COMMENT

• *ABSTRACT*

Recent work in sociological/cultural studies of science (SCS) is characterized by the virtually total absence of any participation by practising scientists. This interdisciplinary barrier appears to be largely a consequence of the relativist approach to the study of science. In addition to having philosophical objections, scientists may reasonably ask whether an approach that effectively renders their interests irrelevant is strategically sound. If there are significant areas of opportunity where the methods and concepts of SCS could have a positive impact on the practice of science, collaboration between scientist and SCSer might well be preferred to the treatment of scientists as non-participatory laboratory specimens.

Science as Culture: A View from the Petri Dish

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Motives and Apologies

> Hibbard: He seemed amused, in his dark soul, and unconcerned.
> Wolfe: Dark soul is an odd phrase for a psychologist?
> Hibbard: I read poetry week-ends.[1]

As a practising scientist with significant interests outside of science, I have become increasingly dissatisfied with the phenomenon of compartmentalization that seems almost naturally to follow. Science is the job; everything else is for weekends; never the twain shall meet. Topics such as history and philosophy of science, which do to a certain extent straddle the great divide, rarely have much to offer that affects the *day-to-day practice* of science. In the last couple of years (although they have been going

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strong for at least two decades) I have become aware of programmes, variously described as 'sociology of scientific knowledge' or 'cultural studies of science', that tend to focus much more on actual scientific practice, as well as on the way science fits into general concepts and concerns of society. Such work appeared to offer the possibility of making a direct impact on my scientific activities, and opportunities for projects that could reunite my divided interests. For convenience I will use the abbreviation 'SCS' (for Social and Cultural studies of Science) to refer to this body of work, and 'SCSers' to refer to its practitioners.[2]

As I began delving into the field, I was struck by two observations. First, although the accounts of particular episodes in scientific practice, as well as the more sweeping conclusions drawn from them, contain many illuminating observations, challenges to facile modes of thought, novel interpretations – in short, much to validate my hopes – the bottom-line picture of how science operates almost always comes out radically different from my own interpretation. Probably that should come as no big surprise. What I *do* find surprising, to the point of dismay, is the virtually total absence of participation by practising scientists, in spite of the constant emphasis on interdisciplinarity that permeates SCS. Just to cite a few representative examples that I encountered at an early stage:

- an article entitled 'What Are Cultural Studies of Scientific Knowledge?' enumerates practitioners from a variety of disciplines – history, sociology, anthropology, philosophy, literary theory – but no scientists;[3]
- a collection of essays presented at a conference intended to 'begin to find ways toward a more direct engagement of literary and scientific perspectives on the very complex problems of "realism" and "representation"',[4] featured participation by professors of English, philosophy, history and humanities; but none of the scientists who presumably were to have been engaged;
- a monograph that advocates SCS for its potential to overcome disciplinary boundaries (and that devotes considerable attention to topics such as the relationships between science and politics, that might be thought to appeal rather directly to scientists' interests) goes to the trouble of identifying, for each segment of its potential readership, the parts of the book on which they should concentrate. Recommendations are given for philosophers of science; all

other philosophers; social scientists; humanists; politicos and policymakers.[5] That a scientist might be a reader must be too improbable for consideration.

In what follows I will argue that this state of non-intercourse originates at least in part from fundamental differences in viewpoint; that unswerving insistence upon one's professional viewpoint will tend strongly to maintain this particular interdisciplinary barrier; that there are still opportunities for a substantial and important common agenda; that the optimal mode for pursuing that agenda is collaborative; and that projects aimed at lowering, if not levelling, the SCS–science barrier should begin to be explored.

I should acknowledge that this is intended not as a piece of scholarly work, but as a polemic, meant to describe the reaction of an interested scientist provoked by a relatively shallow immersion in SCS. I recognize the potential danger of basing arguments on passages that may not accurately represent the position of even a single SCSer, especially when each position may well have evolved over time, and certainly cannot stand for the widely divergent range of positions that make up the entire field. I have tried to focus as best I can on points that seem to be reasonably common to the most visible SCS adherents, but I'm sure I will be charged with distortions. In any case, my main goal is to encourage dialogue; I hope that responses will not be limited to the details that I have got wrong, but will address the more general issues that I have tried to raise.

The Scientific Bestiary

> Guinea pigs do not read books. Biologists do. . . . They have ideas about science in society and society in science. . . . The social scientist, peering thoughtfully into the biology department, finds its inhabitants peering thoughtfully back.[6]

Obviously scientists must play the role of specimen in SCS inquiries. Is it obvious that they can play no other role? Let's examine an instructive case. Collins gives a detailed account of an attempt to reproduce the construction of a novel type of laser, during which he and the scientist actually collaborated in the *physical* labour.[7] When it comes time to analyze what has happened, though, it's all Collins. After presenting his interpretation, he comments that 'scientists are resistant to the sort of account of experimentation that I have just given'.[8] This choice of phrase calls to mind a microbiologist, noting that the bacteria are resistant to the antibiotic that he has just applied. (Whence, obviously, the title of this Comment.) Why isn't that resistance cause for concern that his account might just possibly be incomplete, or misleading, or distorted in some sense?

Collins' book focuses extensively on the nature of replication; the lesson *I* take home from this episode emphasizes one of the (if not *the*) major purposes of attempts at replication: since no two experiments can ever be completely identical in all aspects, we need to know which aspects are essential and which don't much matter. That's what Collins' scientist colleague is after, from all the comments that are quoted; but Collins downplays it almost to the point of invisibility, concentrating instead on replication as a matter of demonstration of competence, pursuit of personal agendas, and so on. This is not in any way to say that Collins' account is not insightful and important, but it is significantly out of balance. Later in the book,[9] Collins discusses the 'sorting problem', which includes (but is not limited to) the activity of determining whether an experiment is a 'competent copy' of the original, and announces that there *is no such problem* in this laser case. In the sense that a competent copy is one that lases, which can be determined unproblematically, that statement may be true; but in an important sense from the scientist's perspective, it isn't true at all.

If SCSers are not to learn from scientists (except in the sense that an experimenter learns from his specimens), should scientists at least learn from SCSers? Apparently not that either, according to Collins. Strict compartmentalization is the (unchanging?) order of the day. 'Science – the study of an apparently external world – is constituted by not doing the sort of thing that the sociology of scientific knowledge does to science; the point cannot be made too strongly'.[10] Elsewhere in Collins' *oeuvre* we find similar statements: 'Natural scientists, working at the bench, should be naive realists – that is what will get the work done'.[11] And: 'There is a sense in which the social view of science is useless to scientists – it can only weaken the driving force of the determination to discover'.[12] We've already seen guinea-pig-scientist and bacterium-scientist; these statements describe centipede-scientist, who is unable to walk if he thinks about how he does it! Surely any programme or philosophy that instructs a significant portion of its potential audience to ignore it must thereby become at least a bit suspect?[13]

Of course, all these quotes are from a single SCSer (and his co-authors), and not everyone agrees with him. For example, two responses to 'Epistemological Chicken' explicitly deny that scientists are or should be naive realists[14] (although Latour elsewhere states that '[for working scientists] black boxes cannot and should not be reopened',[15] which has the flavour of a 'scientists should be naive realists' stance; see below). Nevertheless, can anyone point to an example of an interaction between SCSer and scientist that transcends the experimenter–subject relationship in any significant way?

Is Anyone Listening?

> 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.[16]

What do scientists think about all this? Hardly anything at all, to go by available writings: it seems that scientists are more than willing to follow Collins' prescription of ignorance of the workings of SCS. Two recent books written by scientists (physicist Steven Weinberg, [17] biologist Lewis Wolpert [18]) primarily for the general public, that do take note of the SCS phenomenon, were reviewed by SCSer Steve Fuller,[19] who identifies several common themes. First is the idea, particularly in Wolpert's book (as the title implies), that scientists are fundamentally *different* from everyone else.[20] I have no hesitation in joining Fuller's flat rejection of that thesis. Perhaps there is some merit in contrasting scientific modes of reasoning to some loosely-defined concept of 'common sense'; but do they differ from, say, those of a non-scientist engaged in scholarly work? Not that I can see. I think the late biologist Lewis Thomas had the right idea here:

> I must try to show that there is in fact a solid middle ground to stand on, a shared common earth beneath the feet of all the humanists and all the scientists, a single underlying view of the world that drives all scholars, whatever their discipline – whether history or structuralist criticism or linguistics or quantum chromodynamics or astrophysics or molecular genetics. There is, I think, such a shared view of the world. It is called *bewilderment*.[21]

Surely if SCS has demonstrated anything conclusively, it is that scientists are *not* fundamentally different from anyone else in any social, cognitive, rhetorical or other sense. Collins again: 'Close description of the human activity makes science look like any other kind of practical work . . . . This makes science one with our cultural endeavors'.[22] I shall make use of this argument in the next section.

A second common theme is the perceived hostility of SCS towards science. No doubt there is some, but it is hard to believe that SCSers would go to the trouble of their detailed studies just in order to denigrate their subjects.[23] In any case, a discussion of motives seems rather fruitless. Instead, I will focus on a third (and to me central) theme: the role of relativism in SCS. Or, as Fuller (somewhat condescendingly?) puts it: 'Among the more amusing yet instructive moments in the books under review are the occasions when the authors speculate on why science studies practitioners reject the epistemic uniqueness of science'.[24]

It is this aspect, it seems to me, that is most responsible for the as yet insurmountable barrier between SCS and science. Weinberg explicitly dismisses any potential use to scientists of philosophy of science (see the quote that heads this section, and also gives us bird-scientist for our bestiary); but even so, one can detect a sort of closet admiration for SCS. Discussing Sharon Traweek's studies of physicists, he notes:

> This kind of big science is a natural topic for anthropologists and sociologists, because scientists belong to an anarchic tradition that prizes individual initiative, and yet they find in today's experiments that they have to work together in teams of hundreds. As a theorist I have not worked in such a team, but many of her observations seem to me to have the ring of truth.[25]

From there it doesn't appear much of a leap to conclude that SCS *could* have much to offer to the practice of science.[26] Weinberg's problem, and I would imagine most practising scientists would agree completely, is that he cannot accept what follows:

> It seems to have been an easy step from these *useful* historical and sociological observations to the radical position that the *content* of the scientific theories that become accepted is what it is because of the social and historical setting in which the theories are negotiated.[27] (Emphasis added)

Again, I recognize that this may not be an accurate statement of all (any?) SCSers' positions, but some general version of it appears to be widely held. So why do scientists find it so unacceptable? Typical arguments would be based on the claim that 'Science works!' which apparently does not much impress many SCSers; let me try a different approach.

A (Not too Naive?) Case for Realism

> The logician goes pathetically through the motions that have always worked the spell before, but somehow the monster, Reality, has missed the point and advances still.[28]

What role does Nature (or reality, or whatever you want to call it) play in determining the content of scientific knowledge? The following quote (from a nonbeliever) represents what I take to be a fairly moderate version of the SCS position:

> The deep point of the sociological critique is that the social forces that operate in this modification of practice – the rules for consensus shaping, the conversations with peers, the training process and broader socialization within a larger community – may be sufficiently powerful that the effects of nature are negligible.[29]

What could be the function of such a position in the SCS programme? Any or all of the following: (1) as a methodological prescription to be followed; (2) as an empirical conclusion that follows from the studies; or (3) as dogma. Fuller insists on (1) and against (3):

> As Bloor and his followers employ the term, 'relativism' is not an unconditional epistemological doctrine on a par with, say, rationalism or realism. Rather, it is a methodological heuristic designed to *counteract* the science studies practitioner's own standing prejudices.[30]

I have no quarrel with that; it's perfectly sound scientific practice to exclude one factor from consideration in order to focus on the effects of another. In doing so, of course, one must not forget that the subject of study is now an approximate model, and that the excluded factors may well turn out to be at least as important as the ones being examined. If the role of relativism in SCS goes beyond (1) to (2) – as it certainly does for at least a significant fraction of SCSers – then we need to consider how the conclusion is empirically justified.

Note that the conclusion is the same as the starting assumption; that is not invalidating by any means, but it does call for caution. It is clearly not sufficient only to show that social factors *are* important, to be able to conclude that Nature is not. What if we take the opposite approach, in good Popperian fashion, and try to falsify the assumption? In this regard, many SCSers have recognized that a key question needs to be faced. It has been stated and answered in a variety of forms; here are two:

> Why doesn't scientific culture continually disintegrate as scientific actors develop it in the myriad different ways that are conceivable in principle? . . . On the one hand, actors can be seen as tentatively seeking to extend culture in ways that serve their interests . . . and . . . interests serve as standards against which the products of such extensions, new conceptual nets, can be assessed . . . scientific knowledge has to be seen, not as the transparent representation of nature, but rather knowledge relative to a particular culture.[31]

> If there is less persistence among fashionable literary theories than among fashionable chemical theories, that is a matter of sociology. It is not a sign that chemistry has a better method, nor that it is nearer the truth.[32]

These seem to constitute an acknowledgment that scientific culture is (unexpectedly?) uniform, and attempt to explain that in terms of social factors. The problem is, why doesn't that apply to *any* culture? Why, according to this view, should philosophy, or literary criticism, or sociology, or any field be more fragmented or multivocal than science? Why do scientific debates eventually reach closure, while debates *about* science (see, for example, the artificial debate constructed by Laudan[33]) can go on indefinitely without converging on anything but an agreement to disagree? The social explanation offered above appears to require that scientists are somehow *different* from practitioners of other fields, more ready to recognize and conform to common interests (sheep-scientist?). That argument was rejected in the preceding section. At a minimum, it reopens the question whether rejection of the 'obvious' explanation – that scientific culture is more uniform just because it *is* constrained by Nature – might have at least something to do with dogma.

Flipping one's Whig

> 'Next we'll try Hypothesis Contrary to Fact. . . . Listen: If Madame Curie had not happened to leave a photographic plate in a drawer with a chunk of pitchblende, the world today would not know about radium'.

'True, true,' said Polly, nodding her head. 'Did you see the movie? Oh, it just knocked me out. That Walter Pidgeon is so dreamy. I mean he fractures me'.[34]

Turning to the other side of the argument, Fuller responds to Wolpert's demand for specific examples that demonstrate the social construction of knowledge as follows:

It is not merely the logical possibility of an alternative science radically disjoint from our own; rather, what compels is that contemporary science is *merely* the product of following one of several paths that had been equally open at an earlier point in our own history.[35] (Emphasis added)

He cites Shapin and Schaffer's study of Boyle *vs* Hobbes as a prime example.[36] Similarly Hagendijk, on the same study: 'Modern science would not exist as we now know it if these people in the seventeenth and early eighteenth centuries had not established this particular way of handling these distinctions'.[37]

I understand these quotes to say that it isn't just *conceivable* that an altered outcome back then could have led to a contemporary state with significantly different beliefs and practices; rather, it's necessarily so. I have a lot of trouble finding anything compelling in such an argument, even if we give full credence to Shapin and Schaffer's account. (A sketch of an alternative that puts much more weight on experimental and 'rational' than social determinants,[38] appears at least as convincing to me; of course I'm prejudiced.) A careful and enlightening study of the forces that led to an observed outcome is *not* a demonstration that certain changes in the forces would lead to a particular altered outcome; still less so, that such an altered outcome would have consequences that persist through subsequent history.

Shapin and Schaffer are much concerned about 'Whig history' – interpreting what happened then in terms of what we know now – which they feel permeates 'classical history of science'.[39] In avoiding that Scylla, must one set a straight course for the antipodal Charybdis – that what happened then uniquely determines what we know now? Maybe not *all* roads lead to Rome, but the demonstration of an earlier fork is hardly proof that where we have ended up is 'merely' a historical accident. There is a revealing passage in *Leviathan and the Air-Pump*, where the authors quote a military historian, and compare history of science to history of battle:

The 'von Moltkes' of the history of science have shown similar disinclinations to

> engage with actual scientific practice, preferring idealizations and simplifications to messy contingencies, speech of essences to the identification of conventions, references to unproblematic facts of nature and transcendent criteria of scientific method to the historical work done by real scientific actors.[40]

That's fair enough, and I'm all for engaging with actual scientific practice; but let's extend the analogy a bit further. A battle is won or lost according to all the 'messy contingencies' and the rest; we can't come back to it a few years later, maybe with a new weapon that would have greatly benefited the losing side, and reverse the outcome. It's quite different for scientific controversies, which are often reopened, refought with new and improved 'weapons', and reversed. To suggest that, say, if Napoleon had had tanks at Waterloo, contemporary Europe might look very different, may be reasonable and even convincing. To suggest that if Hobbes had been more adept at enlisting allies and won the debate with Boyle, we might not have vacuum cleaners today, is considerably less so.

Let's try a different analogy. In accounting for whether or not a given reaction takes place and why, chemists distinguish between equilibrium and kinetics. The former refers to the thermodynamic *state* of a system, which is determined once the nature and amounts of reactants are specified, along with temperature and pressure. It does not depend on the *path* that is followed from reactants to products. Kinetics, in contrast, *are* path-dependent and much more subject to control. It is often possible, given a reaction that is thermodynamically allowed but kinetically slow, to accelerate the reaction by means of a catalyst. There is nothing that can be done, in contrast, to cause a thermodynamically forbidden reaction to take place. I suggest that we can make a similar distinction in scientific knowledge, and that SCS tells us much about paths but little about states.

Of course, my analogy has an obvious shortcoming. In chemistry there are a variety of methods for determining or estimating the thermodynamics of hypothetical reactions, so there is usually not much difficulty in deciding whether a reaction is unobserved because of kinetics or thermodynamics. In the resolution of scientific debates, we have no *independent* method of deciding whether the answer that has eventually been reached is the right one. All I can say is that there is at least one empirical argument (see above) that our state of knowledge is in fact constrained by Nature. The argument that we should reject that evidence in favour of an unconstrained, fully constructed picture is less than convincing.

Madness and/or Method?

Research, a poor parallel parker, needs several passes.[41]

If it is necessary to debunk an idealized model of science – fully rational, fully methodical, homogeneous across disciplinary boundaries and history, proceeding via a direct, shortest-possible route from observation to hypothesis to experimental test to acceptance – then there is no question but that SCS has thoroughly done so. I doubt whether many scientists would have found that model an accurate representation of their own practice anyway. However, at least some SCSers seem not satisfied with shattering the idol; they insist on sweeping up all the fragments and throwing them out. If there is no absolute, constant over time, guaranteed reliable scientific method, they argue, then there is no method at all. Underdetermination and the Duhem-Quine thesis always seem to show up somewhere around here.[42] Kitcher has described a typical scientist's response, paraphrasing Gould:

> Scientists, however, sometimes greet this allegedly mundane point with incredulity. 'It's hard enough', they complain, 'to find *one* way of accommodating experience, let alone many. And these supposed ways of modifying the network of beliefs are changes that no reasonable – sane? – person would make. There may be a *logical* point here, but it has little to do with science'.[43]

There appears to be, among SCSers, a general tendency to underestimate seriously the extent to which scientific progress builds upon the existing body of knowledge. This is manifested, *inter alia*, by an *over*emphasis on distinctions and dichotomies: focusing on individual sciences rather than science as a whole; contrasting normal *vs* revolutionary science, frontier *vs* textbook science, discovery *vs* justification, and so on. All of these have some validity and use, of course; but by making too much of them it is easy to distort the overall picture. For example:

> The core consists of a *small* set of theories, analytic techniques, and facts which represent the given at any particular point in time . . . . The research frontier is where all new knowledge is produced . . . the social character of knowledge in these two components differs dramatically . . . . If we look only at core

> knowledge and at what scientists say about core knowledge, we will conclude that science is adequately described by the traditional view. If we look at frontier knowledge, however, we will find little confirmation for much of the traditional view.[44] (Emphasis added)

The key word here, obviously, is 'small'. If there is in fact only a small core of accepted knowledge, then it is easy to conclude that interpretation of frontier work is relatively unconstrained, and hence that frontier beliefs have primarily or even exclusively social origins. On the other extreme:

> Once we get over the distortion of perspective caused by being so close in time to so much new scientific knowledge, we can see that much of it is already essentially complete. By this I mean that the experimental and theoretical basis of some of our fundamental knowledge is so extensive that there is little likelihood of its being changed to any significant degree. This is an astonishing assertion, given the breathtaking pace of discovery today. But the pace of discovery is possible precisely because our fundamental knowledge is so complete.[45]

Even without completely subscribing to Cromer's 'astonishing assertion', I am quite sure that he has the relative proportions of core and frontier much more correctly than does Cole.

Another example of a problematic dichotomy between textbook and frontier science may be found in Collins and Pinch's discussion of Eddington's solar eclipse observations as a decisive confirmation of Einstein's theory:

> While all this was going on, there were still other tests of relativity that had the same mutually reinforcing relationship to these tests as they had to each other . . . . No test viewed on its own was decisive or clear cut, but taken together they acted as an overwhelming movement. . . . We have no reason to think that relativity is anything but the truth . . . but it is a truth which came into being as a result of decisions about how we should live our scientific lives, and how we should licence our scientific observations; it was a truth brought about by agreement to agree about new things. It was not a truth forced on us by the inexorable logic of a set of crucial experiments.[46]

Later in the book, they sum up:

> Why [scientific] debates are unresolvable, in spite of all this expertise, is what we have tried to show in the descriptive chapters of this book. That is, we have shown that scientists at the research front cannot settle their disagreements through better experimentation, more knowledge, more advanced theories, or clearer thinking.[47]

Unless we take a very restrictive definition of 'at the research front',[48] it seems to me that these passages are somewhat contradictory. The debate over general relativity *was* eventually settled by means of better experimentation, more knowledge and the like, even if the road to the early consensus might legitimately be considered an unjustified short cut.

The metaphor that heads this section seems to me very appropriate: nobody is too surprised when a driver misses on the first try or two at parallel parking; but nobody denies the existence of a method on that account. As Hacking comments:

> The constructionalists . . . study the first shift of the factory of facts. Quitting work early in the day, they leave us in the lurch with a feeling of absolute contingency. They give little sense of what holds the constructions together beyond the networks of the moment, abetted by human complacency.[49]

Nickles has aptly described scientific work as 'multi-pass'.[50] Discovery and justification are interwoven as it proceeds, following a tortuous path full of backtrackings and short cuts through an uncertain terrain. SCS may be able to help understand much about how we traverse that terrain, but little about how the terrain itself is shaped.

Setting an Agenda

> So what should the relation between sociology of science and science itself be?[51]

> [A man] fell among thieves, who beat him and robbed him and left him bleeding and unconscious in the gutter. And along came two sociologists who looked down upon him lying there and said, the one to the other, 'The man who did this needs our help'.[52]

Let's return to my original question: why is there no significant level of interaction between SCS and science? Some SCSers seem to feel that scientists *shouldn't* concern themselves with SCS, as seen earlier. Others might well place the responsibility on the scientists' side; there may be *nothing* they could do, up to and including issuing Green Stamps with every article, that would get scientists to pay attention. This situation might be acceptable if SCSers viewed their efforts as of purely academic interest, intended only for each other, with essentially no connection between their agenda and that of scientists; but I take it for granted that no SCSer would subscribe to that view. Fuller explicitly points out its inadequacy:

> Contrary to nineteenth-century hopes, the judgments of critics typically do not feedback into the creation of better art or even better publics for the reception of art. What is produced, instead, is a self-sustaining body of scholarly literature. Any positive impact of critics on the course of art in this century has been fortuitous, much like the impact of philosophy on the course of science today.[53]

Fuller implies that SCS, in contrast to philosophy, need not be (is not?) a sterile exercise of a self-contained group of scholars. Is he right?

Let's first address an SCS argument *against* collaboration:

> We provide a prescription: stand on social things – be social realists – in order to explain natural things. The world is an agonistic field (to borrow a phrase from Latour); others will be standing on natural things to explain social things. That is all there is to it . . . [SSK] wants to use science to weaken natural science in its relationship to social science . . . we want all cultural endeavors to be seen as equal in their scientific potential . . . . What we do want to make different . . . is what happens when natural science comes into contact with other parts of the world . . . if natural things are to be given a role in analysts' explanations, if the culture of science is to enter the analysis of science . . . then it is scientists who must be given the principal word in these areas.[54]

At least this appears to admit the possibility that both natural and social explanations may be important, but how is one to assess their *relative* importance? This position allows only for debates, not cooperation, between the two sides. Furthermore, the reason suggested for separation – that in a joint effort the scientific side must inevitably dominate – doesn't exactly proclaim a high level of self-confidence. That's no way to win a game of epistemological chicken!

Here is a different perspective:

> It can be argued that academic work proceeds best on the basis of sectarian organization, blinkered vision, and intense cultural and cognitive differentiation . . . . The alternative view is that the temptations inherent in the intellectual division of labor are best avoided. In the context of the social sciences, this would imply a continuing awareness that our contribution to the understanding of knowledge and its distribution is necessarily partial and limited, and a readiness to learn from and to incorporate the research of other fields, even if this involves our being far more sympathetic to them than they are to us.[55]

This clearly calls for interdisciplinary cooperation, although it isn't clear whether it would go so far as to include scientists. Bauer similarly argues for an interdisciplinary SCS programme, while pointing out the difficulties of launching it.[56] What might a joint scientist–SCSer agenda consist of? One can conceive of projects aimed at improving the *practice* of science as well as the *management* of science, on both micro and macro levels.

Science management issues are rather obvious. Many scientists as well as SCSers recognize that scientists cannot be allowed the only voice in decisions concerning allocation of limited resources between scientific and non-scientific programmes, among scientific fields, between big and little science, fundamental and applied research programmes; nor should they have the privileged voice in policy debates with high scientific content, such as global warming, ozone depletion, resource management, and so on. Again, though, most of the emphasis in SCS seems to be on engaging debates; the resulting impression is of an ideal model in which scientists, politicians and concerned laymen would all argue their positions, while SCSers would be the final arbiters.[57] High on my list of joint SCS–science projects would be an examination of possible strategies for dealing with such issues.

Management on the micro level – decisions by individual companies and laboratories on how to exploit their scientific resources to best advantage – could also use some help, if my experience in industrial research is any indication. How are negotiations between scientific and economic expertises and interests carried out and settled? What role is played by the unexamined assumptions that are inevitably part of the local culture? These and related issues could potentially benefit from cooperative attention.

What about scientific practice? On the level of individual practice, we have already seen arguments that awareness of SCS would be at best irrelevant and perhaps even harmful to the scientist in his daily work. Collins again: 'We can see all our scientific mistakes when we look backwards, and we can see how to solve them but this does not help with today's science'.[58] I find this stance rather surprising. If scientific practice is truly fraught with unexamined conventions, unproven assumptions and unopened black boxes, why shouldn't it be advantageous to focus some attention thereupon? Returning to my earlier analogy, might there be opportunities for SCS to play a catalytic role in scientific progress?

Let me illustrate with a possible example from my own field (inorganic chemistry): the use of X-ray crystallography for the determination of molecular structure. This technique has become a black box *par excellence*; nominal results straight from the computer are almost invariably taken as gospel. Not a small number of incorrect papers has been published as a consequence. Recently there was a modest-sized controversy about a phenomenon called 'bond-stretch isomerism', which occasioned a significant level of both experimentation and novel theoretical explanations, before it was shown to be an artefact of taking crystallographic results too literally.[59] The need to differentiate between immediate observables (squiggles on an oscilloscope, counts from a detector, and the like) and the results deduced from them has been a common topic in SCS literature. Scientists certainly understand the difference, but perhaps they could benefit from occasional reminders.

This point applies to non-scientists as well: in a collection of essays that address some of these issues, the editor (not a scientist) interpolates her own comment on DNA structure: 'In 1989 new technology enabled scientists actually to "see" a DNA molecule for the first time, which confirmed once and for all Watson and Crick's 1953 hypothesis'.[60] The new technology referred to is Scanning Tunneling Microscopy (STM), about which Hacking commented: 'a type of device barely out of the research stage becomes a black box that the next generation will use as a stable laboratory tool'.[61] Obviously, it didn't take anything like a generation for some! Someone who is sufficiently alert to put quotation marks around the word 'see' should recognize that a phrase like 'confirmed once and for all' is at least equally problematic.[62]

As for scientific practice on the macro scale, we might consider episodes such as the recent cold fusion business. Several SCS accounts have already appeared.[63] One commentator noted:

> I think that from everything we have learned about these controversies in science – controversies which exhibit an extraordinary regularity in pattern over the years – we know enough to say to scientists that this is 'science as usual'. And that scientists should be more adept at dealing with these kinds of disputes. And that is my disappointment with the cold-fusion episode. Despite all our work and understanding of controversies, what has our input been? Zilch. Our message is clearly not getting through, and that is the most depressing thing of all.[64]

This passage is important: it highlights many of my objections to the current state of SCS. First, cold fusion is 'science as usual' only in the sense that there have been a number of such controversies – in the same way, I suppose, as Watergate was 'government as usual' and World War II was 'international relations as usual'. I don't see that simply making that mundane observation, or stating that scientists should be 'more adept at dealing with these kinds of disputes' without in any way indicating how, is particularly helpful. I wholly agree that better methods of handling such disputes are sorely needed – consider the amount of money wasted! – but SCSers cannot provide them on their own, as they don't have sufficient *detailed* understanding of the scientific issues involved. Up to now they have been trying to work without it, and that, I would argue, is the main reason why their input has been 'zilch'.

How can we get significant joint projects underway? First, there has to be recognition of common interests; as outlined above, I don't think that should be so difficult. Next, each side has to be convinced of the potential value of collaboration. There is no question that many scientists are and will continue to be hard to convince on this score, but I think that could improve if and when SCSers move in that direction and away from some of the stronger forms of the positions I have criticized above. SCSers *do* seem to recognize the need to enlist scientists, even if it is couched more in terms of cooption than cooperation:

> . . . the scientist whose practices the social epistemologist criticizes have to be made not only part of the problem but part of the solution as well . . . . If scientists have been so deeply misled about the nature of knowledge production and their own role in it, how can this fact be conveyed to them in a manner that is likely to make them want to cooperate with the social epistemologist to improve the enterprise?[65]

Without getting back into the question of just who is more or less misled, I would suggest that trying to convince scientists to do something based on the premise that they are all wrong is not likely to be very successful, and that if a joint agenda is deemed desirable there may be better strategies for going after it. Surely a programme that emphasizes the role of negotiation in settling questions ought to recognize that! SCSers may well feel misunderstood by those who attack them as dogmatic rather than methodological relativists; but it would be hard to deny that many of the writings quoted here seem crafted to provoke just such a response. Consider a scientist who begins looking into SCS and comes across, at an early stage, a statement such as 'The natural world in no way constrains what is believed to be'.[66] Will that be an inducement to read further and try to understand all the subtle implications, or to give up the whole thing as a waste of time? One might hope that a rational being would have the first reaction, but I guess that the second is much more likely for the socially driven, strictly human being that SCS tells us a scientist is.

Lastly, I should note some hopeful signs in more recent literature: Latour's call for giving natural explanations weight alongside the social;[67] Knorr-Cetina's acknowledgment that the constructivist position may soon (may have?) run its course;[68] and the various expressions of interest, however vague, in a joint agenda, some of which have been mentioned earlier. Perhaps we may yet see, in the not-too-distant future, an SCSer whose first instinct on encountering a scientist is to propose a collaboration, not to whip out a specimen kit.

• *NOTES*

I thank Stephen Weininger for encouragement and valuable comments, as well as for suggesting the abbreviation 'SCS'. (I take full blame for 'SCSer'.) I also thank Harry Collins, Andrea Labinger, Richard Powers, Trevor Pinch and the Editor for helpful suggestions on revising an earlier draft.

1. Rex Stout, *The League of Frightened Men* (New York: Pyramid, 1963), 12.

2. Nomenclature *is* a bit of problem here. 'Science studies' is commonly used and is perfectly adequate for the discipline; but what are we to call its adherents? Obviously not 'science students', while 'science studies practitioners' and variants that have been employed seem rather cumbersome. Abbreviations previously employed such as 'STS' or 'SSK' may imply a focus of attention on particular subsets, which I hope to avoid by use of the new shorthand SCS. This then leads directly to 'SCSers', which I propose as a compact and convenient designation. I will continue to use 'scientists' to refer to the objects of their studies; anyone who senses a pejorative asymmetry therein may mentally substitute 'SCSees'.

3. Joseph Rouse, 'What Are Cultural Studies of Scientific Knowledge?', *Configurations*, Vol. 1 (1993), 1–22.

4. George Levine, 'Looking for the Real: Epistemology in Science and Culture', in Levine (ed.), *Realism and Representation* (Madison, WI: University of Wisconsin Press, 1993), 3–23, quote at 7.

5. Steve Fuller, *Philosophy, Rhetoric and the End of Knowledge* (Madison, WI: University of Wisconsin Press, 1993), xxii.

6. Martin Hollis, 'Social Thought and Social Action', in Ernan McMullin (ed.),

The Social Dimensions of Science (Notre Dame, IN: University of Notre Dame Press, 1992), 68–84, at 68.

7. H.M. Collins, *Changing Order* (Chicago, IL: The University of Chicago Press, 1992), 51–78.

8. Ibid., 74.

9. Ibid., 155–56.

10. Ibid.,188.

11. H.M. Collins and Steven Yearley, 'Epistemological Chicken', in Andrew Pickering (ed.), *Science as Practice and Culture* (Chicago, IL: The University of Chicago Press, 1992), 301–26, at 308.

12. Harry Collins and Trevor Pinch, *The Golem: What Everyone Should Know About Science* (Cambridge: Cambridge University Press, 1993), 143.

13. Perhaps responsible advocates should self-administer what I will call the McGarrigle test (David Lodge, *Small World* [New York: Warner, 1984], 362): what would be the consequences if *everybody* followed your recommendations?

14. Steve Woolgar, 'Some Remarks About Positionism: A Reply to Collins and Yearley', in Pickering (ed.), op. cit. note 11, 327–42; Michel Callon and Bruno Latour, 'Don't Throw the Baby out with the Bath School!', in ibid., 343–68.

15. Bruno Latour, *Science in Action* (Cambridge, MA: Harvard University Press, 1987), 4.

16. Steven Weinberg, 'Newtonianism, Reductionism and the Art of Congressional Testimony', *Nature* Vol. 330 (3 December 1987), 433–37, at 433.

17. Steven Weinberg, *Dreams of a Final Theory: The Search for the Fundamental Laws of Nature* (New York: Pantheon, 1992).

18. Lewis Wolpert, *The Unnatural Nature of Science: Why Science Does Not Make (Common) Sense* (London: Faber & Faber, 1992).

19. Steve Fuller, 'Can Science Studies be Spoken in a Civil Tongue?', *Social Studies of Science*, Vol. 24 (1994), 143–68.

20. A similar theme is sounded in Alan Cromer, *Uncommon Sense: The Heretical Nature of Science* (Oxford: Oxford University Press, 1993). This book does not pay much attention to SCS, except to note that (xi): 'Scientists are usually suspicious of sociological analyses of their work, since some sociologists have used them to devalue science'.

21. Lewis Thomas, *Late Night Thoughts on Listening to Mahler's Ninth Symphony* (New York: Viking Press, 1983), 157.

22. Collins & Yearley, op. cit. note 11, 308.

23. For a vehemently contrary view, by no means limited to SCS, see Paul R. Gross and Norman Levitt, *Higher Superstition: The Academic Left and Its Quarrels with Science* (Baltimore, MD: The Johns Hopkins University Press, 1994).

24. Fuller, op. cit. note 19, 150.

25. Weinberg, op. cit. note 17, 185.

26. In fairness I should note here that if this were the sole context in which SCS should influence science, then the positions of Collins and his colleagues that I criticized earlier appear more defensible, as their prescription of ignorance of SCS was specified to be for the scientist *at the bench*. One could argue that group dynamics and the like, while unquestionably part of scientific practice, are distinguishable from lab bench activities. I would not accept that argument, though: it takes us right back to the 'split brain' mode of existence that got me

started on this in the first place. In any case, I will offer additional contexts for interaction later on.

27. Weinberg, op. cit. note 17, 186.

28. Michael Scriven, quoted in Martin Gardner, *The Unexpected Hanging and other Mathematical Diversions* (Chicago, IL: The University of Chicago Press, 1991), 14.

29. Philip Kitcher, *The Advancement of Science: Science Without Legend, Objectivity Without Illusions* (Oxford: Oxford University Press, 1993), 162.

30. Fuller, op. cit. note 19, 155, emphasis in original.

31. Andrew Pickering, 'From Science as Knowledge to Science as Practice', in Pickering (ed.), op. cit. note 11, 1–26, at 4–5.

32. Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science* (Cambridge: Cambridge University Press, 1983), 62. It should be noted that the quote is a paraphrase of someone else's views (Richard Rorty), not the author's own.

33. Larry Laudan, *Science and Relativism: Some Key Controversies in the Philosophy of Science* (Chicago, IL: The University of Chicago Press, 1990).

34. Max Shulman, 'Love is a Fallacy', in *The Many Loves of Dobie Gillis* (Garden City, NY: Garden City Press, 1953), 47.

35. Fuller, op. cit. note 19, 147.

36. Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton, NJ: Princeton University Press, 1985).

37. Rob Hagendijk, 'Structuration Theory, Constructivism, and Scientific Change', in Susan E. Cozzens and Thomas F. Gieryn (eds), *Theories of Science in Society* (Bloomington, IN: Indiana University Press, 1990), 43–66, at 57.

38. Kitcher, op. cit. note 29, 294–97.

39. Shapin & Schaffer, op. cit. note 36, 8–9.

40. Ibid., 16–17.

41. Richard Powers, *The Gold Bug Variations* (New York: Morrow, 1991), 91.

42. See, for example, Steven Shapin, 'History of Science and its Sociological Reconstructions', *History of Science*, Vol. 20 (1982), 157–211, esp. 157–64.

43. Kitcher, op. cit. note 29, 247.

44. Stephen Cole, *Making Science: Between Nature and Society* (Cambridge, MA: Harvard University Press, 1992), 15–16.

45. Cromer, op. cit. note 20, 6–7.

46. Collins & Pinch, op. cit. note 12, 52–54.

47. Ibid., 144.

48. One could of course define frontier science as that consisting of unresolved debates, in which case the statement reduces to tautology. Alternatively, if we allow for a role for Nature in explaining 'textbook science' but insist on social determination for frontier disputes, we are left to wonder exactly how science passes from the frontier to the textbook.

49. Ian Hacking, 'Statistical Language, Statistical Truth and Statistical Reason: The Self-Identification of a State of Scientific Reasoning', in McMullin (ed.), op. cit. note 6, 130–57, at 131.

50. Thomas Nickles, 'Good Science as Bad History: From Order of Knowing to Order of Being', in McMullin (ed.), op. cit. note 6, 85–129, at 94–95.

51. Trevor J. Pinch, 'Opening Black Boxes: Science, Technology and Society', *Social Studies of Science*, Vol. 22 (1992), 487–510, at 506.

52. Dick Francis, *Proof* (New York: Fawcett Crest, 1985), 159.

53. Fuller, op. cit. note 5, 8.

54. H.M. Collins and Steven Yearley, 'Journey into Space', in Pickering (ed.), op. cit. note 11, 369–89, at 382.

55. Barry Barnes, 'How Not to Do the Sociology of Knowledge', *Annals of Scholarship*, Vol. 8 (1991), 321–35, at 333–34.

56. Henry H. Bauer, 'Barriers Against Interdisciplinarity: Implications for Studies of Science, Technology, and Society (STS)', *Science, Technology, & Human Values*, Vol. 15 (1990), 105–19.

57. See, for example, Fuller's 'World of Tomorrow' (Fuller, op. cit. note 5, 377–82), which seems to me almost to advocate replacing one privileged group by another rather than trying to eliminate privilege altogether.

58. H.M. Collins, *Artificial Experts: Social Knowledge and Intelligent Machines* (Cambridge, MA: The MIT Press, 1990), 128.

59. Gerard Parkin, 'Do Bond-Stretch Isomers Really Exist?', *Accounts of Chemical Research*, Vol. 25 (1992) 455–60.

60. Connie Barlow, in Barlow (ed.), *From Gaia to Selfish Genes* (Cambridge, MA: The MIT Press, 1991), 204.

61. Ian Hacking, 'The Self-Vindication of the Laboratory Sciences', in Pickering, op. cit. note 11, 29–64, at 43.

62. Collins has noted the importance of such points: 'One useful thing is to work out the consequences of the inverted commas, for while they may have no impact for work on the laboratory bench, they do have significance for the relations between science and other institutions': H.M. Collins, 'Scene from Afar', *Social Studies of Science*, Vol. 24 (1994), 369–89, at 373. Again, I would only differ with his pessimism about the potential impact on actual scientific practice.

63. For example, James W. McAllister, 'Competition among Scientific Disciplines in Cold Nuclear Fusion Research', *Science in Context*, Vol. 5 (1992), 17–49; Thomas F. Gieryn, 'The Ballad of Pons and Fleischmann: Experiment and Narrative in the (Un)Making of Cold Fusion', in McMullin (ed.), op. cit. note 6, 217–43; Collins & Pinch, op. cit. note 12, 57–78.

64. Pinch, op. cit. note 51, 506.

65. Steve Fuller, 'Social Epistemology and the Research Agenda of Science Studies', in Pickering (ed.), op. cit. note 11, 390–428, at 423. Elsewhere in Fuller's writing there are passages that make one question his true interest in cooperation, such as (Fuller, op. cit. note 5, 311): 'the social epistemologist should engage in what ethnomethodologists call "participant observation" of scientific practices. In other words, she should learn to ply her trade in the presence of those whose company she is most likely to loathe'. Such sentiments might well revive the charges of hostility that we bracketed out earlier.

66. Harry Collins, as quoted in Fuller, op. cit. note 5, 323. It must be noted that the *actual* quote was 'The appropriate attitude for conducting this kind of enquiry is to assume that "the natural world . . . " ': H.M. Collins, 'Son of Seven Sexes: The Social Destruction of a Physical Phenomenon', *Social Studies of Science*, Vol. 11 (1981), 33–62, at 54. Collins' complaint that 'The minute details of what people said seems to be more important in the eyes of critics (and defenders) than what they did' (Collins, op. cit. note 62, 386–87, fn. 3) may be valid; but he should not be

surprised that dramatic statements (especially if they make nice soundbites) play a disproportionate role in how his work is perceived.

67. Bruno Latour, *We Have Never Been Modern* (Cambridge, MA: Harvard University Press, 1993). I *think* that's what he calls for, but I am not yet (and perhaps never will be) sufficiently adept at SCS to claim any deep understanding of Latour's writings.

68. Karin Knorr-Cetina, 'Strong Constructivism – from a Sociologist's Point of View: A Personal Addendum to Sismondo's Paper', *Social Studies of Science*, Vol. 23 (1993), 555–63.

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RESPONSES AND REPLIES

Cooperation and the Two Cultures: Response to Labinger

H.M. Collins

The latest round of the argument between 'the two cultures' has been characterized by childish name-calling.[1] Jay Labinger may not be entirely right in what he says, but he has demonstrated the

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proper manner to debate the issue.[2] Let us hope that it sets a new tone.

I will speak simply for my interpretation of SSK, as I find less and less sympathy for what has become called 'cultural studies of science'. Crucially, SSK, as I see it, wants to change only science's self-image and its relationship to other cultural endeavours; it harbours no ambition to change the practice or methods of science. On the whole, SSKers are fond of science, think of themselves as doing their work as scientifically as possible, and defend their work in terms of 'accuracy', 'detail', 'scholarship', 'repeatability', 'generalizability' and 'intersubjectivity' – the sort of warrants that a scientist might offer. This does not mean that SSKers are fond of all scien*tists*. Given this, SSKers are delighted to cooperate with interested scientists, so long as this does not subvert their professional practice.

For myself, cooperation has already included the work with laser scientists (documented by Labinger), and full-scale participation in a scientific field (Collins' and Pamplin's contribution to the science of 'spoon-bending' was read by many as the decisive defeat of the claims of child spoonbenders).[3] Nevertheless, it remains difficult to cooperate over *social* analysis because scientists *qua* scientists sometimes have a set of interests and beliefs which sit uneasily with SSK. Thus some scientists, and this seems to include Labinger, have a commitment to a model of reality that makes discussion difficult. Labinger actually *warns* of the damage that talk of relativism will do to the prospects of collaboration with scientists such as himself.

In anthropology, it is a professional and political obligation to respect that which one's informants hold sacred, but this seems patronizing in the case of colleagues who are members of the same academic institutions. In any case, we are talking of cooperation here, not the asymmetrical study of one group by another. In the twentieth century, the natural sciences have led the way in helping us believe strange things; most of us have learned to believe in relativity, quantum theory, multiple universes, black holes and so forth. I am surprised that it is so hard for scientists to extend their imaginative dexterity within philosophy. As it happens I do not think that the argument about ontological relativism is academically productive, but to rule out its discussion as a condition for cooperation with scientists compromises academic integrity and needlessly impoverishes the world. Consideration and application of various forms of relativism is part of the stock in trade of the social analysis of science, just as realism is the stock in trade of chemistry.

Labinger's comments about cold fusion illustrate the need for a relaxation of the scientist's view if we are to face up to the problems of SSK. Labinger just *knows* that cold fusion was a waste of money, and is irritated that the sociologists do not know enough science to see it; we have, he says, been trying to get by with too little science. But what does it mean to know too little science? I presume that Labinger also believes that Pons and Fleischmann (FRS) know too little science to understand cold fusion properly, and this puts the analyst in a quandary. Who should we cooperate with if we want to analyze cold fusion? Should we go to Labinger the chemist, to Harwell, where the scientists seemed to find nothing, or should we go to Pons and Fleischmann at Sophia Antipolis where they seem to be seeing excess heat on a regular basis? The answer must be 'all of them'. The sociologist will want to understand the Harwell/Labinger view, but to use this view alone to inform a study of the cold fusion controversy would be to 'go native' (to know too much science?), and this again would be too high a price for cooperation.

Several books have been written about cold fusion by scientists, but they have not referred to the sociological ideas that have come out of earlier studies of scientific controversy.[4] (I am thinking of ideas such as 'the experimenter's regress', the 'enchanting' effect of distance from the research front, the impact of the evidential context within which empirical claims are cast, and so forth. These show how it is that one scientific controversy is, inevitably, much like another.) Instead, these books offer worn-out clichés about the need for independent replication and the dangers of 'pathological science'. To be positive, I suggest that Labinger, with his appreciation of work in science studies, might carry out his own (or a cooperative) study of some controversy in chemistry in which he would use these (not so) new ideas. If he could make a move in that direction he would be showing how the vast, submerged, experience of scientists could be used in developing a better understanding of the way that knowledge is made. He would have begun to build a bridge between the two cultures that could bear the weight of the populations of both banks.

• *NOTES*

1. For an example, see the letters in *The Times Higher Education Supplement* (London, 21 October 1994), 15.
2. Jay A. Labinger, 'Science as Culture: A View from the Petri Dish', *Social Studies of Science*, Vol.25, No.2 (May 1995), 285–306.
3. Brian Pamplin and H.M. Collins, 'Spoon Bending: An Experimental Approach', *Nature*, Vol. 257 (4 September 1975), 8.
4. See, for example, Frank Close, *Too Hot to Handle: The Race for Cold Fusion* (London: W.H. Allen; Princeton, NJ: Princeton University Press, 1991); John R. Huizenga, *Cold Fusion: The Scientific Fiasco of the Century* (Oxford & New York: Oxford University Press, 1993).

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Responses and Replies (continued)

From Pox to Pax?: Response to Labinger

Steve Fuller

Labinger's own practice offers the best model to date for fruitful science–science studies engagements.[1] Not only is his tone right,

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but he also hits on all the right points, which I have distilled into three.

Why Am I so 'Condescending'?

What I cannot excuse I may be able to explain. First, when I wrote *Philosophy, Rhetoric, and the End of Knowledge*,[2] I had not anticipated much of a natural science audience, and there was (and is) still a battle to be won among humanists and social scientists that the cultural dimensions of the natural sciences can be made central to their own agendas. In this context, a deflationary rhetoric can help loosen up academics who, in spite of themselves, remain in the grip of physics envy. Now that natural scientists have begun to listen, I have tried to deal with them in a civil tongue.[3]

A deeper source of my 'condescension' is the irony that comes from seeing history repeat itself. It is ironic that scientists should be annoyed or perplexed when, say, Harry Collins claims to be just another toiler in the groves of academe, since Collins is doing nothing more than taking a page from science's own rhetoric of self-legitimation. Return to Germany 100 or 150 years ago, and you will find many attempts to show that free inquiry into X does not undermine X, but may instead enhance X by making X more publicly accessible. An especially striking precedent is 'critical-historical' theology, a field whose practitioners revealed the Gospels to be myths but then concluded that myths are a necessary component of religious belief – and that, in fact, the ability to keep the faith in face of demystified scriptures is the ultimate test of the 'true believer'.[4] Of course, 'true believers' reacted to this argument with the same dose of scepticism as scientific believers do to what Collins and the rest of us say. But by failing to appreciate the irony of such precedents, scientists risk becoming the old man who cannot see himself in the child.

'Yes, but Isn't (Natural) Science Still Really Special?'

This question epitomizes Labinger's response to the science studies literature. He gives, so to speak, a 'synchronic' and a 'diachronic' argument in defence. First, at any given moment, there is a uniformity to scientific culture that does not exist elsewhere in society. Second, the nature of science is such that it would have headed in the same general direction, even if it had had a different historical starting point. I shall consider each in turn.

The temptation to treat science's 'uniformity' as bearing the mark of 'nature' or 'truth' often reflects a failure to appreciate science's underlying social complexity.[5] For example, does the absence of explicit disagreement over a theory or result warrant the conclusion that some deep 'consensus' of belief obtains among members of the scientific community? Would it not make more empirical sense to examine institutional mechanisms that discourage scientists from contesting claims beyond a certain point, and then ask why these mechanisms seem to work so effectively? Here an account of the differences in how scientists and other academics are trained would explain a lot. How do scientists become predisposed to find certain displays of epistemic authority persuasive and others not? How do scientists master a common writing style that enables them to translate the idiosyncracies of their work situations into moments in a narrative common to other workers in their field? Is there anything more to the 'uniformity' of science than these factors taken together, as continually reinforced by various gatekeeping practices?

I can see why Labinger might think that this studious avoidance of 'nature' and 'truth' is little more than an attempt by sociologists to colonize the natural sciences. However, there *is* more to the move than that. Seen in historical perspective, appeals to 'nature' and 'truth' have often conveyed an air of spurious finality to the matters being described or explained. 'Nature' is cited as the ultimate barrier to change, and 'truth' as the limit of rational disagreement. Yet, time and again, these obstacles have been moved, circumvented or removed altogether. Sociological discourse has the advantage of drawing attention to this fact by recasting 'nature' and 'truth' in humanly accessible terms. Even the recent 'return to nature' in science studies that Labinger endorses is really an attempt to export quintessentially human properties (such as agency) to things that have traditionally lacked them.

Turning to the question of whether all possible histories would lead to today's science, I think Labinger has hit upon an important issue that our literature has yet to address properly.[6] As he rightly observes, science studies histories tend toward a radically 'under-determined' view of how the world works, such that virtually any change in the past would have caused a significant change in what followed. Now, what aspects of science does Labinger believe would remain invariant across a range of alternative historical trajectories? Surely, he does not believe that alternative paths would have arrived at *exactly* the same version of contemporary science. In that case, if everything that happened in the past is *not* necessary for science being what it is today, then how different could the past have been in order to reach roughly the same state, and does this possibility not imply that the present itself may be altered quite significantly and still enable a proper scientific enterprise to flourish? In other words, the more 'accidental' or 'contingent' the actual history of science is taken to be to how one defines the nature of science, the more one can separate the 'wheat' from the 'chaff' of today's science. And so, I wonder, by how much could the following parameters be varied and still leave science thriving: Its reward structure? Its relations with the state and industry? Its educational practices? Its recruitment patterns? It would be nice to think that any of the above could be varied considerably while leaving the essence of science intact, because it would mean that the interests of scientific enquiry and social justice could be met simultaneously. But I wonder. . . .

Where Do We (Collectively) Go from Here?

Unlike many of my colleagues, I do not want to see science studies become yet another 'separate but equal' academic discipline. I do not consider increasing disciplinary specialization and the associated Kuhnian model of normal science to be either natural laws or regulative ideals of enquiry. Thus, I welcome the 'interpenetration' of science and science studies. However, given the current academic power structure, it is unrealistic to expect that this will happen without the natural sciences yielding some of their autonomy by becoming more receptive to the critiques implied in science studies accounts. A good step in that direction would actually come from leading scientists learning to appreciate the diversity of their own disciplinary constituency, and that, say, not all physicists believe that more resources ought to be thrown into high-energy physics.[7] The fact that Jay Labinger and I can take each other's views of science seriously enough to respond to them is only a first step toward what ultimately needs to be done.

• NOTES

1. Jay A. Labinger, 'Science as Culture: A View from the Petri Dish', *Social Studies of Science*, Vol. 25, No. 2 (May 1995), 285–306.
2. Steve Fuller, *Philosophy, Rhetoric, and the End of Knowledge* (Madison, WI: University of Wisconsin Press, 1993).
3. Much to my (pleasant) surprise, my recent book (op. cit. note 2) was reviewed in the 14 May 1994 issue of the *New Scientist*. My new-found civility in dealing with scientists may be seen in 'Pro-science Is Harder than You Think', *Academic Questions* (in press, 1995). The latter journal is the official organ of the US National Association of Scholars (NAS), a group that aims, among other things, to counter 'academic left' critiques of science of the sort popularly associated with science studies. For accounts of a recent NAS meeting, see Anthony Flint, 'Science Isn't Immune to Cultural Critique', *Boston Globe* (15 November 1994), 1, 28; and Scott Heller, 'At Conference, Conservative Scholars Lash Out at Attempts to "Delegitimize Science"', *The Chronicle of Higher Education* (Washington, DC, 23 November 1994), A18, A20.
4. This argument is normally attributed to the German Enlightenment philosopher-critic, Gotthold Ephraim Lessing. For the role of critical-historical theology in the scientization of religion, see John Hedley Brooke, *Science and Religion: Some Historical Perspectives* (Cambridge: Cambridge University Press, 1991), 263–74.
5. What follows is drawn from Steve Fuller, *Social Epistemology* (Bloomington, IN: Indiana University Press, 1988), Chapter 9; and Fuller, 'The Social Psychology of Scientific Knowledge: Another Strong Programme', in William R. Shadish and Fuller (eds), *The Social Psychology of Science* (New York: Guilford Press, 1994), 162–80.
6. Labinger, op. cit. note 1, 293–95. I discuss the role of counterfactuals in historical judgements of science in Fuller, op. cit. note 2, 210–13; and in S. Fuller, 'Making Agency Count: A Brief Foray into the Foundations of Social Theory', *American Behavioral Scientist*, Vol. 37 (1994), 741–53, at 743–46.
7. This point is elaborated in Steve Fuller, 'Being Civil with Scientists: Reply to Wolpert and Weinberg', *Social Studies of Science*, Vol. 24 (1994), 751–57.

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Responses and Replies (continued)

Cooperation for What?: A View from the Sociological/Cultural Study of Science Policy

Sheila Jasanoff

Jay Labinger most commendably practices what he preaches: his humorously exasperated critique of relativism in science studies ends with a peaceable appeal for more interdisciplinary co-operation between scientists and their 'SCS' critics.[1] His ideal joint agenda would include 'projects aimed at improving the *practice* of science as well as the *management* of science, on both micro and macro levels'.[2] Such an olive branch from a worthy adversary is surely worth taking seriously, although one cannot help wishing that Labinger had read as widely in the segment of the SSK literature that deals with science in public life as he evidently has in the segment of SSK that is primarily concerned with practices in the laboratory.[3] This wider reading might have persuaded him that, before any cooperation is possible, scientists and their most dedicated (if sometimes unfriendly) observers must converge on a mutually agreeable understanding of what 'science' is – in other words, they must sort through just those arguments and counter-arguments that make for the stand-off that Labinger so amusingly and ably describes.

Taking science studies to task for a too-easy relativism may well be a useful way to begin the rapprochement, but as a defender of reflexivity Labinger must see that correcting the SSK critics' possibly misguided notions about reality is at best one half of the problem. (I use 'SSK' here to privilege the self-identifying marker of choice for our community.) The other half, which his paper only

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gestures at, is the scientist's own conception of the work of science. What understanding of this subject does Labinger himself bring to the table as he calls upon the science studies community to join with scientists in 'improving' the practice and management of science? To what extent do views like his perhaps uncritically reflect those of other scientists? And what can the practitioners of science studies do to reframe and possibly make more productive Labinger's invitation to re-engage in meaningful conversations with him and his fellow scientists?

The science management issues that Labinger lists as 'rather obvious' reproduce the old bifurcation in the policy literature between *policy for science* ('allocation of limited resources between scientific and non-scientific programmes, among scientific fields, between big and little science, fundamental and applied research programmes') and *science in policy* ('policy debates with high scientific content, such as global warming, ozone depletion, resource management').[4] Yet, one of the major accomplishments of science studies over the past 20 years has been to problematize precisely these analytic categories. We know that neither 'science' nor 'policy' can be taken for granted as well-defined conceptual fields; each is constructed in relation to the other, through the very dynamics of making, implementing, avoiding and even resisting the formal policy instruments that have been of greatest concern to conventional policy analysis.

More important, the constructivist strain in science studies has allowed us to see distinctly that the boundaries of 'science' and 'policy' are drawn by social actors to sustain a variety of social needs on a continually shifting map of privilege, politics and power. Quantitative risk assessment, for example, is a culturally induced and cross-culturally differentiated response to the vulnerable bureaucrat's need for 'objective' legitimation, the legalistic society's need for rules and clarity, and the publicly supported scientific community's need for policy problems that are amenable to resolution by 'science'.[5] Reliance on particular analytic or legitimating discourses, such as risk assessment, is not equally entrenched even in countries like Britain and the United States that share a common intellectual and political heritage. The public debate on science policy accordingly looks, feels and sounds very different on the two sides of the Atlantic, even when the issues are superficially similar and when they entail seemingly analogous economic and political consequences.

This analysis suggests that the most important times and places for SSK scholars and scientists to interact may be at points quite different from those that seem rather obvious to Labinger. Encounters that would increase each side's reflexivity, and influence its approaches to inquiry, are more likely to occur before, not after, the normal interplay of culture and interest have produced a fixed cartography of what counts as 'science' and what as the associated 'policy'. Of course, important debates may still be had after these domains have been sharply delineated, but the exchanges will then take place within constraints that may limit the renegotiation of fundamental premises – both in SSK and in the sciences. Like Labinger's curiously unreflexive editor, we may be doomed to 'see' only small islands of contested territory, without recognizing the depths of ignorance that lie concealed by the surrounding seas.

How, then, would an SSK researcher with interests in science policy define the conversational agenda between science and its academic observer-critics? My preference would be to begin where so many of the ladders start – in all the places where 'science' is being defined and given content in relation to other social activities, often in ways that redirect resources or power to the scientific enterprise. I would look for constructive engagement with scientists wherever the boundary issues between science and policy are contested and emerging rather than where they have been largely black-boxed and made invisible: thus, I am not so much interested in how to manage global warming or ozone depletion as in the processes by which particular conceptual models of such environmental problems arise and are legitimated; not so much in the validity of default assumptions in cancer risk assessment as in the institutional and social arrangements that account for the remarkable appeal of reductionist policy instruments like risk assessment or climate modelling; not so much in how to 'improve' judicial decision-making on science as in the ideas of justice, rationality, facticity and proof that make this a subject for such intense and unyielding controversy.

Scientists who would be willing to set aside, or at least re-examine, some of their prior commitments in considering these kinds of issues would make wonderful conversation partners indeed for SSK, and may in the process produce more humane directions for scientific inquiry. To make this move, however, scientists will have to reconsider their own role in constructing

Labinger's scientific bestiary. After all, it is scientists themselves who created those powerful images of the lab, the maze and the Petri dish that leave the SSK critic with little choice in most instances but to stand on the outside, looking in.

• *NOTES*

1. Jay A. Labinger, 'Science as Culture: A View from the Petri Dish', *Social Studies of Science*, Vol. 25, No. 2 (May 1995), 285–306.
2. Ibid., 299.
3. For comprehensive surveys, see Sheila Jasanoff et al. (eds), *Handbook of Science and Technology Studies* (Thousand Oaks, CA: Sage, 1994).
4. Labinger, op. cit. note 1, 299.
5. See S. Jasanoff, *Risk Management and Political Culture* (New York: Russell Sage Foundation, 1986).

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Responses and Replies (continued)

The Cultural Reconstruction of Science: A Response to Labinger

David Hakken

Jay Labinger is to be congratulated for his principled effort (which I support) to open a discourse with scholars who do social and

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cultural studies of Science.[1] I believe that such a discourse is necessary if STS is to survive the current backlash.[2] We can only do this if we manage to move beyond the purely critical moment and communicate a clearer image of technoscience practice substantially less vulnerable to the kinds of critique which we make. Like Labinger, I believe that collaboration between STSers and technoscientists is crucial both to constructing a viable alternative practice and to winning acceptance for it.

Labinger focuses appropriately on the terms of fruitful dialogue. Here I respond in the way I believe he would wish me to – concentrating on those of his terms which I find dissatisfying, and offering (very different) alternatives.

Where I part company with Labinger is over his picture of the STS critique of technoscience. By his account, STS is a kind of intensified form of a normal science: the identification of specific examples, even whole 'schools', of bad science – to borrow Hilary Rose's phrase, 'shit shovelling'.[3] In my view, STS offers a more general critique of technoscientific practice: that it is deeply flawed by modernist scientistic presumptions – positivism, empiricism, progressivist rhetorics, and so on – which must be rooted out if a transformed science is to be created.

This critique was fostered by A.N. Whitehead and articulated again most beautifully by Stephen Toulmin.[4] Contra Labinger, the critique is not equivalent to epistemological relativism – I myself work with a realist epistemology – but it does entail something more extensive than merely traversing the terrain Sancho Panza-style, offering an occasional catalytic comment or other 'reminder' as junior partners in an enterprise presumed to be shared. STS calls us to something beyond shit shovelling, to a deconstruction of the manner in which the terrain itself has been culturally constructed, so that our maps of it become substantially less systematically distorted.

The way in which his chosen 'conceit' structures Labinger's argument is a nice example of how scientism limits analysis.[5] As foreshadowed by the Petri dish in his title, Labinger sees STS as a 'scientific' enterprise, a metaphor albeit with a role reversal – the technoscientist in the Petri dish, and the STSer peering into the microscope. STSers are only perceivable to him when we act like 'normal' technoscientists, even to the masculinist idiocy of playing (epistemological) chicken!

Rather than accepting the self-evident validity of everyday scientific practice, STS demands that we act reflexively, recognizing the multiple ways in which 'really existing technoscience' reproduces highly questionable cultural and philosophical premises. His conceit-imposed blindness is the primary reason why Labinger fails to see what STS really offers to day-to-day practice.

Labinger's view also reproduces the typical technoscientist's narrow dualism. His world is one in which the categories 'scientist' and 'SCSer' are mutually exclusive. Yet I am an anthropologist who does STS, and am thus both inside and outside of technoscience. With colleagues who share a critique of scientism, I apply STS in my day-to-day work, both in my anthropological theorizing and in my field studies of and everyday practice with information system developers. In short, a reflexive view of the technoscientific enterprise is a necessary prerequisite to effective STS/technoscience collaboration. I have no interest in 'collaboration' which projects a self-satisfied view of technoscience while denigrating other scholars (see his disparaging sociologist story).[6]

One final example of his non-reflexivity: restricting STS to 'science', whereas I insist on the broader concept of 'technoscience'. He asks rhetorically, 'can anyone point to an example of an interaction between SCSer and scientist [*sic*] that transcends the experimenter–subject relationship in any significant way?'.[7] I do find – am indeed occasionally personally involved in – collaborative, non-hierarchical STS/technoscience interactions among some systems developers, both in Scandinavia and in the US. These interactions work because practitioners from both sides of the two-culture divide share substantial parts of a world view. Reflexive technoscientists and STSers learn to construct and be constructed by the substantial limitations of 'practical' activity. It is precisely because technologically-orientated technoscientists deal regularly with (socially-constructed) real worlds that collaboration is easier for STSers with them than with scientifically-orientated technoscientists, who often need never 'put their feet on the ground'.

Nevertheless, there are depressingly few examples of this sort of collaboration. The best parts of Labinger's Comment help us understand why this is so: obscure phrasings, postmodern posturings, 'in your face'-isms which serve primarily to draw boundaries or impose personal agendas, in exactly the same manner as the technoscientists we sniff at. The worst truth is not even what Labinger's abstract describes as 'the virtually total absence of participation by practising scientists',[8] but that when they do participate, we have no idea how to respond corporatively. I have in mind the presentation at the 4S/HSS/PSA Meeting in New Orleans (12–16 October 1994), by system-developers Morten Haltning, Ole Hanseth and Eric Monteiro, on 'New Communication Standards: Technical Flexibility or Social Uniformity?'. This paper, a serious attempt both to ground an STS problem in a 'concrete' virtual artefact and to show the practical limitations of the STS perspective, apparently failed to connect with our discourse community.

Indeed, Labinger's underlying message is accurate: the STS network performs miserably on those few occasions when bridges are extended across the cultural divide. While I take exception to the design he projects, I am hopeful that Labinger and other 'technoscientists of good will' can be prodded to move beyond shit shovelling. STSers need to engage in a positive (but not positivist) reconstruction of science.

• *NOTES*

1. Jay A. Labinger, 'Science as Culture: A View from the Petri Dish', *Social Studies of Science*, Vol. 25, No. 2 (May 1995), 285–306.
2. In the US, the backlash has been led by Paul R. Gross and Norman Levitt, in *Higher Superstition: The Academic Left and Its Quarrel with Science* (Baltimore, MD: Johns Hopkins University Press, 1994), and its subsequent discussion.
3. Personal communication.
4. Stephen E. Toulmin, *Cosmopolis* (Chicago, IL: The University of Chicago Press, 1990).
5. I use the word 'conceit' here in its strict seventeenth-century, rhetorical sense.
6. Labinger, op. cit. note 1, 300.
7. Ibid., 289.
8. Ibid., 286.

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Responses and Replies (continued)

Response to Labinger

William Keith

Most people have had the following experience: You are sitting at the keyboard, writing, typing away like mad, and suddenly you begin to *watch yourself type*. Even as you watch, the typing that was so natural, so easy, becomes more and more difficult until your fingers are so clumsy you can no longer type at all. Somehow, focusing one's consciousness on an activity interferes with doing it; one cannot do both at once. Jay Labinger's honest and provocative essay seems to exemplify this point, at the discursive level.[1] Labinger points to the apparent mismatch between what he calls SCS and the day-to-day practice of science. His discomfort is symptomatic of a larger problem – the actual or appropriate relationship between science (as practised) and Science (as studied by SCSers).

This problem has many levels and dimensions, but I'd like to point out some of the discursive discontinuities that pervade the relationship between science and SCS. The basic problem is this: a redescription of science in other terms (which every SCS account is) seems unavoidably to destabilize science. Whether one welcomes or bemoans this outcome, the tension between the doing of science and the study of it seems always to manifest itself as attack on science. We thus need to explore how the tension arises, and why it creates such hostility.

Labinger is particularly vexed, as are Gross and Levitt,[2] by their perception that SCS accounts claim that scientists misunderstand their own enterprise, as if to say: '*We* thought it was about seeking truth, while *you* think it's about social arrangements'. While Labinger is willing to concede that 'social factors *are* important',[3] neither he, nor Steven Weinberg,[4] nor any practising scientist I know of, is willing to accept that a description of social relations is

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what science is about.[5] I take it that the continual references in these debates to the ignorance or naïveté of SCS scholars about science boils down to the complaint that 'If you were doing science, you wouldn't say such things, because you'd realize they make nonsense of the whole venture'. Why would anybody continue doing science, one might ask, if its distinctiveness rests only in it being a particular social arrangement? Nature and truth are *not* rhetorically dispensable for scientists, since these are the terms by which they make sense of their own enterprise. While you're doing that activity ('trying to find truths about the world'), watching yourself do the activity ('understanding science as social and cultural') is paralyzing. (This argument goes the other way, too: 'If science is truly objective, then social accounts of science are just a kind of category mistake, an attempt to explain apples by oranges', and so on.) The mere discrepancy between two descriptions cannot explain the hostility flaring between scientists and SCSers.

In particular, SCS accounts are threatening to science because of rhetorical and philosophical choices that were fixed in the seventeenth and eighteenth centuries. Stephen Toulmin, in *Cosmopolis*,[6] argues that the Scientific Revolution and the Epistemological Turn were bound by a common political motivation, born of the horrors of the Thirty Years' War and ubiquitous sectarian strife: to find a way of doing philosophy/science that leaves these insoluble social and cultural differences behind, a mode of understanding the world *independent* of the social and cultural character of scientists. In fact, 'the world' comes to means exactly that part of our experience which is independent of social factors. The terms 'objective' and 'subjective' come into use to describe the relative contributions of the world and our minds to our experience of something. The search for the *objective* thus represents the faith that there is something to be known, to be *said*, beyond what we say *as* Catholics or Protestants or whatever. (That *objectivity* itself can later, in the twentieth century, be implicated in certain kinds of oppression should, of course, surprise no one.) So, subtracting out the social element to discover the residue of reality is perhaps the central mission of science, a mission attacked by the very existence of social accounts that, so to speak, 'go all the way down'.

A clue for this analysis is the immediate response of many scientists and philosophers to the genre of SCS analyses: 'So you're saying that *anything* goes!?'. As much as SCSers shrug it off, 'Of course not . . .', this objection persists. Perhaps if the subjective/objective talk is built deeply enough into scientists' consciousness, they will of course believe that the negation of the objective implies the rule of the subjective. They might also be saying that introducing these social elements explicitly into the practice of science would result in a practice that we would no longer call science. (And they're probably right about that.) In other words, it seems that even scientists sympathetic to SCS, like Labinger, *need* to talk about what they're doing as scientists in non-social terms.

At this point, many readers will be saying to themselves: 'Here we go again. He's reifying philosophical, binary distinctions that are disposable through deconstruction or negotiation'. I don't doubt the justice of this reply, only its relevance. Certainly ethnographers may find that, in working among scientists, they can work out a way of talking acceptable to those scientists in the way anthropologists usually do – but Labinger will still be unhappy with the *idea* of what they're doing, and rightly so, by his lights. The rhetorical problem (why do SCS claims make scientists so hostile?) is tied deeply into a political characterization of the cultural distinctiveness of science. When scientists point to the technological productiveness of science (Astronauts on the moon! Diseases cured!) they are not, I think, just saying that these 'successes' show science is in touch with Nature, but that access to Nature was made possible by the non-social character of science. As Steve Woolgar has often pointed out,[7] to claim that scientists are simply wrong about this is to position oneself as the *real* scientist, which starts the loop all over again. More importantly, it does not take the political problem, or the scientists, very seriously.

I don't have a pat answer to this cluster of problems, and it's unlikely that anyone else does either. But it does point to the location of the conflict, and maybe the paralysis, in the relationship of scientists and SCSers.

• *NOTES*

1. Jay A. Labinger, 'Science as Culture: A View from the Petri Dish', *Social Studies of Science*, Vol. 25, No. 2 (May 1995), 285–306.

2. Paul R. Gross and Norman Levitt, *Higher Superstition: The Academic Left and Its Quarrel with Science* (Baltimore, MD: Johns Hopkins University Press, 1994).
3. Labinger, op. cit. note 1, 292.
4. Steven Weinberg, *Dreams of a Final Theory: The Search for the Fundamental Laws of Nature* (New York: Pantheon, 1992; London: Hutchinson, 1993).
5. Labinger, op. cit. note 1, 292.
6. Stephen E. Toulmin, *Cosmopolis* (Chicago, IL: The University of Chicago Press, 1990).
7. For an early sample, see Steve Woolgar, 'Interests and Explanation in the Social Study of Science', *Social Studies of Science*, Vol. 11(1981), 365–94.

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Responses and Replies (continued)

Collaboration and Scandal: A Comment on Labinger

Michael Lynch

Labinger's 'View from the Petri Dish' is a well-intended and well-documented discussion of social studies of science which invites us to reconsider our treatments of science and scientists.[1] He objects to how our studies often place scientists in 'the role of specimens'.

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FIGURE 1

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A similar image is portrayed by a cartoon in *The Times Higher Education Supplement* (Figure 1).[2] This depicts two looming figures scrutinizing the movements of diminutive scientists trapped in a maze who look up from their microscopes to glower at their captors. The omniscient and wild-eyed observers are, of course, the sociologists of science. The ironic thing about such caricatures is that they depict science studies analysts as though they were obsessed with the kind of oculocentric, detached observation that their own studies have criticized in recent years. Nevertheless, the 'specimens' do have some reason to complain. Some years ago, laboratory ethnographies tended to exaggerate the distance between the social science observer and the 'tribes' of scientists described – forgetting that a quick trip across campus was sufficient to locate a native village, and that entry into such a village often could be gained through the help of acquaintances and, in some cases, spouses. The published accounts of these ethnographies often employed literary devices that created a sense of estrangement from even the most familiar aspects of the laboratory.[3] Some of us analyzed tape recordings of laboratory shop talk, and our narrative voices animated the desiccated utterances in the transcripts, informing readers what the scientists were *really* saying and doing, sometimes in spite of their words.[4] More recently, scientists have become semiotic 'actants' dwelling in textual flatlands alongside their colleagues, audiences and specimens.[5] Still more recently, quite a few writers of social and cultural texts, emboldened by the success of the historical and ethnographic studies, have favoured a more confrontational approach and, as Labinger notes, one spokesman for our field has offered to lead scientists out of their epistemological darkness.[6] It is no wonder that Labinger is reluctant to accept the helping hand. Under the circumstances, we should be grateful that he offers the olive branch of collaboration.

I think that Labinger, through no fault of his own, fails to recognize the extent to which the collaboration he advocates between practitioners and analysts already is a substantial part of the science studies field. The problem is that it has been buried under the polemics of the realist–constructivist debate. Collaborations of the sort that Labinger seems to have in mind are well underway, although less in science studies than in the related field of technology studies. For example, over the past several years, Lucy Suchman, R.J. Anderson, Graham Button, Wes Sharrock, Brigitte Jordan, Susan Newman and others have worked closely with teams of designers, software programmers and other technical staff at Xerox PARC and EuroPARC.[7] At least in principle, there is no strict division of labour between the designers and the ethnographers, because the intention is to expand design work to take better account of social contingencies. Some of the projects currently underway at the Centre for Research into Innovation, Culture and Technology (CRICT) at Brunel University also involve collaboration between science studies researchers and technical specialists and managers.

On a less obvious level, collaboration has been a necessary part of the fieldwork performed by several of the authors Labinger cites, even when this is obscured by their literary devices and analytic postures. Labinger mentions that Harry Collins collaborated with a practising scientist in his participant–observation study of building a transmission laser,[8] but he notes that the scientist only participated in the labour process: 'When it comes time to analyze what has happened, though, it's all Collins'.[9] Although there is some point to this criticism, I would argue that Collins and other ethnographers of scientific work gained *analytical* insight as well as data from the practitioners they studied. If my own experience is any indication, a sociologist or anthropologist who visits an unfamiliar laboratory, observatory or other research facility requires a great deal of guidance before he or she can observe and analyze the relevant activities. The 'guides' in these cases are not limited to the authorities in the sociology or anthropology literatures. More immediately, they are the inhabitants being studied. While conducting the study, far from being an omniscient overseer, the ethnographer is likely to be cast as a novice in need of a lesson or a visitor to be entertained with show-and-tell stories. Much of what the ethnographer observes and records is conveyed by the scientists studied, and this includes some of the lessons that subvert the classic, honorific versions of science and method. Insight also can be gained from an ethnographer's estrangement from the daily routines and common understandings, but when taken too seriously the literary figures of the Martian anthropologist or the fly on the wall obscure the extent to which ethnographic accounts are indebted to the informal tutorials given by local inhabitants.

To say that the scientists are the *sources* of ethnographic accounts of their activities may seem strange in light of the fact that such accounts sometimes are thought to produce *scandalous* versions of science. But this is not so strange when we consider that scandal often arises when the public exposure of what occurs in practice contradicts the versions promulgated by official propagandists. For those of us influenced by ethnomethodology, the point of studying science is not to create scandal, but to come to terms with the difference between formal accounts and what is evident in practice.[10] The 'exposure' of the quotidian aspects of science has not embarrassed the scientists involved, as compared to the way the release of the White House tapes diminished Richard Nixon's reputation.[11] Instead, this exposure mainly seems to have upset a few writer-scientists who, as Gilbert Ryle once put it, forget what they say on weekdays in a working tone of voice when preaching 'in an edifying tone of voice on Sundays'.[12]

To his credit, Labinger seems uninterested in protecting science from our 'scandalous' investigations, and his invitation to collaboration should be taken seriously. However, once again, there already is more collaboration on the scene than he may realize. Some of the collaborative efforts I have mentioned are quite thorough and explicit, and not at all scandalous, while others seem to touch off defensive reactions by scientist-writers who would prefer to exert greater control over the public relations associated with the scientific professions. This is not to deny that further collaboration is in order, but we should not fool ourselves into thinking that collaboration at the scene of research should lead to metaphysical agreement about, for example, the role of nature in scientific investigation. However, by dissolving the presumption of a radical difference between a 'practitioner's' and a 'sociologist's' viewpoints we may find a way to 'show the microbe the way out of the Petri dish'.

• NOTES

1. Jay A. Labinger, 'Science as Culture: A View from the Petri Dish', *Social Studies of Science*, Vol. 25, No. 2 (May 1995), 285–306.

2. Cartoon by Andrew Birch, *The Times Higher Education Supplement* (London, 30 September 1994), 17.

3. See, for example, Bruno Latour and Steve Woolgar, *Laboratory Life* (London: Sage, 1979); Karin Knorr-Cetina, *The Manufacture of Knowledge* (Oxford: Pergamon Press, 1981); and Sharon Traweek, *Beamtimes and Lifetimes: The World of High Energy Physics* (Cambridge, MA: Harvard University Press, 1988).

4. Michael Lynch, *Art and Artifact in Laboratory Science* (London: Routledge & Kegan Paul, 1985), and Latour & Woolgar, op. cit. note 3.

5. See B. Latour, *Science in Action* (Cambridge, MA: Harvard University Press, 1987), and many other sources.

6. Labinger, op. cit. note 1, 286–87, cites Steve Fuller, *Philosophy, Rhetoric and the End of Knowledge* (Madison, WI: University of Wisconsin Press, 1993).

7. See Françoise Brun-Cottan, Kathy Forbes, Charles Goodwin, Marjorie H. Goodwin, Brigitte Jordan, Lucy Suchman and Randy Trigg, 'The Workplace Project: Designing for Diversity and Change', video produced by the Xerox Palo Alto Research Center (Palo Alto, CA, 1991), and Suchman, 'Technologies of Accountability: Of Lizards and Airplanes', in Graham Button (ed.), *Technology in Working Order: Studies of Work Interaction and Technology* (London: Routledge, 1992), 113–26.

8. Labinger, op. cit. note 1, 287, cites H.M. Collins, *Changing Order* (London: Sage, 1985), 51–78. This section is based on an earlier study by Collins, 'The TEA

Set: Tacit Knowledge and Scientific Networks', Science Studies, Vol. 4 (1974), 165–86.

9. Labinger, op. cit. note 1, 287.

10. See Harold Garfinkel and Harvey Sacks, 'On Formal Structures of Practical Actions', in J.C. McKinney and E.A. Tiryakian (eds), *Theoretical Sociology: Perspectives and Development* (New York: Appleton-Century-Crofts, 1970), 337–66; and M. Lynch, *Scientific Practice and Ordinary Action: Ethnomethodology and Social Studies of Science* (New York: Cambridge University Press, 1993).

11. It may be recalled that Nixon 'collaborated' in his own undoing by authorizing the tape recordings of his meetings and phone calls in the Oval Office. He reportedly did this in order to secure his place in history. See M. Schudson, *Watergate in American Memory* (New York: Basic Books, 1992).

12. Gilbert Ryle, 'The World of Science and the Everyday World', in Ryle, *Dilemmas* (Cambridge: Cambridge University Press, 1954), 68–81, quote at 75.

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Responses and Replies (continued)

Other Voices: A Response to Labinger

Harry M. Marks

> America, I'm putting my queer shoulder to the wheel.[1]

Jay Labinger wants SCS practitioners to change their attitude(s).[2] Stop treating scientists and their doings like microbial specimens on a slide, start giving the microbes a little credit for what they have wrought and are capable of wreaking, get out from behind

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that lens and give us (scientists) a hand in building culture, society and science. But first, show a little ritual humility or, as Pope Leo X might have said to Martin Luther: 'Confess your sins and return to Holy Mother Church; we'll talk about your penance later'.

As a historian who studies medical sciences and practices, I'm the odd man out here. I'm neither especially qualified nor motivated to respond to key elements of Labinger's bill of particulars against Harry Collins, Andy Pickering, Steve Fuller et al. I leave it to them to defend or recant their epistemic doxa as they see fit. Nevertheless, Labinger's plaint should concern anyone who writes about the sciences: we are, he insists, going about it in the wrong way. We use the wrong methods, ask the wrong questions, and lack the right manners. 'Not' (as my nine-year-old laconically says).

Labinger advises students of science to pay more attention, in his terms, to the 'state[s]' of science, and less to its 'path–dependent' kinetics.[3] This strikes me as poor advice: would Labinger advise a chemistry student to go look at 'Nature'? – or, rather, with a specific set of instrumentation, materials and premises, to investigate the reaction kinetics of TATA-binding protein in DNA transcription?[4]

It is rather hard to locate the 'science' (noun, singular) which Labinger repeatedly invokes. What is simply an 'error' term for the clinical chemist measuring the concentration of creatine phosphokinase isoenzymes may well be the thermodynamicist's life-long research programme. As a historian writing about the clinical chemist, I need to understand his interests in CPK determinations, as well as *all* the resources available to him: concepts, funding, instruments, status, materials, rhetoric. The global 'state of science' is a rather unwieldy tool for me to use in doing this analytical work.

Like some of us historians, Labinger may be reacting to the sociologist's habit of slipping unobtrusively from a carefully situated analysis of seventeenth-century experimentation to indiscriminate talk of 'the nature and status of experimental practices'.[5] There is a tendency, in the writings of otherwise meticulous sociologists, toward essentialism: speaking about 'science' or 'experiment' or 'the laboratory' as if these were natural, transparent and stable categories. In the ground-clearing efforts which precede some exquisitely empirical studies, there is a related habit of pronouncing on what Science is and is not. We would all be better off, perhaps, if we regarded such talk as analogous to the orchestra tuning up before playing: technically essential, morale building, but a prologue to the real performance.

If Labinger will allow me my professional interests in path-dependence then, I'd have to confess, he doesn't seem a very promising collaborator. If I want to understand the problem of path-dependence in historical time, I'm going to turn to theoretical sociologists like Andrew Abbott or Geoffrey Hawthorn, who have thought long and hard about problems of historical causality and explanation.[6] I am not going to rely on someone who thinks that we owe vacuum cleaners to Robert Boyle's accomplishments at the Royal Society. For Labinger, the consequences of the seventeenth-century scientific revolution are a rhetorical resource not, as Roy Porter has recently suggested, a seriously neglected historical problem.[7]

Labinger appeals for a collaboration between SCSers and scientists. On what terms and to what ends? There is no shortage of historical examples for social scientists serving as handmaidens to industry, the military, colonial administrators, physicians and the like.[8] Labinger's examples of collaboration suggest that he would like social scientists to return to the role they once held as advisor and courtier to the Princes of Science.

My difficulties are not with Labinger's particular choice, but in the menu of choices being offered. Labinger is hardly alone in asking: 'Halt! Who goes there, friend or foe?'[9] I find this way of posing the question unnecessarily claustrophobic. Are there other options?

Social scientists might opt to be partisan *within* a science, as defenders of holism against reductionism in biology, say, or as critics of subjectivist accounts of probability. The examples of Evelyn Fox Keller, Richard Lewontin and the young Ian Hacking come to mind. Among social scientists, perhaps Peter Galison's sympathies for the unrecognized physical experimenter over the lofty physical theorist come closest to such advocacy.[10]

Alternatively, one might elect to operate on a broader canvas, one peopled not just with scientists, their allies and enemies, but with enzyme chemists and epidemiologists; suburbanites and urban gentry; short, fat people from Jefferson Parish, Louisiana; middle-aged men who did and did not make the varsity basketball team in high school; women whose brothers are or are not infected with AIDS. Historians such as JoAnne Brown, Carolyn Steedman and Judith Walkowitz are just beginning to examine the implications of this plurivocality for the way we write history.[11] Labinger may be heartened to hear that the imperious narrative voice of the social scientist is being chastened, if not silenced. Scientists who speak their own history and actions may yet outshout the duly authorized SCS narrators of whom Labinger rightly complains.

One thing seems clear, however, from the plurivocal continent of cultural studies. One should not assume in advance who the actors are, how they will define their interests, or what alliances they will seek.

To take the area of science with which I am most familiar, physicians, statisticians, patients and stockbrokers are all involved in producing and judging experimental evidence about new medical treatments. They differ about the interpretation and significance of particular studies, about the best ways to conduct studies, and about which studies are worth doing now. One finds both inter-group and intra-group differences about the science being done.[12] All of which is to say that Labinger's skirmish with constructivism offers me a very limited palette with which to think about, or participate in, this scientific activity. Perhaps he can ensure that no interlopers intrude on our current debates about the age of the universe.[13] But that does not seem to get us very far in understanding or doing science.

• *NOTES*

1. Allen Ginsberg, 'America', in Ginsberg, *Howl and Other Poems* (San Francisco, CA: City Lights Press, 1959), 43.

2. Jay A. Labinger, 'Science as Culture: A View from the Petri Dish', *Social Studies of Science*, Vol. 25, No. 2 (May 1995), 285–306.

3. Ibid., 294.

4. Charles Klein and Kevin Struhl, 'Increased Recruitment of TATA-Binding Protein to the Promoter by Transcriptional Activation Domains in Vivo', *Science*, Vol. 266 (14 October 1994), 280–82.

5. Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump. Hobbes, Boyle and the Experimental Life* (Princeton, NJ: Princeton University Press, 1985), 3.

6. Andrew Abbott, 'Event Sequence and Event Duration: Colligation and

Measurement', Historical Methods, Vol. 17 (Fall 1984), 192–204; Abbott and Stanley Devine, 'The Welfare State as a Transnational Event', *Social Science History*, Vol. 16 (Summer 1992), 245–74; Geoffrey Hawthorn, *Plausible Worlds: Possibility and Understanding in History and the Social Sciences* (Cambridge: Cambridge University Press, 1993).

7. Roy Porter, 'The Scientific Revolution: A Spoke in the Wheel?', in Porter and Mikulas Teich (eds), *Revolution in History* (Cambridge: Cambridge University Press, 1986), 290–316.

8. See Loren Baritz, *Servants of Power: A History of the Use of Social Science in American Industry* (Middleton, CT: Wesleyan University Press, 1960); Henrika Kuklick, *The Savage Within: The Social History of British Anthropology* (Cambridge: Cambridge University Press, 1991); Peter Buck, 'Adjusting to Military Life: The Social Sciences Go to War, 1941–1950', in Merritt Roe Smith (ed.), *Military Enterprise and Technological Change: Perspectives on the American Experience* (Cambridge, MA: MIT Press, 1985), 203–52; Robert Straus, 'The Nature and Status of Medical Sociology', *American Sociological Review*, Vol.22 (1957), 200–04.

9. This seems to have been the tone of exchanges in both the US and the UK, judging by the accounts to be found in *The Chronicle of Higher Education* (Washington, DC, 5 October and 23 November 1994), and *The Times Higher Education Supplement* (London, 30 September 1994).

10. Evelyn Fox Keller, *A Feeling for the Organism: The Life and Work of Barbara McClintock* (San Francisco, CA: W.H. Freeman, 1983); Richard Levins and Richard Lewontin, *The Dialectical Biologist* (Cambridge, MA: Harvard University Press, 1985); Ian Hacking, *Logic of Statistical Inference* (Cambridge: Cambridge University Press, 1965); Peter Galison, 'The Trading Zone: Coordinating Action and Belief in Modern Physics', paper presented at a conference on 'The Role of Experiment in Scientific Change' (Virginia Polytechnic Institute, 30 March – 1 April 1990). A revised version of Galison's argument will appear in his *Image and Logic: The Material Culture of Modern Physics* (1995, in press).

11. JoAnne Brown, 'Professions', in James T. Kloppenberg and Richard Wrightman Fox, *A Companion to American Thought* (New York: Blackwell, 1995, in press); Brown, 'On Professions: The Cook, the Soldier and her Mother', paper presented at the Women's Studies Seminar, Johns Hopkins University (5 October 1994); Judith R. Walkowitz, *City of Dreadful Delight: Narratives of Sexual Danger in Late-Victorian London* (Chicago, IL: The University of Chicago Press, 1992); Carolyn Steedman, 'Bimbos from Hell', *Social History*, Vol. 19 (1994), 57–67; Diana E. Long, 'Moving Reprints: A Historian Looks at Sex Research Publications of the 1930s', *Journal of the History of Medicine and Allied Sciences*, Vol. 45 (July 1990), 452–68; Philip J. Pauly, 'Summer Resort and Scientific Discipline: Woods Hole and the Structure of American Biology, 1882–1925', in R. Rainger, K. Benson and J. Malenschein (eds), *The American Development of Biology* (New Brunswick, NJ: Rutgers University Press, 1988), 121–50.

12. Harry M. Marks, 'Cortisone, 1949: A Year in the Political Life of a Drug', *Bulletin of the History of Medicine*, Vol. 66 (Fall 1992), 419–39; Marks, *Ideas as Reforms: Therapeutic Practice, 1900–1980* (unpublished PhD dissertation, MIT, 1987); Jeffrey Levi, 'Unproven AIDS Therapies: The Food and Drug Administration and DDI', in Committee to Study Biomedical Decision Making, Institute of

Medicine, *Biomedical Politics* (Washington, DC: Institute of Medicine, 1991), 9–37; Harold Edgar and David Rothman, 'New Rules for New Drugs: The Challenge of AIDS to the Regulatory Process', *Milbank Quarterly*, Vol. 68 (1990), Supplement 1, 111–42.

13. John Noble Wilford, 'Big Bang's Defenders Weigh Fudge Factor, A Blunder of Einstein's, As Fix for New Crisis', *New York Times* (1 November 1994), C1, C10. For the scientific origins of this debate, see George H. Jacoby, 'The Universe in Crisis', *Nature*, Vol. 371 (27 October 1994), 741–42, and Wendy L. Freedman et al., 'Distance to the Virgo Cluster. . .', ibid., 757–62.

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Responses and Replies (continued)

In and Out of the Petri Dish: Science and S&TS

Trevor J. Pinch

Jay Labinger's piece is most welcome in the present climate of hostile polemics from scientists directed towards Science and Technology Studies.[1] It is rare to find a practising scientist who has taken the time and trouble to comment upon our work in a thought-provoking way and, even, in the process to come up with

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a new three-part acronym for what we do. (I prefer S&TS to SCS because it keeps technology in the frame.)

I am less surprised than Labinger at the lack of participation by practising scientists in S&TS. It all takes time and usually everyone is too busy.[2] For instance, our experiences of training people with natural science backgrounds for research in S&TS (and my own experience in 'converting' from physics) is that it can take several years to make the switch. An experimental week-long seminar which we held last year at Cornell, where we engaged scientists in discussions about our work, revealed that the process, although likely to be enriching to both sides, will be lengthy. I welcome such interactions with scientists, but we should be well aware that the two enterprises – science and S&TS – may not always share the same agenda.

Understanding an issue from an S&TS viewpoint may be different from a scientist's own understanding of the issue. Jay Labinger himself provides a good example of these contrasting modes of understanding when he writes that Collins' account of replication is 'significantly out of balance'.[3] Replication for the scientist is a practical issue – how can I get this experiment to work properly?; what do I need to change in the set-up?; which aspects are essential and which are not?; and so on. But the S&TS question for Collins is not the same. What Collins is after is an understanding of the underlying social/epistemological processes which underpin such pragmatic concerns. The notion of a 'competent replication' and the theory of 'experimenters' regress' are Collins' explanation of what is going on. Knowing this will not, in most cases, help the scientist with his or her practical work. Collins' account is not so much out of balance as just a different sort of enterprise.

Such a stance often leaves scientists with a 'so what?' feeling. This is not anything to feel embarrassed about; it is in the nature of some of what we S&TSers do. It would be absurd and pretentious to think that activities like science, which take a vast amount of skill, training and competence, could be significantly impacted upon by the likes of S&TS. This is why Collins and I have expressed the position that we doubt that the main message of S&TS is for practising scientists.[4]

This, however, does not mean that S&TS and practitioners should be like ships in the night. Because scientists (and engineers) are our objects of study, clearly our work could feed back upon them – and, likewise, they have the right to comment upon our work. In such matters of science policy and the general conduct and management of science, S&TS could, and in my view, should, feed back upon science. But that feedback will not be at the level of making a direct contribution to the science, any more than it is likely that scientists will make a direct contribution to S&TS.

Because scientists are our objects of study, we have responsibilities towards them. Our primary responsibility should be that we do not misrepresent their skills. In other words, the stories we tell should at least be recognizably familiar to them, even if they are not particularly useful. In a scientific controversy we also have a responsibility to recover all the different views available. Scientists, too, have responsibilities towards us. The wilful misrepresentation of our work helps no-one. The most useful and productive relationship I see for scientists who are willing to reflect upon our work is to tell us when we get the technical details of the science wrong, and to raise informed questions about our own theory and practices.

On these very issues I found Labinger's piece to be most useful. I think he is right to criticize me for not showing how, in practice, scientists could have dealt better with the cold fusion dispute. However, I think he is mistaken in saying that we cannot provide such recommendations because we lack sufficiently detailed scientific understanding of the issues. A proper analysis of the cold fusion dispute would have to show a detailed scientific understanding of the issues. This is not 'understanding' in the sense of being able to contribute to the field, but gaining enough understanding such that we accurately represent the technical issues at stake. Building on our studies to provide some advice or guidelines, or just to offer a forum to debate such issues with scientists, would be a form of collaborative project which I would greatly welcome.

All this is rather paradoxical. What scientists think we lack – technical understanding – is actually what we pride ourselves on, but what scientists usually notice above all else is the relativism. Scientists in the end, it seems, worry most about epistemology – which is supposed to be our forte – and we worry most about the technical detail – which is supposed to be their forte. We long to be in the Petri dish, and they long to be out of it! In dealing with the hybrid world of technoscience paradoxes abound, and both scientist and S&TSer will be needed. The sooner the collaboration gets going the better.

• *NOTES*

1. Jay A. Labinger, 'Science as Culture: A View from the Petri Dish', *Social Studies of Science*, Vol. 25, No. 2 (May 1995), 285–306. In the UK, the hostile polemics stem from the book by Lewis Wolpert, *The Unnatural Nature of Science: Why Science Does Not Make (Common) Sense* (London: Faber & Faber, 1992). See Aisling Irwin, 'Sociology Row Erupts at BA', *The Times Higher Education Supplement* (London, 16 September 1994), 44, and the collection of items in ibid. (30 September 1994), 17–19. In the US, the polemic begins with Paul R. Gross and Norman Levitt, *Higher Superstition* (Baltimore, MD: Johns Hopkins University Press, 1994).
2. This may be a peculiarity of Anglo-American culture. I understand that in France the links between scientists, engineers and S&TSers around Bruno Latour's and Michel Callon's group at L'Ecole des Mines in Paris have been much closer.
3. Labinger, op. cit. note 1, 288.
4. See H.M. Collins and T.J. Pinch, *The Golem: What Everyone Should Know about Science* (Cambridge: Canto paperback, 1994).

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Responses and Replies (continued)

'Stop Talking about Science!':[1] A Response to Labinger

Alan Stockdale

Jay Labinger has posed some interesting questions regarding the relation of what he terms social and cultural studies of science

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(SCS) to science.[2] He is dissatisfied with the compartmentalization of SCS and science and the differences in viewpoint he perceives. To remedy this situation he proposes collaboration and dialogue focused on some common agenda. In general I am in agreement with these aims, although I am not convinced that there is as much compartmentalization as he assumes, or that there is not already significant dialogue between some scientists and SCSers.

The dialogue that does exist is obscured by the way the problem is framed. The relation of SCS to science is frequently conceptualized as the meeting of two monolithic entities. Although I doubt that many people believe this, debates about the relation between the two are often conducted along these lines. This is particularly true of some of the more polemical outpourings of scientists and SCSers that have appeared recently.[3] The binary framing lends itself to an extremely impoverished and polarized debate. Labinger frames the problem in a similar fashion, but more as a provocation towards expanding productive interaction. I believe that this purpose would be better served by moving to a more substantive discussion of science.

While the terms 'science' and 'SCS' are useful for general purposes, we are in danger of being bewitched by them. Labinger is aware that SCS glosses a 'widely divergent range of positions'.[4] The same point could be made about science. For this reason general discussions about science are misleading. Much of the critical power of SCSs lies in the detailed investigation of examples of scientific *work*. I emphasize work because I think a problem in some of the SCS literature – and scientists' own philosophical reflections – is that science is construed primarily as a representational activity. As a consequence, we have a rather abstract and stagnant debate about whether scientific knowledge is a social, a natural or a literary construction. Treating science as a practical activity disengages us from this more general discourse about knowledge, and forces us to focus on the details of what gets done in particular circumstances and how.

Labinger reads Pickering and Hacking as acknowledging that scientific culture is atypically uniform.[5] This reading seems doubtful, as this is exactly what ethnographies of scientific work do not demonstrate. One finds diversity in the details of what gets done and how it gets done, even where there appears to be a broad consensus of belief or a standardization of practices.[6] What emerges from ethnographies of work is a complex patchwork of interacting, changing, overlapping micro-cultures of practice. A core problem is thus how different groups, with different skills, interests and so on, come to cooperate with one another.[7] In my own work on cystic fibrosis research, I encounter scientists and medical researchers engaged in many different types of activities in different yet related settings. The order that exists is a relative one. Consensus and standardization are an endless problem to which considerable energy is devoted, but at the same time there are constant pressures towards local innovation.[8]

Labinger's problem looks different when it is reframed in terms of many worlds of practice, instead of simply two. Some of the researchers I talk to view my activities as alien, but a more common reaction is to see some connection between their own work and my interest in their work. This is not particularly surprising, as our problems and practices overlap: we are all engaged in reasoning, writing and organizing in some way or another. Recently I interviewed a senior scientist in a DNA diagnostics laboratory. She commented that she liked to talk to social scientists as it was an opportunity for her to think through problems she had to solve. She was acutely aware that she worked in a world that was simultaneously a world of matter and of social relations. Like her, I was someone moving in the vast extended network of molecules, diseases, technologies, skills, molecular biologists, clinicians, patients, regulators, ethicists, venture capitalists, nurses and so on. And part of her job, like mine, although for different purposes, was understanding differences within the network of relationships. Our two worlds intersected.

The relationship that is involved in the ethnographic research I have been discussing here has little in common with the scrutiny of a Petri dish. By its very nature, ethnography is a collaborative enterprise. The only way the research gets done is if others choose to talk to you, or let you observe and participate in their activities. Of course one can come away from this situation and write papers with little regard to the people one interacts with, but I suspect that this is unusual. Anthropological research, at least, tends to be done over long periods of time and involves a close relationship. Anthropologists who work on science seem to share their work with the people they study.[9] This does not necessarily result in a sharing of viewpoints. There is no reason why it should. The value of the relationship lies in there being a difference.

This sort of interaction tends to go unnoticed, even though it is potentially the most productive sort for all parties. It is productive because it involves a sharing of the perspectives of people who are intensely engaged in the complexities of a particular set of circumstances. I am more dubious about the contribution SCS has to make to science at the macro level because this involves a return to a vague transcendental position that misses the localization of cultural processes. There is arrogance in the assumption that SCS has found some general truth about science that should dictate policy decisions or some other general matter. On the whole, the details of SCS research run counter to grand theoretical pronouncements about the nature of science. Labinger says he is 'all for engaging with actual scientific practice'.[10] I agree.

• *NOTES*

I wish to thank Babak Razzaghe-Ashrafi for his helpful comments.

1. The title comes from Michael Lynch's *Scientific Practice and Ordinary Action: Ethnomethodology and Social Studies of Science* (Cambridge: Cambridge University Press, 1993) 315: 'Stop talking about science! Go to a laboratory – any laboratory will do – hang around for a while, listen to conversations, watch the technicians at work, ask them to explain what they do, read their notes, observe what they say when they examine data, and watch how they move equipment around!'.

2. Jay A. Labinger, 'Science as Culture: A View from the Petri Dish', *Social Studies of Science*, Vol. 25, No. 2 (May 1995), 285–306.

3. See, for example, the exchanges in *The Times Higher Education Supplement* (London, 30 September 1994), 17–19.

4. Labinger, op. cit. note 2, 287.

5. Ibid., 292.

6. See Kathleen Jordan and Michael Lynch, 'The Sociology of a Genetic Engineering Technique: Ritual and Rationality in the Performance of a "Plasmid Prep" ', in Adele Clarke and Joan Fujimura (eds), *The Right Tools for the Job: At Work in Twentieth-Century Life Sciences* (Princeton, NJ: Princeton University Press, 1992), 77–114.

7. See Joan Fujimura, 'Crafting Science: Standardized Packages, Boundary Objects, and "Translation" ', in Andrew Pickering (ed.), *Science as Culture and Practice* (Chicago, IL: The University of Chicago Press, 1992), 168–211.

8. In these respects, science is continuous with the wider cultural system of which it is a part. For a general discussion, see Ulf Hannerz, *Cultural Complexity: Studies in the Social Organization of Meaning* (New York: Columbia University Press, 1992). See also Fredrik Barth, 'The Analysis of Culture in Complex Societies', *Ethnos*, Vol. 54 (1989), 120–42, for a discussion of cultural complexity in a different context.

9. Two anthropologists who share their work with the scientists they study are Sharon Traweek and Diana Forsythe. See Traweek, 'Border Crossings: Narrative Strategies in Science Studies and among Physicists in Tsukuba Science City, Japan', in Pickering (ed.), op. cit. note 7, 429–65, and Forsythe, 'STS (Re)constructs Anthropology: A Reply to Fleck', *Social Studies of Science*, Vol. 24 (1994), 113–23.
10. Labinger, op. cit. note 2, 294.

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Responses and Replies (continued)

Out of the Petri Dish Endlessly Rocking: Reply to My Responders

Jay A. Labinger

I am pleased that my Comment has elicited such a set of (on the whole) thoughtful Responses, and also that most of the Responders appear to agree, either explicitly (especially Collins, Lynch and Stockdale) or implicitly, that science and SCS are engaged in fundamentally similar activities; that should make it easier to find common ground.[1] The main exception, Hakken,

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disagrees that STS is like science, and also stands out in calling for a radical reconstruction of 'technoscience' that does not appear to be on the agenda of any of the other Responders. I have little to say about his Response, except to express my bemusement that he objects to the joke about sociologists that I quoted as 'denigrating other scholars', but feels that charging me with 'conceit-imposed blindness' is perfectly acceptable; and to agree with him that he and I are unlikely to have much interest in collaborating.

My attack on relativism – that it is inconsistent with the unity of science in contrast to other disciplines, and hence either Nature strongly constrains scientific belief or else scientists must be somehow different from everyone else – provoked surprisingly little overt challenge. (What do I do with all the rebuttal arguments I'd been saving?) Only Fuller directly confronts my argument, by suggesting (as others have) that the difference between scientific training and that of other disciplines may be a sufficient explanation. Unfortunately that just moves the problem one step back: if scientists are not inherently different but are trained to be so, who does the training? How is it that a system of training in which 'scientists become predisposed to find certain displays of epistemic authority persuasive and others not' has taken hold and persists?[2] I would guess that somewhere in the antecedents of contemporary social science there was probably a similar tradition of training for submission to authority; why didn't it persist *there*? We have here a sociologists' regress that is at least as vicious as any experimenters' regress! Fuller seems to be granting a degree of transcendence to a social phenomenon (scientific education) in order to deny it to natural phenomena: just what Latour warns against.[3] To be fair, what Fuller really proposed is that institutional mechanisms, including training, should be examined, not that they should be assumed to be decisive. I agree that that is what SCSers should be (and are, of course) doing, and I am trying to stay open to the possibility that such a study might convince me to reconsider the force of my argument. So far I've not seen one.

Several Responders questioned whether science really is all that uniform. Discussing the unity of science is complicated by the absence of mutually acceptable terminology, which as Jasanoff notes is a strict prerequisite to fruitful cooperation. (I never said it was going to be easy!) Even the word 'science' is problematic, as Marks and Jasanoff both note. One distinction that I and several Responders sometimes fail to make is between the content of science and the process of science. Thus, when Fuller talks about the possibility of alternative sciences, does he mean content?

> Does he think that in an alternative science organisms are not made up of cells; that DNA is not the genetic material and does not code for proteins; that neurons do not conduct their messages electrically due to changes in membrane permeability?[4]

Or does he mean the 'parameters' of process that he lists in his Response? Of course, SCSers would deny that a clear-cut distinction *can* be made; Fuller wonders how much the parameters could be changed while leaving the 'essence of science' intact.[5] (Which begs the question of what is essential to science, another fundamental problem pointed out by Marks.) I should have made it clearer that my argument based on unity of science was addressed much more to content than process. I agree that in some senses scientific practice can be characterized as rather diverse. I also have no problem with the idea that history could have led to 'a science' that differs substantially from ours in Fuller's parameters. However, this falls far short of the social constructivism that I read in many SCSer positions.

Enough on epistemology (which scientists really don't much worry about, despite Pinch's tongue-in-cheek observation, unless someone else brings it up); let's turn to strategy. Several Responders express varying degrees of concern that I am trying to 'subvert their professional practice'. Marks in particular feels my Comment amounts to a universal attack on 'anyone who writes about the sciences'.[6] I have no idea what elicited that interpretation,[7] but I don't want to quarrel with him *or* his nine-year-old on this score: I don't believe, and don't think I said or even implied, that he and his fellow students of science 'use the wrong methods, ask the wrong questions, and lack the right manners'.[8] Furthermore, I am in full agreement with another common theme in the Responses – that SCS (or studies of science as Marks would broaden the discussion) addresses a wide range of topics and questions. By no means are all of them suitable for collaboration or even, as Pinch points out, of particular interest to scientists. (Certainly there is plenty that I do as a scientist that would have little interest for an SCSer!)

I *do* feel, however, that if SCSers want to engage scientists, they (some of them) need to take more account of how their work is understood – not by some ideal rational scientist (who, SCS tells us, doesn't exist anyway) but by the living, breathing, self-interest-driven human being. Collins says SSK wants to change science's *self*-image; how can you do that (to recall the old joke about the farmer and the mule) without first getting it to pay attention? If some scientists 'have a commitment to a model of reality that makes discussion difficult',[9] what is the SCS concept of *its* model of reality? Granting that 'consideration and application of various forms of relativism is part of the stock in trade of the social analysis of science'[10] (see the discussion of relativism as methodology in my Comment), would it be inaccurate to say that the dominant perception, by outsiders, of relativism in SCS is as an *ideological* commitment? Or that SCSers who are not thus committed (I assume there are some) have done little to try to change that perception?

If Collins is correct, that 'Labinger actually *warns* of the damage that talk of relativism will do',[11] it is only within the above context, and certainly not any form of threat. (I hope he didn't take it so; obviously I'm not in a position to threaten anyone, even if I wanted to, which I don't.) Consider Wolpert's comments:

> It is precisely because I believe the sociology of science to be a potentially valuable area that I am so hostile to the bizarre news it propagates . . . there is the whole area of the relation of science and the public – the public understanding of science, and the scientists' appreciation of public concerns . . . . One would have hoped to find answers to such questions from the sociologists of science. Alas, not.[12]

Jasanoff points out that there *is* a substantial body of such literature (and that she wishes I were more familiar with it, as do I);[13] obviously that isn't what Wolpert thinks of when he hears 'sociology of science'. His perception of relativism looms so large that all else vanishes. Or take Weinberg's comment at a recent meeting of the National Association of Scholars on whether there is a threat to science: 'Yes, I'm scared'.[14] If most scientists don't read SCS, what little they know about it will come second-hand from those who do; right now too many of the latter are alarmists of the Gross/Levitt school.[15] Would it not be worthwhile trying to enlist some allies as well?

I am encouraged by Fuller's acknowledgement that he feels the need for a change in rhetoric now that scientists are listening. Indeed, maybe rhetorical adjustment can take care of most of the problem. I remain unconvinced that SCS can't be done without relativism. For example, Galison (whose work Marks commends) prefaces his SCSish study of physics with an argument against social construction of knowledge;[16] clearly at least one practitioner had no need of that hypothesis! Nevertheless, I certainly don't 'rule out its discussion as a condition for cooperation'.[17] I would much rather see it treated as a potentially useful but approximate model – how far can we get with a strictly social account, and when does observation (not ideology) suggest that we need to let Nature back in? On that basis I think many scientists would be willing to 'extend their imaginative dexterity within philosophy'.[18]

Let me now turn to some specific points that arose in one or more Responses. Several (Collins, Keith, Pinch) objected to my suggestion that SCSers 'don't have sufficient *detailed* understanding of the scientific issues involved' to provide methods for dealing with controversies *on their own*.[19] I meant no aspersions: the operative word was 'detailed', in the same sense that I would lack detailed understanding of a scientific field other than my own. Collins aptly distinguishes between 'critical expertise' and 'professional expertise' in this regard.[20] My point (perhaps the main point of my Comment) is that there are situations that call for both, *working together*.

Keith's picture of self-awareness as paralyzing (see 'centipede-scientist' in my Comment) certainly has merit:[21] if we have to reflect on the foundation of *everything* we believe every time we want to use it, we'll never move. (Maybe that's why relativism is so upsetting to us?) However, the other side of self-awareness is at least equally important. Wolpert's categorization of non-scientific thinking as 'comfortable ignorance of never having considered things could be otherwise',[22] appears strikingly ironic in light of his blanket attack on SCS: what could be more counterintuitive than much of what SCSers do? To answer questions such as those Jasanoff poses, about my understandings of my own field and to what extent they are uncritical reflections of others,[23] I look to SCS for help. Stockdale's account of his interaction with a scientist is an exemplary model.[24]

I wish I had space to discuss my views on cold fusion in detail, but I'll just mention two points. First, Collins and other SCSers appear to equate belief in a *result* with belief in the investigator's competence. I don't claim 'Pons and Fleischmann (FRS) know too little science';[25] just that they got *this* wrong. Every scientist I know makes mistakes, trivial or otherwise (my own first paper was seriously wrong; fortunately I was able to correct it myself); that doesn't make them (or me, I hope) incompetent. In fact, being convinced of error is far more common than SCS appears to allow for. I strongly endorse Weinberg's comment:

> Above all, it is the scientists' experience, of being forced by experimental data or mathematical demonstration to conclude that we have been wrong about something, that gives us a sense of the objective character of our work. How often have Fuller or other science studiers had this refreshing experience?[26]

Second, I agree with Collins that 'going native' could corrupt *some* of what SCS does;[27] as I have already stated, not all of their work is appropriate for cooperation. But let me propose a thought experiment: you are an SCSer who has been looking into cold fusion, and you are invited to testify at the Congressional hearings on whether $25 million should be committed to cold fusion research.[28] What will you say? Pinch seems to agree that 'cold fusion is science as usual' is not very helpful,[29] but feels that SCS *can* make recommendations. It's not at all clear to me what SCS as constrained by Collins could contribute here.

Finally, I am not too concerned about protecting science from scandals (Lynch) or destabilization (Keith) on the part of SCS. As for the former, I support increasing public awareness of the role of science in contemporary culture from (almost) any quarter. Even if some of it is done with hostile intent, I believe that can be adequately countered. In any case, between the public's viewing science as a visibly human (warts and all) enterprise or granting it the virtually uncomprehending worship that Gross and Levitt demand, I prefer the first. As for the second, granting that the term 'destabilization' has many nuances, if one of them is keeping science from becoming static, I'm all for it. What was said last century should always be true: 'La science cherche le mouvement perpétuel. Elle l'a trouvé; c'est elle-même'.[30]

• *NOTES*

1. For the record, the complete exchange, starting with my Comment and ending with my Reply, runs as follows: Jay A. Labinger, 'Science as Culture: A View from the Petri Dish', *Social Studies of Science*, Vol. 25, No. 2 (May 1995), 285–306; H.M. Collins, 'Cooperation and the Two Cultures: Response to Lab-

inger', ibid., 306–09; Steve Fuller, 'From Pox to Pax?: Response to Labinger', ibid., 309–14; Sheila Jasanoff, 'Cooperation for What?: A View from the Sociological/ Cultural Study of Science Policy', ibid., 314–17; David Hakken, 'The Cultural Reconstruction of Science: A Response to Labinger', ibid., 317–20; William Keith, 'Response to Labinger', ibid., 321–24; Michael Lynch, 'Collaboration and Scandal: A Comment on Labinger', ibid., 324–29; Harry M. Marks, 'Other Voices: A Response to Labinger', ibid., 329–34; Trevor J. Pinch, 'In and Out of the Petri Dish: Science and S&TS', ibid., 334–37; Alan Stockdale, '"Stop Talking about Science!": A Response to Labinger', ibid., 337–41; Labinger, 'Out of the Petri Dish Endlessly Rocking: Reply to My Responders', ibid., 341–48.

2. Fuller, op. cit. note 1, 311.

3. Bruno Latour, *We Have Never Been Modern* (Cambridge, MA: Harvard University Press, 1993).

4. Lewis Wolpert, 'Response to Steve Fuller', *Social Studies of Science*, Vol. 24 (1994), 745–47, at 746.

5. We may be about to find out: see David J. Hanson, 'Republican Takeover of Congress: Uncertain Impact on Science Policy', *Chemical & Engineering News*, Vol. 72 (21 November 1994), 39–41.

6. Marks, op. cit. note 1, 330.

7. Marks gets my position on whether students of science should focus on 'paths' rather than on 'states', and whether or not Boyle is the forefather of the Hoover Company, so completely backwards that I won't worry too much about trying to figure this one out.

8. Marks, op. cit note 1, 330.

9. Collins, op. cit. note 1, 307.

10. Ibid., 308.

11. Ibid., 307.

12. Wolpert, op. cit. note 4, 745.

13. Jasanoff, op. cit. note 1, 314.

14. As quoted by Scott Heller, in 'At Conference, Conservative Scholars Lash Out at Attempts to "Delegitimize Science" ', *The Chronicle of Higher Education*, Vol. 41 (Washington, DC, 23 November 1994), A18, A20, at A20.

15. Paul R. Gross and Norman Levitt, *Higher Superstition* (Baltimore, MD: Johns Hopkins University Press, 1994).

16. See Peter Galison, *How Experiments End* (Chicago, IL: The University of Chicago Press, 1987), 10–12. For a critique, see David Bloor's review, *Social Studies of Science*, Vol. 21 (1991), 186–89.

17. Collins, op. cit. note 1, 307.

18. Ibid.

19. Labinger, 'Science as Culture', op. cit. note 1, 301.

20. H.M. Collins, in a multiple book review in *Public Understanding of Science*, Vol. 3 (1994), 323–37, at 335.

21. Keith, op. cit. note 1, 322.

22. Wolpert, op. cit. note 4, 746.

23. Jasanoff, op. cit. note 1, 315.

24. Stockdale, op. cit. note 1, 339.

25. Collins, op. cit. note 1, 308.

26. Steven Weinberg, 'Response to Steve Fuller', *Social Studies of Science*, Vol. 24 (1994), 748–50, at 750.

27. Collins, op. cit. note 1, 308.
28. See Gary Taubes, *Bad Science: The Short Life and Weird Times of Cold Fusion* (New York: Random House, 1993), 249–52.
29. Pinch, op. cit. note 1, 336.
30. Victor Hugo, cited in Jean-Marc Lévy-Leblond, 'The Mirror, the Beaker and the Touchstone, or, What Can Literature Do for Science?', *SubStance*, Vol. 71/72 (1993), 7–26, at 13.

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Responses and Replies (continued)

Co-Construction and Process: A Response to Sismondo's Classification of Constructivisms

Peter Taylor

Any classification into types can clarify our view of the whole while, at the same time, distracting our attention from hybrids and the processes by which they are formed and sustained.[1] In this light, the recent review by Sismondo, which teases out some of the multiple meanings given to the term 'construction', and his subsequent exchange with Knorr-Cetina,[2] should leave us troubled. Many of us are interested in the processes of science in the making, in which scientific theories, materials, tools, language, institutions and wider social relations are being co-constructed, and are trying to analyze the diverse 'resources' drawn upon by agents in such co-construction processes.[3] Sismondo's classification makes little space for that strand of social studies of science, focusing as it does on the type of thing being produced, not the processes of their production. Knorr-Cetina does not take issue

Social Studies of Science (SAGE, London, Thousand Oaks, CA and New Delhi), Vol. 25 (1995), 348–59