths and angles, as well as the packing of molecules to make up the overall structure of the crystal — was virtually indistinguishable. Such a situation was unprecedented and seemed highly unlikely. Over the next 20 years a number of additional examples were reported, as well as a theoretical study that supported the possibility of that form of isomerism; but in the early '90s a reexamination showed convincingly that the central evidence — the crystallography — was artifactual, a consequence of sample impurity.

What does this have to do with science studies? It is fairly easy to see that the original misinterpretation, and its persistence for two decades, are in large part

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12. Harry Collins and Trevor Pinch, The Golem: What Everyone Should Know About Science, Cambridge: Cambridge University Press, 1993; The Golem: What You Should Know About Science (2nd edition), Cambridge: Cambridge University Press, 1998.
13. William J. McKinney, "When Experiments Fail: Is 'Cold Fusion' Science as Normal" in A House Built on Sand, ed. Noretta Koertge, Oxford: Oxford University Press, 1998, p. 147.
14. Gerard Parkin, "Do Bond-Stretch Isomers Really Exist?" Accounts of Chemical Research, 1992, 25, 455-460.

due to two aspects — both concepts that feature in science studies[15] — of X-ray crystallography. First, it has become a highly "black-boxed" technique: much of the collection, analysis and even interpretation of data is automated. Second, it is a "privileged" technique. In the realm of molecular structure determination, all other methods such as various forms of spectroscopy have become subordinated. (In seminars one almost invariably hears phrases like "To settle the structure once and for all, we turned to X-ray crystallography….") Indeed, one of the more striking features of the evolution of the bond-stretch controversy is that warning signs, coming from spectroscopy and even old-fashioned chemical analysis, that something might be amiss, were essentially ignored in the face of the "indisputable" crystallography.

Now, one could certainly claim that this is a fairly trivial application of science studies, and that scientists would be perfectly aware of, and capable of addressing, such issues without any help from outsiders. True enough; but it still seems to me that having them called to one's attention from an outsider's perspective may be particularly effective in raising one's sensitivities, and increasing one's alertness to the possibility of analogous situations. Certainly my own initial reaction to the bond-stretch case resolution (before I'd become significantly aware of any science studies literature) was pretty much along the lines of "well, good, they've cleared up a mistake," rather than trying to place it in a much broader (and much more appropriate) context. Would this work in any useful sense for others? It would be interesting to try to find out.

We can go beyond such specific cases to larger questions. As many have pointed out,[16] science cannot possibly progress if it spends too much time constantly questioning its fundamental credos; but progress is equally impossible if <u>no</u> such revisiting and revising takes place. Scientists seem confident that there is sufficient internally-generated reflection, but others are not so sure.

For example, Sokal responded to Bruce Robbins' suggestion that truth can be oppressive as follows:

> "It was not so long ago," Robbins explains, "that scientists gave their full authority to explanations of why women and African-Americans…were inherently inferior." But <u>that</u> isn't truth — it's ideology posing as truth, and objective science demonstrates its falsity. This error is repeated throughout Robbins' essay: he systematically confuses truth with <u>claims</u> of truth, fact with <u>assertions</u> of fact, and knowledge with <u>pretensions</u> to knowledge.[17]

No doubt the distinctions Sokal draws — between truth and claims, fact and assertions, etc. — are legitimate from a philosophical point of view. But his

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15. Bruno Latour, <u>Science in Action</u>. Cambridge: Harvard University Press, 1987.
16. See, for example, Thomas S. Kuhn, The Essential Tension: Selected Studies in Scientific Tradition and Change. Chicago: University of Chicago Press, 1977, pp. 225-239.
17. Alan Sokal, "Truth or Consequences: A Brief Response to Robbins" in <u>Tikkun</u>, Nov/Dec 1996, p. 58.

dismissal of Robbins' argument seems to me much too facile. It was "objective" science that was taken to demonstrate, earlier this century, what Sokal now calls ideology. How are we to know, today, whether some of our "truths" are in fact "claims," that might some day be revealed to be false? But any such determination would have to be preceded, clearly, by a willingness to suspend belief; and it seems plausible that, again, in some cases an external impetus might be more effective in producing that state. In the case of racial inferiority, the visible example of Nazi Germany may well have done much more to provoke challenges to accepted scientific wisdom than anything initiated by the scientific community itself.

Basically, I'm just suggesting that we need an occasional fresh look from a completely different perspective — even if it looks plainly wrong — to shake us out of too-comfortable thinking patterns. In this regard, Sokal and Bricmont's comment about metaphorical aspects seems to me too restrictive:

> Some people will no doubt think that we are interpreting these authors too literally and that the passages we quote should be read as metaphors rather than as precise logical arguments. Indeed, in certain cases the "science" is undoubtedly intended metaphorically, but what is the purpose of these metaphors? After all, a metaphor is usually employed to clarify an unfamiliar concept by relating it to a more familiar one, not the reverse.[18]

Certainly that's one role of metaphor, but not the only one, maybe not even the most important one. I would focus on the "relating" rather than the "clarifying" aspect: metaphors can forge new connections, and even if most of them lead nowhere, who knows which one will spark a novel, productive insight?

So perhaps science studies —maybe even the "antiscience, postmodern" work assailed by so many critics — is well situated to carry out that function. Even such a non-sympathizer as E. O. Wilson has acknowledged that role:

> As today's celebrants of unrestrained Romanticism, the postmodernists enrich culture. They say to the rest of us, Maybe, just maybe, you are wrong. Their ideas are like sparks from fireworks explosions that travel away in all directions....a few will endure long enough to cast light in unexpected places. That is one reason to think well of postmodernism....Still another, the one that counts most, is the unyielding critique of traditional scholarship it provides. We will always need postmodernists or their rebellious equivalents. For what better way to strengthen organized knowledge than continually to defend it from hostile forces?[19]

History tells us that Roman generals, riding their chariots in triumphal parades, were accompanied by a slave who would ride behind and continuously whisper in their ear about the transient nature of the world: what's on top today will be

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[18]. Alan Sokal and Jean Bricmont, Fashinable Nonsense: Postmodern Intellectuals' Abuse of Science. New York: Picado, 1998, p. 10.
[19]. E. O. Wilson, "Back from Chaos" in Atlantic Monthly, March 1998, pp. 61-62. (I should note that the ellipses in the quote represent much less complimentary comments!)

dust tomorrow. History doesn't tell us, so far as I know, whether the generals ever indulged the inevitable urge, whenever a slave got sufficiently irritating, to turn around and whack him in the chops. While in no way pushing the analogy too literally, I would suggest that scientists have been rather quick to respond to voices of skepticism with annoyance. If instead we consider them as invitations to re-examine, even if only occasionally, all that we take for granted, we might well find in them useful spurs to think in new ways.

2. SPLIT PERSONALITIES, OR THE SCIENCE WARS WITHIN

Jay Labinger

A number of contributors to this volume have suggested that what we have here is a failure to communicate, and have offered a number of correctives. Most of these can be broadly characterized as: try to avoid words that might be interpreted as something not really meant; try to avoid reading messages that aren't really there. (I was struck by the number of contributors who remarked on the tendency of readers to interpolate the phrase "nothing but" or the equivalent into passages where it may never have been intended.) Among the many themes of discord and concord that have been generated, I would like to concentrate on two that appear, occasionally, to reflect some degree of <u>internal</u> division, where individual commentators seem to be making use of both sides of an issue.

I emphasize the words "appear" and "seem" in the above: few if any of the issues we deal with are so black-and-white that one could or should line up unambiguously on one side or the other, and that's certainly the case with the two dichotomies in question. But even the perception that the author is trying to have things both ways, I believe, can be a significant contributor to the misunderstandings and tendencies to talk past one another that have been so well documented in the first part of this book.

First: Is science very much like common sense, or very different from common sense? Entire books could be (and have been[20]) written on this theme, and several of our contributors, including Shapin (4) and Lynch (7), have addressed it, representing it primarily in terms of different opinions held by different scientists. The aspect I am specifically concerned with is to what degree one is justified in extrapolating from common-sense or "easy" cases to claims made with respect to science in general. For example, Sokal & Bricmont use the "obvious" question of whether or not it is raining as a starting point to address the role of reality in determining belief. They do recognize that there is an issue here: one can question whether "ordinary" and "scientific" knowledge should be treated the same. Yes, they conclude: if reality constrains ordinary knowledge it constrains scientific knowledge even more so, because experimental science is carried out expressly to make that the case (13). Thus science would seem to be just an elaborate form of common sense.

On the other hand, S & B acknowledge that science is <u>difficult</u> (10). Hence it is far from clear that intent — the basis of the claim above — can guarantee outcome, or that common-sense arguments can be freely carried over to the consideration of cutting-edge science. It may be straightforward to go from observation to belief in the case of rain; but it is considerably less so in the cases examined by science studies, which are aimed precisely at whether experimental

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[20]. At least two entire books have been written on the first: Lewis Wolpert, <u>The Unnatural Nature of Science: Why Science Does Not Make (Common) Sense</u> (London: Faber & Faber, 1992); Alan Cromer, <u>Uncommon Sense: The Heretical Nature of Science</u> (Oxford: Oxford University Press, 1993).

activity is capable of making "Nature itself constrain our beliefs" as S & B claim. So the appeal to common-sense and science may well belong to separate realms, and an argument that seems to freely hop from one to the other is not very convincing.

A particularly problematic connection between the familiar and the not-so-familiar is the analogy between science and the law. S & B suggest (15) that the statement "it is true that X is guilty…this truth came into being as a result of decisions about how we should licence our police investigations…a truth brought about by agreement to agree" sounds very odd. Really? If I substitute "O. J. Simpson is not guilty" for "X is guilty" in the above, the statement sounds like a pretty accurate one to me!

I expect S & B would counter this by contesting my usage of the word "true" here. Their intuitive notion of "truth" (8) presumably has little to do with a formal jury verdict. But if we have no access to that kind of truth, as must frequently be the case in the realm of criminal investigations, arguments based upon it are not particularly helpful; and science studies would say, I presume, that applies in the realm of scientific investigations as well. Here again they appear to be alternating modes of argumentation, starting from a non-rigorous, common-sense understanding of "truth" and proceeding to a closely argued position on whether methodological relativism logically entails philosophical relativism. This gives the non-sympathetic reader an easy opening to dismiss the argument and avoid more productive engagement.

The secoond issue, stated generally, is: Are the foundations of science studies primarily theoretical or empirical? Obviously this is a huge topic, that far exceeds my space and resources to deal with adequately. I focus specifically on one aspect that science studies seem to appeal to frequently, either explicitly or implicitly — the principle of underdetermination, or the Duhem-Quine thesis. Both Mermin (6) and Sokal & Bricmont (7) characterize this idea — that there are always any number of ways to interpret any finite set of observations — as logically correct but practically irrelevant. I would have to agree, in the sense that no scientific case history that I know of (even as represented in science studies) reveals any such multiplicity of interpretation after a substantial amount of evidence (a vague term, admittedly) has been collected. What such studies do show (perhaps "claim to show" would be more appropriate, depending on who is reading them) is that consensus can often run ahead of what the data would seem to compel.

But that falls well short of justifying, from an empirical standpoint, any sort of radical underdetermination thesis. If anything, overdetermination is the more common situation: we can find cases where at some point in time no theory is found to be compatible with all existing data by the community.[21] That directly contradicts assertions like the following (not from this volume), and makes them rather hard to take:

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[21]. See, for example, the account of the "missing" solar neutrinos in The Golem.

> "It is unproblematic that scientists produce accounts of the world that they find comprehensible: given their cultural resources, only singular incompetence could have prevented [high-energy physicists from] producing an understandable version of reality at any point in their history."[22]

Such sweeping claims tend to undermine credibility for those readers who perceive therein a hefty degree of prior ideological commitment. Science studiers proclaim the empirical nature of their programs; but their empirical work appears to be informed by philosophical tenets that are unsupported — or even contradicted — by observation. It becomes all too easy to dismiss such studies, even when solid data have been collected.

Unlike S&B, who call upon science studiers to abandon their "misguided epistemology" (18), I wouldn't deny anyone the right to a theoretical framework. But I do feel that a little more care over how much weight theoretical grounds are given might help avoid some of the problems of perception. I note Shapin's comment about a connection between weak methodological discipline and success in the natural sciences (5), and wonder whether it might be usefully (and symmetrically!) applied to science studies.

One thing a reader can do is dissect out and discard any theoretical component of such a study, and concentrate on the residue, which will often be a valuable, if mainly descriptive, account. For example, whether or not one agrees with Perutz' reaction to the Geison book, discussed by Dear, it would be hard to argue that recovering the contents of Pasteur's (highly illegible) notebooks was not a heroic effort. Likewise, Saulson characterizes Collins' collection of aural archives as a great service, whatever one thinks of his conclusions (2).

I recognize that the above may well sound at best like damning with faint praise; it is reminiscent of the "Sandwich Theory" of reading that Pinnick offers (and rejects) with respect to a particular work:

> [The book] just needs to be read with charity. You take the first and last chapters with a large degree of intellectual tolerance (those are the parts to ignore, with all the bad argumentation and the trumpeting about sociology of knowledge); then, in between, there is a good historical account to read.[23]

I wholeheartedly support the idea of "reading with charity," but I don't think it implies any of Pinnick's evident condescension. In many of the scientific papers in my field that both report and interpret data, I find the data extremely useful even though I consider the interpretation to fall somewhere in the range of

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22. Andrew Pickering, as cited (and criticized!) by Peter Galison, How Experiments End (Chicago: University of Chicago Press, 1987), p. 10.
23. Cassandra L. Pinnick, "What is Wrong with the Strong Programme's Case Study of the 'Hobbes-Boyle Dispute'?", in A House Built on Sand, Noretta Koertge, Ed. (New York/Oxford: Oxford University Press, 1998), p. 228.

unconvincing to utter nonsense. But that doesn't mean the endeavor wasn't worthwhile.

Even an effort based on an unconvincing premise can make a valuable contribution. Lynch comments on the potential value of pushing apparently far-fetched ideas, such as Fish's baseball analogy, as far as one can: we can learn from how it breaks down (9). Physicist-turned-biologist Max Delbrück said something similar. In trying to apply Bohr's notions of complementarity to biology, he considered the conceptual transfer as just another research tool. He wanted to push it as far as it would go, recognizing that it would surely break down — but the point where it did break down would teach us something fundamental.[24] Perhaps, as Collins' focus on "strangeness" seems to suggest, this sort of "defamiliarization" is the most useful contribution that science studies can make.

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[24]. Nils Roll-Hansen, source to come.

3. LET'S NOT GET TOO AGREEABLE

Jay Labinger

Both Pinch and Weinberg comment on the level of agreement this "conversation" seems to be approaching, which Pinch notes is the hallmark of a "real" conversation. (I know nothing about the field of Conversation Analysis, but from my own experiences, I suspect that the finding that "real" conversations tend to end in agreement might have at least something to do with how a "real" conversation is defined. But never mind.) In this, my last opportunity to speak here, I want to offer some thoughts on the Benveniste case, which has been mentioned by a number of the commentators, and also to respond to S&B's criticism of my original contribution. I expect that each of my colleagues will find at least <u>something</u> they can disagree with herein.

Gregory notes that Benveniste's original work was replicated elsewhere, received positive peer review, and was published in <u>Nature</u>, which is "usually good enough" to start establishing the results as truth; and that anyone who understood these processes would be baffled by the subsequent developments. I'm not sure how well that holds up even as a general description. Peer review and publication is at best tentative validation. The review process (which hardly ever includes replication) is good at weeding out claims that can be seen to be inconsistent, either internally or with previously established results. But it is not so good at detecting claims that are plausible but incorrect. So we have <u>many</u> results that make it into print and are subsequently found to be wrong. Usually that happens when someone needs to make use of the finding for further work and is unable to do so successfully, rather than because someone simply wanted to replicate the work. (At least, that's the case in my field, chemistry; perhaps it's significantly different in other areas of science, but I'd be surprised if so.)

Leaving generalities aside, though, I believe that someone who understands the "truth-generating" processes would also see how atypical (I hesitate to say abnormal, in the face of all the arguments over what counts as normal science) the details of the Benveniste case are, and quickly overcome bafflement. All of the putative warranties for reliable truth — replication, peer review, and publication in a prestigious journal — were compromised at best. Replicability was by no means 100% — sometimes the experiment worked, sometimes it didn't, even in the principal investigators' own lab. The referees (according to an editor's note at the end the original paper; their actual reports are unavailable to me) did <u>not</u> believe the results, but were unable and/or unwilling to debar publication in the absence of unambiguous proof of error. And the journal editor's reservations were documented <u>twice</u> in the issue containing the paper, as well as in the report on the subsequent investigation he led (more on that in a moment).

Most disconcertingly, Gregory seems (I admit I may be misreading her here) to feel that the work's great potential significance — "challenging our understanding of many areas of physics and chemistry" — is another reason to be surprised at its rejection. <u>Au contraire</u>: accepting the truth of the memory of water would not just challenge but require us to discard wholesale quantities of quite well established science — what some call "textbook science" — such as the nature of molecular motions in liquids. Call me reactionary if you will; but I, for one, am not going to even begin to think about doing that without much more compelling evidence than has been offered. Rather, I'll assume that something is wrong, even if I can't see exactly what it is — much as if I were to see a demonstration of a purported perpetual motion machine. When experiments sometimes give negative results and the authors can offer no explanation why that should happen, it doesn't require much of a leap of the imagination to suppose that the positive results are artefactual. (Recent similar episodes with which I've been somewhat familiar — polywater, cold fusion — have reinforced my belief in that strategy.)

I do agree with Collins and Pinch that Benveniste was treated quite inappropriately, and unfairly, by the editor of Nature and his debunking team. Their report (J. Maddox, J. Randi and W. W. Stewart, Nature (1988), 334, 287) does identify a number of problems with the work — unreliable replicability, inattention to statistical issues, possibly slipshod experimental protocols. But all of this could, and should, have been examined by experts, not the self-admitted "oddly constituted group" that actually made the visit. Indeed, it should have been done before Nature's grudging acceptance of the work for publication. The actual event, including such flamboyancies as taping a sealed envelope containing the encoding of samples to a ceiling (which of course implies, without actually charging it, suspicion of outright fraud), completely sensationalized the negative report. Benveniste felt he was deliberately set up by this sequence of publication followed by investigation (J. Benveniste, Nature (1988), 334, 291), and it's hard to disagree. Nonetheless, the "marginalization" of Benveniste has, in my opinion, much more to do with the contrast between the marginal character of his reported observations and the major significance of the claims he based on them.

Turning to S&B: they dismiss my analogy between scientists examining the effect of one variable at a time, and sociologists excluding the actual facts as a possible cause of belief, as "absolutely wrong." I freely concede my analogy is not perfect — what analogy is? The one they counter with, that the sociologists are doing something like investigating causes of lung cancer without asking about smoking, is at least equally flawed. On an obvious level, the latter alludes to a statistically-based study where effects of such variables can be accounted for explicitly and quantitatively: how can that be compared straightforwardly to a non-quantitative examination of a single case? But that's a trivial point.

Much more important is the point both I and Collins raised in our commentaries: the convictive force of arguments based on extrapolation from "easy" to "hard" cases is questionable. Yes, we all know lung cancer is related to smoking, just as we all know wine doesn't turn into blood. But sociologists of scientific knowledge (as I understand it) are interested in how it happens that we all come to know such things, which means they focus their studies on the times (or places, I suppose, for the wine/blood example) when we didn't all know them, and hence to treat them as known would potentially be distorting. This is obvious, as Collins notes, with respect to studies of ongoing controversies where the "truth" of the matter is still unknown. One could argue, I suppose, that such studies are premature and useless; but I don't hear anybody making that argument. Therefore, a study of a historical case done as if it were similarly unresolved should also be reasonable. At least, S&B's charge that so doing amounts to inferior methodology and/or tacit commitment to philosophical relativism seems to me quite unreasonable. If I may indulge in another (shaky) analogy, it's as if S&B's lung cancer studiers were to be criticized for failing to control for a factor that is not known to operate, say left-handedness.

On the other hand, many scientists and traditional historians of science will look at historical episodes precisely from the viewpoint S&B favor: the (currently accepted) truth of the matter, and how it came to be accepted in terms of rational reactions to empirical evidence. They are engaged in a different project. To be sure, though, these projects overlap, and here I must agree with S&B: some of the claims made by SSK reflect little of the modesty that would seem to be logically entailed by the "bracketing out the facts" approach.

No doubt a lot of this has to do with rhetoric — what Collins calls the power of words. S&B's suggestion that a simple name change could eliminate all their concerns seems to be giving a little too much power to words. Would methodological relativism carried out under the banner of the "weak programme" no longer require a commitment to philosophical relativism for logical consistency? Does ontology thus recapitulate philology?

However, this problem can go beyond rhetoric to substance. I'll close by citing a couple of examples (neither is particularly egregious, to be sure) from the cold fusion saga. In his dialog with philosopher William McKinney (Trevor Pinch, "Half a House: A Response to McKinney", Social Studies of Science (1999), 29/2, 235-40) Pinch remarks, as practitioners of SSK have always emphasized, on the difficulty of assessing epistemology and methodology in the midst of an active controversy, and illustrates with one of the claims used against Pons and Fleischmann, that their cells were not stirred properly. He criticizes McKinney for not taking into account possible responses to this and other such attacks, and states, "Pons and Fleischmann were able to use a dye tracer to show that bubbles of gas produced in the reaction stirred the cells adequately." If that is intended to be simply a demonstration that scientists under attack usually have resources to defend themselves, fine. But as written, it seems to be just the sort of assessment of the merits of a piece of experimentation that Pinch says can't be made.

As written, it's also wrong: the fact that a dye, added at a single point in time, disperses evenly throughout the cell in a short time says nothing about whether that degree of stirring is adequate to ensure that the temperature is uniform throughout the cell — the real point in question — when heat is being continuously generated in one region and lost in others. That's not to say that the stirring was necessarily inadequate (I think it was, but that's not important here), just that this dye experiment is irrelevant to the issue; and by making such a statement Pinch is acting inconsistently with his own precepts and doing exactly what he criticizes McKinney for.

The other example (also Pinch's) comes from a discussion of the role of rhetoric in resolving controversy (Trevor J. Pinch, "Rhetoric and the Cold Fusion Controversy: From the Chemists' Woodstock to the Physicists' Altamont", in Science, Reason, and Rhetoric (Eds. Henry Krips, J. E. McGuire and Trevor Melia), Pittsburgh: University of Pittsburgh Press, 1995, pp. 153-176). It's a very entertaining and interesting piece; but the conclusion includes the sentence "In the cold fusion case I claim that the outcome of the debate has to a large part been shaped by particular rhetorical performances." How can he make such a claim?

Of course, we need to do some second-order rhetorical analysis on the claim itself. I assume he does not believe that we would all now be powering our homes with little cold fusion generators if only Pons and Fleischmann had been a little more rhetorically adept. Does he merely mean that the precise course of the verbal debate would have differed in some details, which is obviously but trivially true? Or that the outcome would have been different in some substantial aspects — for example, that Congress would have given the multimillion dollar award requested to keep us ahead of the competition, or that there would be centers for cold fusion research still thriving in the US? I can't say that's impossible (although I don't believe it for an instant), but to make the claim implies a judgment of the relative importance of the causative forces at work, and if some of those are not to be examined, on what basis can one judge?

These are minor examples, but I think they are representative, and support S&B's complaint: if science studiers want to insist on agnosticism with regard to the merits of scientists' truth claims, they ought to be more circumspect about some of their own more sweeping claims, when the latter imply some elements of evaluation of the very issues they've said they can't evaluate. As a certain philosopher not entirely unknown to the field once remarked, "Whereof one cannot speak, thereof one must be silent."