

On sociology and STS

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Abstract

This paper starts by exploring the development of Science, Technology and Society (STS) in the UK in the late 1960s, emphasising its interdisciplinary roots, and comparing and contrasting it with the concerns of Sociology. It then turns to more recent developments in STS, outlining the importance of material semiotics to important traditions within the discipline including those influenced by actor network theory, feminism, and postcolonialism. It notes, in consistency with the Foucauldian approach, that material semiotics implies that knowledge traditions are performative, helping to create the realities that they describe. The paper concludes by exploring the implications of this performativity for the politics of research methods and for the future character of social science research.

Introduction

Where and how do science and technology relate to the various traditions of British sociology?

One answer must be: they are central. Any sociology that draws upon Marxist or Weberian roots locates technology and its ownership at the core of the social (Goldthorpe *et al.*, 1968; Lockwood, 1989). Any sociology that lies within the ameliorist tradition is, at the very least, bound to wrestle with consequences of industrialisation and its material forms (Young and Willmott, 1986). And then there are many specific sociological approaches that also, and centrally, treat with technologies and their knowledges. These include: traditions of work on sociotechnical systems and organisations (Burns and Stalker, 1961); on the gendering of social relations in the materially-organised workplace (Cockburn, 1983); on the knowledge economy and its infrastructures (Forester, 1980); and on the importance of media and communication technologies (Silverstone, 1994). And these are just a few of the versions of sociology that bump up against and treat with technologies.

So a concern with technology is braided into sociology. But so too, though sometimes slightly less obviously, is a concern with science. Since the 1950s, and especially in the work of Robert K. Merton (1973), natural science has become a sociological topic in its own right. But, and more important for sociology as a discipline, there has been a continuing preoccupation with

science as *method*. In one version of the history of sociology the latter is understood as a discipline that grew up in a space defined by classicism on the one hand, and romanticism on the other (Gouldner, 1973; Mannheim, 1953). In this way of thinking Marx, Weber and Durkheim each responded to the opportunities and problems posed by that space. Is 'the scientific method' appropriate to sociology? What, indeed, *is* the scientific method? How might we distinguish between good scientific practice in sociology on the one hand, and poor practice on the other? These are issues that preoccupied the classical sociologists, and they have never gone away. Indeed, they have become foregrounded since the seventies with the 'post-modern' erosion of epistemological certainties. And they have been tackled both methodologically and politically. For here is an important input from Marxist and Marxist-influenced traditions: how to distinguish science from ideology? How to offer critique? And how to ground knowledge in ways that are not saturated by domination if there *is* no recourse to epistemological certainty.²

So sociology is and always has been preoccupied with both technology and science, including its own status as a science. And yet there is an oddity here. Since around 1970 the social analysis of science and technology has been the core concern of a discipline that only partially overlaps with sociology. This 'Science, Technology and Society' (STS) has drawn from a wide range of disciplines including anthropology, education, geography, history and history of science, organisational analysis, philosophy of science and sociology. Along the way it has run under a number of labels including 'science studies', 'science and technology studies', 'the sociology of scientific knowledge', and 'social studies of science and technology'. The differences between these labels are not insignificant, but whatever it is called it is a small academic discipline, not usually taught at an undergraduate level, that is largely distinct from, sociology. In this piece I am interested in exploring the differences as well as the overlaps between STS and sociology. And I am particularly interested in what this might signify for the future of sociology.

Any history is a more or less artful construction that might be done quite differently.³ What follows is thus a specific and situated account of some of the origins and features of STS. Along the way, and as a part of this, I compare and contrast these with some of the preoccupations of sociology. Again, and as a part of this, I try to locate STS as part of a larger intellectual and political set of movements. I conclude by considering the possible significance of STS sensibilities and approaches for the future of sociology. As I hope will become clear, I believe that these might be quite significant.

Origin stories

Institutionally STS in part reflects a developing preoccupation by elites in European countries and in North America to understand and control science and science-based technologies, together with public attitudes to such tech-

nologies. Science has been important to parts of technology since at least the 19th century, and became more important in the interwar period. However, the links between the two became indissoluble during the Second World War, most notably with the Manhattan Project, and the science-based innovation that has underpinned most advanced technologies since 1945. The latter include pharmaceuticals, biomedicine, military technologies, nuclear power, chemicals, agriculture, IT and communications.

In his *Little Science, Big Science*, Derek de Solla Price (1963) described the dramatic exponential growth of science in the developed economies and especially the US in the post-war period. He graphically described the shift from small university laboratories as science became a major factor of production integral to industry and the economy. He also, and as a part of this story, showed that it had become very expensive both for industry but also for nation states. His book dramatised the new problems faced by the post-war elites: how to control this vastly expensive monster with its promises of prosperity and health, but also of possible destruction? How to think well about where to locate resources? More recently these questions were joined by another: how to legitimate technoscience in the face of increasing public scepticism, both secular (fear of nuclear weapons or genetically modified organisms) and religious (creationism, or resistance to stem cell research)?

Different Western countries reacted to these challenges at different times and in different ways. In the UK the response was partly shaped by C.P. Snow's 1959 lecture on 'the two cultures'. He argued that the illiteracy of scientists was matched by the stunning *scientific* illiteracy of otherwise highly educated elites. What was needed, he said, was a double form of literacy. Though this argument has been contested (Edgerton, 2006), it was highly influential, and one of its consequences was the creation of a handful of 'science studies' departments in the early 1960s (at Sussex, Manchester, and Edinburgh). The idea was that scientists and social scientists both needed a 'broadening element' in their education, and that, albeit modestly, there was space for this in the expanding British university system.

So what *was* this science studies? The answer is that nobody knew. More precisely, it was that it might be many things. One possibility was that it would take the form of studies of innovation and its management. This was (and remains) popular in the management literatures and schools which began to grow in the late 1960s, but it was distinct from and marginal to the early growth of science studies. A second was the analysis of science policy. This became a major focus for the Sussex Science Policy Research Unit at Sussex, and has become increasingly significant in STS over the last twenty years (Gibbons *et al.*, 1994). But it is the third and the fourth possibilities that were to become most important for the early history of the new science studies in Britain. The third was a critique of science as ideology which developed in various locations including Cambridge, London and Manchester. The inspiration here was primarily Marxist (feminism became important in the 1980s), and the approach institutionalised itself in part around the Radical Science

Collective and its *Radical Science Journal* (later *Science as Culture*). And fourth there was the development of a naturalistic ‘sociology of scientific knowledge’ which explored how science was conducted in practice, and drew in part on micro-sociology including symbolic interactionism. Though this too grew up in a variety of British locations, it became the primary focus of Edinburgh’s Science Studies Unit (which like its Manchester counterpart was located in the science faculty) and led to the creation of another new journal, *Science Studies* (later *Social Studies of Science*).

The story of science studies (or STS) between 1968 and about 1980 in the UK is predominantly the story of the interaction between these two approaches: Marxist-informed critique on the one hand, and the sociology of scientific knowledge on the other. Important for this story is that each of these strands drew on resources that have nothing to do with sociology. Perhaps most important was the (philosophical) history of science. Represented by a number of authors, the best known was Thomas Kuhn’s *The Structure of Scientific Revolutions* (1962). The writing of Kuhn and his colleagues was crucial to science studies in at least three ways. First, it could be used as a licence to conceive of science as a form of *culture* as opposed to a special form of truth lying outside normal social practice. Second, it attended to the informal and *practical* features of science. And third, it made its arguments in the form of *case studies*. A word on each of these.

a. Science as culture

The new history of science refused both Whig history and post-hoc epistemological critique whether in the form of logical positivism or Popperian falsificationism.⁴ Controversially, it suggested that if we want to make sense of scientific change we need to understand first its specific circumstances, second the character of already existing scientific knowledge, and third the resulting (albeit situated) rationality of those practitioners. Looked at from a social science point of view, this is not very far from a *verstehende* sociology or indeed from cultural or social anthropology. Thus, though Kuhn did not put it in this way and expressed reservations about how his work was used in the sociology of scientific knowledge, it was easy to treat his paradigms as versions of culture, that is as packages of cultural, cognitive and practical resources for making sense of the world. And once this argument was made, a crucial implication followed: all the resources of social science could be marshalled and used to create what had not previously been imagined – that is, a sociology of *scientific knowledge*.⁵

This was highly productive but it also implied a form of relativism. What if anything might be said about the *truth* of scientific knowledge from this point of view? The sociology of scientific knowledge responded to this question with a form of methodological pragmatism. Adopting what David Bloor (1976) called the ‘principle of symmetry’, it argued that the validity or otherwise of

the science being examined was not important if the object was a general and naturalistic analysis of the growth of scientific knowledge. In this way of thinking the latter was a pragmatic cultural tool actively used by professionals to make sense of the world. To understand how this worked it was necessary to set questions about the validity or otherwise of that knowledge on one side. A general analysis demanded that true knowledge should be explained in the same terms as false.

But how? The authors in the sociology of scientific knowledge investigated this in several ways, mostly working through historical cases and contemporary controversy studies.⁶ They explored the possibilities of a Durkheimian sociology of knowledge (especially mediated through anthropology of Mary Douglas (1982)). They also argued, in a version of the sociology of knowledge that can be traced back to Marx, that the ways in which puzzle-solving scientists *use* their culture reflects natural and social circumstances, including social interests. In this way of thinking, the task of a sociology of scientific knowledge was to explore the shaping of scientific culture at the hands of practitioners as the intersection of natural phenomena, social interests, and prior cultural resources. The approach was descriptive and explanatory in focus, and not primarily preoccupied with critique. Thus, though Barry Barnes (1977) talked of the operation of concealed and illegitimate social interests, he also argued that nothing could be imputed about the *validity* of the knowledge so generated.

This was immensely productive, but it also meant that the sociology of scientific knowledge cut itself adrift from two of the major preoccupations of British sociology. First, and more obviously, if science was simply a form of culture, this meant that a straightforward political *critique* of scientific knowledge was not possible. Thus though knowledge might routinely be used to legitimate power, it was neither *intrinsically* distorted nor ideological. Second, the *categories* most important to macro-sociology also tended to go out of focus. In the sociology of scientific knowledge, class and its correlates such as occupational status, and later gender and ethnicity, became more or less incidental to the analysis of scientific puzzle solving. This was because scientific culture does not necessarily immediately relate to such macro-sociological categories. Some authors in the tradition indeed sought to link knowledges to class (and later gender) interests (MacKenzie, 1981; Shapin, 1979), but there was no necessary link. In practice, at least in the earlier work of the sociologists of scientific knowledge, smaller-scale *professional* interests were often more important (and more easily seen?).

For the critically-minded *Radical Science Journal*, *RSJ*, authors this was less than satisfactory. As they wrestled with their own version of the culturally complex and socially situated character of scientific knowledge, they much more actively sought to explore the structuring and restricting nature of large scale social interests. As a result, they were much more likely to detect both domination and distortion in science. This means that in contrast with the Edinburgh school sociology of scientific knowledge, they sought

both to criticise, and to link their analyses to macro-social categories of interest to critical sociology and politics.⁷

The problem for *RSJ* and other radical authors was different. Indeed it can be seen as an early expression of an issue familiar to many radicals in an era of eroding epistemological foundations. Thus at a time when British critical sociology was being strongly influenced by Althusser's insistence on the distinction between science and ideology, the *RSJ* writers were wrestling with a quite different problem: if science (including social science) does not guarantee truth then what is the possible basis for a political and analytical critique of ideology? If all science is culture, and all culture reflects its social circumstances, then how might one avoid relativism? These issues were tackled in different ways within those affiliated to the radical science movement. Some indeed sought to hold on to relatively reductionist versions of Marxism (Rose and Rose, 1976), but others experimented with more libertarian resources, including Lukács and Gramsci.⁸

b. Science as practice

If Kuhn's writing implied a form of *verstehende* sociology, it also stressed the importance of *practice*. Though there were exceptions (Polanyi, 1958), at the time Kuhn started writing most historians and especially philosophers of science tended to treat science as a cognitive and formally rational set of activities. Formalisms, laws and theories were emphasised at the expense of embodied skills (including perceptions), craft work, laboratory manipulation, and apprenticeship.

Kuhn's book shifted what one might think of as the epistemological centre of gravity of science from the former to the latter. In the picture of science that he painted, symbols, laws and theories all remained important, but the formal parts of science achieved their significance in the context of informal activities and ways of seeing. Drawing from the later Wittgenstein, Kuhn argued that rules such as scientific laws do not include their own applications. Instead, apprentice scientists are taught to see phenomena generated in carefully orchestrated experimental settings as resembling others that they already know – and therefore as further expressions of symbolic generalisations. Scientific puzzle solving activity, the process that Kuhn calls 'normal science', is to be understood as the artful extension of existing rules to carefully constructed novel experimental situations. All in all, this is hard work, it is extremely creative, it is embodied, and it is thoroughly *material* in form. This is also why scientific training is as much about apprenticeship – learning to see and to handle – as it is about formal instruction.

Whilst this approach sits uneasily with the more 'normative' functionalist sociologies of writers such as Robert K. Merton, it sits easily with many other forms of sociology. Theories of practice are common within both critical sociology and its more descriptive cousins such as symbolic interactionism. Cer-

tainly, as I indicated above, there were many resonances between an embodied and material form of *verstehende* or interpretive sociology on the one hand, and Kuhn's understanding of scientific practice on the other.

c. Science as case study

Finally, and crucially, Kuhn works through *case studies*. Indeed, his book can be frustrating for those who seek a brief account of the essential features of his argument. This is because he takes us to exemplary historical moments and episodes, and describes them for us. We learn about his theory of science – and his approach to doing history of science – through instances. We never learn about them in the abstract.

Two points. One, this means that how Kuhn *describes* science resonates with his theory *of* science. Theory, he is telling us, cannot be detached from its instances. The parroting of formalisms is empty. The latter only become significant if we know what they mean in practice by being able to link them to, and see them at work in, different specific circumstances. Of course Kuhn cannot take us into Lavoisier's laboratory, but he does the next best thing. He describes it in its material and theoretical complexity, and asks us to see that the formal articulations of science are about seeing, manipulating, and noticing – even perhaps creating – systematic similarities and differences between otherwise diverse sets of circumstances. Scientists experiment, and historians of science work through case-studies. This is all of a piece.

Two. Importantly, this is a mode of working that has been borrowed by STS and made its own. STS writing is not only highly theorised, but also works on and in theory. Its core concerns often have to do with epistemology (the theory of knowledge), and (more recently) ontology, the character of the real (I will come to the latter below). In theory it might make its arguments in an abstract manner (and there are some signs of movement in this direction), but its major mode of self-expression, discovery and exegesis has usually been through case-studies. More strongly, its practitioners predominantly *think* through materials. They extend their ideas and conduct their controversies through cases, which act as an empirical (but not straightforwardly empiricist) stimulus and irritant. STS is not utterly distinctive in this respect, but it is unusual.

This means that there is considerable potential for misunderstanding in the places where it overlaps with standard forms of sociology. For instance, traditionally STS practitioners have usually been cautious about theory in the form of grand narrative. (I think, for instance, of notions such as the 'risk society'). They tend to speak somewhat austere, to want to know both what large scale generalisations or theories mean in practice, and about *where* they apply. Indeed, they are prone to ask whether such generalisations mean anything at all. They also, and in a related way, tend to avoid buying into a theory/data distinction. This is because in STS theory is not first created and then applied empirically. Theory and data are created together. However empirical it may

be, everything is already theorised. And empirical case-studies, at least in principle, are important because they articulate and re-work theory. So, for instance, Harry Collins's (1975) relativist argument that there is nothing outside social relations and esoteric and embodied professional culture to determine truth and our knowledge of the real world was developed in a careful case study of the controversy about the detectability of gravity waves. Bruno Latour's argument about the way in which scientific facts circulate through laboratories into other sites was worked up in a series of case-studies, including his work on the Pasteurisation of French agriculture (1988b). A counterintuitive correlate of this argument (that if science is to work outside the laboratory it is necessary to reconfigure other locations in certain respects *as* laboratories) is similarly developed empirically. My own work (Law, 1987) on what became known as 'heterogeneous engineering' (the idea that theories, documents, technologies and social relations are all – and necessarily – created together) was also developed through a series of case studies including a study of the fifteenth century Portuguese maritime imperial expansion.

Writers from outside STS sometimes find this frustrating. Why cannot STS writers express their theory concisely? Why is it that anyone who wants to read this literature and understand what is going on has to wade through case studies about solar neutrinos (Pinch, 1980) or the measurement of blood haemoglobin levels (Mol and Law, 1994) when all they want to know is how STS is theorising (say) the character of experiment or the supposedly malleable character of objects that are usually taken to be stable? The response – I have already given it – is that in the core STS sensibility abstraction is only possible by working through the concrete. Or, to put it more conventionally, theory is done in the form of case studies.⁹

This difference expresses itself in several ways. One is that when STS theories are transmuted, as sometimes happens outside the discipline, into brief abstractions that remove them from their context, this makes little sense to an STS sensibility, and so does violence to the related STS idea that since theory cannot be sensibly detached from its uses it does not travel lightly. And a second is a continuing temptation for STS authors to respond to the debates that periodically capture the social science high ground (I am thinking, for instance, of discussions of the risk society or globalisation) by responding in similarly large-scale and relatively non-contexted terms – a propensity that causes a degree of unease within STS.¹⁰

Development stories

I've told a story about the origins of British STS and its links with and differences from sociology. Though I have introduced some more recent case studies in the form of illustrations, this story takes us to around 1980. I want to add to this picture and bring us forward to the year 2000. Again the history is schematic, constructed, and substantially arbitrary. In this version it comes in five parts.

One, within STS historical or contemporary historical case studies (for instance of scientific controversies) were subsequently matched by laboratory ethnographies. The first of these was *Laboratory Life* by Bruno Latour and Steve Woolgar (1979). We can see a series of influences at work here. There is a gesture towards anthropology. There is what one might, with the benefit of somewhat enhanced hindsight, call post-colonialism. (The importance of north-south relations is clearer in some of Latour's other work (1990)). Importantly, there is ethnomethodology.¹¹ There is an ever greater insistence on the materiality of the laboratory, its apparatuses, its physical organisation, and its practices. There is post-structuralism (more on this shortly). But (here is the bottom line) none the less this is still a *case-study*. It is another highly theorised intervention cast in empirical mode. And with the exception of ethnomethodology, it is not clear that it is borrowing from any major sociological tradition – though the importance of sociology can be seen more clearly in some other laboratory ethnographies.¹²

Two, many STS writers shifted *topics* and started to work on technologies. Thus they moved from talking about the social shaping of scientific knowledge to what became known as the social construction of technology (SCOT for short – the field is overburdened with acronyms). But, as this way of putting it suggests, initially at least, the form of the argument was often similar. It was suggested, not very counter-intuitively, that technologies may be treated as forms of material culture that are shaped by the operation of social interests.¹³

How much does this shift have to do with sociology? Perhaps again the answer is: not a lot. More important, at least in the first instance, was the work on large technical systems pioneered by historian Thomas P. Hughes and his students. And this is my third point. Hughes's argument was that entrepreneurs such as Thomas Edison conceived of the world in system terms. Science, technology, economics, law and politics, all had to be aligned if (for instance) a successful electricity power generation and supply network was to be built (Hughes, 1979; 1983). Hughes assumed, controversially, that certain gifted entrepreneurs were especially able to do this. But more relevant for the story of STS is the system metaphor central to his picture. This is because we see here a *relational* logic at work. In another and different post-structuralist version (that I touched on above when I mentioned work by Latour and Law) this was going to become crucial to STS. But the bare logic is simple. It is that elements in a system are significant – and indeed achieve their form and character – *only in relation to one another*. Marx's 'all that is solid . . .' beckons here, and, more directly, so too does the systems thinking of the engineer-entrepreneurs studied by Hughes.¹⁴

And it is at this point that we alight at the origins of a tension both within and beyond STS. This concerns the explanatory status of the social. The question is whether or not social interests or structures (for instance to do with class or gender) *shape* technologies. Some of the SCOT writers persuasively argue that they do. Gender, for instance, appears to be embedded in the shaping of the safety bicycle (Bijker, 1995) and print technologies (Cockburn,

1999). But (here is the tension) any move to a system logic tends to undo social foundations as an explanatory resource. This is because it assumes that since systems have their own relational logic, the latter is likely to reshape the social just as much as the technical. And this in turn means that the social is unable to *explain* anything. Though it exists, it is just as much in need of explanation as the technical. This signals, then, on the one hand a large explanatory gap within STS, between parts of STS and sociology, but also, at the same time, some emergent similarities that relate to a move away from forms of reductionist explanation.¹⁵

But which parts of STS have moved from reductionism? My fourth point (and we are still in the 1980s) has to do with what became known as actor-network theory. This starts (as does Hughes) with a system logic because it traces *how* elements in a web or a network take the form that they do in more or less precarious interaction with one another. People, technologies, 'natural' phenomena, documents, non-human life forms, knowledges, social facts, collectivities and phenomena – all of these are relational effects, materials, being done in interaction. Actors, then, are also networks that hold together for long enough to act in relation to something else. (This is why Michel Callon talked of the 'actor-network', a term now so familiar that its original status as an oxymoron is no longer apparent).¹⁶ Again – we've just been there with Hughes – this means that they can not be used to *explain* anything. Everything in the web is revisable. Everything is uncertain. Everything is relational. And nothing is foundational. Not even the entrepreneurs described by Hughes.

How to think about 'actor-network theory'? It does not relate in any direct way to social network theories – though like these it may be generally understood as an expression of a longer term trend, visible for a century but more strongly since 1945, towards system-like and relational modes for understanding the natural, the social and the technical.¹⁷ Indeed, for the reasons I've just touched on, actor-network theory doesn't really count as a form of sociology. Unsurprisingly, it is a source of frustration for those who seek strong social explanations for the origins of phenomena. For similar reasons it is also frustrating for those primarily concerned with social critique. For instead of asking *why* things happen it asks *how* they occur. *How* they arrange themselves. *How* the materials of the world (social, technical, documentary, natural, human, animal) get themselves done in particular locations for a moment in all their heterogeneity. And *how* they go on shifting and relating themselves in the processes that enact realities, knowledges and all the rest. And this is why I think it may be understood as a version of post-structuralism. Here's my proposal. We may think of it as a little-narrative, thoroughly empirically-grounded, very material, small-scale relative of (say) Foucault's larger-scale epistemic project, or Deleuze's rhizomes.¹⁸ Actor-network theory is what resulted when a non-humanist and post-structuralist sensibility to relationality, materiality, process, enactment and the possibility of alternative epistemic framings bumped into the theoretically informed, materially-grounded, practice-oriented empirical case-study tradition of English language STS.¹⁹

And if this is right, then it also suggests that in the UK context, despite their differences, those who work in so-called actor-network theory and those who work in governmentality occupy related conceptual spaces.²⁰ And why both tend to fit uneasily with stronger, more foundational, and more critical sociologies.

Development stories. Here's a fifth, and it has to do with *feminism*. Let's say first that gender has become more important in the STS stories about science and technology (it was notably absent from most 1970s work).²¹ But let's also say that feminist epistemology has also become important too. This folds back into and works on the uncertain relation between epistemological foundations (or their absence) and critique that I mentioned earlier. So there have been arguments in favour of standpoint epistemology – the idea that underprivileged groups including women have privileged access to social reality (Harding, 1986). But perhaps more important in the present context, there have been arguments that stand the idea of objectivity on its head. I'm talking here of the work of Donna Haraway (scientist, feminist, STS scholar and material-semiotician).

Let me backtrack for a moment. In 1985 sociology of science-influenced historians of science, Stephen Shapin and Simon Schaffer (1985) published a central text for the new STS. This was on the English seventeenth century origins of modern science. They argued that scientific experiment became possible in post-Restoration England because practitioners developed ways of trusting one another reliably to witness and report on experiments. The argument was that this trust depended on the simultaneous creation of three technologies (Shapin, 1984): *literary* (the creation of a modest style of writing about matters of fact, with no expression of personal opinion), *technical* (the creation of specific forms of appropriate laboratory experimentation in specific locations), and *social* (the designation of a class of people taken to be reliable because they were 'independent' witnesses of those experiments and able to write about this – which in the seventeenth century excluded women and servants, and was essentially restricted to men of independent means). Shapin and Schaffer argue that this was a crucial moment in the creation of science and its indirect 'modest witnessing'. Indeed, they plausibly suggest that this set of technologies, with variations, still frames much twenty-first century science, and helps to explain why in scientific papers the voice is passive, the figure of the author tends to disappear, and nature appears to speak for itself.

Move forward again. Haraway notes that the tradition of modest witnessing is gendered,²² but also that despite its self-effacing literary mode, it is also profoundly immodest. This is because witnessing pretends to speak objectively on behalf of (some aspect of) nature by separating subject from object and appearing to make the object speak for itself. In reality, however, it conceals the circumstances (social, technical, literary) that produce this form of witnessing and the object that is being witnessed. Then, and this is the crucial move for our purposes, she proposes an alternative form of modest witnessing

that is locatable, responsible and accountable. 'Feminist objectivity,' writes Haraway (1988, 583), 'is about limited location and situated knowledge'. It is about accepting and describing the located character of truth claims. It is about avoiding 'the god trick of [claiming to] see . . . everything from nowhere' (1988: 581). And, since knowing is also about *performing*, it is therefore about accepting the responsibilities that go with knowing. This, then, is a new kind of located and situated critical project, one that is profoundly political, but not foundational.

So where have we got to now?

This version of history has brought us to the year 2000. And I have suggested that sociology is one, but just one, of the sources that feed STS. I've tried to show that these also include: the history and philosophy of science; the Marxist critique of ideology; Wittgensteinian philosophy; the history of technology; ethnomethodology; social and cultural anthropology; semiotics; post-structuralism; and feminist epistemology. I've also tried to show that while some of the concerns that have traditionally animated British sociology (including an interest in the macro-social determinants of social change and inequality, and attention to social critique) are *leitmotifs* running through important parts of STS, there are other parts of it that are non-foundational, relational, place no explanatory weight on the macro-social, and are better understood as descriptive, empirical and specifically material expressions of a post-structuralist sensibility.

That said, it is also possible to bring the story up to date, and note the growth of new analytical and (post?) critical possibilities. I turn, then, to the importance of performativity, multiplicity, and the possibilities afforded by what has become known as ontological politics.

a. Notes on performativity

When STS came into being it tended to talk of 'the social construction' of scientific knowledge. Though many in STS would still feel comfortable with this location, others would not. Two points.

One, as I've tried to say above, in much of STS the social has been dissolved as an explanatory and foundational category. If the social exists separately at all within the web of heterogeneous relations detected by the toolkit of actor-network theory and its successor projects, or in Haraway's feminist material-semiotics, (and of course often it does), then it is a temporarily stabilised *effect* of those webs in which particular parts of that web are generated and treated as 'social'. So, first point, the 'social' has disappeared as a basic analytical category.²³

Two, the term 'construction' has also been eroded. Though the term is endlessly contested (Velody and Williams, 1998), it may be understood as a place for exploring versions of anti-foundationalism. That said, put simply the metaphor tends to conjure up an image of something like a building site. The possible implication is that stuff is, indeed, constructed, but once it is up it is up. Everything else being equal, it stays there. But this does not work so well if the focus is on process – and more importantly, *continuing* process. This is because, in this alternative way of thinking, the webs of relations only hold if they are enacted, enacted again, and enacted yet again – which may or may not happen in practice. But if we think in this way then we're no longer on the metaphorical equivalent of a building site. Instead we are in a world of performance or enactment.

Here then, STS in its (feminist and actor-network) material-semiotic forms is in the same conceptual space as Foucault's archaeology or Judith Butler's (1993) feminism, not to mention a great deal of contemporary political theory, cultural studies, and human geography. It is about performativity. It is arguing that *realities* (including objects and subjects) and *representations* of those realities are being enacted or performed simultaneously. It is, as I noted above, post-structuralist in inclination, albeit in a particular and materially-oriented mode. This means that it is also profoundly non-humanist (beware, performance here has nothing to do with Erving Goffman's sociology). Shift the verb from making to doing – to *doing* realities – and we catch what is at stake. To put it in formal language, what is at stake is not simply epistemological. We are also in the realm of *ontology*.

b. Notes on multiplicity

Here STS reflects and diffracts a much larger intellectual movement. As I noted earlier, the erosion of foundational categories – and foundational epistemological tools – has characterised many parts of social science including sociology in the last forty years. It is indexed, for instance, in the partial shift from the world according to Marx to the world according to Foucault – and then within the latter, the attempt to re-create versions of criticism in a world of always situated and always located materialities and discourses. But what, then, is to be done? What might a post-critical engagement with the-social-and-the-material look like?

The basic proposition that comes from STS and its cognate disciplines is this. Since the real is relationally enacted in practices, if those practices were to change *the real would also be done differently*. Foucault tells us this. He talks of the conditions of possibility set by an episteme, and then of the heterotopic spaces that lie at or beyond the margins – an argument extended and instanced by sociologist and geographer Kevin Hetherington (1997). But now we get to a crunch. This has to do with the *location* of those heterotopic spaces. Where are these to be found? The easiest answer to extract from Foucault is that they

are more or less indefinitely remote. Thus remember that, for all his radicalism, Foucault proposed that the modern episteme started spreading through and organising practices, realities and knowledges in the eighteenth century, and that we are still utterly in its productive grip. To misquote Althusser who was caught in a similarly uncomfortable political and analytical predicament, Foucault is telling us that we're all playing from more or less the same modern score. So the standard answer is that heterotopic spaces are far distant. But as feminist STS philosopher Annemarie Mol notes (2008: 91ff), *material-semiotic STS does not agree*. Instead it says that the heterotopic is everywhere. So what is the intervention?

It turns around the question as to how big you want to think. And then it depends on how widely you want to spread the epistemic and ontic strategies that are taken to be carried in, and recursively ordering, the relations of practice. If you have ambitions to be large, if you think the strategies are general or common to an epoch, then you are lost. But if you don't, and in particular if you think that practices are ramshackle, differ from one another, and relatively poorly co-ordinated, then you are moved, instead, to the conclusion that more or less different realities are also being done, moment by moment, in those different practices. And this is the position that has been articulated in several of the post-structuralist variants of STS including 'after actor-network theory' and feminist material semiotics. And it is here, I suggest, that STS's refusal of grand narrative and the macro (social or otherwise) and its grounding insistence on the specificity of the case study may have helped to save the post-critical day.

How this might work can be seen for actor-network theory. Its earliest studies tended to explore strategies for translation that extended into and ordered (or failed to order) asymmetrical networks of relations. So, for instance, Michel Callon looked at electric vehicles and scallops (1986a; 1986b) and, as we have seen, Bruno Latour analysed the networks of Pasteurisation (1988b) while John Law explored the sociotechnologies of the Portuguese maritime imperialist expansion (1986). These were studies, in some ways resonant with those of Foucault, which show how knowledges, realities, and productive but potentially asymmetrical versions of power may all be done together. In these studies other orderings existed, but they existed outwith those being attended to. Difference was a constant threat (these networks were constantly at risk), but like the heterotopic, it lay *outside*.

If we move forward we find that this has changed. More recent 'after-actor-network' studies have started to bring difference in from the cold. So, for instance, Annemarie Mol explores the practices for doing lower-limb atherosclerosis in four locations in the health services of a town in the Netherlands. And what she discovers is that since the practices in each of these locations is each different, so too are the realities that these enact. In theory the disease is one (this is assumed if you read a textbook), but in practice it is not. Hence the oxymoronic title of her book, *The Body Multiple*.²⁴ In practice the disease is more than one but less than many. The proposal, then, is that the world is not

simply epistemologically complex. It is ontologically multiple too. Or, to put it differently, the heterotopic lies within.

c. Notes on ontological politics

This is counter-intuitive and readily misunderstood, especially by those (including sociologists) who are comfortable with the idea that though there are many different perspectives on reality there is, none the less, a single reality in the end. But the turn to performance leads not simply to epistemological but also to ontological multiplicity. And then, since the heterotopic has been brought back in, it leads to a series of post-critical strategies for thinking about politics and intervention.

Perhaps the simplest way of making the point is to suggest that *reality is not destiny*. For if there are multiple realities then these may be played off against one another. Importantly, some will be preferable to others (though such judgements are themselves likely to be complex). This is the point of an ontological politics (Mol, 1999). It is to work within and upon difference, to make differences. But what does this imply? The answer, for instance in the context of health care, is that it leads away from an external critique of professional patriarchy and biomedical power towards more specific internal interventions in which the differences between different medical practices are played off against one another. There are no general rules here. Differences are more or less specific. Mol, however, tends to call for attention to the effects of medical interventions on people's daily lives, rather than on isolated bodily parameters. In line with this, she has recently argued against currently fashionable neo-liberal doctrines of patient choice, and in favour of a logic of care that tinkers with technologies in practice.²⁵

Though her vocabulary, and indeed in some ways her politics, differ from those of Mol, Haraway's interventions work within a related political and analytical space. Again, for Haraway there *is* no single and comprehensive reality. Instead, whether we are talking about biology, gender, or companion species (Haraway, 2008), there are different realities being enacted in more or less power-saturated practices. The question becomes: how to interfere in and diffract realities in particular locations to generate more respectful and less dominatory alternatives. How to *trope*, to bend versions of the real, to strengthen desirable realities that would otherwise be weak.

This, then, is the *leitmotif* of this turn to the ontological. It is to refuse to be overawed by seemingly large systems, and the seeming ontological unity of the world enacted by large systems. It is, instead, to make the problem smaller, or better, to make it more specific. To deal with the materialities of specific practices. To discover difference. And then to intervene in ways that might make a difference to those differences. This is the sensibility shared by the interventions of Mol and of Haraway – but also of post-colonial STS writer Helen Verran who describes the practices in Nigerian classrooms when

western numbering and that of the Yoruba encounter one another. Are these two great ontic-epistemic systems? Many would say yes. Literatures have been written that insist this to be the case. But Verran describes the ordered chaos of the classroom, and how bits and pieces of these versions of numbering are heterogeneously assembled to allow the class and its students and teachers to go on well together.²⁶

Conclusion

STS started as a discipline that explored the epistemic practices of science by means of contemporary and historical empirical case studies, sometimes naturalistically and sometimes critically. Then it added to its topics by including technology, and added to its intellectual resources (originally mainly from history and philosophy of science, parts of sociology, anthropology, and Marxist critical theory) by including approaches borrowed from the history of technology, feminism, semiotics, post-colonialism, and post-structuralism. Subsequently, it moved from its initial preoccupation with science and technology to explore other empirical areas. Perhaps most important in the post-2000 period has been health care, medicine, and genomics²⁷ (a shift that again in part reflects changes in state-related policy priorities). But STS, or STS-related approaches are also to be found on studies of 'nature' (Donaldson, 2008), geographies (Bingham, Enticott and Hinchliffe, 2008; Hinchliffe, 2001), spatialities (Murdoch, 1998), education (Verran, 2006), markets (Callon, 1998; 2007; MacKenzie, Muniesa and Siu, 2007), information technologies (Suchman, 2007) and organisational behaviour (Bloomfield and Vurdubakis, 2005). And in many instances, with a turn to performativity, STS has started to explore the ontological, the doing of the real, as well as the epistemological or the representational.²⁸ Its preoccupation with the specificity of case studies has meant that its practitioners have been well placed to detect ontological multiplicity in a post-structuralist idiom, and so to start to characterise a range of approaches to a post-critical ontological politics. So what lessons might this material-semiotic STS hold for sociology?

Perhaps the most important is that 'the social' is always material. The two cannot be distinguished, except as outcomes or effects, and it makes no sense to try to do so. A second is that the 'micro' and the 'macro', and more generally any attempts to measure and scale, are similarly relational effects. STS tells us that the macro-social is a precarious achievement, and the challenge is to attend to *how* it and its power effects get done rather than assuming their importance or stability.²⁹ A third is that the human-non-human distinction is similarly a consequence or effect of relations rather than a primitive explanatory category (Latour, 1993). A fourth concerns the role of case-studies. On the one hand sociology has a vital empirical tradition. On the other hand it tends to distinguish between empirical research and social theory. This division makes little sense in STS which develops its theoretical arguments through

case studies.³⁰ This implies questions for sociology. Instead of 'applying' social theory or imagining that it is describing the world, would it be better for empirical sociology to reconceive of itself as a case-study mode of carrying and constituting theory? And, a complementary move, what would sociology lose if it were to jettison its propensity to grand narrative?

These are all questions suggested by a material-semiotic STS. But I want to spend a little more time on a fifth issue, that of *method*. Thus I have argued that a core concern of STS is with how practices enact representations on the one hand (epistemology) and realities on the other (ontology); with how *methods represent and enact the real*. But we need to be clear what this means. Thus, though they may include experimental apparatus, it would be misleading to imagine that methods are primarily pieces of kit (STS has always rejected technological determinism). Instead methods are more or less (always more or less!) routinised practices that do reals and representations of reals. These practices stretch far beyond the laboratory or any formal notion of scientific apparatus.³¹ To put this slightly differently, the core claim of STS is that technoscience does its realities as well as the representations of those realities: that technoscience, in all its complex multiplicity, *enacts worlds that are fit for its methods*.³² But what happens if we turn this round and apply it to the social, to social science, and in particular to sociology?

One answer is that sociology would be understood as a discipline composed of (theoretically freighted) methodological practices for producing descriptions of reality *and* the realities that correspond with those descriptions. *Sociology would be understood, in other words, as a set of devices for doing reality*. Fractured, of course, since practices are specific. But nevertheless productive.³³ So what does this imply? The answer is that new questions would start to form.

- First (as Savage and Burrows (2007) suggest) we might propose that it would be interesting and productive to recast our understanding of the discipline as a set of *methodological reality practices* rather than following Talcott Parsons and thinking of it as a set of theoretical traditions applied to particular subject-matters.³⁴
- Second, we might ask whether we are well-served by the methodological reality practices that currently define the subject-matter, the representations, and the realities of sociology. We might ask whether these are supple – and subtle – enough, and whether we are casting our discipline as the methodological expression and enactment of a set of past realities.
- Third, and as a specification of this, we might ask about the relations between our own methods practices and those of our objects of study. As is obvious, the latter – industrial and retail organisations, global finance, state agencies, NGOs and the multiple organisations of civil society – have their own representational and reality-producing techniques. So the question arises: how well are sociologists currently able to track and trace the realities that these are creating? Is there a risk that we are being left behind?³⁵

- Fourth, and similarly, we might ask how well our *formal* understanding of our methods fits the picture of their performativity that is beginning to emerge. Accuracy and precision will not disappear as goods – there is much to be said of these and they need revisiting – but if methods also enact realities then they cannot be the only goods to be explored. Perhaps we are too concerned with the hygiene of our methods, and insufficiently interested in their possible fecundity and productivity. To put it differently, if we want to enact alternative and better versions of the real we might ask how our methods – appropriately extended – might productively interfere, both with one another, and with the reality-making practices that lie beyond the boundaries of sociology.³⁶
- Finally, we might ask about our own place in the world. The turn to performativity robs us of the belief, the hope, or the pretence that our methods simply describe. They are not neutral. They help to *enact* realities. Haraway argues that we need to be accountable for our practices and their knowledges. Following such authors as James C. Scott and especially Timothy Mitchell³⁷ I would add a more specific version of this political question. What, I would ask, is the relation between our sociology and those – often branches of the state – that sponsor it? In what measure might it be argued that sociology in its current versions inadvertently helps to make a world fit for state power? And what would we make of this if we thought that it did?

I have my own views on these questions, but here I am primarily interested in discussion. For our methods have become richer over the last decades. Visual methodologies, innovative methods for performing, the growth in popularity of focus groups and citizen's juries, a range of versions of discourse analysis, methods for tracking and tracing electronic realities, these and many more have been added, albeit sometimes marginally, to the suite of social science research methods. So I am cautiously optimistic about the creativity of sociology – so long as it is also understood that our methods are, indeed, performative: that they do not simply describe but in some measure help to do the realities that they discover.

A final thought. If the materially post-structuralist and performative vision of the world offered by contemporary STS and some of its cognate disciplines makes sense, then reality is complex: it is a reality multiple. But this also tells us that understanding complexity is not simply a technical task, something to which we might hope to approximate as our methods improve. Any such aspiration is a chimera because we are *part* of that complexity, we are helping to *create* that complexity, and we could never get *outside* the social to view it from above and as a whole. To put it differently, our attempts to know and order will be both performative *and* limited. Or, to put it differently again, looked at from the point of view of a desire for social science order and completeness, the world is chronically messy and will defy summary in any one particular way.³⁸

So what does this imply? Again we may debate. But for me, made as I am in a particular version of STS, a specific more or less critical conclusion follows. It seems to me that methods that imagine the world to be relatively neat and tidy and try enact it in that way, are missing the point. Worse, they are seeking to stipulate and so to enact an order that is epistemologically mistaken, ontologically unrealistic, and politically obnoxious. I sense this every time I have to fill in a questionnaire. Usually, almost always, it seems that the questions do not quite fit. And I feel it, too, when I have to respond to social science inquiries about the rigour of my approach to research, to research methods, and to research hypotheses. Usually, for instance, I cannot tell beforehand how the data will be analysed. Often, indeed, I have no idea what will and what will not count as data. The forms of ordering implied in such inquiries do not match the social realities with which I wrestle. No doubt this is sometimes a straightforward failure in my sociology. But I think it is also a failure in how we collectively imagine our methods and their relation to the real. My hypothesis is that our collective understanding of method seeks, albeit imperfectly, to enact forms of order, but that the realities always escape.

All this leads me to say that we are in need of methods for knowing and enacting non-coherence. Indeed I believe this to be urgent. This is because there is a theory of domination hiding here, a sensibility to inequality that we have not yet quite managed to articulate. This is that domination is often *not* a system effect, the consequence of a coherent order. Rather it is a result of *non-coherence*. Of elements of structuring, ordering, that only *partially* hang together. Of relations of subordination that are relatively invulnerable precisely because they are not tightly connected. Invulnerable because when one is undone the others are not pulled down with it.

How to think this well? How to interfere in the non-coherent structures of domination? For me this is the great challenge for sociology in the 21st century. I have no answers. But of this I am certain. Research methods that describe and try to enact coherence by imagining domination as a structured whole count as part of the problem rather than as part of the solution.

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Notes

- 1 Thanks to Kristin Asdal, Nick Bingham, Steve Hinchliffe, Ruth McNally, Maureen McNeil, Annemarie Mol, Ingunn Moser, Tom Osborne, Mike Savage, Vicky Singleton and Claire Waterton.
- 2 The first of these questions was central, in particular, to the work of Althusser. See, for instance, his highly influential (1971).
- 3 For another version that more closely reflects STS's own self-understanding, see Law (2008).
- 4 For the debates with falsificationism see Lakatos and Musgrave (1970).

- 5 As I noted earlier, before the creation of the sociology of scientific knowledge, the sociology of science had been shaped (and indeed invented) by Robert K. Merton, who explored the institutional and reward systems within which science took place, but, at least in general, took the empiricist view that scientific knowledge itself was, or at least should be, governed by technical and logical norms for gathering and rationally manipulating data. See Merton (1957).
- 6 For exemplary studies from a large tradition see Collins (1975) and MacKenzie (1981).
- 7 For instances of this body of work see: McNeil (1987), Young (1977; 1985) and Yoxen (1983).
- 8 For more discussion see Moser (2007), and especially Maureen McNeil's interventions from page 334.
- 9 The difference can be articulated in other ways. In particular, the STS sensibility can be treated as a monadology. See Latour (1988a), Kwa (2002) and Law (2004b).
- 10 So, for instance, Bruno Latour's work is justly recognised as being of seminal importance not only within but also far beyond STS. In some measure this is because parts of his recent writing – for instance his *Politics of Nature* (2004) – engage with 'large scale' debates (in this instance to do with ecology and political theory). This is a response that implies, however, a shift in idiom from specificity and the idea that there are no generalities outside links between such specificities, to a willingness to talk (for instance in this case) of constitutions as more or less general procedures for adjudicating the competing claims of not very stable human and non-human realities. If this kind of intervention is becoming more common then it may be that STS is shifting its intellectual character, or (depending on your point of view) displaying signs that it is starting to lose its soul. My own prejudices lead me to the latter view. For an attempt (unlike the present piece) to offer an account of STS that remains faithful to its case-study idiom see Law (2008).
- 11 This is reflected in the sensitivity of these authors to the importance of modalities.
- 12 See, for instance, Knorr Cetina (1981) and, in the form of ethnomethodology, Lynch (1985). But anthropologists were also writing STS ethnographies. See Traweek (1988) and Suchman (1987).
- 13 For the edited collection that set this line of working going see Bijker, Hughes and Pinch (1987).
- 14 It is no coincidence that one of the dominant metaphors in contemporary STS (and most particularly actor-network theory) is that of the network.
- 15 Savage and Burrows (2007: 896), in part drawing on recent STS, point to the way in which sociology is poor at causality, and suggest that it might instead 'embrace an interest in *description and classification*'. Savage (2008) in this volume offers an account of the relationality and psychological non-reductionism of the move to social network theories of such authors as Elizabeth Bott.
- 16 See his case-study on scallops, Callon (1986b), and Latour (1987).
- 17 In the present paper we have already seen several instances of this. Hughes's work on large technical systems picks up and reflects the logic he saw at work in the innovations of such entrepreneurs as Thomas Edison. The prevalence of systems and cybernetic metaphors is visible in many parts of engineering. I discuss this at some length in my own case study of a military technology (Law, 2002) where I distinguish between three explanatory forms: system, genealogy, and interest. For an account of cybernetic-like explanations and their limitations in ecology see Taylor (2005).
- 18 Latour (1999) talks of replacing the term 'actor-network' by 'actant-rhizome'.
- 19 It may be useful to enter a note of clarification about the use of the term 'material'. The 'material semiotics' of parts of STS (including Haraway's writing, which is perhaps where the term first appears, and actor-network theory) attends to *materialities* rather than being materialist. Since its metaphysics is relational it makes no a priori assumptions about the stuff of the world. The semiotic argument may be traced from the American tradition through the writing of Haraway (1989: 84–111) and from the continental tradition through the writing of Latour and his collaborators. See Latour and Fabbri (1977) (translated as Latour and Fabbri (2000)) and Latour and Bastide (1986). In this way of thinking 'the material' is a relational effect rather

- than being foundational as would be the case in a metaphysics of materialism. However, in its commitment to practice and the stuff of the world actor-network theory does not privilege discourse, unlike some traditions influenced by post-structuralism.
- 20 The governmentality work grows, especially, out of Nikolas Rose's writing. See, for instance, his (1999). For instances of the overlap see Law (1994) and especially Barry (2001).
 - 21 See, for instance, Cockburn (1999) and Wajcman (1991).
 - 22 Unlike Shapin and Schaffer she argues that it is actively productive of new versions of gender subordination. See Haraway (1997).
 - 23 As an index of this change, it disappears seven years on from the subtitle of the second edition of Latour and Woolgar (1986) which becomes *Laboratory Life: the Construction of Scientific Facts*.
 - 24 Mol (2002). Analogous arguments are made for other materials by Law (2002), Law and Singleton (2005), and Moser (2008).
 - 25 Neo (2008). There is much to be said about 'goods', and about how these might be known. Current neo-liberal orthodoxy assumes that these can be isolated, measured, quantified, and centrally compared. In a logic of care this works poorly because within such a logic goods and bads are understood to be multiple, substantially contextualised, in more or less tension, iterative, only partially discursively available, and their assessment is a matter of temporary and collaborative judgement. The dissonance between these two modes of knowing goods was nicely caught by the announcement by the UK Secretary of State for Health of a National Health Service hospital 'compassion index'. For scathing contemporary newspaper commentary see Caulkin (2008).
 - 26 Verran (1999; 2001). See also Verran's related writing on land and landholding in the Australian encounter between white and aboriginal Australians (Verran, 1998).
 - 27 Indicative references in what is a huge literature would include: Berg and Mol (1998), Epstein (2007), M'charek (2005), Mol (2008), Moreira and Palladino (2005), Moser (2006), Roberts (2007), Singleton (2005), Thompson (2007) and Timmermans (1999).
 - 28 This is one of the key claims of the new STS of economic markets.
 - 29 For an early exploration, see Callon and Latour (1981).
 - 30 For exemplary studies see Haraway (2008) and Mol (2002).
 - 31 See, for instance, Latour and Woolgar (1986), and for a recent analysis, Barad (2003).
 - 32 Counterintuitive though this may seem, it is in good philosophical company. See, for instance, Hacking (1992) and Rheinberger (1997).
 - 33 The argument has been explored in the context of social science by Osborne and Rose (1999), Law and Urry (2004), Law (2009), Savage and Burrows (2007) and Savage (2008).
 - 34 Parsons' *Structure of Social Action* was originally published in 1937, and is widely held to have established the sociological 'canon'. See Parsons (1949).
 - 35 As an example, there are substantial current methodological innovations within STS that are seeking to tackle relational realities that may only be visible by electronic means. Thus, to take one instance, it is well known that pharmaceutical companies have highly developed patenting strategies intended both to protect their own inventions, and to impede those of their rivals (or oblige the latter to pay royalties). There are also large issues to do with intellectual property rights and indigenous peoples, and the patenting of naturally occurring products or gene sequences. Some evidence of these strategies and differences surfaces anecdotally in the press. However, such strategies depend on the access of companies to a series of expensive electronic patent databases. Current work at the Sociomics Core Facility of the ESRC Centre for Economic and Social Aspects of Genomics is seeking to develop tools for tracking and tracing these strategies. For an example of this work see Oldham (2006). The Facility also uses a suite of digital tools developed by Richard Rogers and colleagues at www.govcom.org to track links between web pages in order to detect the formation of public issues, and the locations in which these arise and are enacted. On this 'issue crawler' methodology see McNally (2005) and Marres and Rogers (2005).
 - 36 Consider, for instance, the controversies over such contestable technologies as nuclear power and genetically modified crops. Within STS and its related fields a series of techniques have

been developed that are intended to give a more effective voice to citizens and other participants, lay and professional, in ways that are not quantifiable in standard ways (for instance in terms of economic rationalities and formal risk assessments) and as a consequence therefore tend to be marginalised in UK inquiries into those technologies. Some of these techniques (for instance focus groups) are well known (Waterton and Wynne, 1998), whereas others, for instance multi-criteria mapping (Stirling and Mayer, 2001) and deliberative mapping (Burgess *et al.*, 2007) are relatively new. However, there are two ways of looking at these innovations. One is to treat them as tools for discovering pre-existing realities. The other, consistent with the argument about performativity developed in this paper, is to say that they are also helping to enact reals – new versions of publics that might circulate and display effectivity in (for instance) state-related sites.

37 I am inspired by James C. Scott's (1998) *Seeing Like a State*, and Timothy Mitchell's (2002) *The Rule of Experts*.

38 For this argument at greater length see Law (2004a). For a low-tech example of non-coherent method at work, see Law (2007).

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