Andrew Pickering (ed.) (1992) Science as Practice and Culture. Chicago: University of Chicago Press. 474 pages. ISBN: 9780226668017

Helen Verran

Helen.Verran@cdu.edu.au

Why does *Science as Practice and Culture* warrant review twenty-five years on from its publication when most STS books published that year have already sunk, unremarked, into obscurity? I propose that in 2017 *Science as Practice and Culture* is a useful diffraction grating by which to examine contemporary STS. I re-read the book attempting to identify both the major currents, and the 'back-channels' of STS collective life twenty-five years ago with the aim of illuminating the present.

In 1993, Malcolm Ashmore, began his review of the book this way: "This volume… has a mission encapsulated in the following slogan or rallying cry: sociology of knowledge (SSK) is dead; long live sociology of scientific practice!" (Ashmore, 1993: 489). One way to understand this review then is as an answer to the question 'What have we made of sociology of scientific practice twenty-five years on?' I am doing the reflexive work urged on us back then by scholars like Ashmore. One story of what we have made of it began to emerge for me as impressions gleaned in working with groups of graduate students in Holland, Denmark and California earlier this year. The groups seemed to share a working imaginary of what STS is nowadays, including a particular story of its pasts. I later found it explicitly articulated (Puig de la Bellacasa, 2017: 31).

The story goes like this: in STS epistemics first we had the contest between objectivism and constructivism. This was an STS version of the very old, but still hot contest otherwise known as realism versus relativism, or rationalism against scepticism. STS came of age with the triumph of 'social constructivism' in the forms of SSK, ethnomethodology, EPOR and symbolic interactionism. And then along came 'ontological constitutionalism', in the form of ANT, material semiotics (which allowed among other things, feminist and postcolonialist issues to emerge as ethical concerns), and later for inquiring into modes of existence (and immodestly proposing redesign of modernity's institutions). In some versions, this latest chapter of the STS story also included non-representational theory. In this working imaginary, objectivism, social constructivism, and ontological constitutionalism all now thrive as variant STS epistemic practices in their own niches. No single methodology dominates, and each adopts a civil demeanour with respect to the others, each insisting on staying distinct, but also prepared to work with the others.

The story envisages the politico-epistemics of the modern state as a sort of cosmopolitics, where governance inevitably involves working with those who think otherwise. To some degree this reality of governance shapes and is perhaps also influenced by, STS. I find the story both useful and entertaining; I can go along with it as a working imaginary. But immediately, I wonder if there is evidence that would support such a story of multiple STS pasts working together. And, further, what might be left out of the story? What lines of STS inquiry might have been silenced in

that story of the co-constituting of the politico-epistemic landscape of the modern state and STS? These are the questions I attend to in offering a reading of *Science as Practice and Culture* a quarter of a century after its publication.

In 1992 I was co-ordinating and teaching a Master of Science in Society, the first masters coursework program established in the Arts Faculty at University of Melbourne. Its establishment had been funded in a partnership arrangement with the state government, also a first of its kind. Under this program, mature age students recently made redundant in Australia's first wave of privatisation of state enterprises were eligible for free tuition. A new global order was in the making and STS too was changing.

I had hoped to find material I could use in teaching in this volume, so it appeared on my bookshelves soon after publication. I no longer remember if I did use any of the articles, but I do remember being a bit put off by the collection as a whole. Back then the section headings used to group the articles made little sense, and when in 2017 I go back to reconsider the book, they still seem less than useful. A cursory glance at the chapters has them falling into two groups: those that provoke and are provocatively responded to, and those that more quietly argue on the basis of empirical evidence, that if you wish to understand the roles and place of science in society, science is usefully read as practice and culture instead of theory and methodology. In offering a reading of the book in 2017, I will apply this grouping. I separate off those articles which speak directly to each other, from those that in various ways, argue and evidence the claim that the concepts of practice and culture are analytically useful when it comes to understanding the sciences.

The idea of analysing science as an expression of organisational practice and culture, rather than as a means of generating epistemically valid objective facts to support the functioning of the state, was still controversial in the early 1990s. Accordingly, in Britain the book was reviewed (negatively) in *The Times Literary Supplement*, and (positively) in the *Times Higher Education Supplement*. In France the prestigious *L'Année sociologique* offered a long review from a rather puzzled sociologist. From the reviews I have found, it seems that while historians of science, and sociologists took the opportunity to catch up on what was happening in STS, the 'new kid on the block', predictably the book was ignored by philosophers of science. It seems not to have been reviewed in the STS journals, however, in meetings of science studies scholars it was a hot topic. As I remember the 1994 4S meeting in New Orleans, where I first came across Andy Pickering the editor of the collection, the provocative articles collected together in the book were still hotly debated.

Yet hot exchanges do not age well, so in 2017 these papers read as a rather bad tempered exchange amongst seven protagonists, all identifying as sociologists. To use the Australian idiom, this group of articles could be summed up as 'a verbal punch-up between seven blokes, most of them Brits, but with a couple of French guys in there, who came out swinging.' The radical consequences of the epistemic practices of social constructivism was the bone of contention. A practice-focussed variant of strong social constructivism (the empirical program of relativism) promoted by Harry Collins and Steven Yearley opposed other variants in the form of ethnomethodology (Mike Lynch) and sociology of scientific knowledge (David Bloor). Establishing dividing lines seemed to be the aim. The issue that caused most heat was what was read as two recent developments to manage the radical epistemic consequences of social constructivism. The first was the shift of some British sociologists of science towards reflexivity (Steve Woolgar is the representative included in the collection), and the second was actor-network theory as developed by 'the French school'—Michel Callon and Bruno Latour. These two groups were seen as pushing things too far, and as likely to generate counter-productive outrage amongst the likes of the readers of *The Times Literary Supplement*, and *L'Année sociologique*. These groups were accused of playing "epistemological chicken".

This set of papers was often raked over in the years that followed the publication of the collection, and I can add nothing new. Let me acknowledge the passionate arguments for what they are, and note that in the moment of the coming together of the collection, the insistence of 'the French school' that they wanted to invent a new

game in STS, to leave aside the conversation over epistemology that is an outcome of the particular ontologies embedded in the contest between social constructivism and objectivism, was largely ignored. Perhaps we should be thankful that only a few years later it would be widely accepted that in order to account for the many ways scientists bring in nonhumans, STS analysts must learn to occupy many ontological positions, and to entertain a whole range of ontological possibilities. Philosophical insights from Stengers and Whitehead began to enrich the analytic capacities of actor-network theory, to develop 'ontological constitutionalism.' As knowledge making practice this approach opens up possibilities for critical discussion of the ontological constitution of entities known in science; they parochialize or provincialize the ontological practices of both social constructivism and objectivism, having abandoned claims to be concerned with truth.

In the papers collected together in this book then, while social constructivism is much in evidence, and shows no sign of comity, we see clearly that by the early 1990s ontological constitutionalism was more than holding its own. But what about objectivism? Do we see a civil STS objectivism that might be said to have settled comfortably into a niche in a landscape accepting of differentiated (distinct and connected) STS epistemic practices, which articulate—albeit not too loudly, incommensurable metaphysical commitments? Here I turn to the group of less controversial papers. Is there evidence in the collection that by the 1990s some modest forms of objectivism had emerged? The epistemic practices of such objectivisms would be robust enough to offer possibilities for effecting objective truth mobilizing a notion of truth as corresponding, when necessary. Yet while insistently distinct, such a truth form would have a (limited) capacity to connect to other truth forms, such as the coherence truth form of social constructivism.

In their different ways the papers by Ian Hacking and Steve Fuller which more or less book-end the collection, articulate viable versions of objectivisms that could be worked in that way. Hacking is keen to engage with what he calls the motley of science, proposing what might be named as a form of objectivism subject to socio-materialist limits. He is *not* arguing that what laboratories sciences generate "are mental or social constructs, but rather for down-to-earth materialism" (Hacking, 1992: 30). Acknowledging the moderating effects of the socio-material actualities of laboratories allows for an objectivism that recognizes its limits. Steve Fuller's objectivism by contrast recognizes psycho-social limits by focusing on the actualities of scientists' behaviours. In 1992, the epistemic practices of 'other' STS scholars may still have been experienced as alien (or wrong), but STS as a landscape of multiple methodologies, many sets of truth practices, both distinct and connected in various ways, is certainly discernable in *Science as Practice and Culture*. The STS recognition that science *is* organizational practice, with the corollary that it expresses many particular institutional cultures that effect various specific epistemic standards and ontological strategies, was perhaps prescient in 1992, but its salience for developing possibilities for critique of the politico-epistemics emerging in the versions of the modern (neo)liberal democratic state that were already then in evidence, is not in doubt.

As a collection of papers then, *Science as Practice and Culture* seems to express (and record) some of the moments by which today's complex STS analytic terrain came into being. This tentative conclusion brings me to my second question. Are further analytic currents discernable in the collection; streams of analysis that have so far remained unremarked? Here I turn to the very final paper in the collection, by anthropologist of science, Sharon Traweek. Beginning in the 1980s her ethnographic empirical studies were carried out in a Japanese high energy physics laboratory. There, as a tall, red-haired woman engaging the epistemic practices of American cultural anthropology, of course she stood out as distinct, but she was also multiply connected. She stayed there in-place, committed to going on collectively, doing many differentiations with the Japanese men who were her knowledge making colleagues, she went on, simultaneously separated from and connected to those who thought otherwise.

In this final paper (it has the feel of an afterword) I detect a further –ism that I suggest should be added to the line up of –isms that emerged unbidden in graduate student seminars I

participated in earlier this year, a version of which I came across later, in a recently published book—a story of what contemporary STS *is*. Traweek's paper enacts a truth form we might name as situationism. I suggest it is a truth form, a figure animating epistemic practices, that is widely enacted in STS. While situationism has been present in science studies since ethnographies of science began in the 1980s, as truth practice, it has remained more or less unnoticed. Yet I suggest it is this very truth form that mediated the emergence of complexity in contemporary STS epistemic practices, covertly enabling the actual doings of its various empirical objectivisms, social constructivisms and ontological constitutionalism together and separately.

A situationist methodology articulates a truth form that is not representationist (like those of objectivism and social constructivism) but which does offer possibility for accounting 'how we know we know'. While not fully fledged, not admitting (to itself?) that it is a truth form, a set of epistemic practices, it is exemplified in at least some of the "string of stories" (Traweek, 1992: 461) that constitute Traweek's paper. The ethnographic stories of Japanese physicists' naming practices comparatively embedded in the paper's extended elaboration of American cultural anthropological naming practices, exemplifies this pragmatic and situated truth form, enacted in particular deeds, peculiar to ethnography yet not owned-to by any school of anthropology.

I suggest that nothing stands in the way of STS ethnographies owning-to that situationist truth form enacted in ethnography. Less subject to the Hermes complex which in anthropology often takes the paralytic form of not belonging anywhere (and of which Traweeks's paper is a stellar example), STS ethnography in my experience openly, even promiscuously, expresses both and neither belonging and not belonging—like all its fellow STS methodologies and their truth forms. In concluding my re-reading of *Science as Practice and Culture*, this is the truth form I point to and celebrate as a constituent truth form in contemporary STS, and as there flourishing in 1990s science studies as an unnoticed back-channel.

References

Ashmore M (1993) Review *Science as Practice and Culture*. *Contemporary Sociology* 22(4): 489-490.

Hacking I (1992) The Self-Vindication of the Laboratory Sciences. In: Pickering A (ed) *Science as Practice and Culture*. Chicago: University of Chicago Press, pp. 29-64.

Puig de la Bellacasa M (2017) *Matters of Care. Speculative Ethics in More than Human Worlds.* Minneapolis: University of Minnesota Press.

Traweek S (1992) Border Crossings: Narrative Strategies in Science Studies and among Physicists in Tsukuba Science City, Japan. In: Pickering A (ed) *Science as Practice and Culture*. Chicago: University of Chicago Press, pp. 429-466.