Engaging Science, Technology, and Society

Working at the Edges of Institutions During their Transformations: A Response to Sharon Traweek

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Abstract

In the 2020 Prague Virtual Conference of the Society for Social Studies of Science (4S), Sharon Traweek was awarded the society's John D. Bernal Prize jointly with Langdon Winner. The Bernal Prize is awarded annually to individuals who have made distinguished contributions to the field of STS. Prize recipients include founders of the field of STS, along with outstanding scholars who have devoted their careers to the understanding of the social dimensions of science and technology. This essay is a commentary on Traweek's work from the perspective of Sandra Harding with respect to their shared backdrop of the science wars, the value of standpoint theory and of Traweek's 'meshworks,' and their work in different non-US/European STS contexts.

Keywords

Bernal prize; Sharon Traweek; science wars; meshworks; standpoint theory; women's movement

Introduction

Sharon's work has been important to my own. Here I first point out both the inherent challenges and benefits in working critically at the edges of institutions, as both she and I have done. I review what I can now, at a safe distance from them, characterize as the fascinating moment of the so-called science wars, in which our work was all too often the target of intense criticism from natural scientists. And I point to the important role of "meshworks," as she names them, at precisely those kinds of intellectual and institutional moments. I conclude by noting the significance for the field of science and technology studies of projects such as the new journal, *Tapuya: Latin American Science, Technology and Society.*

I first became aware of *Beamtimes and Lifetimes: The World of High Energy Physicists* (1988) when she was a beginning professor at MIT. As I remember it, I wrote her a fan letter and we began a conversation. Then we both arrived at UCLA in the early 1990s. We went to each other's lectures in those early years. And at the end of the first year she told me something about my professional strategy that I have ever since kept in mind:

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"What you do, Sandra" she said, "is you position yourself on the edge of an institution and you refuse to go away." I have subsequently delighted in that accurate perception at which I could not have myself arrived. I suspect that it also characterizes her own history of institutional positions. Evidently Bernal appreciates this strategy!

With the benefit of hindsight, I can now see that at moments of broad scale institutional and intellectual transformation, moving one's critical powers to the edges of dominant institutions permits kinds of insights from those positions that are not possible when one is "innocently" performing those institution's restrictions. Back then the field of STS was beginning to take shape in ways that challenged the institutional authority of conventional knowledge-producing disciplines in both the natural and the social sciences. How that was happening in actuality, and its challenges to dominant epistemologies and ontologies, was visible from the edges of those disciplines in ways not possible from their centers.

However, she is an ethnographer, and I am not. She uses those institutional edges to produce valuable insights about how such institutions work. She began this research at an extraordinary moment in the late 1980s, when the criticisms from the social justice movements of the '60s and '70s of authoritarianism in scientific research and in universities were just beginning to have effects on such institutions. Her careful observations of the professional practices of scientists in high energy physics laboratories in the United States and Japan emphasized the power of institutionalized social practices to shape just what information the sciences could and could not produce. The characteristically long-term research projects in Japan could produce different information than the typically much shorter research projects necessitated because of typical US career advancement practices.

Her studies were all the more powerful for the social justice movements because they were not overtly directed to social justice projects. The poor people's movement, the civil rights and early feminist movements, and soon thereafter the LGBT and disability movements—all were demanding that the sciences produce the kinds of information that these groups needed to advance their projects. Their issues were about mostly unconscious institutional discrimination, rather than about individual racists, sexists, etc.,—ugly as encounters with such people can be. Institutionalized social practices did have effects on the results of research, her studies showed.

Of course, some disciplines were easier targets than others at that historical moment. Apart from criticisms of hiring and promotion practices, high energy physics could certainly seem to be one of the more difficult challenges for the social justice movements. Nevertheless, it did not pass unnoticed that huge government funding of physics came from the Department of Defense. US militarism clearly had interests in the production of natural science knowledge. And Thomas Kuhn's *The Structure of Scientific Revolutions* (1962) had pointed to a different logic of scientific discovery than the familiar positivist one. Scientific advances occurred also through larger social changes, not only through the heroic advancement of "pure reason." Now, many decades later, she is writing at the next stage of those social justice movements, where the Me Too, Black Lives Matter, and climate change movements demand institutions provide increased funding and

research focuses on those issues. So far, those changes in funding and focus on the sciences have only barely begun.

She has consistently enlightened me about my own experiences and the institutions with which I have interacted. I suspect that unconsciously I have often used her insights to guide my own career strategies. I wonder how many other readers have been relieved to recognize their own situations in Traweek's accounts, along with unintended hints for how to successfully move through their careers in the context of today's massive transformations in universities and research fields, only now beginning in the face of Covid-19 and increasing evidence of looming climate change disaster.

She rightly criticizes typical graduate school training, where students are taught to "stand on each other's necks." Of course my doctoral field of analytic epistemology and philosophy of science is infamous for promoting such practices. Yet, entering the field as I did at the very beginnings of the women's movement in 1968 in the US enabled the development of a "meshwork," as she names it, from virtually the moment I arrived at NYU. I could not have survived without it. There was at that time only one other woman among 30 or so philosophy grad students, and both of us were "returning students"—evidently among the very first admitted to that department. There were no women faculty in the department, and several of my professors were younger than I was. At least several of the faculty made clear that they presumed that we were not serious students, but only bored housewives.

I commuted to classes in NYC from Albany, New York (where my husband taught): 2.5 hours on a bus each way, and then a 30 minute subway trip each way down to NYU and back to the Albany bus. I had two babies when I started back, a one-year old and a two-year old, and a husband who after nine years of marriage still couldn't boil an egg. I figured out to prepare ahead of time a day's worth of food for all three, and get a babysitter for the 12 or so hours I would be away whether or not my husband was in fact teaching. I managed to take all of my graduate classes on the same day: two per week for most terms, but three for the terms where I was required to be full-time.

However, when I started back to graduate school, as mentioned, the women's movement was just beginning in Albany, the state capitol, which had passed a liberalized abortion law in 1969—three years before Roe v. Wade and the federal law. I and five friends had started the first "conscious raising" group in Albany a few months earlier. All six of us were either brand new faculty at SUNY-Albany, or like me, just returning to grad school. We met weekly for two hours or so, with lots of laughter as well as occasional tears, and truly inspiring strategizing. This was a serious "meshworks"! That group subsequently expanded and after my departure for my first faculty position at the University of Delaware, they produced multiple other groups, and soon, significant institutional changes in university and government educational policy throughout the larger Albany region. It was an incredibly exhilarating moment in history. Nevertheless, I note that in the late '60s and early '70s, none of us even thought to raise the issue that we had never even seen a person of color in our respective departments, nor had we been assigned writing by any such. A couple of years later I had the privilege of joining a reading group consisting of faculty and graduate students from literature departments to read writing by women of color; none of us were "of color."

By my second year at NYU, the Danforth Foundation had started up a grant program specifically for returning women students. I received three years of graduate school fully paid for by this program. I greatly valued the three day annual conferences Danforth organized for us with valuable programs on the specific kinds of challenges we were encountering. I came to have lasting friends from those conferences. Danforth ended that program after ten years. That was exactly the number of years that Danforth had again excluded women applicants from their regular huge fellowship program, after finally adding them some years earlier. They did so, they said, because a study had shown that women tended at a high rate not to complete college. When they replicated the test eight years later, but extended it from four to six years to complete the undergraduate degree, they discovered that women completed college at a higher rate than men! So I benefitted from this program because I, too, had had more than a three-year gap in my higher education.

Sharon makes me ponder how I might well have "defended the borders" of the field of standpoint methodology users. I recollect two cases where I tried to do so, but failed. One grad student, an animal rights activist, wanted to write his final paper on the standpoint of animals. Another wanted to write it on the standpoint of the disabled. I initially pointed out to each that neither group could organize to change institutional standards and practices, as could members of the other social justice movements from which standpoint research strategies emerged. But they convinced me that if Fido could recruit a grad student to defend her rights, and if living with his brother's disability recruited the other grad student to help to form a disability rights movement, then I had no border defense case.

Sharon's work, like mine, was from its earliest appearance a target of the so-called science wars. Nobody likes to be studied by an outsider. We all fear the worst: evidence of motives, strategies, and influences on us that we prefer to forget when thinking about our projects and our careers. And certainly the highest status groups, such as physicists, especially resist such observations. The first strategy of the newly observed group is usually to ignore the outsiders. And then when that no longer works, they engage in stronger measures. I know that Sharon has always had to think about this issue, and especially when applying for funding or for a job.

At their most heated, the science wars lasted only a few years, from the 1994 publication of Gross and Levitt's *Higher Superstition: The Academic Left and its Quarrels with Science*, through to mathematician Alan Sokal's 1996 article in *Social Text* (and in Ross 1996), ending in the early 2000s with several conferences intended to bring the two sides together (see, for example, also Ashman and Baringer 2001). I have always thought that what the scientists referred to as their critics' offensive postmodernism was instead the beginnings of the field of the social studies of science and technology. The feminist authors who were the main target of vicious criticisms were simply the easiest target for the virtually entirely male group of offended scientists as they first encountered specifically social and cultural studies of their institutions and practices (Harding 1996).

I suggest that one reason the criticisms died out so quickly was the increasing awareness in that same period of the newly emerging anti-colonial science and technology studies. These were often authored by distinguished male scientists and scholars from around the globe, who were making many of the same kinds

of criticisms of modern Western sciences that the 'academic left' had made. It was one thing for the Western male scientists to attack 'their girls.' It was quite another to find themselves taken on by senior male scientists and historians from countries in the global South in the context of charges of continuing Northern colonialism (see Harding 1992; 2011). Today the science wars of two decades ago are barely remembered as modern Western sciences confront, on the one hand, waves of post-truth anti-science critics about Covid-19, and on the other hand entirely legitimate critics of sciences' slow responses to rapidly increasing climate change.

In some respects, my work is parallel to Traweek's in another way. Where she revealed the powerful physics work in Japan, a non-Northern society, for several years I have been working to establish the basic infrastructures for *Tapuya: Latin American Science, Technology and Society.* This now highly successful young English language journal is completing Volume 4, 2021 as I write. Its central mission is to reveal the distinctive and valuable history and present practices of knowledge production that occurs in Latin America. A significant difference between me and Sharon is that she is fluent in Japanese while I am illiterate in Spanish and Portuguese, the first languages of most of the Latin American authors of the journal's publications. However, I work with an otherwise entirely Spanish and Portuguese first-language editorial team, all also fluent in English. And all editorial decisions are made by scholars working at Latin American institutions—not by me at UCLA. I bring to the journal a long history of standpoint methodology and its strong objectivity, work on racism in Northern science and technology, experience editing a huge English language journal *Signs: Journal of Women in Culture and Society,* acquaintances with many significant Northern STS scholars who can make contributions to the journal's project, as well as crucial start-up funding for the journal from UCLA.

Traweek has two more books soon to appear. One is on Japanese big science, and the other on crafting cultural studies of science, technology and medicine. I look forward immensely to getting to read them, as well as her several new essays which are currently in draft. I hope that these writings contain at least the beginnings of her work on how universities are changing in the new global political economy in which we find ourselves these days.

Author Biography

Sandra G. Harding is an American philosopher of feminist and postcolonial theory, epistemology, research methodology, and philosophy of science. Harding is currently a Distinguished Professor Emerita of Education and Gender Studies at UCLA and a former Distinguished Affiliate Professor of Philosophy at Michigan State University. In 2013 she was awarded the John Desmond Bernal Prize by the Society for the Social Studies of Science (4S).

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