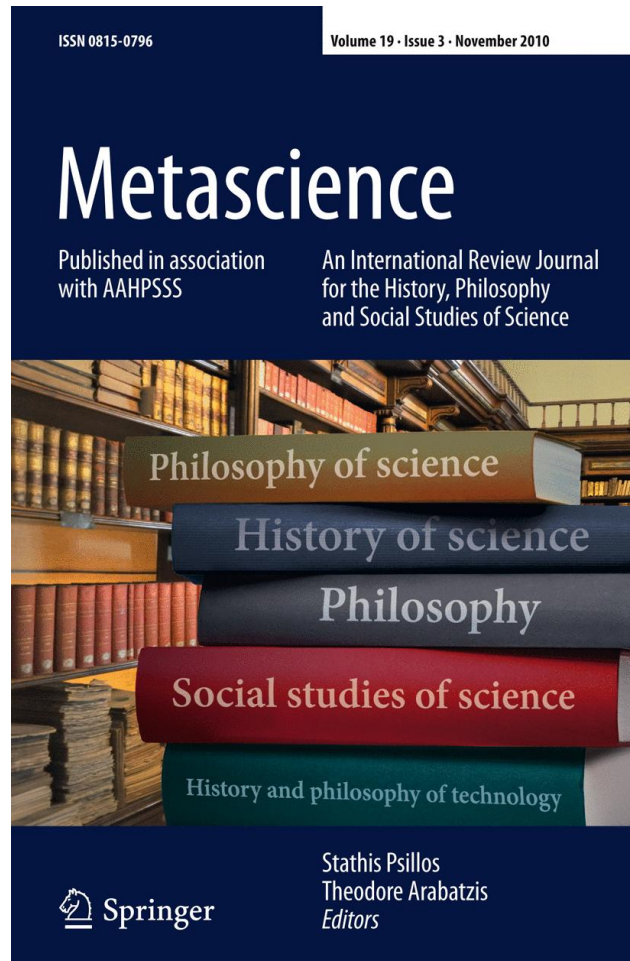


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## Fact and friction

**Park Doing: Velvet revolution at the synchrotron: biology, physics, and change in science. MIT Press, Cambridge, Mass., 2009, viii + 152 pp, £20.95, US\$28.00 HB**

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Three decades ago, ethnographers descended on California's laboratories to watch scientific knowledge being made. These "lab studies" described workplaces closely resembling newsrooms, legislatures, restaurant kitchens, and other epistemically unprivileged sites. In *Velvet Revolution at the Synchrotron*, Park Doing pushes lab studies into exciting new territory, with sophisticated analysis of labor and disciplinary politics and contentions among builders, users, and managers of experimental apparatus. Doing's site is the Cornell High Energy Synchrotron Source (CHESS), where he has worked for nearly 20 years—as an "operator", then an operations manager, a graduate student in Science and Technology Studies, and now as lecturer in history and ethics of engineering at Cornell. The product of those two decades is a dramatic picture of CHESS' evolution and the frictions among its occupants. However, that picture is muddled by Doing's provocative but ultimately unconvincing claim that his ethnography supersedes all previous lab studies because they have never before shown "how contingent practice coerced the endurance of the [or any] particular fact claim" (33).

I'm sympathetic to the quality control Doing brings to lab studies—just because some practice is contingent, embodied, or messy doesn't destabilize the fact-making in which that practice is implicated. Yet Doing often stretches quality control into dubious indictments of his predecessors. For instance, he points out, rightly, that those first ethnographies ducked the question of calibration technologies brought into end controversy. As he says of *Laboratory Life* (London, 1979), "if mass spectrometry did in fact decide the matter ... it should have been Latour and Woolgar's *main* purpose to analyze mass spectrometry as a 'social historical' phenomenon" (31), rather than identifying contingencies in the Salk Institute lab's contribution to the controversy. For Doing, the calibration offered by mass

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spectrometry makes the local contingencies Latour and Woolgar unearthed ultimately irrelevant. For those contingencies to matter, there would have to be further contingencies in mass spectrometry—and, by extension, in every tool used to calibrate that tool, and every tool used to calibrate those tools, and every tool....

Following those chains of calibration and contingency all the way out would be an impossible task for one study—which is why laboratory ethnographers always coordinated with researchers drawing on archival documents, interviews, multi-sited ethnography, prosopography, etc. As it happens, there are now good social histories of mass spectrometry: Carsten Reinhardt's *Shifting and Rearranging* (Sagamore Beach, Mass., 2006) and Michael Grayson's *Measuring Mass* (Philadelphia 2002). Bruno Latour's *Science in Action* (Cambridge, Mass., 1987), articulates *how* to read these works alongside *Laboratory Life*: they trace the widening, cross-linked networks of actors and actants encompassing mass spectrometry that, at a critical moment, Latour's Salk Institute lab stitched itself into to leverage those networks against competitors.

*Velvet Revolution*, however, resists such a rereading of the classic lab studies. Instead, the book's theoretical sections exclusively critique science and technology studies (STS) icons (Lynch, Knorr, Latour, Collins, Pinch, Pickering, Galison), neglecting the ecology of scholars (such as Grayson and Reinhardt) those icons live among. Moreover, *Doing* often besieges straw men versions of those icons. His critique of Harry Collins' *Changing Order* (London, 1985), for instance, claims Collins' "account reads like a conventional treatment of science—calibration settled the dispute. We are simply told by Collins that in principle the episode could have gone otherwise *and been accepted as scientific*" (33).

This ignores the architecture of *Changing Order's* argument. Collins begins with a single-site ethnography of laser-building, where consensus on calibration seems easy (if the laser fries concrete, it works). Even so, Collins shows that lab practice is so messy that even participants don't know exactly what they've done. He then moves to a multi-sited study of gravitational radiation detection, where consensus on calibration is fraught. Combining interviews and ethnography, he shows that the inability of participants to know exactly what they themselves have done, much less ascertain exactly what other labs are doing, means they must interpret calibrations using social cues.

For *Doing*, such contingent cues fall within the context of discovery, whereas calibration is part of the context of justification, a distinction he says lab studies ignore. In his view, "while the assertions of knowledge-making ability put to use in practice at the lab were expedient coercions of accepted reality that endured, this coercion itself was not implicated, in the end, in the ultimate status of fact claims that were produced 'in' and emanated 'from' the lab" (143). That's difficult to deny "in the end," but when exactly do we get there? We can't see facts from the end of history. STS has shown that, *in medias res*, contingent social cues are implicated in the current status of fact claims, in that they are part of a documentary method of interpretation. Scientists integrate what they know of their organizations and research communities into their understandings of technical measures. As the logjam of measures grows, scientists' understandings of their social worlds (who's competent, who's crazy, which disciplines are "sloppy" or "careful") shift,

inextricably, with their understandings of nature (such that “social” and “natural” are entangled).

*Velvet Revolution* isn't incompatible with such a view of STS. Indeed, Doing's portrait of how biologists captured a high-energy physics stronghold is a wonderful example of sociotechnical calibrating. Doing charts how, in physicists' eyes, biologists transformed from irrelevant “sloppy” experimentalists into valued partners in synchrotron science. That transformation was tied to wider cultural shifts in the disciplines' relative prestige, exemplified by Nobel Prizes and changes in federal funding priorities. CHESS physicists' changing evaluation of biologists was more than simplistic cultural calculus. Rather, post-Cold War America's new appreciation for biomedical research meant biologists could (in Latour's terms) stitch themselves into networks encompassing expensive actants such as charge-coupled devices and supercomputers. Biologists' access to these new technical measures fostered physicists' revised understandings of both biologists (colleagues) and biology (facts). Those shifts were inseparable yet contingent: CHESS lost a mid-'90s bid to extend its high-energy research, encouraging Cornell physicists to lend their considerable weight to the view that the synchrotron was (perhaps primarily) a biological tool and that biologists were savvy enough experimentalists to influence synchrotron technology.

Yet Doing doesn't explore such compatibilities with STS. Rather, he insists that his status as a CHESS employee gives his study “a recursive dynamic that is unprecedented in laboratory studies” (42), and therefore that his ethnography is the yardstick compared to which the rest of STS falls short. That's tendentious in several ways. Participant-observer lab studies aren't unprecedented. David Edge and Michael Mulkay's *Astronomy Transformed* (New York, 1976) deals with events from Edge's time as a radio astronomer. Collins and Trevor Pinch's *Frames of Meaning* (London, 1982) is based on the authors' paranormal experiments (the only such experiments then funded by the British government). Many laboratories have employed anthropologists—Laura McNamara, Ben Sims, Amy Wolfe, Lucy Suchman, etc.—within their organizations' regimes of knowledge production. Going further back, Ludwik Fleck's descriptions of his bacteriological research provide a precedent for Doing and all of STS. Doing's long-term commitment to CHESS is extraordinary, and the many different roles he has played there give his study an intensely personal, clear-eyed flavor. But often Doing strikes a too-bold pose that will make readers question even the book's more grounded observations.

To this reader, *Velvet Revolution* offers three enduring contributions. First, Doing's “site” is far more multidisciplinary and fragmented than most lab studies. His meeting-by-meeting description of (literal) negotiations among high-energy physicists, condensed matter physicists, and biologists tells readers exactly what is at stake. Some of the multidisciplinary negotiations affecting CHESS' experimental set-up take place elsewhere, particularly conferences. As Anne Beaulieu (*Social Studies of Science*, forthcoming) argues, lab studies should focus on “co-presence” rather than “co-location.” Doing's off-site commentary nicely exemplifies that distinction.

Second, Doing's study masterfully builds time and change into laboratory ethnography. This, again, isn't unprecedented: Collins' *Gravity's Shadow* (Chicago, 2004) moves in that direction, as do Sharon Traweek and Hugh Gusterson in David

Kaiser's *Pedagogy and the Practice of Science* (Cambridge, Mass., 2005). Still, Doing gives an up-close, nuanced view of CHESS' disciplinary regime shift from physics to biology. Anyone interested in the post-Cold War institutions of science affected by that shift should read this book.

Finally, Doing gives a thoughtful, compelling picture of labor politics at CHESS that STS dearly needs. He does this by leveraging all his fluid identities within that organization: participant and ethnographer, but also operator and middle manager. Chapter 3 argues that skills (and their sources) are political attributions that help define the authority of facts and persons within organizations. Moreover, the politics of skill have history: when CHESS was new, the operators who built its equipment and the scientists who managed them confronted each other over who understood which aspects of experimentation. But as the lab's scientists and equipment aged, a new generation of operators looked to management as an authoritative source of facts, not friction.