

STS as Method¹

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Introduction

How do science and technology shape the world? Or medicine and engineering? And how does the world in turn shape them? These are core questions for STS, and its authors tackle them by asking how science (and technology and the rest) work in practice. They work, that is, on the assumption that what they often call ‘technoscience’ is a set of social and material practices. Then they note that those practices work in different ways in different locations. So STS authors talk about laboratories, firms and hospitals, and also (since STS interests are wide) about financial traders, farms, care homes and indigenous knowledge practices. They look at how theories, methods, and material pieces of equipment are used in practice in specific social, organisational, cultural and national contexts – and they look at the effects of those practices. So the first lesson is this: STS attends to practices, and it is practical itself.

The second lesson follows from this. STS works through its case-studies. If you want to understand STS you need to read it through its cases. How fisherman and scallops interact in practice. How engineers and military chiefs create a warplane. How work in a laboratory generates new theories about physical forces. How primatology helps to reproduce patriarchy. How environmental scientists learn from aboriginal people. It is impossible to understand STS theory without looking at cases. These are where we do our theory. Some find this difficult: they think of theory as abstract. But in STS there is little or no theory/empirical divide. Instead it rolls theory and method and empirical practice together with social institutions (and sometimes objects) and insists that they are all part of the same weave and cannot be teased apart.

How did this arise? The answer is that STS started by looking at ‘the scientific method’ and showed that scientists don’t usually follow philosophers’ rules. Science is powerful, but in practice ‘the scientific method’ is material and messy. (The same is true for social science method too.) More than forty years have passed, but still STS looks at messy methods, scientific and otherwise, in practice. As I said above, how those methods work and get shaped is a core concern for STS. But more recent STS also tackles a further methods-related question. It asks: what is it that scientific (and technological and medical) practices actually do? One answer is that these are methods for shaping and reproducing the social world. The argument is that practices – scientific and social scientific – are methods for formatting society.

In one way or another method therefore lies at the heart of STS. This Chapter explores both STS’s own methods, and what it says about methods in the practices it studies. In the next section, ‘Shaping’, I describe how early STS rejected philosophers’ stories about scientific method and argued that the latter is shaped by social interests. In ‘Formatting’ I draw on feminist STS to show that technoscience methods also shape or format the social. The section on ‘Methods as the centre’ pushes this further by suggesting that everything, social and natural, is formatted in practices. The section on ‘Difference’ looks at how this is done differently in different practices and opens up the possibility of a ‘politics of things’. In ‘Knowledge Spaces’ I briefly review the weave that links methods together with subjects, objects, expressions and representations, and institutions, and the Conclusion reviews the argument of the Chapter as a whole. Along the way there are text-boxes which briefly introduce particular STS case studies. As I said above, it is the case studies which lie at the core of our discipline.

Shaping

What's wrong with epistemology: how STS started

Technoscience is shaped by society: scientific ideas or technologies reflect social interests. Many in STS say this. But where does the argument come from? I foreshadowed the answer in the Introduction. Fifty years ago most believed that science is special because it uses 'the scientific method'. Philosophers debated its character, but the general consensus was that the scientific method is especially good at: one, collecting accurate data; two generating logical generalisations which explain that data; and three, testing those generalisations. Philosophers generally said that scientific knowledge – good, true or accurate knowledge – will grow if people follow the scientific method (Popper: 1959). On the other hand, if they get distracted by personal prejudices or social bias then they don't see the truth and the quality of science suffers.

The earliest sociology of science shared this view (Merton: 1957), but STS came into being by reacting against it in two quite different ways. Some said that this was a nice picture in theory, but that in practice scientific methods in a class or gendered society cannot escape the social. In our power-asymmetrical world scientific knowledge is therefore irredeemably ideological. Others took a different tack. They argued that science is necessarily social. Scientists are trained to see the world in particular disciplinary (and therefore social) ways. They learn to work with appropriate experimental arrangements and theories, to identify core scientific puzzles and what will count as appropriate solutions to those puzzles. They also learn whom they can trust – about scientific authority. This line of argument is called the sociology of scientific knowledge (SSK) (Barnes: 1977; Bloor: 1976; Collins: 1975) and drew on the work of historian T.S. Kuhn (Kuhn: 1970b). It said, in effect, that science, its knowledge, its methods and its practices, are disciplinary (and therefore social) cultures, and that scientific knowledge is shaped in interaction between the world itself on the one hand, and the culture of science including its methods on the other.

Note three points before moving on. First, in this SSK it doesn't matter whether scientific knowledge is true or false. Since the same kinds of social processes are at work in each we need the same methods to explain both. (David Bloor (1976) called this the 'principle of symmetry'.) Second, scientific knowledge doesn't reflect nature. Instead it is a practical tool for handling and making sense of the world. In this philosophically pragmatist position either those tools do the job or they don't. Knowledge that works in practice is taken to be true. That which doesn't is taken to be false. And there is no other way of knowing truth. And then third, following Kuhn, SSK added that theories, methods, perceptions, practices and institutional arrangements are all mixed together. This is important because it suggests that methods are not simply techniques but carry personal, skill-related, theoretical and other agendas (Kuhn: 1970a; Polanyi: 1958; Ravetz: 1973). The implication is that the STS concern with methods spills over into much that is not obviously methodological, because methods cannot be separated from their social context. And methods themselves? These become cultural, practical, materially-based, theoretically implicated, institutionally located, and socially shaped routines or procedures. They are, as I just said, all ravelled up with everything else.

Social shaping

SSK opened up space for a theoretically diverse range of laboratory studies which ethnographically explored the construction of knowledge (Knorr Cetina: 1981; Latour and Woolgar: 1986; Lynch: 1990; Traweek: 1988). But for early SSK the core question was a little different. It asked how science, its methods, and its findings are shaped. SSK answered this question in two closely related ways. First it made the argument that I've just laid out. It said that scientists work with cultural

tools, and these are social. And second, it said that scientific knowledge is shaped by social interests. Donald MacKenzie (MacKenzie: 1978) explored this for a controversy about statistical correlation (see the box). Statistical procedures (like other scientific theories or methods) are tools for making sense of the world. But (this is the new move) how we construct those tools depends on the tasks that we set them. Yule's method for calculating correlation might have been a useful tool in many ways (indeed it turned out that it was.) But it was never going to do the kind of work that Pearson sought to do with his tetrachoric coefficient of correlation. It simply wasn't going to help to show that some kinds of people are superior to others.

MacKenzie's case study is much more sophisticated than in this brief account suggests. For instance, he argues that both professional and broader social class interests are at work. It's also important to understand that interests may shape science in ways invisible to those involved. (SSK is not saying that scientists are necessarily badly motivated.) In addition it is also important to understand that showing that interests are at work tells us nothing about the validity of the science involved. Good knowledge is necessarily shaped by social interests, and sometimes by those we disapprove of. And finally, MacKenzie's study is just one example of SSK at work. Jon Harwood wrote, for instance, on the science of race and intelligence (Harwood: 1976; 1977)), and the same approach was developed to explore technology in the social construction of technology (SCOT). Why, for instance, did the penny-farthing give way to the safety bicycle? Wiebe Bijker (1995) showed the penny-farthing was

Shaping statistics: case study1

Correlation: Intuitively we know what this is. It's a way of measuring how two variables relate to one another. Here's an example. Vaccination (or not), that's one (nominal) variable, and catching a disease (or not), that's a second. If none of the vaccinated people catch the disease and all of the vaccinated people do, then the two variables are highly (in fact perfectly) inversely correlated. We tend to take statistics for granted. But measures like correlation are invented. They are tools for handling data that can be quantified. And since they are invented, they may be constructed in different ways, and statisticians may get into disputes about them.

SSK writer Donald MacKenzie looked at one such dispute. In 1905 the protagonists – George Udny Yule and Karl Pearson – had invented two different ways of measuring correlation. The controversy between them went on for ten years. Yule's approach was straightforward. Pearson's was more complicated. He assumed that variables reflected normal distributions. Why? MacKenzie makes two arguments. They are both about the how interests shaped Pearson's approach. Pearson had previously worked on normal distribution (the 'bell curve') so he found it natural to think about correlations in this way. It was in his 'cognitive' interests to do so. But his approach to correlation also chimed in with his social agendas. A 'bell curve' way of thinking about correlation made it easier to think about the supposed superiority of middle class over working class people. The middle classes (including Pearson) were towards the top of the bell curve, and the working classes (in need of eugenic improvement) were towards the bottom. MacKenzie suggests that his complex way of calculating correlation was also in Pearson's social interests. MacKenzie (1978)

linked to macho forms of masculinity. Women – and many men – couldn't ride it. But since this meant that the market for bicycles was small it was in the interests of manufacturers to create a bicycle that was both safer and more modest. Here gender and commercial interests together shaped a technology and its production. And (another example) Cynthia Cockburn (1999) showed that the technologies of the pre-computer print trade expressed and reproduced both class and gender interests: the creation of heavy manual work was just one of the mechanisms working to exclude women. (For further 'SCOT' studies see Bijker, Hughes and Pinch (1987), and Bijker and Law (1992).)

Objectivity, nature and culture

In contemporary STS the idea that science can be separated from the social has all but disappeared. But the idea that technoscience and society are woven together did not simply come from SSK. For instance feminist STS author Donna Haraway (Haraway: 1988) talked of the 'God trick' to describe the mistaken and self-serving claim that science speaks objectively. (Think, for instance, of the way in which authors disappear from their own texts. These are written as if nature were simply reporting about itself.) Instead, she said, knowledges and methods are situated. The stories they tell about the world necessarily both reflect their location and reproduce social agendas. Achieving the God-like status of being above everything is impossible – though the myth that this can be achieved retains a powerful grip.

To say this is not to object to science. We are all located somewhere. But does it also mean that everything is subjective? Haraway response is that we can hang on to objectivity if we make two methodological and political moves. First scientists and social scientists need to acknowledge their own social location. And second they need to treat that location and its prejudices and blind-spots as a matter of critical inquiry in its own right. For Haraway objectivity is doubly 'partial': because it knows that it is one-sided, and because it also knows that it is incomplete. Her argument is therefore that to achieve objectivity scientists and social scientists need to be accountable for what they write, rather than hiding behind the fiction that what they are reporting comes direct and unmediated from nature. A similar argument comes from Sandra Harding. What she calls strong objectivity grows out of a self-critical examination of the social basis of knowing – a way of doing science or social science that explores the position (and questions the assumptions) of those producing knowledge (Harding: 1993). The idea is that knowledge-makers are part of what they study and that their methods should reflect this.

But where did the idea of objectivity as impartiality come from in the first place? Steven Shapin and Simon Schaffer tell us that this was created in very particular social circumstances in London in the 1660s and the 1670s (see the case-study 2 box). At this historically important moment 'nature' was separated from 'the social' and 'the political' and this separation was successfully institutionalised. Natural science came into being in Europe – and later across the world. A passive nature that might be known and mastered was divided from people who were active – and male. At the same time objectivity was separated from subjectivity and opinion and impartiality from partiality. In short, both the epistemological and institutional divisions that underpin science, give it its authority, and separate it the social and the political, were established in London in the 1660s.

Separating science from society: case study 2

In London in the 1660s in the newly created 'Royal Society' Robert Boyle was wrestling with question: how can we reliably learn about nature? The answer wasn't obvious. For instance, the Bible was full of powerful stories the about Creation. And nature was mixed up with the social order as well as God because it was widely believed that while subjects were answerable to kings, kings were answerable only to God.

Boyle was interested in air pressure. He was a devout Anglican and a royalist but he also wanted to separate facts about nature from politics and God. He did this by making a radical proposal. We can learn about nature, he said, if we do three things. First, we need to conduct reliable experiments. We need an experimental apparatus, an air pump. It was large, complex, expensive, and difficult to run. But the very idea of an experiment was a novelty. This is the first innovation. Historians Shapin and Schaffer, call this a material technology. But more was needed. The experiments needed witnessed, but not everyone could come to London to see the experiments for themselves. They needed to be told about them. This led to the creation of a second literary technology in which experimental accounts were written in a long-winded, modest and matter of fact way. Most important, it meant sticking to the facts, and excluding opinions and speculations. This was the second big innovation. But there was a third question: who could trusted as a reliable witness? To answer this question Boyle drew on the English legal system. In a court of law reliable witnesses were independent. Servants could not be trusted: they were not disinterested because they were not independent of their masters. Neither could woman: they were beholden to husbands, fathers or brothers. And this is the third innovation: the creation of what Shapin and Schaffer call a social technology.

This is the foundation of contemporary technoscience. Nature is separated from the social. It is imagined that facts can be described in ways that separate them from opinions and social contexts. And only disinterested specialists can decide about those facts. This is where the God trick came from. (Shapin: 1984; Shapin and Schaffer: 1985)

Formatting

Haraway (Haraway: 1997) raises questions about aspects of Shapin and Schaffer's account, but most, including Haraway, accept its overall significance. This is the moment when the God trick was embedded in science, and the methods of the latter (appeared) to step outside the social. But the stories about Boyle and indeed Pearson, together with Cockburn's work on class, gender and technology in the print trade, hint at something more. They suggest that technoscience is not simply shaped by the social world, but also helps to shape it. Indeed many in STS argue that knowledges and methods are often shaped in ways that are gendered, racist, class-based, and/or imperialist: that they help to reproduce such inequalities. But how do the methods and outputs of technoscience help to format the social world?

Feminist cultural studies of science

Look at the third case study. This tells us that one version of primatology carried and reproduced a whole range of social concerns (and horrors) including sadism, masculinist self-birthing, patriarchy, anxieties about child-rearing, and assumptions about functional nuclear families. It was shaped by concerns that could not be separated from those of educated middle-class mid-twentieth century America. But, at the same time, it helped to give shape to those concerns: to reproduce them. Social concerns fed into technoscience practice, and technoscience fed these back into social agendas. This is why I want to talk about formatting (the term is mine, not Haraway's). These were practices – methods – that give simultaneous form to science experiments and social structures. Removed from concerns about nuclear families, child-rearing, and gender roles it is very difficult to make sense of the Wisconsin experiments at all.

Primatology: case study 3

Primatology is the study of the great apes. But if we want to understand their social life, then how should they be studied? Some – think of Jane Goodall – the scientist lives with his or (usually) her subjects. This (say its protagonists) allows primatologists to observe natural behaviour. Others place their apes in laboratories which (say their supporters) make it easy both to observe behaviour and to control important variables. Donna Haraway tells the story in her book Primate Visions (1989).

In the 1950s and the 1960s Harry Harlow's Primate Research Laboratory at the University of Wisconsin-Madison was one of the most important centres for primate research. In part this was because Harlow was a master-communicator. He told stories and those stories circulated in the media. But in part it was also because he was working on topics and questions that spoke to the anxieties of post-World-War Two America. The focus was the nuclear family. The big question was: how was it holding up in an era of stress? How was it responding to the demands of consumerism? So the focus was gender roles and child-rearing. Were children suffering as middle class American woman came under pressure to go out to work? What was the importance of maternal love? How might children be brought up happily and healthily given all these pressures?

Where better to study these questions than experimentally with those close relatives of human beings, primates? Harry Harlow's laboratory ran experiment after experiment. The experiments were well planned, well managed, and often sadistic. Here is one example: the surrogate mother experiment. This was designed to find out what infant primates needed to be secure. Perhaps something to hold on to of a vaguely simian shape, a surrogate mother? A wire shape with something like a face? What was the minimum needed to secure a version of maternal love? Harlow and his team reduced many young simians to psychosis (there was a freezing 'ice mother'), but along the way they created what one might think of as the minimally functional version of the mother. This was the 'cloth mother': a frame covered by a blanket with a caricatural face and a feeding teat. Infants, it turned out, survived with the latter.

But how to study that formatting? Haraway draws on feminist cultural studies. The key term here is narrative. Narratives are embedded in texts, materials and methods, and in turn draw on tropes. Tropes are figures of speech or metaphors. Think, for instance, of phrases like 'society is an organism' or the notion of 'scientific discovery' and compare these with the idea that 'society is a machine', or 'scientific invention'. Such tropes shape our narratives. Every time we use words they carry clouds of connotations. This is not a complaint: tropes make up the weave of language and culture. They help to make us what we are. But they also carry political and social agendas. And this has been the insight of feminist STS: that formatting work is being done by storylines and the

practices in which they are embedded. So, for instance, other authors explore the character of the technoscience stories which work to naturalise sex-gender differences. Anthropologist Emily Martin talks about metaphors of bounded bodies in pregnancy (Martin: 1998) and immune system discourse (Martin: 1994). Cultural analyst Jackie Stacey explores the role of the monstrous in cancer (Stacey: 1997). And the insight that power generates silences has been also been explored in feminist writing (Ryan-Flood and Gill: 2010). But if stories format common-sense and science alike (think again of Harry Harlow), then how can we narrate and create better alternatives? Haraway answer is to create alternative tropes that interfere with those that are dominant. So, for instance, she creates a feminist cyborg (Haraway: 1985). In its her version this is no longer a cold war-created destructive masculinist military machine-human enhancement. Instead it is a set of partial connections that blurs boundaries including the distinction between fact and fiction. It offers a path to emancipation – alternative non-militarist futures might be imagined. It makes a difference, politically, theoretically and methodologically. And so too should STS (Haraway: 1997).

Performativity

So technoscience practices are shaped by but also shape the social. They help to format the world. This means that they are performative. ‘Performativity’, a term from linguistic philosophy, says that words are sometimes

also actions (think of the words ‘I do’ in a wedding ceremony (Austin: 1965)). We can link this to the dramaturgical idea that social life can be understood as a performance, and its corollary that performances may have real effects that order the social (Goffman: 1971). This double move suggests a new question for STS: that we might think about how methods are staged.

Think, for instance, about the performativity of social surveys. These (see

the box) stage and format people as respondents. In practice this means that people: have telephone lines; speak the appropriate language; are willing to answer questions; are willing to be classified as men or women; that they understand ordinal scales; and that they are willing to admit that they ‘don’t know’ (which suggest that they are buying into something like a ‘knowledge society’). None of this is exceptionable, but neither is it given in the order of things. The survey works because

Surveys: case study 4

No doubt social research methods are socially shaped, but what do they do?

I asked this question for a Europe-wide survey, the Eurobarometer (Law: 2009). In 2007 this interviewed around 29,000 people in the different EU countries. The sample was stratified by country – about 1,000 respondents were interviewed by phone in each. They were asked about their attitudes to farm animal welfare, and how (or whether) this influenced them when they bought meat products. The survey concluded that farm animal welfare was seen as important by European consumers. ‘Please tell me on a scale from 1-10 how important it is to you that the welfare of farmed animals is protected’. This was one of the questions, and the mean score was 7.8. There were significant country differences. For instance Scandinavian respondents trusted the state to look after animals more than people from southern Europe. And many said that they took farm animal welfare into account when they went shopping.

So fine. But what was the survey doing? The answer is lots of things, but here are two. One it was formatting interview subjects. (A person is not necessarily an interviewee. You need to be formatted right.) And two, it formatted collectivities in particular and specific ways. (A collectivity is not necessarily a country, for instance, and a country is not necessarily a collection of people.)

people are being made to fit, even if they don't. (What of transsexual people, or those who don't work with ordinal scales?)

Unsurprisingly it turns out that survey research is an historical social-science achievement (Savage: 2010). It didn't exist until the twentieth century, when people learned that it is acceptable for strangers to ask them questions. But if surveys perform people in their methods of data-collection they also stage them in their findings. For instance (for the Eurobarometer survey in the box) people are formatted as sets of attitudes seeking information to decide whether to buy animal-sourced products. And collectivities are being done too. These become collections – European wide or national – of individuals. The collectivity is formatted as a statistical collection of isomorphic social atoms in a homogeneous conceptual and geographical space.

To say this is not necessarily to criticise. There is no God trick and all methods, quantitative and qualitative alike (Waterton and Wynne (1999)), narrate and format the world. This means that general complaints about what methods do – their performativity – miss the point. Any criticism needs to focus on particular forms of performativity. So, for instance, if Eurosceptics say that the Eurobarometer stages the European Union as a collectivity this is right, though whether this is a criticism depends on one's attitude to the EU. And the sociological critics are right to say that people don't necessarily have stable attitudes which shape how they behave Shove (2010). But what to make of this? We might simply want to say that the Eurobarometer is flawed. But there is a less obvious and more interesting STS argument. This is to tie validity to location. So in the shops the survey is probably wrong. The extent to which peoples' attitudes actually shape what they buy is limited. But in other places the survey is (taken to be) right. Pragmatically it works, for instance, in the European Commission. In the latter the figure of 'the consumer-with-attitude' is successfully staged. It becomes real because it is epistemologically and politically performative. The conclusion? STS tells us truths are practice-embedded, but as Bruno Latour showed when he showed why Pasteur was so successful (Latour: 1988), it also tells us that truths are location-dependent. If French farms were to be 'Pasteurised' they needed to be reformatted as laboratories. And if people are to be treated as attitude-carrying decision makers then receptive administrative and political audiences need similarly to be created.

A final point. The Eurobarometer tells us that 'Hungarians' believe this, whereas 'Italians' believe that. This tells us that it is staging the nation state together with national citizens. But how? Note that national terms are used unproblematically and matter-of-factly. The survey makes no argument for the nation state, but does this mean that its performative effects of nationality are weak? I want to say no. Indeed on the contrary, I want to suggest that formatting is often most powerful when it is almost incidental. The Eurobarometer simply does nationality, as it were, below the radar. Nationality is all the stronger precisely because it is built unproblematically into the survey's frame: because it is simply taken for granted. My suggestion is that methods, social scientific and otherwise, powerfully enact such incidental 'collateral realities' (Law: 2011a) by assuming them. Surely Haraway is right. It is one of STS's tasks to scrape away the self-evident in methods to understand and question how they format the world.

Methods at the centre

SSK author Harry Collins (Collins: 1975) long ago showed that knowledge and methods and scientific authority may all be negotiated together. In a different idiom Thomas Hughes (Hughes: 1979; 1983) made a related argument about system building. Hughes argues that when Thomas Edison created the New York public electricity system he negotiated a heterogeneous web of social, legal, political, economic, geographical, scientific and technical relations all at the same time. Everything was ravelled up to together. But if everything is woven together, what is the best way of thinking about such interconnectedness? STS has tackled this question in various ways. For Hughes system builders were specially gifted at fitting together heterogeneous components. One of the successor projects to SSK and SCOT, co-construction or co-production, explores how the social and the scientific are constructed together – for instance in the form of regulatory frameworks (Jasanoff: 2004; 2015; Shackley and Wynne: 1995). As we have seen, feminist material semiotics uses narrative analysis to understand the forms taken by heterogeneous relations. But and differently again, actor-network theory (ANT) has also tackled interconnectedness, and it has done so in a way that puts methods at the centre.

Actor-network theory

ANT is radically relational. So Michel Callon (see the box) (drawing from post-structuralism (Deleuze and Guattari: 1988; Serres: 1974) and innovation studies (Callon: 1980)) created a conceptual toolkit for talking about heterogeneous relationality: a method for mapping how every object or actor is shaped in its relations. Here nothing has a given form, so the differences between scallops and fishermen grow in the web of relations and don't pre-exist those relations. The implication is that scallops and people might be different elsewhere, and that we need to explore specificities without prejudging their form or shape (he calls this the principle of 'generalised symmetry'.) This is radical in explanatory terms: it represents a substantial shift from SSK. Here the social doesn't shape or explain anything. There is nothing behind the drama shaping it. Society and nature, humans and non-humans, people and technologies, essential divisions have simply disappeared (Law and Mol: 1995). So the macro-social doesn't explain anything either – like everything else the 'macro' and the 'micro' are relationally formatted (Callon and Latour: 1981).

Is everything method?

So what is there left to study? I have just suggested the answer. It is relations, networks, and webs of practice. If we want to understand the world we cannot go anywhere else. We need to look at how webs assemble themselves to stage effects such as actors and objects, not to

mention larger binaries such as nature and culture, human and non-human, or indeed macro and micro. But this is a profound methodological shift. With this move STS is changing from explanations (like social interests) which lie behind the scenes it is describing, and turning itself into the study of methods for assembling. Whatever is going on is being understood as an expression of strategies or tactics. Indeed the case studies of ANT and its related projects can be seen as a list of methods for assembling, stabilising or undoing realities. These methods include delegation into durable materials (Latour: 1987), the creation of circulating immutable mobiles (Law: 1986) or fluid and mutable objects (de Laet and Mol: 2000; Yates-Doerr: 2014), inscription devices (Latour: 1998), and the preformatting of distant locations (Latour: 1988). They also include the logic of tactics (Callon: 1986), and multiple modes of ordering which work together to secure temporarily robust human and non-human arrangements (Latour: 2013; Law: 1994; Law: 2002; Thévenot: 2001).

It is easy to see why the critics say that actor-network theory is Machiavellian. Often it is guilty as accused, but not, I think, always, for it is not necessarily cynical to explore how power is done. On the contrary, if we want to undo power it may help to understand its methods. Perhaps the similarities between ANT, feminist material semiotics, and Michel Foucault's history of the present are instructive. Despite the differences all attend to material and linguistic heterogeneities, and how these generate effects including asymmetries and dualisms. All insist that these are not given in the order of things (Foucault's phrase) and might be otherwise. And all argue that patterns recur: that the world isn't a different place every morning. Perhaps (I doubt this is true for early actor-network theory) they are also all saying that there are sustained patterns of inequality. At any rate, they are all assuming that a methodological microphysics of power is systematically at work that is both productive and excludes alternatives. (Think of Haraway on primates, and Foucault on judicial torture (Foucault: 1979).) And crucially, none works on the assumption that strategies are inevitably explicit or cynical. The argument, then, is that ANT is not necessarily Machiavellian. An analysis of the methods of power and their productivity – a history of the present – may, on the contrary, be used radically to make a difference.

Scallops: case study 5

In 1986 Michel Callon (Callon: 1986) published what may be the most cited article in STS. This was on the scallops, the fishermen and the scientists of Saint Brieuc Bay. The story is about the decline in the scallop population, the attempts by three scientists to understand that decline, and then to create zones protected from fishing in which the scallops might breed, go through their life cycle, and mature. The story traces the attempts by the scientists to create collectors for scallop larvae, the success of this strategy, how the scallops attached themselves to those collectors and started to grow. It details the negotiations between the scientists and the fishermen to create non-fishing zones, and it concludes with the dramatic moment when the agreement broke down and the fishermen scraped the protected areas clean of scallops. However the success or the notoriety of Callon's article has little to do with the scallops themselves. Instead it arises because Callon treats the fishermen, the scientists, and the scallops in the same terms. All are actors. All are strategists and tacticians. All seek to enrol others in their schemes. At Callon's hands, there is no difference in principle between scallops, fishermen or scientists.

Difference

So everything is radically relational and STS case studies work by looking at the patterns of relations in practices: for Callon entities become actors in networks of revisable relations. Essential differences disappear. Everything is endowed with a 'variable geometry', and what becomes important are the tactics and strategies – the methods – embedded in the practices that generate those patterns. No assumptions are made about what will be found. But there is a knock-on effect. Since practices may vary, so too may the entities that they are formatting. This means that 'the same' object may be one thing in one place, and another somewhere else. This is the problem of difference.

Multiplicity

Mol explores this for lower limb arteriosclerosis. She shows (see the box) that the practices that perform this condition different in different places. Then she makes the claim that I just mentioned: that the objects being enacted in those relations are being formatted differently too. Her counterintuitive conclusion takes us to the problem of difference. She says that in practice there isn't a single

arteriosclerosis, there are four. But the practices that format arteriosclerosis aren't independent of one another. This means that arteriosclerosis is a complex pattern of intersections, an object that is more than one but less than many. The different arterioscleroses may line up, contradict, include one another, or never meet up. Like Haraway's cyborg, arteriosclerosis isn't a unity but a set of partial connections (Haraway: 1988; Strathern: 1991). We live in a world of ontological multiplicity.

Disease: case study 6

What is arteriosclerosis? Annemarie Mol explored this in an ethnographic study of lower limb arteriosclerosis in a Dutch town (Mol: 2002). She visited GP's surgeries and listened to patients worrying pain in their legs when they walked. In the hospital she watched technicians taking radiographs which showed the circulatory system in the form of tracery of curves and lines. She visited the ultrasound department and watched the specialists looking for Doppler differences reflecting changes in the speed of blood flows. And then she watched surgeons, opening up blood vessels, and scraping out arterial plaque in the form of a white, putty-like substance.

Four practices, each about lower limb arteriosclerosis, but what is this condition? The standard story says that long term changes in the blood lead to the build-up of arterial plaque which limits the blood flow which in turn starves the muscles of oxygen and causes pain. In practice sometimes these signs and symptoms from Mol's different fieldwork sites fitted together nicely, but sometimes they didn't. The Doppler might say one thing, and the radiograph something different. These would be the kinds of differences hammered out at a case conference. Mol notes that this worked because it assumed that there is an object out there, and the specialists had different perspectives on it. However, her own argument is quite different and very far from the common sense of this standard story. She says that different practices enact different arterioscleroses. These practices and their arterioscleroses relate to one another in theory but not necessarily in practice.

A word on ontology. Philosophers use the term to talk of what there is in the world, or what reality out there is made of. Most Western philosophers assume that the stuff of reality is constant and that we share the same reality-world. Then we explain our disagreements about reality by saying that we have different perspectives. But recent STS is pushing back against this. In the way I have just

suggested, it is saying that ontologies are relational effects that arise in practices (Barad: 2007), and that since practices vary, then so too do objects. This argument is important in STS because it softens realities – it means that they are not given (Abrahamsson and others: 2015). It also means that we might imagine realities that are better. In short, an ontological politics (Mol: 1999) or a cosmopolitics (Stengers: 2005) becomes possible because different normativities and realities are being woven together in what Mol calls ‘ontonorms’ (Mol: 2012)). So a feminist cyborg may be better than one that is militarist, or the arteriosclerosis of physiotherapy might sometimes have advantages over the one performed in surgery.

Two further points. The first is a caution. Performing objects is tough, even in this relational world. It is difficult and costly (think of Mol’s hospital departments). We can’t just dream new realities up. (Latour and Woolgar: 1986; Law: 2011a). Second we need to ask: where is difference? We can debate, but the intuition that underpins Mol’s intervention is that we will always find it if we go looking for it, and that finding it is, so to speak, an analytical and normative choice. To this I would add a methodological rider: we need to be wary of stories about consistency and coherence. Instead it might be better to cultivate a sensibility for mess (Law: 2004). Though, of course, there is also an art in distinguishing between mess that is politically and methodologically important and that which is not.

Method and difference

So contemporary STS asks questions that are simultaneously about realities and politics or normativities. Recognising its own performativity it understands that makes a difference. But what kind of difference does it make? The answer is that it tries to find ways of living together well. It does this in many ways, but here I will pick out two.

In a world in crisis economically, socially, and environmentally it is clear that we urgently need to find better ways of living together. STS tells us that technoscience in its present form is part of the problem. Separated from the political it is destructive because it likes to assume that reality is fixed. So how to think about this? One set of STS tools for tackling this question comes from democratic political theory and practices. Democracy is about living together well in a common world. Yes, the old ways of reconciling difference democratically – parliaments and their analogues – have failed because they reproduce the nature-culture divide, fix nature and exclude it from politics. The task, then, is to invent new methods for softening realities, reworking social collectivities, and melding these productively and democratically together. Many have wrestled with this, but none more systematically than Bruno Latour. He has talked of non-modern constitutions, of parliaments of things, of matters of concern, of new forms of political ecology, of the importance of due process, and the need for diplomacy to hold together different conditions of felicity or modes of existence (Latour: 1993; Latour: 2004a; Latour: 2005; Latour: 2013). Throughout his urgent task has been to imagine ways of generating common responses to common problems in a common world. Less ambitious but somewhat similar concerns inform, for instance, the work of Michel Callon, Pierre Lascoumes and Yannick Barthe (Callon, Lascoumes, and Barthe: 2009) who experiment with hybrid forums which mix experts, non-experts and politicians. The object is to melt the categories of nature that were previously hardened and fixed in professional silos. As a part of this they undertake

experiments 'in the wild' to secure collective learning and recompose a better common world. Again the interference is procedural and methodological.

How can we go on together well in difference? This question – adapted from Helen Verran (2013) – takes us to the second strategy. Similar in many ways to the first, it is more modest because it makes no assumption about common frameworks. If democracy wants to reconcile difference overall, then the second strategy is not about democracy. Neither does it try to generalise. Rather it is about detecting and handling difference well, case by case (Law and others: 2014). Mol, for instance, argues that the arteriosclerosis of physiotherapy may be better for some patients than that of surgery (Mol: 2010).

Tsouvalis and Waterton (see case study 7) work locally on the environmental problem of algal bloom to soften scientific and social categories. And Ingunn Moser's work on dementia care shows how Marta Meo care methods enact patient competences that don't fit into textbook medical science (Moser: 2008). Like Mol she is chipping away at the dominance of biomedical realities and treatment regimes by talking up processes of care (See also (Pols: 2006) and Singleton (2010)). Importantly, none of these authors offer general prescriptions.

Similar power-

asymmetrical encounters across difference are common in North-South relations where Southern realities about land, gods, animals, people, bodies and social ordering are typically turned from realities into mistaken 'beliefs'. So people are not visited by spirits: they are psychotic (Bonelli: 2012). The land is not a living thing: it is empty (Verran: 1998; 2002). The mountain is not a God: it is a mineral-rich resource (de la Cadena: 2010). A food additive does not reduce children's malnutrition: this is a fiction (Marques: 2014). Bodies don't have meridians: they are neuromuscular entities (Kuriyama: 1999). Glaciers don't take offence: they are ice flows (Cruickshank: 2012). In all these encounters two realities are being staged, but one is refusing the other (Law: 2015). The issue

Eutrophication: case study 7

Loweswater is a lake in the English Lake District. Romantically pretty, relatively remote, the valley is a location of economically marginal highland sheep farming, long term out-migration by farmers, and their replacement by incomers. It is also a lake which suffers from blue-green algal bloom. Why? And what might be done to present this this?

Between 2007 and 2010 Judith Tsouvalis and Claire Waterton ((2012; 2015)) brought together a mixed group of farmers, residents, social scientists, environmental scientists, and representatives of the Lake District National Park to focus on the problem. Everyone was clear that something needed to be done. The issue was: what? Tsouvalis and Waterton worked to facilitate a collective experiment of the kind recommended by Bruno Latour. They looked for ways of opening things up and slowing them down. How were the scientific findings produced? Might these be discussed and questioned? Was it possible to situate them and put them alongside other kinds of framings, economic, social or recreational? Could scientific findings be softened (Latour: 2004b) from stabilised 'matters of fact' to situated 'matters of concern'? The answer, it turned out, was yes, at least within limits.

Was it possible to foster a tolerant but reflexively critical approach within the group, and open discussion up to different kinds of framings? Was it possible to move forward on the understanding that there are limits to all forms of knowing, those of technoscience included? Was the group capable of working with the idea that human-non-human relations are complex? And that there were no definitive solutions and that humility in the face of complexity might be what was needed? Again the answers to these questions were yes. Yes, there were also many frustrations, but a more relationally fluid collectivity, one that attended to the importance of process, was provisionally tinkered, into being.

then becomes: how to discover techniques for undoing this refusal and going on well together in difference. As I noted above, this phrase comes from Helen Verran (1998; 2013) who charts how the Australian legal system and Australian Aboriginal people have learned how to respond to one another across difference. Is land an area, or is it part of a continuing creation? The solutions are far from perfect, but Australian law has created practices which recognise ownership in both senses. Such techniques for living well with difference do not always work and they need to be crafted case by case. Perhaps the job of STS – and here it works with post-colonialism – is to chart differences, articulate these, and help to craft ways of going on well together in difference (Blaser: 2009; Feit: 2004; Turnbull: 2000; Verran: 2002).

Knowledge spaces

STS tells us that methods are never simply techniques. Theories, methods, the empirical, modes of writing, disciplinary structures, audiences, authorities, and realities, all are staged together. Other candidates are jostling to join this list including organisational structures, career concerns, and social, economic and technical infrastructures (think, for example, of everything that goes into publishing), and imaginaries, national and otherwise (Felt: 2015; Jasanoff and Kim: 2015). The argument (it's at work in all the empirical examples above) is that knowing and its methods are materially complex and performative webs of practice that imply particular arrays of subjects, objects, expressions or representations, imaginaries, and institutions. Perhaps we might think of these heterogeneous arrays as knowledge spaces (Law: 2011b) because: they set more or less permeable boundaries to the possible and the accessible; they are defined by patterns of relations which enact those gradients of possibility and accessibility; and they intersect with and are implicated in the generation of alternative knowledge spaces that cannot be included (think of Darwinism and Creationism).

Now think about the power and the obduracy of these knowledge spaces. The point is simple. In any given location it is easier to know in some ways than in others. It may be challenging to publish in major journals, but the literary conventions, procedures, required competences, appropriate topics, and acceptable theoretical frameworks are all – albeit often implicit – generically clear. Together they enact academic knowledge spaces within which it is comparatively easy to operate. As a part of this they also substantially define what is possible in an academic career. In practice STS knowledge spaces are multiple, but if we shift beyond their conventions, knowing becomes progressively more difficult for an academic. The wrong topic? A case study that is not of interest to an 'international' (a US?) audience? The wrong language? A strange theory? Inappropriate methods? Excessive commitment to activism? A mode of writing that doesn't look like a standard journal article? This is getting risky. So here is the question. Is it possible to imagine alternative STSs? And (almost the same question though it sounds quite different), is it possible make a career within STS by alternative means? By writing poetry, or dancing? By writing popular texts? In a reciprocal embodied regard? In simulations, or in mounting exhibitions, or immersive ethnography? By being an activist? By working in the space between science and art? Or by consulting with the spirits?

No individual could do all these things, but I have chosen these particular examples because they suggest hybrid or unconventional knowledge spaces that are indeed possible. Some have

successfully worked through exhibitions (Latour and Weibel: 2006), or by writing poetry (Cole: 2002), or poetry in combination with other media (Watts, Ehn, and Suchman: 2014), or semi-popular texts (Raffles: 2010), or in simulations (Guggenheim, Kraeftner, and Kroell: 2013), reciprocal human-animal interactions (Despret: 2013), immersive ethnography, activism (Haraway: 2008; Wynne: 1996), artworks (Jones: 2011; Neuenschwander: 2008), art-science intersections (Gabrys and Yusoff: 2011; Kraeftner and others: 2010; Shared Inc.: 2014), or in participative methods (Waterton and Wynne: 1999). Others have done so in dance (Cvejic: 2010; Myers: 2012) or by consulting with the spirits (Smith: 2012) – a way of knowing which is very important in some post-colonial contexts. Such efforts represent brave efforts to experiment with hybrid knowledge spaces. But all that said, creating different knowledge spaces is tough. It is slow, it is hazardous, it is often lonely, and uncertain – as is amply illustrated by many of the fascinating cases described by Raffles in his Insectopedia. And, to pick up a theme touched on in the previous section, the unwitting ‘Northern’ character of STS knowledge spaces sets stark limits to alternative ‘Southern’ forms of STS. What, for instance, might an alternative ‘Chinese’-inflected STS look like? Who knows, but one thing is certain: its theories and its methods would look quite unlike those in current STS. (Lin and Law: 2014)

Conclusion

In this chapter I have argued: that methods are shaped by the social; that they also shape, stage and format the social; that they are performative and heterogeneously enact objects, worlds and realities; and that they are situated, productive, essentially political and normative, and that they might be otherwise. Then I have argued that with the decline of larger explanatory schemes STS has increasingly attended to the tactics and strategies of practice – that is to methods and how these stage the world. I have also suggested: that since practices vary between locations they generate different realities and normativities; that the relations between these are uncertain; and that much STS is currently struggling in one way or another to generate methods that recognise, properly attend to, or stage better ways of handling difference.

The story I have told is therefore about method in a double sense. It has been about both the methods embedded in the objects and the processes that we study and those that make up our own STS practices. As is obvious, the two are intertwined. What we detect in the world arises in the interference between our own practices and those of the world. The implication is clear. This chapter – like every other in the Handbook – should be understood as its own situated intervention. Even-handedness is not possible. The God trick is out. Coming from a space between actor-network theory, feminist material-semiotics and post-colonialism, I have therefore staged relationality, specificity, difference, binary breakdowns, and politics or normativities in ways which others might not. Essential categories and realities have been reinterpreted as relational effects, and the search for causal explanations has been played down in favour of a multiplicity of links. As a part of this I have adopted an expansive or generous understanding of method. And again as a part of this, I have gone searching for non-coherences both as a matter of taste and of politics. My object has been to suggest, both implicitly and explicitly, that it is the urgent task of STS first to attend to difference, and second to craft specific ways of going on well together in difference – ways of being that are therefore multiple. There are no single solutions. What it means to go on well together in difference is necessarily contested. At the same time we need to remind ourselves that the world is not open

and that not everything is possible. We cannot invent realities or better ways of living by simply dreaming up new methods. But this does not mean that we cannot try, just a little, to open up and enact alternative and better possibilities. The hope is that in this way we can avoid giving comfort to a politics that denies that it is political. We can resist the claim that reality is destiny. So perhaps in the end the enemy is hubris. Things never have to be the way they are. That is the point of this STS of method.

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