

of Africa and Australia, often show mysterious straight lines of a kind not found in most other maps. (There are no straight lines in nature...) The only way in which we can understand these straight lines is to relate them to the history of particular treaties and, beyond that, to the colonial system which produced them. But treaties are not things which can be explained in terms of physiography or vegetation or electrons. Nor can they be explained in terms of the neurones of the people who make them, any more than vice versa. The only way to explain treaties is by thinking about human history and human purposes. And this is talk which cuts into the cosmic cake – so to speak – at quite a different angle.

Understanding the relation of history to physiography is not (then) like relating two places on the same map. It involves relating two maps – two different ways of thinking – to one another. And when we consider problems about how consciousness relates to the physical sciences, and more generally to the rest of life, that is what we have to do.

This work is philosophical. But that does not mean that it has to be left to the philosophers. It is a co-operative venture, to which all citizens of the intellectual republic can contribute. And it is a much more interesting and useful occupation for them than the wars recommended by competitive imperialism.

NOTES

- [1] His impressive account of this search in the opening chapters of the *Discourse on Method* is no doubt true enough in itself but it is by no means the whole truth.
- [2] Ziman, 1995: 65–82. The map metaphor is discussed at length by Stephen Toulmin (Toulmin, 1953) and by John Ziman (Ziman, 1978). Indeed, this metaphor has been put forward independently without special comment by a number of philosophers, starting perhaps with Wittgenstein.

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See, for instance, his image of a representational grid which might just as well be triangular or hexagonal as square *Tractatus Logico-philosophicus* (Wittgenstein, 1961: §6.341), his discussion of alternative descriptions in *Philosophical Investigations* (Wittgenstein, 1963: §24), his remark that 'A philosophical problem has the form "I don't know my way about"' (Wittgenstein, 1963: §123) and above all his image of language as an ancient city whose shape cannot be explained as conforming to any single pattern (Wittgenstein, 1963: §18). Images such as these resemble map-talk in having the advantage that they do not require us to talk of truth and falsity. We do not describe grids or maps as true or false. Since, however, maps undoubtedly are more or less accurate, more or less correct or misleading, this talk still keeps to the standards of everyday realism. On the wider question of what guarantees we can reasonably expect here, see *On Certainty* (Wittgenstein, 1969).

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Hilary Rose

Debating Science Studies in the Age of the Sound Bite

Cultural struggles in Britain have been privileged to date, in that Gross and Levitt's *Higher Superstition: The Academic Left and its Quarrels with Science* (1994) has, except in specialist circles, received rather little attention. It has, to borrow Margaret Thatcher's useful metaphor, been denied the 'oxygen of publicity'. The British version of the culture wars so evidently raging in the US has been less a generalised attack than a series of assaults – two of the most conspicuous being on psychoanalysis and on the sociology of scientific knowledge – known to its practitioners as SSK. Perhaps this transatlantic difference derives from the scale of the lurch to the right in the US, by comparison with the slow implosion of the official Right in the UK accompanied by the adoption of an increasingly centre-right programme by the Labour party. By contrast with the US situation the attack on both psychoanalysis and SSK are in Britain not in any obvious sense attacks by the Right on the Left, instead the attack is mounted by self identified right thinking persons (so far men only), speaking on behalf of

'Science'. From the standpoint of this single entity – which I shall describe with a capital S to distinguish it from the heterogeneity of the sciences and their methods – psychoanalysis is criticised for not being Scientific and SSK for taking a constructivist and repudiating a realist theory of natural science.

In these wars, the self appointed defenders of Science are seeking to police the boundaries of knowledge, and to resurrect canonical knowledge of nature, against the attempts of the Others (who potentially include feminists, anti-racists, psychoanalysts, post-colonialists, leftists, multi-culturalists, relativists, postmodernists, etc.) to extend, transform or maybe even dissolve the boundaries between the privileged truth claims of Science and other knowledges. But first, just because any of us may find ourselves among the Others under attack, this commonality may not automatically generate bonds of solidarity between this 'us'. My enemy's enemy is – only perhaps – my friend.

For example some postmodernists have

claimed an alliance between their project and that of the feminisms, while numbers of feminists have claimed that postmodernism depoliticises and weakens feminism. In this vein Jane Flax (1993) writes of her distress as a feminist theorist at a bruising attack on her postmodernism by a feminist audience. Conversely, from a critical realist standpoint, the editor of *Race and Class*, A. N. Sivanandan (1995), criticises postmodernism's attempt both to rewrite a new non racialised subjectivity for white working class youth and to clear ground for this new approach by deploring 'moralising anti-racists.' In his judgement the literary turn may diminish discrimination, but cannot meet the challenge of racist violence on the streets.

In such times alliances between 'us' are likely to be provisional and built; the innocent appeals of an old ungendered and 'unraced' solidarity of class are no longer available; gone too is the 'innocence' of universal sisterhood. Instead, while not abandoning commonality, recognition of complexity and the need to pay meticulous attention to context, not least our own, are the name of the game. Positioning myself within a particular reading of the feminist critiques of science – and they are plural and diverse – I want to explore the debate around what was spoken of as "the social standing of science" Who is speaking is as important as what is being said and in what location the debates take place.

Importing Problems

One of the problems for the English speaking inhabitants of medium sized offshore island, is that belonging to an increasingly global language system, generates some real negativities – as well as possibilities. It assists the mediatrixes of key weeklies in fanning cultural conflicts by importing cultural conservatives from the US scene – where they seem to grow with distressing abundance – into the UK, as well as enthusiastically reporting our own indigenous talent. Thus not having been particularly

attentive to feminism the anti- and post-feminist backlash in which women claim notoriety if not fame by rubbishing the work of feminists, is received by the media, with enthusiasm.

While debates about psychoanalysis sit somewhere at the edge of British cultural life, those about Science are central. The most recent exchanges around the nature of scientific enquiry, have been grumbling away in the background for some time, are in many ways an extension of the old assumption within the Anglo-Saxon tradition that Science equals the natural sciences. The concept of *Wissenschaft*, which holds all of systemic inquiry together in other cultural traditions, is simply not available in an Anglo-Saxon context. This background grumble took centre stage with a public debate held at the 1994 meeting of the British Association for the Advancement of Science between Harry Collins, a leading sociologist of science, and developmental biologist Lewis Wolpert, Chairman of the Royal Society's Committee on the Public Understanding of Science (COPUS). Collins, together with fellow sociologist Trevor Pinch, had published *The Golem: What Everyone Should Know about Science* (1994), and Wolpert, *The Unnatural Nature of Science: Science Does Not Make (Common) Sense* (1992). These rival and polarised texts claim to extend the public understanding of science, the former by claiming that science is socially constructed, the latter by its insistence that scientific knowledge is unitary, with unique truth claims derived from its capacity to hold a mirror to nature visible to its illuminati.

The confrontational debate made a considerable impression on those who witnessed it as a vituperative and unpleasant event. To the extent that considerable sections of the academy have given up those highly adversarial and masculinist exchanges, long criticised in feminist circles, as fostering the non meeting of minds (Moulton 1983), the debate was a return to a past from which we are not yet free.

The quarrel was then transferred to the pages of the Times Higher Education

Supplement (THES). The options for those sociologists who disagreed with Collins or those biologists who disagreed with Wolpert, to say nothing of those who as feminists thought both were unreconstructedly macho in thought and style, was shrivelled down to the binary choice of either Collins or Wolpert. Within a few months the meeting was re-enacted at Durham (financially supported by THES who saw they were onto a fine controversy) and although Collins would not participate in a repetition of the earlier savagery, preferring to expound his views on television, Wolpert was very much present. Despite there being many more contributors so that the adversarial structure was less acute, the meeting still had many unpleasant exchanges.

PUS: the Contested Turf

The ground under contest is the Public Understanding of Science (PUS) which formed the title of a report produced in 1985 by a Committee of the Royal Society chaired by Walter Bodmer. The problem, as perceived by this elite grouping, was that science was losing its popularity. This sensitivity to loss of trust and thence anticipated loss of support is being played out, not yet in the UK, but in the USA as the centre of the global research system, in terms of the one third cut to the science budget currently on the floor of the House. The Bodmer report sought to overcome this by proposing that a more scientifically literate public would be more supportive.

In response the Royal Society established the Committee for the Public Understanding of Science (COPUS) under Bodmer's chairmanship, with the mission of encouraging scientists to assist the public in understanding science. A whole battery of science festivals, hands-on experiences for children, prizes for authors of books which enhance PUS, Science Engineering and Technology weeks for schoolchildren and research awareness for industry were brought into being. One central element in

this package is the advocacy of science as "fun", fun being ingredient X which will render science attractive to the young who will then wish to study it and to the citizen who will be willing to continue financing it. Scientists have grumbled that this ideology of fun misrepresents laboratory life. Those of us outside science with miserable memories of authoritarian teaching styles and rote learning, followed by writing up experiments which had already invariably failed in terms of what should have happened, the attempt to be interesting is far from unwelcome.

However the new approaches are conspicuously uneven. Within the museum sector they have been part of the Thatcherian cultural revolution which abandoned Victorian pedagogy and put the customer and the customer's choice in its place. While some of the new representations of science are intellectually ambitious, such as the Wellcome's attempt to set the development of biomedicine in social context, we also have the paraphernalia of the gift shop. There have been some culture shocks. Those with a lingering commitment to the museum as a site of scholarship, were, for example, considerably disturbed to learn that the curatorial staff of the Natural History Museum had been despatched to Disneyland by a new Director.

Whatever the pluses and minuses of the new representations of science, the central Bodmer credo that the increased public understanding of science bred trust and thereby support, was indeed a matter of faith. A number of both quantitative (Durant *et al.* 1989) and qualitative (Irwin and Wynne 1996) social science studies have pointed to the association between increased scientific literacy and increased scepticism about science. However, the relationship between the social sciences and the natural sciences in Britain is far from one of mutual respect. The programme of social science studies of the diverse publics understanding of science, have had no impact on the COPUS credo. Indeed its committee members were conspicuous largely for their absence when the Economic and Social Research Council

show cased their PUS programme at the Science Museum. COPUS relaunched itself in 1995 into its second decade with a continuing commitment to one way communication. The job of the public is to listen and learn.

Communicating science to the public through the media is a long and honourable tradition, pioneered in the 1940s by the marxist, geneticist and essayist, JBS Haldane, with his regular column in the *Daily Worker*. Before that the lecture hall was the chief form. In the second half of the twentieth century this task of communication has become largely the task of professional science writers, who have extended it into popular science magazines (*New Scientist*, *Focus*), the broadsheets, and television.

Recruiting scientists themselves into public understanding of science activities as an officially sanctioned activity is relatively new in Britain. Radical and feminist scientists have long been at this game, but from a critical standpoint. What remains peculiarly difficult for the British is to move beyond communication as monologue. Unlike some other countries such as Denmark, which has a strongly democratic approach to technical decision making and takes for granted dialogue between the producers of, for example new biotechnology, and the public as end users, Britain, apart from one cautious experiment at a biotech consensus forum, remains incredibly apprehensive that a non technical expert should have an opinion and start talking as well as listening. Such anxiety has two origins: the first is the pathological commitment to a culture of secrecy endemic to British society; the second is specific to scientists and reflects a deep anxiety about letting others talk about scientific matters.

Science is one of the few cultural activities where the practitioners have always sought to stay in charge of the story about science. Despite rather few practising scientists in the Anglo-Saxon context having any training in the history and philosophy of science, there is more than a whiff of an ideology of the authority of the ultimate expert who is alone qualified to say what it and what is not

Science. Historians and philosophers, it goes, are all very well in their place, but the ill concealed subtext is that place lies in being deferential to the natural scientists. The remembered happy marriage is of that relation of mutual admiration between the distinguished immunologist Sir Peter Medawar and the philosopher of science Sir Karl Popper.

Twin groupings born in the late sixties, one outside the academy – the radical science movement – and one within – the new post-Kuhnian social studies of science – broke with this deference. Science, in this analysis, was not outside culture, independent, uncontaminated by the social, and ‘pure’, but was itself an integral part of culture. The social studies of science, whether radical, feminist or mainstream understood themselves as having a more or less critical and no longer deferential relationship to science.

Struggles over the social standing of science are not confined to visible debates; the strategic positioning of key actors in the committees which bring together Science and Society is far from accidental. Thus within the life science one of the fiercest recent debates, which has mobilised particularly, but not only feminists, concerns the potent link between the new reproductive technologies and the new genetics with their claims to diagnostics and therapy. Together these threaten to determine not only which fetus shall survive but who shall be mother. As the disciplines of embryology and genetics have come under intense public scrutiny, elite members of these disciplines (elite in the UK corresponds substantially with being a Fellow of the Royal Society (F.R.S)) have mobilised as very visible players within the PUS discourse. There is an intense concern that the public gets the right understanding of genetics and embryology, so that those scientists who successfully enter the PUS discourse and shape it in ways acceptable to the research community becomes the new heroines and heroes.

Thus following the “test tube baby” de-

bates of the eighties, the Warnock Committee was established by the Government to consider the ethical issues raised by human embryology and to suggest any necessary regulation. As a distinguished embryologist, F.R.S and experienced committee woman not least in working with the Royal College of Gynaecologists on the artificial insemination issue, Anne McLaren was an ideal member member. Even the Thatcher administration recognised that this was a woman’s issue and in addition to the predictable clutch of theological ethicists, scientists etc, actually allocated half the membership to women.

McLaren herself, perhaps because of her radical political background but also her gender, was rather different from her male scientific counterparts, in that she was manifestly able to engage in dialogue with various different publics, not least with feminists. The location of such exchanges, such as the popular 1980s Communist Summer University were far from obviously friendly to science. She advocated a solution which both permitted research to proceed and simultaneously allayed mainstream anxieties. In large measure it was this position which was eventually adopted by the majority of the Committee. In recognition of her work McLaren was awarded the Royal Society’s Faraday Medal in the Public Understanding of Science.

That the first chair of COPUS was the geneticist and leading figure within the International Human Genome Organisation, Walter Bodmer (also awarded a Faraday medal for his work in promoting the understanding of the new genetics) and that the second was Lewis Wolpert is expressive of this desire of scientists from disciplines under criticism to stay, at least strategically located, if not in charge of the public understanding science. In the case of Wolpert with his very public and hostile views about the sociology of science, there is a further dimension in that he is also chair of the Medical Research Council’s (MRC) grant-giving committee on the Ethical Legal and Social Aspects (ELSA) of the new

genetics.

This very British strategy of natural scientists not only in staying in detailed charge of their own discourse but in seeking to control the discourses of other disciplines about PUS is not simply misconceived, but that it will for good reason increase distrust. Indeed the recent House of Commons Select Committee report on Human Genetics has taken on the argument that ELSA should not be under the exclusive control of the MRC and has recommended that it is jointly managed with the Economic and Social Research Council. Thus the struggles around the public understanding of science go on at a number of levels and places, not only in head-on confrontation as in *Golem versus Unnatural Science*.

Golem Science

Drawing on the mythical Jewish figure of the Golem, Collins and Pinch (C&P) set out to explain the political role of science and technology. For them science is a Golem, wobbling in their text between gender neutrality and masculinity. I have many sympathies with C&P’s political project, which is to increase the public understanding “about” science rather than merely to increase the public understanding “of” science, in the COPUS model. I might use different language but we share a common sociological impulse that people are expert in their own lives and that a desirable cultural and political objective is to move to a dialogue between the several publics and the sciences.

Their book discusses eight case studies of scientific controversies, from the worm runners (a sixties claim that tissue from the brain of trained worms could be injected into the bodies of untrained worms and that memory was thus transferred) through experiments to prove relativity, to cold fusion theorists. Set plain the C&P thesis is that “the scientific community transmutes the clumsy antics of Golem Science into a neat and tidy myth”.

For me the central theoretical problem is the lack of reflexivity in C&P's sociological stance. Thus while they show us the scientists as actively socially constructing their "neat and tidy myths", we are invited to believe that their own sociological accounts of science are real, and not subject to the same analysis. They tell us their subjects construct science, while they offer one true sociological story which every one should know. Arguably they thus reproduce for sociology the authoritarian scientific voice they criticise. Worse their tactless use of the word, "myth", to describe the slow patient work of laboratory scientists, pretty much forecloses the possibility of dialogue with them. For myth, used in what appears to be a vague everyday sense here, includes any narrative having 'fictitious' or 'imaginary' elements.

Sociobiologist Richard Dawkins is quick to pick up the myth word and to attack relentlessly. "It is often thought clever to say that science is no more than a modern origin myth. The Jews had their Adam and Eve, etc.... What is evolution some smart people say, but our modern equivalent of gods and epic heroes, neither better nor worse, neither truer nor falsier... There is a fashionable saloon philosophy called cultural relativism which holds in its extreme forms than science has no more claim to truth than tribal myth: science is just a mythology favoured by our modern Western tribe." (Dawkins 1994: 31)

Collins, and for that matter Steve Fuller's claim, that SSK is only methodologically relativist, does not quite match up to the ontological slippage which is taking place in C&P school of SSK which renders it vulnerable to Dawkins' attack. A more sophisticated – and politically more sensitive – account of science as stories about nature is also used by Donna Haraway in *Primate Visions* (1989), but as a fully reflexive scholar, she has the grace to acknowledge that her accounts are also stories. Further, like most feminists working in this area she is sensitive to the need for natural scientists to be realists in order to construct their accounts of nature. Contemporary SSK in all

its variants recognises that a deconstructionist would have a hard time in a lab but they handle this with varying degrees of sensitivity. But the more general point that I want to make by contrasting C&P with Haraway, whose alleged postmodernism has precipitated a conference of mainly environmentalist scientists to discuss its implications for the protection of nature (Soule and Lease 1995), is that there is a tremendous range of positions available within the social studies of science.

What separates the feminists from the mainstream professionals is that feminists are committed to building alliances, not least with other feminists, and consequently are very sensitive to the delicacy of the relationship between the feminist critics of science and feminists in science. While C&P deliberately rule out the possibility of their scientist subjects entering into negotiation with their social realist account of science, feminists are willing to listen to those voices, and some want to include Nature herself as part of the actor network. Apparent oxymorons bind the feminist discourse: feminist science, feminist empiricism, feminist objectivity; feminist rationality: these mark out a normative discourse and delineate it from the deliberately non normative discourse of mainstream SSK. Key figures in the production of these oxymorons would include: Lynda Birke, Patricia Hill Collins, Donna Haraway, Sandra Harding, Nancy Hartsock, Ruth Hubbard, Helen Longino, Elizabeth Potter, Hilary Rose, and Vandana Shiva. There is much more attention among the feminisms to choosing language which is likely to foster dialogue as against that which is likely to foreclose it. By contrast C&P's sociology of science shares more than a little of the same clumsy Golem-like qualities of the scientific Golem they want us all to know about.

There are always problems with popularising research and in using case studies to resolve truth issues. Thus I read the original case study on, for example, the worm runners, rather differently, and think that C&P as popularisers go rather beyond

the claims being made. The worm runners' thesis had hilarious and very obvious social possibilities, not least for educational practice – should we eat or mainline the professors' brains? As such it produced lots of media discussion and some wonderful cartoons. However, its reception within the biological community, as against its presence in the popular scientific weeklies always looking for a controversial story to maintain readerships scarcely matches the notion of the establishment of 'a neat and tidy scientific myth'. Indeed a conversation with the sociologist David Travis, who carried out the original research, confirmed that practically all the neuroscientists he interviewed were deeply sceptical about the worm-runners' claims, both for theoretical reasons within biological discourse and because they did not trust the accounts of the experimental procedures.

C&P assert that both the psychologist McConnell's worm and the pharmacologist Ungar's later rat-based claims for memory transfer has not been disproved on what they speak of as 'decisive technical evidence' (C&P 1993: 25). But the very notion of decisive technical evidence begs the question, for it sets aside the possibility of theoretical biological criticism. Although the unrepeatable experiment plays a part in the resistance to the establishment of a scientific fact, not least when carried out with the full theatre of multi-authorship or multi-location, the production of a fact is not convertible to one decisive moment but requires a cumulation of evidence. Hence it is entirely possible for me to take considerable pleasure in their own meticulous empirical work showing the processes of constructing scientific facts at the micro level, while still finding myself uncompelled by their theoretical project.

C&P's case studies, as a form of renewed internalism, also set to one side the larger context in which scientific claims are made. Both the 1971 analytic predictions by Jerry Ravetz of the nascent problems of industrialised science and Dorothy Nelkin's *Selling Science* (1987) remind us of the lure

of press releases in a grant funded research system- especially where there might be potential commercial developments. While some have already gone down the 'smart drug' claim drain, the more modest likelihood, increasingly under discussion among neuroscientists, is the possibility of finding a way to intervene chemically to slow down the neural degeneration associated with Alzheimer's. The point I am driving at, which C&P seem to allude to in some sentences where they acknowledge expertise, but exclude in crucial others as when they sum up Golem science as 'myth making', is that science is concerned with the test of performativity. As a cultural project modern western science has never been content only to represent nature, but to act upon it; hence performativity is not a criterion that can be lightly set aside. It is this which enables Richard Dawkins (1995:32) to explode with: "Show me a cultural relativist at thirty thousand feet and I will show you a hypocrite. Airplanes built according to scientific principles work."

The understanding that science is socially shaped finds increasingly wide cultural acceptance. Thus the European Union Framework Programme for Targeted Social and Economic Research (1995) speaks of the 'social shaping of science and technology', but it does not follow that because scientific knowledge is socially shaped that it is interchangeable with myth or even stories. Nor is this current debate quite so new as there was a recent precursor in 'the science is social relations debate' in the mid seventies radical science movement (Rose 1994: 260; Putilnik 1995:22–43). What is called the good science / bad science debate remains – just as it does in sociology or for that matter plumbing and dressmaking. The trouble with Collins is that where fringe science is concerned, it is as if he wants to make heroes (never heroines) insisting that because they have the tools of the trade and appear to follow the procedures, no-one is allowed to say that the plumbing leaks at every joint. Ian Hacking suggests that the social constructionists focus on the early

stages in the construction of scientific facts, but that they leave the scene too early so that we are "left with a feeling of absolute contingency. They give us little sense of what holds the constructions together beyond the networks of the moment, abetted by human complacency" (Hacking 1992:131).

Indeed Collins has appointed himself as the defender of fringe science. When the biologist Jacques Benveniste made his homeopathic claims and received the full *Nature* treatment – the editor John Maddox plus a magician and a scientific fraud buster visited Benveniste's Paris laboratory to witness a replication experiment. Collins claimed that this was epistemology in action and that replication could not prove or disprove the claim. But while most biologists were as sceptical as *Nature* about the claim, many thought that *Nature's* style was bullying and offensive.

C&P insist that they are concerned with the political role of science and technology; well so am I. But their refusal to acknowledge the now substantial body of feminist scholarship which has explored the sexualised, and racialised representations of nature constructed by an androcentric and eurocentric science, articulating the social processes through which women have been excluded from science and how, is also political. Their construction of the political is pretty much synonymous with Sandra Harding's concept of "weak reflexivity" as they persistently restrict their analyses to the "micro processes of the laboratory explicitly excluding race, gender and class relations" (Harding 1991: 162). For genuinely smart field workers, their inability to see such social relations at work is quite an achievement.

Inability to see feminist research is not exclusive to C&P, but has been a general weakness of British mainstream sociology of science, and would include the work of Mulkay, Bloor, Barnes, Woolgar, and Ashmore. Nor did Latour as the most authoritative voice across the Channel do any better until his enthusiastic appreciation of Haraway (Latour 1993). In his work on reproductive technology Mulkay begins to

cite a small number of feminist texts (Mulkay 1995) and Woolgar too is beginning to acknowledge feminism in relationship to constructivism (Grint & Woolgar 1995).

Until these hints of conversion this highly professionalised grouping has been hostile to normative critics of science, dealing with them by erasure and silence. By contrast *Science*, the voice of US science has for some years had an annual issue devoted to feminist debates in science, and even *Nature*, its UK counterpart, reviewed Evelyn Fox Keller's biography of Barbara McClintock, while the lead British SSK journal *Social Studies of Science* did not. Instead their sociology, despite their claim to be interested in the political role of science, has chosen to mirror science's claim of being a gender-free culture. C&P's radical impulse concerned with the political role of science and technology is so hedged in by a professionalised and pale male constructions of the political that its capacity to build alliances with other critics, whether within or without the sciences, is severely restricted. Arguably the unquestioned success of this highly professionalised British approach to SSK is beginning to run out (Knorr Cetina 1993). There are hints that more normative approaches such as those of feminism look to be more fruitful.

More optimistically this exclusivity of mainstream SSK does not imply that the Others cannot borrow their intellectual tools. These will unquestionably need adapting, as Audre Lorde's epigrammatic question "can the master's tools tear down the master's house" does not go away. There are also encouraging cracks in the masculine culture of science studies, as feminist research students wishing to work on the social studies of science and technology pressure the departments from below. Gradually the departments are beginning to hire feminists and the possibility of whole new conversations comes into existence. My reading of *The Golem* is that it contributes to these new conversations almost despite itself.

Unnatural Science

The Unnatural Nature of Science is, for anyone with a more than cursory familiarity with current philosophy of science, an essay in glassy mirror ideology (GMI). It sees no gap between the word and the thing. GMIs, such as Wolpert, simply do not appreciate the lethal criticism of the mirror theory offered by the Picasso joke. A man troubled by Picasso's portraits with eyes facing both frontwards and sideways asks the painter why he does not paint realist pictures. To make his point clear the man takes out a photograph of his wife and says: "Like this". The artist looks at the photo and mildly observes: "Small, isn't she?"

Scientists, unlike postmodernists and other ontological relativists, believe that there is something 'out there' and that by following the practices of science they can represent that thingyness faithfully. This realism, while a crucial belief for everyday laboratory practice, is not transferable into a theory of representation with the simplicity of the man with the photograph. Such unreconstructed mirror theory would have a hard time within the philosophy of science and an even harder time within the new ethnographic accounts of science classically represented by Latour and Woolgar's *Laboratory Life* (1986). Many laboratory scientists report their pleasure in this book as a meticulous mapping of the socio – technical process through which scientists take the inscriptions emanating from their equipment and gradually turn them into scientific facts. They acknowledge that persuading other scientists working in the area that these are the only possible interpretations of the inscriptions – that these are indeed the new facts – is at once both technical and deeply political. But Latour and these scientists, both know that while science is always social, it is not only the social which writes the science (Latour 1993).

Numbers of scientist contributors to the discussions in both THES and at Durham were entirely unalarmed by the concept of science as a social construct, and showed no special discomfort with the idea that

science is not independent of the culture in which it is produced. Robert Banks as a biologist, observed that "Biology is grossly biased towards those organisms which closely resemble ourselves, or which irritate us, like *ecoli*". But equally natural scientists insist that science is also not independent of observation and experiment on the natural world. By contrast the GMIs seem to need their Science to act with a more god like cultural certainty, thus perhaps it is not by chance that two of the more influential voices to support Wolpert are committed to militant atheism with a positively nineteenth century fervour. The elision between science and social progress possible in that century dissolved in the between C. P. Snow writing rather more than fifty years ago, saw scientists as "the men with the future in their bones". That certainty about either scientists or the future was lost at Nagasaki and Hiroshima which came, and not only for Robert Oppenheimer, to represent the scientists knowing sin. GMIs may try to dismiss that learnt cultural distrust of science, but dismissal and heroic nostalgia for an innocent past cannot help in our present cultural uncertainties.

Wolpert takes the localised belief of every scientist in the laboratory that there is one right answer for every phenomenon, and turns it into a universal precept. He makes no space for the different discourses of the different sciences, for the possibility that an explanation of the same phenomenon within biophysics is likely to be very different from that within biochemistry, to say nothing of the physicists' long-standing capacity to accept both wave and particle theory. For that matter that the account of say 'money' will be very different in the discourses of anthropology or economics as against that of the physical sciences.

His central argument is that modern science has strongly defined boundaries; that it is unique and unitary as a way of knowing nature; that its roots lie entirely in ancient Greece; and that it is radically different from something he speaks of as common sense. He thus sets aside, or is unaware of,

historical accounts such as that of Martin Bernal (1988) who is concerned with the black African roots of Athenian science, and dismisses as technology or trivial the ethnosciences, whether those of the Chinese, the Egyptian, Indian, Islam or the Mayan – to name but a few of the many seeking attention. At Durham, sociologist of science Mammo Muchie, sought to bring third world critiques of science into the discussion, arguing that because modern Western science is hegemonic, it appears as natural and universal. Its achievement is to appear as a culture of no culture. In similar vein to the feminist arguments I was trying to make, Muchie spoke of the exclusion of emotion and ethics from the construction of rationality. Against such arguments for the possibility of Other more localised and more environmentally and social responsible sciences: Wolpert insists that there is only Monoscience.

Science he claims, is unnatural knowledge in that many of its truths run counter to everyday beliefs – for instance that the sun goes round the earth or that heavy bodies fall faster than lighter ones. However, he never tries to define what he means by common sense, or to consider whether it too is a culturally relativist concept, that today's common sense is merely yesterday's 'good science'. Or by common sense does he want to invoke the counterintuitive, which is surely the stance of every systematic approach to knowledge from the arts to the sciences and by no means the unique property of any single one.

As a sociologist I would want to argue that not least because we live in a deeply scientific and technological culture, 'lay' people, and outside our narrow expertises we are all 'lay', pick up particular areas of science, typically those which are important or have some special interest for them. Often people do this without claiming that their knowledge is 'science' but instead speak modestly of 'hard facts' or 'reliable knowledge'. Thus in my own PUS research on people with a cholesterol genetic disorder, most of them knew more about saturated,

poly and unsaturated fats than was asked in a parallel quantitative study ascertaining public levels of scientific literacy. In the same series of sociological studies of PUS, Wynne's Cumbrian sheep farmers rapidly acquired a richer appreciation of radiation in the food chain than the Ministry of Food and Agriculture scientists (Irwin and Wynne 1996). For that matter effective natural scientist PUS practitioners perceive their publics as having tacit knowledge of say probability theory, or of biomedicine, but also recognise that as non scientists, they often do not equate their knowledge with the formal categories of mathematics or science. The practitioners use this perception as a building block for their pedagogy. Wolpert's insistence on the sharp line between science and common sense is thus hard to reconcile with his commitment to COPUS. He also sets aside scientists' ownadage that 'yesterday's science is today's common sense and tomorrow's nonsense'.

To return to the debate which followed the original Collins/Wolpert clash, other leading scientific figures strongly supporting Wolpert included Richard Dawkins – recently appointed to a new, privately funded Chair in the Public Understanding of Science at Oxford, and the physical chemist Peter Atkins. The three were as one in their contempt for SSK; as Dawkins put it, it is just 'Chic drivel'. Wolpert's view of the sociology of science is matchingly intolerant; 'I've never come across anything that wasn't either obvious, trivial or wrong. I think they have made zero contribution.' Atkins resists the very idea of science as a social construct: "The universal character of science, by which I mean its independence of lasting national, racial and religious and political influences, must argue strongly against science as a social construct" (Atkins 1994).

At one level I cannot see why C&P and the mainstream SSK in Britain are so under attack for they never question, as feminists and radicals do, the larger political role of scientific knowledge such as sociobiology, or for that matter the new genetics. Sociobiology's endorsement of rape,

polygamy, male violence, male dominance, etc. over the last two decades surely merits criticism both from within their own canon and without.

For example after a passage about genes reproducing Dawkins continues: "The world is full of organisms that have what it takes to become ancestors". As a sociologist interested in biology I might be sceptical, but I would have to leave effective critical analysis to a biologist. For good reason I would look to biologists willing to enter normative debate such as: Fausto Sterling, Gould, Hubbard, Lewontin, and Rose. But when Dawkins goes on to make, what appears to him to be a self evident claim about the social: "A body that actively works as if it is trying to become an ancestor... that is why we love life and love sex and love children" (Dawkins 1994:2), then social scientists need to engage. While the cadences of the sentence flow easily, if this purports to be a realist account of the social we have surely entered the sociological counterpart of the *Hello* magazine. Does such crude biological reductionism in its cheerful sentimental universality purport to explain everyday life in Oxford, let alone mass rape and genocide in Bosnia?

But the business it seems of mediatics on this occasion was to promote adversarial debate about Science as a monolithic entity which minimised the possibility of any complexity or any interchange of views, positions merely became more entrenched. The psychological need of these GMIs to speak for all of Science with absolute ontological certainty and not to talk in detail about the messier more provisional discourse of any particular science points to their unease. Indeed their resistance to talking about their own experiments and observations, but issuing grandiloquent claims about Science is a hallmark of their style.

Occasionally such adversarial displays eased but only with a significant effort such as that made by sociologist Michael Lynch. At the Durham meeting he made a close textual analysis of *The Unnatural Nature*

which simply left its author excusing his assertions, explaining that he was only an amateur in the history and philosophy of science with Lynch saying "why don't you talk to us about embryology then. That could be really interesting". For what Lynch had shown was that Wolpert made assertions without evidence, invoked a common sense concept of realism and had confused a scientific grasp of the issues with a scientist's grasp of the issues.

But adversariality was built into the meeting so even this more scholarly critical exchange did not stop Wolpert denouncing the sociology of science lock stock and barrel. My hunch is that outside such adversarial arenas, the full blown glassy mirror ideologues (GMIs) are relatively isolated and that there is a widespread cultural understanding that science is a human activity and as such is socially shaped, in consequence both the boundaries and the nature of science are continuously subject to change over time and place. Nonetheless the GMIs do command a considerable amount of media and other cultural space and cannot be ignored. The question is how to cope with them, to me it seemed that Lynch's approach of patient public critical opposition was exemplary, even if it was transient in its influence on the author.

The media itself is not a bystander in this, it promotes and feeds off these vicious and unproductive soundbite exchanges. It makes little space for thoughtful analysis. Instead it has valorised intolerance, ignorance, and a plurality of authoritarian Moses each coming down from the mountain with holy writ. Worst of all the stagey culture wars get in the way of quieter and more serious arguments as to whether postmodernism is depoliticising, whether we can give up the truth claims of science and is there still the possibility of a limited conception of objectivity, and whether and how the abstract rationality of science can be replaced by a new socially and environmentally responsible concept of rationality. Should the gently squabbling 'we'- which includes only a handful of natural

scientists – be endeavouring to engage in dialogue with many more, admitting the scientist as collaborator rather than as object? Labinger's (1995) plea from the Petri dish surely deserves treating with respect.

Or should we set aside these debates which are less than productive, and try to examine the new ideas about the changing production system of knowledge (Gibbons *et al.* 1994) so that it is moving towards 'post academic science' (Ziman 1995). Surely if there are structural changes taking place in the production of knowledge – and without endorsing either or both of Gibbons or Ziman's particular analyses – there are enough changes which have taken place and are continuing to command our analytic interest, those who work in the social studies of science following what ever approach surely have a common concern to reflect on what such changes mean for the production of our knowledges as well as those of the scientists.

At my most optimistic, I read these changes as perhaps opening the possibility of many new actors entering the production system of knowledge, which could indeed include those Others historically excluded by modern western science – not least Nature herself. If the old Science and even the sciences have lost public trust, and that there is in consequence a clear political danger to the public support of systematic knowledge, then the restoration project of the GMIs is both a mistaken and futile strategy. The only effective and creative response is to try to reshape the sciences by bringing the other Others in. There is nothing mechanical or guaranteed about these possibilities as the new production system could, as in the emergent Conservative British model, seek to exclude everyone except industry and the elite technoscientists, but this new system is developing in a context where dreams of localised, embodied, responsible knowledges press from multiple currents in both the South (Shiva 1989) and the North (Haraway 1989; Harding 1991; Rose 1994). Dreams come into existence, not through binary confrontations and the soundbite, but

through multiple conversations and complex alliances. For my part, anyone who is prepared to help make space and enter seriously – and pleasurably – into such conversations and alliances is my friend.

Beyond the Acronyms

Another more conventional language of speaking about constructing cultural space and building friendly alliances is the political project of the democratisation of science and technology – leaving STS, even feminist STS, and going back to the policy debates. The intense efforts of the radical science movement of a quarter of a century ago could not at that moment succeed, not least because of the ways the project was understood together with the bluntness of our analytic tools. Perhaps now that we have a rather better understanding of the complexity of social shaping fostered by those acronyms of STS, SCOT and SSK and all the rest, the time is ripe to return to the urgent task of thinking about and working on science and technology policy. Part of that process will require new institutions, able to foster the social (re)shaping of science and technology in ways friendlier to the diversity of people and nature alike. I am not suggesting that such (re)shaping, even in the light of all the social shaping research, is going to be easy, but it is reasonable to suggest that the diversity and strength of our social science 'know how' increases our chances. Democratising science and technology looks to be both socially more worth while and also more intellectually challenging than death by soundbite.

NOTE

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