Science as Culture: A View from the Petri Dish

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• ABSTRACT
Recent work in cultural/sociological studies of science 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 cultural/sociological studies of science could have a positive impact on the practice of science, collaboration between scientist and culturalist/sociologist might well be preferred to the treatment of scientists as non-participatory laboratory specimens.

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 <u>day-to-day practice</u> of science. In the last couple of years (although they have been going 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 <u>do</u> 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 leveling, 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 responses (if any) will not be limited to the details that I have gotten 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 <u>physical</u> 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 <u>different</u> 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 <u>bewilderment</u>.[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] (My italics)

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 <u>counteract</u> 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 the mere 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] (My italics) 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 <u>all</u> roads lead to Rome, but the demonstration of an earlier fork is hardly proof that where we have ended up is 'merely' an historical accident. There is a revealing passage in <u>Leviathan and the Air-Pump</u>, 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 <u>state</u> 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 <u>path</u> that is followed from reactants to products. Kinetics, in contrast, <u>are</u> 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 <u>independent</u> 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 <u>one</u> 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 overemphasis 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] (My italics)

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 <u>shouldn't</u> concern themselves with SCS, as seen earlier. Others might well place the responsibility on the scientists' side; there may be <u>nothing</u> 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 <u>against</u> 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 <u>par excellence</u>; nominal results straight from the computer are almost invariably taken as gospel. Not a small number of incorrect papers have 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 artifact 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 <u>detailed</u> 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 <u>do</u> 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 and 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.

[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 and 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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