

Handbook of Science and Technology Studies

Fourth edition

Edited by Ulrike Felt, Rayvon Fouché, Clark A. Miller, and Laurel Smith-Doerr

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In one way or another STS almost always works through *case studies*. These evoke, illustrate, disrupt, instruct, and help STS to craft and recraft its theory (Heuts and Mol 2012; Yates-Doerr and Labuski 2015). Though the status of case studies is a source of controversy (Beaulieu, Scharnhorst, and Wouters 2007; Gad and Ribes 2014; Jensen 2014), nonetheless, if you want to understand STS—and STS theory—you need to read it through its cases. Such changes include how fishermen and scallops interact in practice (Callon 1986), how engineers and military chiefs create a warplane, how work in a laboratory generates new theories about physical forces, how primatology reproduces patriarchy, and how environmental scientists misunderstand aboriginal Australians. Some outside the discipline find this difficult: they think of theory as abstract. But the STS focus on practice means that theory, method, and the empirical get rolled together with social institutions (and sometimes objects). They are all part of the same weave and cannot be teased apart.

In the next section, “Shaping,” I describe how early STS rejected philosophers’ stories about scientific method and argued that science is shaped by social interests. In the “Structuring” section I use feminist STS to show that technoscience methods also in turn shape or format the social. The “Methods at the Center” section pushes this further by suggesting that everything, social and natural, is shaped in practices. The section on “Difference” shows how this varies in different practices and opens up the possibility of a politics of things. In “Knowing Spaces” I briefly review how methods link with subjects, objects, expressions and representations, and institutions; and the “Conclusion” reviews the argument of the chapter as a whole. Along the way feature boxes give a flavor of particular STS case studies.

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 an answer in the introduction. Fifty years ago most of those who thought about science 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 collecting accurate data, generating logical generalizations which explain that data, and testing those generalizations. Philosophers generally said that scientific knowledge—good, true, or accurate knowledge—grows if people adopt the scientific method (Popper 1959). In response to Nazi and Soviet political interference, they said

prejudice distorts how scientists observe phenomena, erodes logical reasoning, and undermines objectivity.

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 power (Slack 1972). This means that scientific knowledge is irredeemably ideological. Others argued that science is necessarily social. Scientists are trained to see the world in particular disciplinary (and therefore social) ways. They 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. This “sociology of scientific knowledge” (SSK) (Barnes 1977; Bloor 1976; Collins 1975) drew on the work of historian Thomas Kuhn (1970b). It said that science, its knowledge, its methods, and its practices are disciplinary cultures and that scientific knowledge is shaped in interaction between the world on the one hand and the culture of science, including its methods, on the other.

Note three points before moving on. First, in 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 scientific 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: that methods are not simply techniques but carry personal, skill-related, theoretical, and other agendas (Kuhn 1970a; Polanyi 1958; Ravetz 1973). This means that the STS concern with methods spills over into much that is not obviously methodological because methods cannot be separated from their social context. These become cultural, practical, materially based, theoretically implicated, institutionally located, and socially shaped routines or procedures, all raveled up with everything else.

Social Shaping

SSK opened up space for laboratory studies which ethnographically explored the construction of knowledge (Knorr Cetina 1981; Latour and Woolgar 1986; Lynch 1990; Traweek 1988). It also, and a little differently, asked how science, its methods, and its findings are *shaped*. SSK answered this question in two closely related ways. First it said, as I have just noted, that scientists work with cultural tools. And second, it said that

Box 1.1**Statistics: Case Study 1**

Correlation is a way of measuring how two variables relate to one another. Here's an example. Vaccination (or not) is one (nominal) variable, and catching a disease (or not) is a second. If none of those vaccinated catch the disease and all those unvaccinated do, then the two variables are highly 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 (1978) looked at one such dispute. In 1905 the protagonists—George Udny Yule and Karl Pearson—had invented two different ways of measuring correlation. Yule's approach was straightforward. Pearson's was more complicated: he assumed that variables reflected normal distributions. Why? MacKenzie makes two arguments about 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* interest to do so. But his approach to correlation also fitted 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 toward the top of the curve, and the working classes (in need of eugenic improvement) lay toward the bottom. MacKenzie suggests that his complex way of calculating correlation was also in Pearson's *social* interests.

scientific knowledge is shaped by social interests. Donald MacKenzie (1978) explored this for a controversy about statistical correlation (see Box 1.1).

Statistical procedures (like other scientific theories or methods) are tools for making sense of the world. But (this is the new move) how those tools are constructed depends on the tasks that we set them. Yule's method for calculating correlation was useful in many ways, but it was never going to do the kind of work that Pearson sought to do with his tetrachoric coefficient of correlation, namely, to help to show whether some kinds of people are superior to others.

MacKenzie's case study is much more sophisticated than 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 both that interests may shape science in ways invisible to those involved and that the fact that interests are at work tells us nothing about the validity of the science involved. Knowledge that works, "good" knowledge, is necessarily shaped, and sometimes by social interests of which we disapprove (Barnes 1977). And finally, MacKenzie's study is just one example of SSK at work. For instance, Jonathan Harwood (1976) wrote on race and intelligence, and the same approach was

developed to explore technology in the *social construction of technology* (SCOT). Why did the penny-farthing bicycle give way to the safety bicycle? Wiebe Bijker (1995) showed the penny-farthing was linked to macho forms of masculinity. Women—and many men—couldn't or weren't supposed to 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 safer and more modest. Here gender and commercial interests together shaped a technology. And (another example) Cynthia Cockburn (1999) argued that the technologies of the precomputer 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 almost disappeared. But the insight that technoscience and society are woven together also came from feminism. For instance, Donna Haraway (1988) talked of the “God trick” to describe the mistaken and self-serving claim that science speaks impartially. The idea—or the ideal—of objectivity has a long and varied history. (See Daston [1999] and case study 2 below.) Usually this implies impartial detachment from local prejudices, blinkers, and idiosyncrasies. But Haraway argued that knowledges and methods are irredeemably *situated*. The stories they tell about the world always 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. But does it also mean that everything is subjective? Haraway's response is that we can hang on to objectivity by making 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, its prejudices, and its blind spots as matter of critical inquiry in their own right. For Haraway, objectivity is doubly “partial” because it knows that it is one-sided and because it also recognizes that it is incomplete. Her argument is that to achieve objectivity, scientists and social scientists need to be *accountable* for what they write rather than hide behind the fiction that what they are reporting comes direct and unmediated from nature. Sandra Harding makes a similar argument. 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

Box 1.2**Separating Science from Society: Case Study 2**

In London in the 1660s in the newly created Royal Society Robert Boyle was wrestling with the question: how can we reliably learn about nature? The answer wasn't obvious. For instance, the Bible was full of powerful stories about the creation of the world. Boyle was interested in air pressure. A devout Anglican and a royalist, he also wanted to divide facts about the world (or 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 (1985) call this a *material technology*. But more was needed. The experiments needed witnesses, but not everyone could come to London to see those experiments for themselves. They needed to be told about them. This led to the creation of a *literary technology*, in which experimental accounts were written in a modest and matter-of-fact way, sticking to the facts, and excluding opinions and speculations. This was the second big innovation. But there was a third question: who could be trusted as a reliable witness? To answer this question, Boyle drew from the English legal system. In a court of law reliable witnesses were independent. Servants could not be trusted because they were not independent of their masters. Neither could women: 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 what Haraway calls the "God trick" came from (Shapin 1984; Shapin and Schaffer 1985).

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? Steven Shapin and Simon Schaffer (1985) suggest that this was created in very particular social circumstances in London in the 1660s and the 1670s (see the case study in Box 1.2).

At this historically important moment "nature" was separated from "the social" and "the political" and this separation was successfully institutionalized. 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.

Structuring

Haraway (1997) raises questions about parts 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, Pearson, and class, gender, and technology in the print trade hint at something more. They suggest that technoscience is not simply shaped by the social but helps in turn 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 and also that they help to reproduce such inequalities. But how?

Feminist Cultural Studies of Science

The third case study (see Box 1.3) shows how 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 and reproduce them. Social concerns fed into technoscience practice, and technoscience fed these back into social agendas. These were "structuring" practices—methods—that give simultaneous form to science experiments, structures or forms of knowing 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.

But how to study that structuring? 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 contrast these with "society is a machine," or "scientific invention." They do different kinds of work. Such tropes shape our narratives and 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 done in storylines and the practices in which these are embedded. So, for instance, technoscience stories may naturalize sex-gender differences. Anthropologist Emily Martin talks about metaphors of bounded bodies in pregnancy (Martin 1998) and immune system discourse (Martin

Box 1.3**Primatology: Case Study 3**

Primatology is the study of the great apes. But how should they be studied? Sometimes—think of Jane Goodall—scientists live with their subjects. This allows primatologists to observe natural behavior. Others place their apes in laboratories, which makes it easy both to observe behavior 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 an important center for primate research. Harlow was a master communicator who told media-savvy stories. But he was also working on topics and questions that reflected the anxieties of post-World War II America. The focus was the nuclear family. The big question was: how was this 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 women 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 such pressures?

Where better to study these questions experimentally than with those close relatives of human beings, the primates? Harry Harlow's laboratory ran experiment after experiment. These were well planned and managed—and often sadistic. One example is 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 in the end 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 caricature of a face and a feeding teat. Infants, it turned out, survived with the latter.

1994). Cultural analyst Jackie Stacey (1997) explores the role of the monstrous in cancer. The insight that power generates silences has also been explored in feminist writing (Ryan-Flood and Gill 2010), while Lorraine Daston (1999) describes the shifting history of objectivity. But if stories structure common sense and science alike, then how can we narrate and create better alternatives? Haraway's answer is to create alternative tropes that interfere with those that are dominant. So, for instance, she creates a feminist cyborg (Haraway 1985). No longer a Cold War-created destructive masculinist military machine-human enhancement, this is a set of partial connections that blurs boundaries including the distinction between fact and fiction. It offers a path to emancipation in which 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 (like “I do” in a Christian wedding ceremony [Austin 1962]). We can link this to the dramaturgical idea that social life may be understood as a performance and to its corollary that performances may have real effects that order the social (Goffman 1971). This double move suggests a new focus for STS: to explore *how methods are staged*.

Think, for instance, about the performativity of social surveys. These stage and structure people as respondents (see Box 1.4). 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, understand ordinal scales, and are willing to admit that they “don’t know” (which suggests 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 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 (Igo 2006; Savage 2010). It didn’t exist until the twentieth century, when

Box 1.4

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 to 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. But what was the survey *doing*? The answer is lots of things, but here are two. It was shaping *interview subjects*. (A person is not necessarily an interviewee. You need to be formatted right.) And it shaped 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 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, in the Eurobarometer 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 of individuals: collections of isomorphic social atoms in a homogeneous conceptual and geographical space.

To say this is not necessarily to criticize. There is no God trick and all methods narrate and format the world (Waterton and Wynne 1999). This means that general complaints about the performativity of methods miss the point. Any criticism needs to focus on particular forms of performativity.

So, for instance, Eurosceptics correctly argue that the Eurobarometer stages the European Union as a collectivity, and sociologists are right to say that people don't necessarily have stable attitudes shaping how they behave (Shove 2010). Perhaps we simply need to say that the Eurobarometer is flawed. But there is a less obvious and more interesting STS argument. This ties validity to location. So in shops the survey is probably wrong: the extent to which attitudes shape what people buy is limited. But in other places the survey is (taken to be) right. Pragmatically, for instance, in the European Commission 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 (1988) showed when he explored why Pasteur was so successful, 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 similarly need to be created.

A final point. The Eurobarometer says that "Hungarians" believe this, whereas "Italians" believe that. This tells us that it is staging the nation-state as well as national citizens. But how? Note that national terms are used unproblematically. The survey makes no argument for the nation-state, but does this mean that its performative effects of nationality are weak? I want to suggest, on the contrary, that formatting is often most powerful when it is almost incidental. Nationality is being done strongly 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 to understand and question how methods structure the world.

Methods at the Center

SSK author Harry Collins (1975) long ago showed that knowledge and methods and scientific authority may all be negotiated together. In a different idiom Thomas Hughes (1983) made a related argument about system building. Hughes argued that when Thomas Edison created the New York public electricity system he generated a heterogeneous web of social, legal, political, economic, geographical, scientific, and technical relations. Everything was raveled up to together. But 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; Shackley and Wynne 1995). As we have seen, feminist material semiotics uses narrative analysis to understand the forms taken by heterogeneous relations. Differently again, actor-network theory (ANT) has also tackled interconnectedness in ways that put methods at the center.

Actor-Network Theory

Actor-network theory is radically relational. So Michel Callon (see Box 1.5) (drawing from post-structuralism [Deleuze and Guattari 1988; Serres 1974] and innovation studies [Callon 1980]) created a conceptual tool kit for talking about heterogeneous relationality, a method for mapping how every object or actor is shaped in its relations.

Box 1.5

Scallops: Case Study 5

In 1986 Michel Callon 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 of the scallop population, the attempts by three scientists to understand that decline, and efforts to create zones protected from fishing where scallops might breed and mature. The story traces the successful attempts by the scientists to create collectors for scallop larvae. 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 he treats the fishermen, the scientists, and the scallops in the same terms. *All* are actors. *All* are strategists and tacticians. *All* seek to enroll others in their schemes. At Callon's hands, there is no difference in principle between scallops, fishermen, and scientists.

Here nothing has a given form. The differences between scallops and fishermen grow in the web of relations and don't preexist those relations. So scallops and people might be different elsewhere, and it is important to explore specificities without prejudging their form or shape (he calls this the principle of "generalized symmetry"). This is radical in explanatory terms: it represents a substantial shift from SSK. For Callon, the social doesn't shape or explain anything. Society and nature, humans and nonhumans, people and technologies—essential divisions have simply disappeared (Law and Mol 1995). So the macrosocial doesn't explain anything either—like everything else the "macro" and the "micro" are relationally generated (Callon and Latour 1981).

Ordering Methods

These conclusions are controversial. Many in STS remain attached to macro-micro distinctions. But if we follow its logic, we need to study relations, networks, and webs of practice. We need to look at how webs assemble themselves to stage effects such as actors and objects, and binaries such as nature and culture, human and nonhuman, or indeed macro and micro. But this is a profound methodological shift, because with it STS moves from explanations (like social interests) which lie behind events to attend instead to methods for assembling. Whatever is going on is seen 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, stabilizing, 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 together secure temporarily robust human and non-human arrangements (Latour 2013; Law 1994, 2002; Thévenot 2001).

It is easy to see why the critics say that actor-network theory is a Machiavellian description of ruthlessly successful political tactics. Sometimes 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 if we understand its methods. Here the similarities between ANT, feminist material semiotics, and Michel Foucault's (1979) history of the present are instructive. Despite 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 (early ANT excepted) they are also 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, instead, be used to make a political difference.

Difference

So in a material semiotic way of thinking everything is radically relational. Essential differences disappear. Everything is endowed with a “variable geometry,” and it becomes crucial to explore the tactics and strategies—the methods—embedded in practices. 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. In STS this is called the *problem of difference*.

Multiplicity

Mol (2002) explores multiplicity for lower-limb arteriosclerosis. She shows (see Box 1.6) that the practices that perform this condition are different in different places.

Then she makes the claim that I just mentioned: that the objects being enacted in those relations are being differently shaped 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, never meet up—or combine some mix of these. 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*.

Philosophers use the term *ontology* 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, that we share the same reality-world, and that we disagree about reality because we have different perspectives on it. 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; Law 2002) and that since practices vary, so too do objects. This softens realities—it means that they are not given (Abrahamsson et al. 2015). It also means that we might imagine better alternative realities. A “politics of

Box 1.6**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 GPs' surgeries and listened to patients worrying about leg pain 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 white, puttylike, arterial plaque.

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 buildup of arterial plaque, which limits the blood flow which in turn starves the muscles of oxygen and causes pain. In practice Mol found that sometimes these signs and symptoms fitted together nicely, but sometimes they didn't. If this happened, then the differences were hammered out at a case conference. Mol notes that this worked because everyone 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.

things," 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 have advantages over the one performed in surgery.

Two further points. First, 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 up new realities. (Latour and Woolgar 1986; Law 2011a). Second, we need to ask where we might find difference. We can debate, but the intuition that underpins Mol's intervention is that we will always discover it if we go looking for it, and that doing so is an analytical and normative choice. But this implies 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. There are no rules here, but simply noting that the world

is non-coherent is not a discovery. We also need to know what kind of a difference we are hoping to make.

Method and Difference

In this version contemporary STS asks questions that are simultaneously about realities and politics or normativities. Recognizing its own performativity, it understands that it makes a difference. But what kind of difference does it make? The answer is that it typically tries to find ways of living together well. It does this in many ways, but here are 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 takes reality to be fixed. So how to think about this? One answer draws on democratic political theory and practice. Democracy is about living together well in a common world. Perhaps 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, 2004a, 2013). Throughout, his urgent task has been to imagine ways of generating common responses to common problems in a common world. Less ambitious but related concerns inform work on publics (Marres 2007) and the work of Michel Callon, Pierre Lascoumes, and Yannick Barthe (2009), who experiment with hybrid forums which mix experts, nonexperts, 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. Though similar 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 generalize. Rather it is about detecting and handling difference well, case by case (Law et al. 2014). So Mol (2010) argues that the arteriosclerosis of

Box 1.7**Eutrophication: Case Study 7**

In Western ways of thinking “nature” is divided from “culture.” Nature is taken to have particular attributes. Science seeks to reveal these. In contrast, culture is known to be variable: different groups of people believe different things. But what to make of the STS idea that science is cultural too? Situated? Potentially revisable? How to think about intractable problems that are both natural and social?

Between 2007 and 2010 Claire Waterton and Judith Tsouvalis (2015) brought together farmers, residents, social scientists, environmental scientists, and administrators to discuss the persistence of blue-green algal bloom in Loweswater in the English Lake District. Everyone was clear that something needed to be done. The issue was what? Waterton and Tsouvalis looked for ways of opening up scientific and social uncertainties. How were the scientific findings produced? Might these be discussed and questioned? Was it possible to situate them alongside other kinds of framings, economic, social, or recreational? Could scientific findings be softened (Latour 2004b) from stabilized “matters of fact” to situated “matters of concern”? The answer, it turned out, was yes, at least within limits.

Was it possible to appreciate that there are limits to all forms of knowing, those of technoscience included? Was the group capable of working with the idea that human-nonhuman relations are complex, 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. There were many frustrations too, but a more relationally fluid collectivity, one that attended to the importance of context and process, was provisionally tinkered into being.

physiotherapy may be better for some patients than that of surgery. Michel Callon and Vololona Rabearisoa (2004) explore the intersection of different forms of morality and humanity and the role of silence for the case of a patient with muscular dystrophy. Waterton and Tsouvalis (2015) (see case study Box 1.7) work locally on the environmental problem of algal bloom to soften scientific and social categories. And Ingunn Moser’s work on dementia care suggests that Marta Meo care methods enact patient competences that don’t fit textbook medical science (Moser 2008). Like Mol, she is chipping away at the dominance of biomedical realities and treatment régimes by talking up processes of care (see also [Pols 2006] and Singleton [2010]). Importantly, none of these authors offer general prescriptions.

Postcoloniality

Similar power-asymmetrical encounters across difference are common in North-South relations where alternative 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). The forest does not depend on shamans: it is a place to mine gold (Kopenawa and Albert 2013). A food additive does not reduce children's malnutrition: this is a fabrication (Marques 2014). Bodies don't have meridians: they are neuromuscular entities (Kuriyama 1999). Glaciers don't take offense: they are ice flows (Cruikshank 2012). Chinese medicine is not experiential: it is theory-deficient (Zhan 2014). In all these encounters two realities are being staged, but one is refusing the other (Law 2015). The issue then becomes how to discover techniques for undoing this refusal and going on well together in difference. As I earlier noted, this phrase comes from Helen Verran (1998, 2013), who charts how the Australian legal system and 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 recognize 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 a task for STS—working here with postcolonialism—is to chart differences, articulate these, and help to craft ways of going on well together in difference (Blaser 2009; Feit 2004; TallBear 2014; Turnbull 2000; Verran 2002).

Knowing Spaces

STS suggests 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 organizational structures, career concerns, social, economic, technical and publishing infrastructures, and imaginaries, national and otherwise (Felt 2015; Jasanoff and Kim 2015). The argument (as in 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, metaphysical assumptions, normativities, and institutions. Perhaps we might think of these heterogeneous arrays as “knowing 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 knowing spaces that cannot be included (think of Darwinism and Creationism).

Now think about the power and the obduracy of these knowing spaces. In any given location it is easier to know in some ways than in others. It may be challenging to publish in major academic journals, but at least the appropriate literary conventions, procedures, competences, appropriate topics, and theoretical frameworks are reasonably clear. Together they enact academic knowing spaces within which it is comparatively easy to operate, and they substantially define what is possible in an academic career. But if we shift beyond these conventions, knowing becomes progressively more difficult for an academic. The wrong topic? A case study that is not of interest to an international (a U.S.?) audience? The wrong language? A strange theory? Inappropriate methods? Excessive commitment to activism? The “wrong kind” of activism? 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?

In practice, the answer is yes: hybrid or unconventional knowing spaces 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 semipopular texts (Raffles 2010), or in simulations (Guggenheim, Kraeftner, and Kroell 2013), reciprocal human-animal interactions (Despret 2013), activism (Haraway 2008; Wynne 1996), artworks (Jones 2011; Neuenchwander 2008), art-science intersections (Gabrys and Yusoff 2011; Kraeftner et al. 2010), or in participative methods (Waterton and Wynne 1999). Others have done so in dance (Cvejic 2010; Myers 2012) or by consulting with the spiritual realm (Smith 2012)—a way of knowing important in some postcolonial contexts. Such efforts represent brave efforts to experiment with hybrid knowing spaces. But creating different knowing spaces is slow, hazardous, and often lonely and uncertain. And, to pick up a theme touched on in the previous section, the unwitting “Northern” character of STS knowing spaces sets stark limits to alternative “Southern” forms of STS. So, for instance, in a “Chinese”-inflected STS, theories and methods might look quite different (Lin and Law 2014).

Conclusion

In this chapter I have argued that methods are shaped *by* the social; that they *also* shape, stage, and structure the social; that they are performative and heterogeneously enact objects, worlds, and realities; 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, to methods, and to 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 recognize, properly attend to, or stage better ways of handling difference.

The story I have told has been both about the methods in 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. And this is why this chapter should be understood as its own situated intervention. Even-handedness is not possible, and the God trick is out. Coming from a space between actor-network theory, feminist material-semiotics, and postcolonialism, I have staged relationality, specificity, difference, binary breakdowns, and politics or normativities in ways which others might not. I have reinterpreted essential categories and realities as relational effects and searched for multiplicity rather than causal explanations. As a part of this, I have adopted an expansive or generous understanding of method and sought noncoherences as a matter of both taste and 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 but multiple ways of going on well together in difference. There are no single solutions. What it means to go on well together in difference is necessarily contested. Though we need to remind ourselves that the world is not open and that not everything is possible, 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, and 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. Such is the point of this STS of method.

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