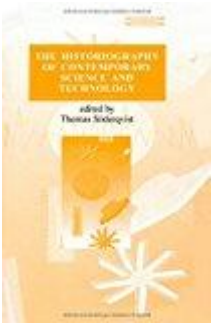
H-Net Reviews
in the Humanities & Social Sciences



Thomas Soderqvist. *The Historiography of Contemporary Science and Technology.* Amsterdam: Harwood Academic Publishers, 1997. xii + 264 S. $52.00, cloth, ISBN 978-3-7186-5906-7.

Reviewed by Stuart W. Leslie (Johns Hopkins University)

Published on H-Sci-Med-Tech (June, 1998)

"Who's Afraid of the History of Contemporary Science?" asks Steve Fuller in his provocative concluding essay to this collection. Apparently just about everyone practicing it, judging from the book's other papers. Fuller reminds us that until the end of the last century, scientists still took the history of science seriously, not merely as a way of settling priority disputes, but as a guide for future research directions. The divergent paths of science and science studies since, so forcefully underscored by Gross and Leavitt's ***Higher Superstition***, and by the controversies surrounding exhibits at the National Air and Space Museum on the Enola Gay and at the National Museum of American History on Science in American Life, raise troubling issues about what, if anything, working scientists and those of us who study them have to say to one another.[1] As Fuller points out, some of the gains historians have made in distancing themselves from their subjects may have come at the cost of less critical self-reflection on the part of scientists, who, when they worry about history at all, worry about saving it from the historians.

At the same time, there seems to be a divergence between historians of science and other historians that may be far more damaging to the history of science in the long run. Fuller might be exaggerating when he says that "science seems to have an internal sense of development only when historians of science write about it." As part of the general history of the West, it is more common for science to be portrayed as " manifestation of ambient cultural forces" (p. 250). He may be surprised to discover, as I have, how often even distinguished colleagues in general history still turn to Herbert Butterfield's classic study of the scientific revolution as their sole text on the history of science--and seem largely unaware of what those of us in the history of science consider the major historiographical revolutions of the last generation. It will be intriguing to see how quickly, if at all, Steven Shapin's new text on the scientific revolution makes its way into general history courses.[2] I suspect that Fuller's call for "a grand narrative" along the lines of J.D. Bernal's magisterial ***Science in History*** as a counterweight to the prevailing progress narrative of the scientists may have as little impact on the social consciousness and social

commitment of mainstream scientists as did the original, even if someone is bold enough to attempt such an ambitious synthesis. Daniel Boorstin's ***The Discoverers*** for all its virtues, provides a discouraging example of how easily an otherwise imaginative general historian can fall into what Fuller calls "the myth of scientific progress" when turning his hand to the history of science.[3] Still, I share Fuller's sense that the distance between historians of science and other historians deserves at least as much attention as the more visible rift between scientists and historians of science.

Distressingly few contributors to this volume seem to notice that general historians generally ignore historians of science, if not the history of science. Is the big problem that historians of science don't know enough, or care to know enough, science, as Alan Shapiro asserts in a recent op-ed piece in ***The Chronicle of Higher Education***?[4] Or is it that historians of science don't know enough, or care to know enough, history? Only Ronald Doel's careful review of the literature on science and foreign policy in the cold war and Jeff Hughes' witty essay on "Whigs, Prigs and Politics" give any sustained attention to this issue and its implications for framing the history of science.

Do historians of recent science really face such "new and unfamiliar methodological and theoretical problems" (p. vii), or do they simply not pay that much attention to how other scholars in the humanities and social sciences have handled similar challenges? Most of the supposedly novel features of working in the history of recent science--so many documents, so little time, the unreliability of oral testimony, classified or inaccessible archives, elusive electronic media, quarrelsome subjects--are by no means unique to history of science. Jeff Hughes rightly notes that political, military, and business historians, indeed anyone interested in making sense of modern, bureaucratic organizations, faces the same daunting task in gaining control over sources and interpretation and is just as likely to find himself at odds with his subjects. Corporate executives, politicians, and veterans are also willing to fight for their versions of history, often with more resources than scientists can muster. While I agree with Hughes that "history is always contested intellectual terrain," I cannot agree that "the history of contemporary science is especially contested" (p. 22). More contested than race? Than gender?

Most contributors follow Jean-Paul Gaudilliere's lead in asserting that there is nothing particularly distinctive about the methodological issues facing historians of recent science, and then complaining about what Gaudilliere calls "the living scientist syndrome" and its demands on the historian. Certainly Gaudilliere recognizes that historians of science have something to learn from other scholars about interpreting invented traditions,and about the political context of intellectual history. But while he offers some suggestive comments on the links between the politics of molecular biology, leftist politics in France, and the broader culture of control and regulation, Gaudilliere's account, like most others in this collection, ends up looking at science from the inside in. Perhaps this has something to do with credentials and how they are presented to the subjects of inquiry. As Gaudillere admits, "We were usually not perceived as historians, but rather as "scientists temporarily involved in the writing a piece of 'our' history" (p. 116). Given that common (mis?)perception, which historians of science generally do nothing to discourage, can we honestly expect sufficiently critical distance? Skuli Sigurdsson, in a quirky essay on his experiences writing a commissioned history of the electric power company in Iceland, cautions that forgetting can be as important as remembering, and that what's remembered and what's forgotten often depends on who's paying the bill.

Larry Holmes, who has enjoyed an equally distinguished career writing about scientists in the near (Hans Krebs) and more distant (Lavoisier)

past, provides some badly needed perspective on the difference. He underscores the paradox that "we claim that we should immerse ourselves in the science of the past, to evaluate scientists or the scientific work of any earlier era in its own terms, according to the standards and the general state of knowledge of that time, not according to later knowledge and standards. But when we write about events so recent that we have no later standards to apply, we view their absence as a liability" (p. 166). He then resolves the paradox with the hope that writing about the near and more distant past might somehow end up being complementary. Those of us who may find Holmes' notion that writing about early science is the best preparation for writing about the present slightly intimidating--who still feels confident writing about several different centuries?--can take some comfort from knowing we may have the first, but by no means the final, word. Holmes also argues, in refreshing contrast to so many scientists and historians of recent science, that mastering, say, nineteenth century physiology can be just as difficult as mastering its modern counterpart, especially if we insist on doing so on its own terms.

I'm more troubled by Holmes' metaphor of historians of science as an expeditionary force or occupying army. Given the scope of our subject, and our relatively small numbers, Holmes asks, should we spread ourselves evenly but thinly or concentrate our efforts on a few strategic sites? He contrasts the history of molecular biology, which has attained something of a scholarly critical mass, with the scant attention paid to metabolic regulation, a mature and less dynamic, but perhaps for that reason a more representative field of the modern life sciences. "If we move selectively into a few favored niches, we risk distorting the overall picture by further exaggerating the relative prominence of fields and persons who have already achieved high profiles by the time we begin to examine their historical places," he says. "The dilemmas we face due to the smallness of our numbers in proportion to the vast scale of the activity of those we seek to study are so deep, I believe, that they overshadow all of the many other problems we may pose for the writing of the history of contemporary science" (p. 175). Maybe so. What's striking to me, however, is how he (self?)-consciously borrows the discovery and conquest metaphor from the master narrative of the scientists, and so undermines the very notion of history as a joint construction of historian and subject.

There is plenty of hard won practical wisdom here for those of us who prefer not to reinvent the wheel. Soraya de Chadarevian, reflecting on her experiences interviewing British molecular biologist, wonders whether historians comfortable with the traditional interpretation of texts are adequately prepared for the complex and rich experience of the interviewing process which we are not trained to decipher or to use for the stories we write (p. 59). Will lifting the mask from our subjects, she asks, by confronting them with original documents and contradictory evidence, only end up creating a new mask equally difficult to decipher? I am sympathetic to her idea that historians must be responsible to their informants, while remembering what Joan Didion has said about writers ultimately betraying their subjects. Ilana Lowy reports on going native in a cancer clinic only to discover how the attitudes and behavior of her sponsors changed as a promising clinical trial took an unexpected turn for the worse. Failure might be more interesting to write about, but it obviously heightens tensions between the historian and her informants, especially if there as no clear understanding about who will have the final say. Joe Tatarewicz, with surprising candor, details how funding and cooperation from NASA has distorted the history of the American space program, and comments on the dilemma of being caught between different audiences with very different expectations. Faced with tough choices, he recalls, "I resolved to waffle firmly" (p. 81), acknowledging publicly what many of us have done privately. He also raises a key issue about the public display of the history of science. For every reader of a schol-

arly history of recent science, there will be a hundred or a thousand visitors to the National Air and Space Museum or the British Museum. Consequently, doesn't interpreting that history for a general (casual?) audience deserves serious attention, especially in light of recent political firestorms?

Susan Lindee (a former journalist) and Susan Cozzens (a card-carrying sociologist) provide an instructive contrast on how to visualize our subject. Though Lindee hesitates to privilege any one disciplinary perspective, she does insist that history must name names and seek to explain the specific as well as the general. She also has the journalist's nose for a fishy story, and she recounts two telling examples, one from her own interviews and another from biographies of Marie Curie, to illustrate the perils of taking a good story literally. I am not sure I accept her distinction between responsibility to the living and the dead--"there is no implied trust between the historian and the subject who is dead," she says. In oral history, there is indeed an implied trust (p. 44). Was Voltaire right all along? But I do share her sense that what distinguishes history (and journalism) is an emphasis on the particular. That does not mean, of course, that we shouldn't look for larger patterns. In contrast to historians like Holmes, who study recent and distant science in much the same way, Cozzens wants to a contemporary research practice of neurosciences. While properly questioning whether detailed statistical mapping of citations, publications, key words, and so on actually tells us anything we didn't already know or couldn't discover some other way, Cozzens provides a genuine methodological alternative to conventional history of science. I only wish she included an example, and an illustration. Perhaps the answer to Holmes' fears about the relative imbalance of scientists and historians of science is not more strategic deployment but more carefully chosen analytical tools, including more attention to the theories and practices of the social sciences, such as statistical models, which were, after all, invented to deal with many of the same issues that we as historians now confront.

Paul Forman's essay on late-modern and postmodern sciences stands like a Robert Venturi facade against an international-style skyline. Having immersed himself in Zygmunt Bauman and other philosophers of postmodernity, Forman now urges us to reconsider the entire historical and scientific enterprise in light of the overproduction, instrumentalism, and boundedness he finds so characteristic of knowledge production in the postmodern world. Forman is asking the toughest questions in the collection, and if his answers sometimes share the ambivalence, ambiguity and irony of his subject, he nonetheless offers some sharp insights into what happens as knowledge itself increasingly becomes a commodity. His notions of market and responsibility may remain frustratingly abstract, but his claim that "postmodernity begins where the production of bound and interested knowledge is unequivocally accepted" (p. 188) rings true, and opens up questions that few of us have even thought to ask. Like Cozzens' maps, Forman's call for a theory appropriate for its times offers a real alternative to business as usual, and reminds us that in the end it as not only how we handle our sources and our subjects that matters, but how we define our larger purpose and the tools we craft to help us achieve it.

Notes

[1]. Paul R. Gross and Norman Levitt, ***Higher Superstition: The Academic Left and Its Quarrels with Science*** (Baltimore: Johns Hopkins University Press, 1994), Martin Harwit, ***An Exhibit Denied: Lobbying the History of Enola Gay***, (New York: Copernicus, 1996), and Edward T. Linenthal and Tom Engelhardt (eds.), ***History Wars: The Enola Gay and Other Battles for the American Past***, (New York: Metropolitan Books, 1996).

[2]. Herbert Butterfield, ***The Origins of Modern Science, 1300-1800***, (New York: Macmillan, 1958) and Steven Shapin, ***The Scientific Revolution***, (University of Chicago Press, 1996).

[3]. Daniel J. Boorstin, *The Discoverers*, (New York: Random House, 1983).

[4]. Alan E. Shapiro, "Historians of Science Must Again Master Scientific Substance," *The Chronicle of Higher Education*, XLIV, 21 (February 20, 1998), B4-B5.

If there is additional discussion of this review, you may access it through the network, at https://networks.h-net.org/h-sci-med-tech

Citation: Stuart W. Leslie. Review of Soderqvist, Thomas. *The Historiography of Contemporary Science and Technology.* H-Sci-Med-Tech, H-Net Reviews. June, 1998.

URL: https://www.h-net.org/reviews/showrev.php?id=2097