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Genealogies of STS

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Human societies fashion themselves through rites of memory, gilding and illuminating some pages of the past while consigning others to forgetfulness. Official memories reinforce dominant cultural narratives, asserting continuities where skeptics might see breaks or ruptures, contradictions or untruths; indeed, revolutions, scientific or otherwise, can be thought of as violent breaks with comfortable connections between past and present. To see the power of such story-telling, consider for example the myth of the durability of the US Constitution, a myth that proclaims the unchanging identity of that founding document through a secession, a civil war, and numerous democratizing amendments that totally transformed the look of the nation's voting polity. To this day, that myth legitimates styles of constitutional analysis that challenge, under the rubric of strict constructionism, attempts to treat the Constitution as a source of living and evolving principles. Or, at the opposite pole of the Earth, take Australia's ritualistic annual observance of Anzac Day. That remarkable celebration connects the forging of the nation's identity to Gallipoli: to a dawn landing on faraway shores almost a century ago, in a war of others' making, to a failed military enterprise that ended in an inglorious evacuation, following a bloody, months-long stalemate.

These reflective essays written to commemorate two seminal works published a quarter century apart – by Thomas Kuhn (1962) and Bruno Latour (1987) – constitute just such an exercise in group identity-making. Only here the collective served is an academic field, science and technology studies (STS), rather than a nation, a religious group, or an ethnic community. Revisiting these two milestone works with the wisdom of hindsight is but a portal, I submit, to the more challenging task of thinking through

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what is at stake for STS in such acts of commemoration. What do we gain and lose through selecting these books by these authors from the multitude of writings that have shaped the field?

Not many books can claim to perform their own central argument. Kuhn's *Structure of Scientific Revolutions* does this in a sense. In describing how revolutions occur in science, Kuhn in effect caused a revolution in the description of scientific change. *Structure* has been cited an astonishing 53,433 times according to Google Scholar.[1] 'Paradigm shifts' have entered the everyday vocabulary of the English language, with application to everything from cooking to politics.[2] So much popularity comes at a price. Marshall Sahlins, the brilliantly irreverent Chicago anthropologist, took down both Kuhn and Thucydides, comparing their narrative strategies to rhetorical styles in baseball history (Sahlins, 2004: 127–38). The analogy (which Sahlins invoked with a nod to its cultural parochialism) rests on the contrast between the pennant race of 1951, the year the New York Giants unexpectedly beat the Brooklyn Dodgers and made it to the World Series, and 1939, a year the Yankees predictably won the American League championship. The Giants' win, Sahlins observes, was 'evenemential', a one-time thing, whereas the Yankees' achievement was 'developmental' – very like the difference between revolutionary science and normal science. Chuckles aside, Sahlins is concerned here with a theory of history, the jump of registers from collective to individual, from a team performing at its machine-like best to a single, nameable hero (Bobby Thomson) hitting the winning home run, from Foucauldian genealogy to a great man theory of history.

Asking what such switches of register accomplish leads us to the central irony of celebrating Kuhn as the man who hit the first home run for STS. For Kuhn's descriptions of how science works, how it moves and changes, grows if anything more claustrophobic with time. It reads like a 'Christina's World' of scientific discovery. As in Andrew Wyeth's famous 1948 painting of a polio-disabled woman crawling up a hillside, Kuhn's science is a constricted space, seen through the eyes of those most at home in it. In Wyeth's painting, close-up details are rendered with loving precision: 'minute blades of grass, individual strands of hair, and nuances of light and shadow' spring into view (New York Museum of Modern Art, 2011), but there is little hint of sociality – unless one sees the distant house on the hill as emblematic of an inhabited world. Ian Hacking, himself no fan of constructivism, noted Kuhn's narrowness of vision more than a decade ago:

> Kuhn said little about the social. More than once he insisted that he himself was an internalist historian of science, concerned with the interplay between ideas, not the interactions of people. His masterpiece, ever fresh, is now over thirty-five years old – truly the work of a previous generation. … Yet for all that Kuhn emphasized a disciplinary matrix of one hundred or so researchers, or the role of exemplars in science teaching, imitation, and practice, he had virtually nothing to say about social interaction. (Hacking, 1999)

About the role of women, for instance. It was 1962, a year before the publication of *The Feminine Mystique* (Friedan, 1997 [1963]), and it is startling to see how completely Kuhn's world was still a man's world.[3] A few examples will serve:

> If science is the constellation of facts, theories, and methods collected in current texts, then scientists are the men who, successfully or not … (Kuhn, 1962: 1)

> But there are always some men who cling to one or another of the older views … (p. 19)

> Almost always the men who achieve these fundamental inventions of a new paradigm have been either very young or very new to the field … (p. 90)

> That reaction comes particularly easily to men just entering the profession … (p. 203)

The gendered, masculine view of science and scientists was hardly unique to Kuhn. As Friedan observes, 'But girls would not study physics: it was "unfeminine"' (Friedan, 1997: 60). The point is not that women are erased from Kuhn's account, but that gendering is symptomatic of a deeper poverty of the social imagination. After all, women were not wholly absent from the scientific workplace even then. One of my good friends at Radcliffe College in those years was a physics major, another a historian of early science and mathematics, and I myself was in a math-science track. By contrast, it is striking to find this sentence in Ludwik Fleck's account of a study of bacterial variability: 'I therefore suggested to my colleague that she find out whether our strain split into lighter and darker colonies' (Fleck, 1979: 88). No more than a pronoun, casually dropped, and not in pursuit of any overt agenda; yet it conjures up a more completely peopled world.

Fleck's (1979 [1935]) *Genesis and Development of a Scientific Fact* is in some ways better entitled to claim paternity with respect to contemporary work in STS, and not merely because it came first.[4] Where Kuhn offers men forming disciplined scientific paradigms through shared exemplars and language games, Fleck concentrates on the formation of new subjectivities, the 'thought collectives' who for him are carriers of science's development through the evolution of distinctive 'thought styles'. While this is not the place to engage in a detailed comparison of Kuhn and Fleck – that work has been done in part by others (Harwood, 1986; Mößner, 2011)[5] – it is worth asking why it matters whether we speak of Kuhn in isolation from Fleck in tracing STS's ancestry. It is not the similarities between the two but the differences that underscore the importance of including both in historicizing STS, adopting in Foucault's terms genealogical rather than archaeological perspectives.

Fleck's scientists are situated in culture and embedded in history, deriving their manner of thinking from a wider array of influences than theory-internal heuristics and exemplars. They are affective creatures, moved by 'reverence for an ideal – the ideal of objective truth, clarity, and accuracy' (Fleck, 1979 [1935]: 142). And they acquire and give up their communal sensibilities gradually, through developmental (Yankee style) rather than evenemential (Giants style) transformations of allegiance and belief. Though Fleck, like Kuhn, speaks of the importance of training in building scientific communities, his way of talking about the emergence of thought collectives is quite different from Kuhn's: more psychological and subjective, and phenomenological. Thus, Fleck analogizes the propagation of new ideas to making a tune audible to people not previously conditioned to hear it. August von Wasserman, developer of the first test for syphilis, 'heard the tune that hummed in his mind but was not audible to those not involved' (Fleck, 1979: 86). To establish the test's validity, it took a period of 'tuning' between those who produced the new sound and those who learned to hear it. It takes little stretch of the imagination to connect this account to recent developments in STS

that pay greater attention to the sensory and perceptual dimensions of scientific practice, such as 'sound studies' (Pinch and Bijsterveld, 2011; Pinch and Trocco, 2002; Thompson, 2004). Parallels with paradigm shifts would be far less apposite.

Latour's *Science in Action* has not quite completed its transit into history. As a result, its contributions to STS cannot be evaluated with as cold an eye as can be those of Fleck and Kuhn. Its impact on the way STS scholars work and the terms in which they do it seems for the moment to be more profound than that of its forerunners. If Kuhn hit the first home run, then Latour set new records for homers, with bases loaded. Latour's image of a living science, constantly in motion and advancing through attack and counterattack, gripped the imagination of a generation of STS analysts. His injunction to study science and technology by following scientists and engineers while their work is still 'hot' inspired countless studies of laboratory science. It is hard to imagine the texts of our field shorn of the signature Latourian vocabulary: black box, inscription, obligatory passage point, allies, networks, immutable mobiles, centers of calculation, trials of strength. Though several these concepts had germinated through work on actor-network theory by Michel Callon and others, it was Latour who mobilized them for uptake by audiences beyond the École des Mines. His playful encounters with the inanimate, mechanical, and written instruments of science remain eye-opening to many. His sprightly rules of method have acquired near-axiomatic status, while provoking much misunderstanding and some vitriolic critique, especially Rule 3, which states: 'Since the settlement of a controversy is the *cause* of Nature's representation, not its consequence, we can never use this consequence, Nature, to explain how and why a controversy has been settled' (Latour, 1987: 258).[6]

Once again, however, the key obligation for us is to ask what is at stake in selecting this book as one of two main exemplars for the field. What paradigm of STS does the book anchor? Latour's ebullient account of the social aspects of laboratory science is no less partial in its way than Kuhn's, even though science as Latour renders it is a refreshingly human, flesh and blood enterprise. His lab inscriptions, for instance, thoughtfully enroll both male and female spokespersons (for example, pp. 70–71). One way to see what is missing in this concededly more social world is to look more closely at his use of the term controversy.[7] Controversies in *Science in Action* are mostly carried out in textual space, with scientists fighting to dethrone opposing papers and acquire ever more allies and citations in support of their own: 'We go from conversation between a few people to texts that soon fortify themselves, fending off opposition by enrolling many other allies. Each of these allies itself uses many different tactics on many other texts enrolled in the dispute' (Latour, 1987: 43).

Yet if one thinks back to what was happening in American science in the mid- to late 1980s, controversies were playing out on much larger fields of action than individual laboratories or the pages of professional journals. Those were years when climate modeling came into its own with all of its potential for scientific controversy – first through the nuclear winter debate and later, in 1988, with the triple play of an exceptionally hot summer, atmospheric physicist James E. Hansen's testimony on global warming to the US Congress, and the formation of the Intergovernmental Panel on Climate Change. In 1987, the US Congress authorized the construction of the Superconducting Supercollider, whose eventual demise caused consternation in the physics community and helped light

the fuse for the 'science wars' of the 1990s that pitted STS against scientists. The 1980s, too, were problematic years for the life sciences, plagued by prominent cases of fraud that culminated in the high-octane, ultimately inconclusive, investigation of Nobel Laureate David Baltimore's laboratory at MIT (Kevles, 1998). Many trials of strength occurred in and around the sciences during those heady years, but they forced scientists to confront actors equipped with vastly different resources from the visible stuff of laboratories: law, money, political influence, enforcement capability, regulatory authority, media access, the power to make and unmake institutions.

Any STS scholar worth her salt would recognize these *social* resources as constructs too. The question is whether a version of technoscientific controversies that approaches these elements by following 'science in action' can offer a full enough way of accounting for science (and technology) in action in society. For that more co-productionist project, we have to enlarge our horizons to take in the laboratories of society, where the stakes are never about knowledge alone, where values and cultural practices matter, and where power reigns through far ruder means than the accumulation of textual allies and citations.

STS, of course, had established roots in controversy studies long before 1987. Dorothy Nelkin, the most prolific contributor to this genre in the US, published her first book in 1971 on the public controversy over an ill-conceived attempt to site a nuclear power plant on Cayuga Lake, near Ithaca, New York (Nelkin, 1971). By 1979, she had edited a textbook on the subject, entitled quite simply *Controversy* (Nelkin, 1979); it was required reading in the STS course she taught for many years at Cornell University. Her books were not seminal in the sense that STS scholars are taught to regard the works of Kuhn and Latour as seminal. Largely devoid of social theory and lacking verbal pyrotechnics, Nelkin's work stayed pretty close to the surfaces of things. Yet in the citation accompanying her selection as recipient of the 1988 Bernal Prize of the Society for Social Studies of Science, the philosopher Ronald Giere credited her with inventing the field of controversy studies, saying: 'More than anyone else, she was responsible for the idea that controversies over science and technology provide a kind of natural laboratory for studying the operations of science and technology and their interactions with the surrounding society' (Giere, 1988).

It matters hugely for our sense of STS as a discipline whether we see the laboratory as the site par excellence for studying scientific controversies or social controversies as laboratories for studying how science and technology work in society. The former project, as Latour so engagingly argued, can be accomplished by following scientists and engineers as they go about producing facts and artifacts. But its horizons are limited to a large extent by the imaginations of the protagonists, who have to varying degrees cloistered themselves and their ways of knowing away from the rest of the world.[8] As in most actor-centered accounts (the home run again), these studies do less well at illuminating institutions, culture, or society. The latter project calls for an altogether more expansive imagination and a more layered sensitivity to modes of meaning-making other than the epistemic. There is room in that project to ask not only who wins and who loses in particular struggles over representation, but who benefits, to what ends, by what means, and at how great a cost. Studies in this vein allow the researcher to examine the interplay of structure and agency, to investigate how issues get reframed and new

identities or solidarities emerge, and to acknowledge that matters of fact are not the only matters of concern to people. Importantly, controversy studies rooted in society are not simply about coming up with the winning representation (the home run), and the settlements they describe are normative as well as cognitive.

Toward the end of his reflections on baseball history, Sahlins abandons Thucydides and Kuhn for Foucault and archaeology. It is Foucault's method of genealogy that I would sooner advocate for an STS concerned with its own self-fashioning, an approach that integrates the epistemic and material creativity of technoscience with other ways of constructing knowledge and power (for example, Jasanoff, 2004). Against great man theories, too, genealogy offers a useful corrective: it allows lineages to be traced in terms of maternity as well as paternity.

Notes

I would like to thank Emma Frow and Ben Hurlbut for their perceptive comments on an earlier draft.

1. By contrast, *Science in Action* registers 12,305 citations in Google Scholar as of this writing.
2. A political scientist writes of the Occupy Wall Street movement, for example, 'it is clear that a political paradigm shift is taking place before our very eyes' (Harcourt, 2011).
3. According to the Google Books count, there are 41 occurrences of 'men' in *Structure*; there are no results for 'women'.
4. The Society for Social Studies of Science (4S) implicitly recognized this point in naming its major book award after Ludwik Fleck, although other factors no doubt also played their part in shaping that choice.
5. Harwood arguing that Fleck's work suffers from incoherent and fragmentary concepts, thus not adding up to full-blown theory; and Mößner arguing that Fleck's work takes a wider view of the social than Kuhn's and more fully discusses the influence of the social on individual cognition.
6. For a biting rejoinder, see one of the books that launched the 'science wars' in the US: Paul Gross and Norman Levitt's *Higher Superstition: The Academic Left and Its Quarrels with Science* (1988: 58).
7. Thanks to Google's search function, we learn that the word 'controversy' appears 58 times and 'controversies' 31 times in *Science in Action*.
8. Instructively, Latour's former colleagues at the École des Mines in Paris refer to this kind of laboratory-based research as 'secluded science'. See Callon et al. (2009).

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Biographical note

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