



**Review: [Untitled]**

Reviewed Work(s):

*The Historiography of Contemporary Science and Technology* by Thomas Söderqvist  
Alfred I. Tauber

*Science, Technology, & Human Values*, Vol. 24, No. 3. (Summer, 1999), pp. 384-401.

Stable URL:

<http://links.jstor.org/sici?sici=0162-2439%28199922%2924%3A3%3C384%3ATHOCSA%3E2.0.CO%3B2-5>

*Science, Technology, & Human Values* is currently published by Sage Publications, Inc..

---

Your use of the JSTOR archive indicates your acceptance of JSTOR's Terms and Conditions of Use, available at <http://www.jstor.org/about/terms.html>. JSTOR's Terms and Conditions of Use provides, in part, that unless you have obtained prior permission, you may not download an entire issue of a journal or multiple copies of articles, and you may use content in the JSTOR archive only for your personal, non-commercial use.

Please contact the publisher regarding any further use of this work. Publisher contact information may be obtained at <http://www.jstor.org/journals/sage.html>.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

---

The JSTOR Archive is a trusted digital repository providing for long-term preservation and access to leading academic journals and scholarly literature from around the world. The Archive is supported by libraries, scholarly societies, publishers, and foundations. It is an initiative of JSTOR, a not-for-profit organization with a mission to help the scholarly community take advantage of advances in technology. For more information regarding JSTOR, please contact [support@jstor.org](mailto:support@jstor.org).

## REVIEW ESSAY

---

*The Historiography of Contemporary Science and Technology*, edited by Thomas Söderqvist. Amsterdam: Harwood Academic, 1997, 264 pp.

Most scholars would agree that history and philosophy of science began as part of science proper. Prior to our own century, history of science was primarily a rhetorical and theoretical tool in showing how new science was part of a progressive, and rational, process. Review of the historical development of a particular science was an integral component of the scientific report. When Goethe wrote on color theory, Priestly on electricity, and Lyell on geology, these so-called natural philosophers used history to legitimate their own work. Even into our own era, history of science (when still entertained as relevant to science) was often seen as exercising a beneficial influence on practice; for example, the laboratory scientist might profit from history used as an analytical tool (Kragh 1987, 33-34). Although the historical perspective as a value in itself governed such innovators as Giambattista Vico, confusion about historical interpretation as an important scholarly activity distinct from doing science itself was untangled rather slowly. In many respects it remains, in large measure as a response to Kuhn's (1962, 1970) *The Structure of Scientific Revolutions*. Kuhn drew radical inferences from a simple observation: scientific memory is short.<sup>1</sup> According to Kuhn, the practicing scientist had little if any historical consciousness.

More historical detail, whether of science's present or its past, or more responsibility to the historical details presented, could give only artificial status to human idiosyncrasy, error, and confusion. Why dignify what science's best and most persistent efforts have made it possible to discard? The depreciation of historical fact is deeply, and probably functionally, ingrained in the ideology of the scientific profession, the same profession that places the highest of all values on factual details of other sorts (Kuhn 1970, 138). This was an important insight that produced at least two ideas. The first was the fundamental question of how and why science proceeds without this self-consciousness of its own method. The second—and this was the one Kuhn himself, and later the entire discipline, pursued—was to what extent science might be characterized by some historical self-reflexiveness. In a sense, the first set of questions would in effect be answered as a shadow response to the second set.

By now, that scientists take little heed of the historical context of their research is an adage, even a precept, of science studies. To the extent that scientists address the past, they constantly refashion the antecedents of their work in the image of contemporary prevailing theory and explanation. Thus, an objective historical account of the development of scientific understanding or an acknowledgment of possible

nonrational factors at play in the evolution of the scientific discipline accordingly plays no significant role in professional scientific endeavor. Kuhn (1970) similarly observed the general irrelevance of philosophy of science to scientists; or, as he wrote, scientists "have not generally needed or wanted to be philosophers" (p. 88). For that matter, this dismissal is safely extended to the relevance of science studies as practiced in all of its various formats. According to Kuhn, only in crisis periods of revolution would scientists seek out the rules and assumptions of their work. In normal science, the research edifice is so stable that such self-consciousness is not required and, thus, not sought. In fact, the relative distance, or objectivity, required for such assessment simply has no function in the everyday consciousness of the working scientist. In short, to do science, and so to be a successful scientist, historical or philosophical awareness is unnecessary. And, just as a lawyer need not know sociological theory of small groups to practice law before a jury, so a scientist hardly requires a sociological analysis of his own competitive group to function successfully within it. So the question remains, Where does the historian of science fit?

## I

Today, more than a generation after Kuhn's observation, we might still ask: What is the relation of history of science—or, more broadly, science studies in general—to its subject, science? An important aspect of Kuhn's (1977, 3-20) own response in answering this question was to shy away from any consideration of contemporary science, and in this regard alone we witness an interesting divergence in the trend of historians to focus more of their effort on contemporary science. This collection of essays gleaned from the International Workshop on the Historiography of Contemporary Science, Technology and Medicine (Göteborg University, Sweden, 16-17 September 1994) addresses the particular challenge of writing the history of post-World War II science and, more generally, the relationship of history writing to its object of study. Some, like Isaiah Berlin, would opine that such narratives are not history but journalism (cited in Hughes, 19), and indeed the traditional demarcation of the past as, perhaps, events occurring fifty years prior to their interpretation has served as a common hallmark characteristic of "history." But the past is only of degree. Certainly, events occurring before the lifetime of the historian constitute uncontested history, but why should recent past events not also qualify as "the past" and, thus, be suitable grist for the historian? What is sacrosanct about a half-century, or for that matter a full- or a quarter-century? In fact, yesterday is the past. The issue is not the chronological degree of separation between historians and their object of study but the particular intimacy afforded by the contemporaneous time frame. The historian has a different kind of relationship to the persons, events, and artifacts that he scrutinizes in the current or recent past, since in a sense these can often "talk back" in a manner not available to subjects deeper in time.

To be sure, there are unique problems in writing the history of near-contemporary events, including the richness (sometimes overabundance) and the restrictions of

source material, the centripetal pull of the interviewed scientist in presenting the “best” case, the potential conflicts of interests exerted by institutional support on scholars engaged in writing their histories, and so on. Some of the present authors focus on such matters, and these are of course important issues to consider in writing contemporary history, but I believe that those articles that deal with the deeper challenges related to the very nature of current historiography are the most useful for the entire audience of science studies practitioners. I refer to the various efforts attempting to define the fundamental relation of history writing to its object of investigation. The most self-conscious efforts in this regard (e.g., Soraya de Chadarevian, Jeff Hughes, Susan Lindee, Ilana Löwy, and Thomas Söderqvist) strike me as having the most general interest, but the combination of such articles with those more narrowly configured, which assess the problems of writing institutional histories (e.g., Joseph Tatarewicz and Skuli Sigurdsson), results in an excellent collection of critical self-reflections on the state of contemporary history of science studies ranging from frank philosophical concerns to particular technical obstructions. The contributions, although refracting the issues in different ways, are remarkably consistent in their high intellectual content, offering the reader a provocative and thoughtful account of the historical craft.

The attempt to historically characterize the past fifty years of science is not primarily due to the fact that 90 percent of science (papers, participants, budgets, etc.) has occurred in this short period (and thus, as Söderqvist argues, it is important to historically characterize science that has so largely been practiced in our own era); rather, it is because historians have an opportunity to exercise their craft in a particularly interesting fashion when focused on contemporary science. Why? The answer to that question leads these essayists to attempt to characterize their historiography, and one must conclude that pastness per se is not the defining element of history. I am not sanguine that a single “answer” is forthcoming, and this in itself is an important characteristic of the historians’ practice today, but we may ponder the various responses and derive a composite picture of the art of contemporary history writing.

First is the self-conscious acknowledgment that “History is always positioned. . . . Histories are written *by* particular people, *with* particular interests *in* particular settings *for* particular purposes” (Hughes, 25-26). This prepositional description of historical writing alludes to our almost obsessive self-reflective assessment. This self-critical attitude has been gathering force since Carr’s (1961) anti-positivist polemic written a generation ago:

Study the historian before you begin to study the facts. . . . The facts are really not at all like fish on a fishmonger’s slab. They are like fish swimming about in a vast and sometimes inaccessible ocean; and what the historian catches will depend partly on chance, but mainly on what part of the ocean he chooses to fish in and what tackle he chooses to use—these two factors being, of course, determined by the kind of fish he wants to catch. By and large the historian will get the kind of facts he wants. History means interpretation. (p. 26)

Beyond the admission concerning the inability to completely capture the past in any record, Carr, and many others before and after him, argue that such choices are

ultimately political, and thus the conceit of some purified objectivity must be abandoned. History writing then becomes “contested intellectual territory” (Hughes, 22), and this leads to the second defining characteristic of contemporary historiography: the object of inquiry ostensibly may be the “past,” but historians deliberately grapple with the present in their attempts to understand that past. As Hughes observes, “For many, the present has been, and continues to be, the primary object of concern when they approach the past” (p. 20).

The “presentness” of history resides in the moral and epistemological character of history writing—moral because we seek meaning in history; epistemological because, as Augustine famously observed in his *Confessions*, the past does not in fact exist, only the thought of it does in the present. Again, this is an old adage, for as Lindee reminds us, Collingwood (1946) wrote in *The Idea of History* that all history is contemporary history, since all history is a rethinking of the past in terms that bring it into the present. The relativism inherent in such an orientation can hardly be regarded as a radical admission, but its implications have raised a cry from stalwart positivist-inclined scientists engaged in the recent so-called science wars.

As Hughes (citing Jenkins 1991) cogently observes, “‘history finds meaning, after all, in its use’” (p. 27), and thus the history of contemporary science, in particular, becomes most contested, for “‘it is here, in the land of the living, not the archive, that the problems of contemporary scientific history really lie’” (p. 27). It is precisely when the historian is seen as crossing the line into the laboratory, where he or she is regarded as a participant in science and not just a chronicler, that the role of historian is disputed. No longer just an observation, when the interpretation becomes part of the fabric of the “contested intellectual territory”—the object itself, science, and its interpretation, history, become conflated—then the stakes have been radically redefined. The traditional historian, because of his separation from his subject in time, could no longer influence the drama he described, but in the contemporary setting, the active scientist will see the historical interpretation no different in kind from the laboratory researcher’s own writing of history. After all, scientists in large measure are writing their own scientific biographies as they report results, but more important perhaps, they are situating their findings and claims into the larger scientific construct of theory and claims of discovery and priority. This contested contextualization of the facts move those reports beyond the domain of raw scientific knowledge (whatever that might be) to a more complex sociology of knowledge replete with power politics. Thus, the scientist as a self-interpreting autobiographer will challenge those who seek to interpose their own perspective. Thus, “history” is no longer an exclusive dispute between historians; rather, it becomes the contested territory between historians and the historical actors themselves.

My own experience in this arena well illustrates the point. I recently published a study with Scott Podolsky (1997) on the first applications of molecular biology to immunology. *The Generation of Diversity* is the last of a trilogy on the history and development of immunology’s fundamental theory. Unlike its companion volumes (Tauber and Chernyak 1991; Tauber 1994), this contemporary history profited from interviews with the principal scientists involved in the elucidation of antibody manufacture (an important biological question concerning gene rearrangement and the

economy of protein diversification), and we were most pleased to receive their respective insights to help flush out the hidden details of the story. When we sent the manuscript in its penultimate form to various investigators for comment, we received some unexpected (because we were naive) responses. From those who were given more credit than heretofore bestowed, we obtained detailed and thoughtful replies. But from the three alpha competitors for the Nobel Prize—Susumu Tonegawa, Leroy Hood, and Philip Leder—we received no constructive responses, despite repeated inquiries concerning specific issues. Hood and Leder declined comment, and Tonegawa, who alone won the coveted award in 1984, told me in the last of several conversations that he would not deign to discuss the manuscript, for it was “worthless,” by which he explained that historians could not delve into such matters. When I pressed to know whether there were factual errors or interpretations that might be argued differently, he simply blistered, “You have no idea what I was thinking!” And when I asked him to be specific about where we might have made mistakes, he abruptly terminated the discussion. Tonegawa, needless to say, was very much disheartened by our description of his role in the discovery of immunoglobulin synthesis, and although he was cooperative when he thought we would write in support of his own rendition of that history, he abruptly changed posture when he discovered we were critical of his claims and the history he had written of his own accomplishments. My experience is hardly unique. As Lindee writes, “The historian of contemporary science can wound and be wounded in ways that the historian of eighteenth-century science cannot” (p. 42). But this is the price of active interactions, and beyond the sociology of such “interventionist” history writing, there may be a deeper restructuring of the historian’s relation to his object of inquiry in this contemporary setting, a topic I wish to focus on, since it directly addresses the imbroglio of contemporary historiography of science. Specifically, I am concerned with the “boundary issue.”

## II

Söderqvist is forthright in ignoring any firm line demarcating science and the study of it. He readily acknowledges his own desire to actually participate in science by reforming it:

Most important the historiography of recent science may add some thoughtfulness and prudence to the technoscientific system, the prudence gained by years of accumulated experience. . . . Maybe it is time for historians of science to cooperate with mellowed scientists to infuse some *phrenosis* [practical wisdom] into the wildly galloping contemporary technoscientific system. (p. 12)

This is a recurrent theme of Söderqvist’s view of history of science. As a historian who self-consciously has fashioned himself as an existentialist biographer, he has sought to study the deepest motivations and choices made by the immunologist Niels Jerne to understand the life of science as a window to Jerne’s character. Thus, on one level, Söderqvist’s project is to be read as an exploration of what it means to be human

in the most general sense; the life of a scientist is simply the particular terrain of explanation. The same kind of analysis might be made of a politician, an actor, or a financier. But, in further commenting on his craft, Söderqvist (1996) offers a fascinating datum about his own motivation as a practitioner of science studies:

I believe that one good existential biography of a scientist can better contribute to a *remaking of the practices of science* than a score of revealing social historical and sociological investigations of science. (p. 77; emphasis added)

Söderqvist's espousal of historical analysis as part of the effort to remake science sounds a bit like a confessional, whereby announcing or discussing transgressions leads to their absolution. This is the religious confession and the psychoanalytic exposure. In history, it is the ideological interpretive turn that "explains" the course of events, and in that examination we might hope to alter human evolution by a reassessing function that would teach us how to mend our ways and avoid making the same mistakes again. His is a bald assertion. Although he asserts that he has no designs on the cognitive enterprise, only on the moral demeanor of doing science, I find the distinction difficult to maintain. Söderqvist has now drawn an inclusive circle around science and those disciplines that examine scientific practice as an object of historical, philosophical, anthropological, or sociological investigation. Science studies is now to be regarded as part of science—namely, that branch that offers scientists opportunities to self-consciously reexamine themselves in the hope of becoming better scientists. Here is revealed a nontraditional role for the historian, one perhaps present implicitly by all judges of history, but now quite explicit in the proactive role Söderqvist and some others have espoused for science studies more generally. To the extent that such a proactive stance is taken, a different genre of historiography is emerging, and I believe it is this species that has raised the ire of those who have declared a science war. I am confident that the boundary has been blurred; the question is to what extent a particular student of science allows his or her interpretation to merge with the topic of study. In simple terms: Where is the line drawn that separates interpretation from its object?

Although all history is contextualized in the present, interpreted for our understanding, in the particular instance of contemporary science the participatory element becomes an explicit component of the project. In judging current events, an attempt to adjudicate the "right" story is only part of the agenda, the epistemological aspect if you will. The other element is moral, a judgment on the nature of science, its process, and results. Hughes puts the issue quite plainly: "It could be argued that a struggle is going on to define what the history of science is and what it should be. This, in places, is turning out to be a struggle to define what science *is*, and what *it* should be" (p. 29).

There is a divide that is for the most part accepted among scientists and their interpreters. As sociologist Harry Collins has remarked, "I don't expect my studies to make any difference to them, to the scientists' practice. I'm not trying to change physics'" (quoted in Ashmore 1989, 23). It is generally acknowledged that the study of science offers important insights into epistemology and metaphysical thinking if one is a philosopher; into the nature of science as a social institution if one is a sociologist;

into the interpretation of the evolution of science if one is a historian. But underlying each of these interpretations is the attempt to characterize that activity we call "science." And in that characterization there is the implicit presupposition that we are capturing some essential quality of science. Why is the interpretation important? Does it adjudicate the claims of science? Would the validity of a scientific theory or fact be disputed on the grounds of the underlying interpretation of the science used to elucidate that claim? The insularity of the scientific fact itself is generally left unperturbed by any such analysis *within its own scientific environs*. This suffices for the scientific enterprise.

Nevertheless, the scientific establishment is carefully and sometimes painfully scrutinizing science studies where this implicit boundary has been broken. Kuhn made the key observation: Scientists do not require history or philosophy or sociology to do science, and when scientists ask the cogent question, What have these disciplines contributed to science? the answer provided by most of them is, rare cases excepted, nothing. That response is predicated on a narrow view of what science is; namely, from their perspective, the internally driven accumulation of facts. But the historian counters that the very construction of science employs historical reconstructions as the current "science" is reconceived in a reworking of its own historical evolution. As Hughes notes, "History is a crucial resource for scientists not just in terms of the politics of ex post facto legitimation but in the very constitution of scientific knowledge at the laboratory bench itself" (p. 26). He goes on to quote Harrison's (1987) provocative diatribe in *Nature*, one of the earliest salvos of the science wars:

In scientific research, where ideas form and dissolve in a state of flux and at any present moment present countless potential futures, scientists retain their bearings by contrasting past and present ideas. Awareness of temporal depth in science forms an integral part of scientific research. . . . Scientists grope their way, seeking to divine where they are going from where they are coming: they reach into the future as well as the past, and their diachronically extrapolated conceptual networks are not whiggish sins but the essence of science in action. (pp. 26-27)

Thus, history is accepted as integral, but not interpretative history under the aegis of the externalist camp. Clearly, history is seen as constitutive by both sides, but the deeper ideology guiding the practice and use of history is divergent: a contest between a normative vision and one framed by the contingencies of myriad sociopolitical, metaphysical, psychological, and cultural forces. In short, for those who see the truth claims of science made with some insularity, a category mistake has been made by science studies: the object of inquiry and its interpretation have been fused together.

If only the sociologists and historians had simply remained confined to their own departments and argued with each other over the deeper contextualized "meaning" of any given fact or theory (in whatever sociological, historical, or philosophical discourse they chose), the scientists could have safely ignored their discussions. But science studies somehow overspilled its banks and now rushes onward, threatening to sweep away the normative definition of science held by most scientists themselves. According to these interlopers, facts and theories cannot reside within the exclusive

domain of an inner-driven laboratory imperative. These new interpretations would dispense with normative definitions to account for the undeniable empirical success of science. Pristine scientific method and rationality are no longer held as sacrosanct principles, and the internalist—the doer, the researcher—would be left with a skeptical vision of his own efforts. But the success of the researcher is not dependent on understanding the deeper whys and wherefores of science. Pointing to the empirical success of the laboratory, he safely disregards his critics and plods on in his inquiry oblivious to the deeper contextualization of his work. From this perspective, history, philosophy, and sociology of science have little to offer the practicing scientist, because these can only serve as interpretive descriptions of the activity we call science.

One must not confuse the description with its object. For example, a bottle is described in every detail, and even if photographed from a dozen different angles, it is never *the* bottle. As Magritte wrote at the bottom of his painting of a pipe, “Ceci n’est pas une pipe.” The dispute really resides at the defense of an undeclared boundary—between the object of inquiry, science, and its characterization. On this view, the interpretive disciplines can offer only descriptions of science, just as paintings offer descriptions of the world. Bhaskar ([1975] 1997) has defined the issue in terms he describes as the imperative to distinguish between intransitive inquiries (the object of science, the unchanging natural world) and transitive ones (scientific object, a conceptual object that may change):

Any adequate philosophy of science must find a way of grappling with this central paradox of science: that men in their social activity produce knowledge which is a social product much like any other, which is no more independent of its production and the men who produce it than motor cars, armchairs or books, which has its own craftsmen, technicians, publicists, standards and skills and which is no less subject to change than any other commodity. This is one side of “knowledge.” The other is that knowledge is “of” things which are not produced by men at all: the specific gravity of mercury, the process of electrolysis, the mechanism of light propagation. None of these “objects of knowledge” depend upon human activity. If men ceased to exist sound would continue to travel and heavy bodies fall to earth exactly the same way, though *ex hypothesi* there would be no-one to know it. Let us call these . . . *the intransitive objects of knowledge*. The transitive objects of knowledge are . . . the raw materials of science—the artificial objects fashioned into terms of knowledge by the science of the day. They include the antecedently established facts and theories, paradigms and models, methods and techniques of inquiry available to a particular scientific school or worker. (p. 21)

Bhaskar’s philosophy of science hinges on the capacity to sustain these different modes of knowledge—the social character of science and the independence from science of the objects of scientific thought. I need not delve into his own response to this challenge other than to alert readers to the important distinction he has so clearly drawn. These problems of distinction Bhaskar regards as crucial in establishing an ontology for the philosophy of science, and they allow for a realist interpretation.

Plainly stated, the realists argue that science studies may gaze upon the subject, but to confuse that gaze with the subject itself is to make the most basic of category errors.

Goethe in the eighteenth century committed it in his *Theory of Colors* when he attacked Newton, and science studies in the second half of the twentieth century has often been marked by the same conflation. Those scientists who reject the post-Kuhnian critique would support Laudan (1996, 201), a philosopher who accuses science's radical critics of the *fallacy of partial description*; the false supposition that because science is a social activity, it is best understood and explained in sociological terms. Of course, science is a social activity, but it is many other things too; and because of its multifaceted nature, sociological, historical, and philosophical analyses can refract its character only imperfectly. More fundamentally, the argument not only rages over the choice of hermeneutic, but to what extent any interpretation might capture its object of study, much less merge with it. This is hardly an implicit or hidden agenda, for it is precisely on this point that the science wars are being fought.

### III

At the most innocent level, we are witnessing the often ill-defined ontological and epistemological demarcation between science studies and science itself, a problem of methodological identification. This division is especially blurred when the boundary of sociology is extended to include social sciences, which have for decades spun theories to justify the very existence of the discipline. Central to that self-definition is the aspiration of objective scientific analysis. It is one thing for sociology, and history for that matter, to attempt to obtain facts and analysis by means that have proven so successful in the natural sciences. This may be a legitimate project, but a not-too-subtle inversion takes place when, rather than an application of natural scientific method to social analysis, the historical or sociological study is made integral to science itself. The boundary between science and its study then becomes hopelessly blurred.

For instance, Barnes, Bloor, and Henry write in their recent *Scientific Knowledge: A Sociological Analysis* (1996) that they "see the sociology of scientific knowledge as part of the project of science itself, an attempt to understand science in the idiom of science" (p. x; emphasis added). To be sure, at one level, the authors are asserting their methodological legitimacy as social scientists (i.e., using the "idiom" of science and thereby "honour[ing] science by imitation" [p. x]). At another level, they do more than ape scientific method in the project of understanding science, for these sociologists maintain that they are engaged in the "project of science" itself, which must include the study of human organization and behavior. Science then extends beyond the examination of the natural world to include culture; and the unique standing of the scientific endeavor is now open to other kinds of analysis that includes sociological or historical comment. Here, we see an interesting conflation between the object of study by science (Bhaskar's intransitive natural world), the object of study (transitive scientific knowledge), and analysis of science (a second-order transitive investigation we call science studies). If the net is cast wide enough, the entire enterprise becomes one piece. The archaeology of knowledge then has no boundaries or subdivisions, and history and sociology have extended their imperialistic domain to include science

itself within their own bailiwick. The implicit challenge is over the very definition of science, which no longer, from this point of view, can claim insularity according to more traditional divisions.

If the traditional understanding is invoked, scientists in the laboratory do science, whereas those who report on science do so from the outside, whether it is as sociologist, historian, or philosopher. But this designation breaks down when the notion of scientific knowledge and the modes by which it is obtained are perceived as in essence no different from other forms of human understanding. This perspective is now widely held in science studies. Some regard the issue as an instance of simple politics. As one wag has put it, little demarcates science “from other social activities save the efforts of its practitioners to create and service such a demarcation” (Hamilton 1988, 7). Woolgar (1988) writes, “Science itself is not SCIENTIFIC except in so far as it represents itself as such” (p. 107). This position rests on what Laudan refers to as an epistemological conceit, which is attributable to two principle insights. (1) With the emergence and eventual triumph of the fallibilistic perspective in epistemology, it became generally accepted that science offers no apodictic certainty and that all scientific theories are then corrigible and may be subject to serious emendation. Science and nonscience are then no longer distinguishable by assimilating that distinction to the difference between knowledge and opinion. Within this fallibilistic framework, scientific belief then turns out to be a species of opinion (Laudan 1996, 213). (2) The extraordinary heterogeneity of the activities and beliefs customarily regarded as scientific suggests the futility of seeking an epistemic version of a demarcation criterion. Since there appear to be no epistemic invariants, one can not assume their existence, and moreover, even to say there is a boundary problem presupposes the existence of such invariants (Laudan 1996, 221). Laudan summarizes this position:

I am not confident that what we call “the sciences” have any special set of methodological principles or epistemic credentials that clearly sets them off from the supposedly “non scientific” forms of cognition. What I am confident of is the claim that no one, philosopher or sociologist, has yet set out any acceptable account of what cognitive or methodological features demarcate the sciences from the nonsciences. (p. 189)

If one adopts this general orientation, the separation of observer from his or her object of study—whether science or history—may be easily blurred.

The attitude taken toward this question has, not surprisingly, polarized the science studies community. Some, like Latour and Woolgar (1979), strongly embraced the “naive” observer approach, attempting to remain detached observers; indeed, as Löwy describes them, “strangers to the culture of modern laboratories. . . . According to this view only a totally naive observer may avoid the pitfalls of non-critical acceptance of the scientists’ point of view” (p. 92). Various contributors argue a different case, admitting the trials and tribulations of “intimacy,” they still maintained that they must practice their task as informed, indeed expert, observers (e.g., de Chadarevian, Löwy, Tatarewicz). Although these historians espouse an objective distancing, there is a continuum of practice whose opposite pole to Latour and Woolgar is best exemplified by Garfinkel (1977), who has promoted “ethnomethodology.” He has argued that

there is a “gap” in the sociology of occupations created by the methods and interests of studies about occupations as compared with the methods and interests that make up the what (or “quiddity”) of the practices themselves (Lynch 1993, 270-71). This observation seems to me commonsensical, but rather than draw the line of separation between the activity and the interpretation, Garfinkel wishes to erase it altogether. Like Latour and Woolgar, Garfinkel urges the sociologist to carefully examine practice, tools, and daily discourse; that is, to fully assimilate himself into the laboratory to obtain firsthand, primary data for analysis as opposed to looking at research papers, institutional structures, statistics of large behavior patterns, and so on. But in embarking on a practice-oriented approach, Garfinkel found himself on a slippery methodological slope (one Woolgar also slid on, even more radically<sup>2</sup>). Beyond Garfinkel’s call to simple empiricism about scientists’ behavior, he made a more radical proposal suggesting that ethnomethodology might be “hybridized” with other disciplines such as natural sciences so that instead of reporting on exotic practices of a physicist or geneticist, the “product” of this hybrid research would in fact be a contribution to the original object of research. In other words, the sociologist in examining laboratory practice would—in this integrated approach—become one of the research participants and, thereby, contribute to the final assessment.

There is an interesting conclusion to this program; namely, that as a sociological project, it must fail. The ethnomethodologist becomes a scientist, like an anthropologist going native, and although a report is forthcoming, it is by its very nature incomplete and in a sense distorting to the actuality. As Lynch (1993) describes the imbroglia,

The ethnomethodologist would not “go in” to the discipline studied in order to “come back” with a cognitive map or other representation of the culture, since no map would be sufficiently complete to recover the scenic details implicated by a competent reading of the map’s semiotic features. Short of delivering an entire constellation of details that make up the practical worksite, the only uniquely adequate “news” that could be delivered to the professional sociologists would be an apology to the effect, “You would have had to have been there.” (p. 276)

This is not a unique observation. For instance, Baigrie (1995) writes,

We can consult scientific treatises and articles, laboratory notebooks, tables of data, etc., in order to reconstruct some scientific achievement, but *there is no simple historical analogue to practice.* (p. 106)

This recognition raises severe problems for doing science studies, and, although this is not my specific concern, at the very least, according to Garfinkel (1977), it seems that the historian/sociologist qua science studies scholar has forfeited his autonomous professional perspective in favor of going native. There are long, and to my mind tedious, theoretical justifications Garfinkel makes in defense of his new “sociology,” but for our purposes his ideas are interesting as illustrative of the fluidity of the boundaries separating science studies from science itself in both intransitive and transitive modalities. More to the point, in considering this collection of essays, we

appreciate how these issues are directly germane to doing contemporary history of science, where the same kind of subject-object conflation may be operative. At the very least, even an observer such as Löwy, who seeks to remain distanced from her subject, admits to the schizophrenic tension inherent in the tightrope she walks as a historian in the contemporaneous setting:

My external/internal status affected my self-perception. . . . I oscillated between two frames of reference: the “scientific” and the “historical/sociological.” Occasionally I experienced a feeling of superiority, due to my supposedly broader, “bilingual” and “bicultural” point of view; at other times I felt myself a “native of nowhere”—an inadequate immunologist and an awkward historian. (p. 93)

Indeed, the identity conflict led to interpersonal conflict as well. Löwy goes on to admit that she has, in fact, “disturbed” the system:

I was not very happy with my role as a disturbing intruder. On the other hand I was not sure that it is possible—or desirable—to study present science while steadily maintaining good relationships with all the scientists one is studying. (p. 96)

#### IV

Löwy, like Garfinkel, directly examined the very content and nature of scientific knowledge but without going native. The contemporary science historian’s or the sociologist’s thirst for immersing himself or herself into laboratory practice as a sort of participant observer is to my mind a search for an epistemological “objectivity” in contrast to the strong theory-driven sociological analysis to which he or she is reacting. This methodological debate has a strong appeal in the social sciences, including history, and represents a historical and continuing struggle between “idealism” and “observation” that divides disciplines in so many ways. But there are pitfalls to be avoided, as Löwy cogently observes:

Observers, native or naive, may disturb the observed setting. Ethnographers and sociologists are familiar with this problem, and students of present science discover it. Discussing with actors, interviewing actors about their role, confronting them with documents and with their previous declarations, *may change the way they perceive past events, but also influence their present actions.* (p. 93; emphasis added)

(I would call this the “measurement effect,” drawing an analogy with the observer effect in the atomic universe, where determination of either momentum or position obviates accurate determination of the other parameter.) Löwy, unlike Söderqvist, sees the historian’s effect as an untoward passive by-product of the doing of contemporary history, whereas Söderqvist differs from Löwy in seeing the historian as having the opportunity to effect a proactive role. Garfinkel (1977) has a similar view, and

some like Paul Forman (pp. 198-201) would even argue that this is a moral opportunity that should not be squandered.

Forman regards an “activist” attitude as a manifestation of a general sense of “responsibility” that marks postmodernity (pp. 192-201). Echoing Feyerabend (1978), he sees science as simply too important to be left to the scientists themselves, and thus critics of science cannot sit in some neutral niche of insular objectivity but instead must engage the world of technoscience actively in the moral economy of use. Löwy (pp. 103-105) similarly picks up this theme in citing Snow’s (1964) call for a class of interpreters to mediate the growing gulf between technoscience and the illiterate public, again because of the dire consequences of not assuming responsibility. The problem is, of course, that no consensus on such an adjudicating role has been agreed upon by the scientific community or, for that matter, conferred by the public at large. The science wars resulted in large measure because science studies critics who challenged normative views of science (and specifically not those who held more traditional convictions) assumed this interpretative role without prior consent. I am not suggesting in any sense that permission is *required*; I only point out the sociology of the contested intellectual terrain.<sup>3</sup> The implications of this struggle are vast and can hardly be underestimated, not only for the future trajectory of the writing of history but in the very way we understand science and its product, the modern world.

Steve Fuller, in a concluding summary, reiterates that “the availability of multiple pasts [the interpretative turn] is an apt rhetorical fulcrum for leverage into an array of alternative futures” (p. 247), for history, beyond its pedagogical function, “might be a resource that bears critically on contemporary issues” (p. 248). On this view, historians can, and do, function as “epistemic referees, ensuring that credit is given where due and criticism addressed where posed” (p. 252). I contend that this role is coyly understated. Following the central tenet that “all history is created” (Brettler 1995, 1), historians have witnessed a radical shift in centering their profession—a shift of focus from history to historiography. Thus, self-critical debate about the discipline’s theory, methods, and assumptions has taken center stage. Such practice, however, cannot claim methodological absolution, since following deconstruction (irrespective of the resulting self-analytic maze) we must continue to probe our query, perhaps with more circumspection, but without an Archimedean point of reference. With such a floating platform for analysis, the important distinction that remains is how the interpretation is to be distinguished from the object of study, and the interpreter distanced from the actors in the historical drama, if at all. Most sociologists would challenge Garfinkel and most historians would distance themselves from Söderqvist with regard to the points raised here. But I think that those who embrace the ready fusion of interpretation to the science project they explore are simply offering a more honest account of the motivations guiding much of current science studies.

Here we come to the crux of the matter: Many perceive that the assault of contemporary science studies is not only on a positivist vision of science but more deeply on the rationality upon which the entire enterprise is based. The challenge is no less than to what extent science may make its truth claims; that is, by what criteria are we to judge its avowals of Rationality and Objectivity. The scientific enterprise is

committed to a kind of verification not found (and usually unattainable) in other domains of knowledge. Despite the obvious successes that rely on this claim, objectivity as understood in the scientific context rests on a complex philosophical foundation that remains contested. The “objectification of nature” has been challenged by the “sociology of knowledge.” This is a highly convoluted issue, but in its simplest formulation the argument concerns how criteria are chosen for successful prediction and verification. The normative standards and methods heretofore governing our characterization of science are being confronted by a social constructivist vision, which maintains that consensus dynamics are not only operative but in some instances determinative. So-called defenders of science maintain that we may never possess certainty, but at least we may with certainty proceed rationally in our attempt to discover nature’s secrets. Science, they concede, may be “wrong,” “biased,” “trivial,” or used for evil, but this is all irrelevant to the essential point: the truth of scientific propositions. The rejoinder, tinged with irony, is that those who would disclaim a postmodernist interpretation do so against the very same modernist mode of self-referential critiques that spawned and undergirded science itself. And that analysis has self-evidently shown that science is immersed in the tribulations and politics of its supporting culture. Because of its tight intercontextualization within an intricate matrix of philosophical, historical, and cultural contingencies, science cannot possess a singular universal and proscribed method of discovery. Thus, to recognize that science lurches forward by rules neither rigidly formalizable through logical analysis nor insular to its narrow interests is to forfeit an old conceit and embrace a much more comprehensive understanding of this complex activity. This allows us to circumspectly assess science’s appropriate intellectual claims and the power of its vision, not only of nature, but of society and ourselves.<sup>4</sup>

This circumspection about our ability to define scientific inquiry and the rules that govern it has given rise to a more general concern about our reliance on science as a paragon of human knowledge, given that other kinds of knowledge are possible and, within their own domains, legitimate. The argument really boils down to whether the critique of science emanates from a sound reading of history and philosophy, reflecting an honest brokering of the record, or whether science studies of the more radical (and perhaps insidious) constructivist variety are themselves part of a more complex ideological program within which science—in the name of some higher moral vision—is the object to be dismantled.

The historian is embattled. The stakes are high. The outcome is far from clear.

## Notes

1. It is interesting that only two (Paul Forman and Steve Fuller) of the thirteen contributors to this anthology cite any of Kuhn’s publications. So much for historical self-consciousness(!), for it was Kuhn who clearly defined the agenda addressed here. If science, to proceed, does not rely on understanding its own origins and tracing its historical evolution, then to what extent does it adhere to such rational categories of development? The answer Kuhn offered—and later aggres-

sively pursued to the point of his own repudiation—was that scientific evolution was a process that was governed by many factors of which the objective characterization of nature hardly stood alone. Famously, paradigms were the conglomerate of cultural, political, economic, aesthetic, and sociological ingredients that became additional factors in an aspired positivist description of nature. The pursuit of truth remained an ideal, but truth, rationality, and objectivity evinced by the historical record were contingently constructed, and historians were responsible for demonstrating that contingency. Albeit subject to the restraints of nature, and highly flexible in its interpretative schemata, science lost its normative footing. In short, uncertain objectivity yielded to constructivism. Bequeathing the problem of a nonnormative interpretative schema, the essays considered here each, at least implicitly, struggle with Kuhn's formulation.

2. Perhaps one dramatic way of illustrating the point is to examine a radical critique, not as necessarily representative of science studies, or even prescient for its future, but as a case that clearly defines the issues that many see as still implicit in current studies. Consider a most revealing testament offered by Woolgar (1988). His critique takes on several fronts, the most easily discerned being the boundary issue. Woolgar expressly attempts to demolish any distinction between science and its critics: Science is "constitutively social," which means to "abandon the idea of science as a privileged or even separate domain of activity and inquiry" (p. 13). That is a common theme in science studies today, but Woolgar is unusually frank about his agenda. The argument he employs and the implications he draws go to the very root of Western epistemology. He writes that his own project, the social study of science, seeks to undercut the very basic subject-object dichotomy that has pervaded Western thinking and is exemplified by science. In his radical critique, Woolgar attacks his sociological peers from suffering the same ideological limits as those they study:

The disciplines which produce the critique of science share an important epistemological position. Although they might be characterized as distinct from science on disciplinary grounds (that is, in terms of their object), they share with science the *ideology of representation*, the set of beliefs and practices stemming from the notion that objects (meanings, motives, things) underlie or pre-exist the surface signs (documents, appearances) which give rise to them. A critique of science is empty, or at least, severely restricted, if it fails to address this ideology. The problem for science critics . . . is that any attempt to dismantle this ideology, rather than a particular set of claims which emerge from a specific disciplinary (natural scientific) application of this ideology, appears tantamount to dismantling one's own discipline. (p. 99)

No wonder, for as Woolgar further observes,

The pervasiveness of the ideology of representation is such that it informs the practices of critics of science as much as those of scientists. . . . The critic of science is the mediator between the objects of his study (science, scientists' behaviour, etc.) and the signs (texts) which represent those objects, just as the scientist (observer) is the mediator between objects and their representation. (p. 103)

Thus, Woolgar espouses an attack on both science and its study, each of which employs a subject-object dichotomy. With little to support the project other than reflexivity in an endless self-referential exercise, he makes his own agenda explicit: "The task is not just to understand the moral order which sustains the ideology of representation, but also seek ways of changing it" (p. 105).

The foundations of Woolgar's (1988) position lie in some deep suspicion of science and the basic ethos it draws upon. Whether this is the authority of knowledge, or the power of technol-

ogy, or the “dehumanization” of man, or whatever radical agenda is at work, there is clearly a resurrectionist mentality afoot. Without further delving into the specifics of Woolgar’s agenda, one must nevertheless be struck by the overtone of his charge. His critique attempts to present a moral program of challenging science, the “dominant and enduring feature of Western thought since at least the time of Bacon” (p. 11):

The discourse of science is to be understood as a discourse which structures and sustains a particular *moral order* of relationships between agents of representation, technologies of representation and their represented “objects.” (p. 14; emphasis added)

Woolgar ends with an understated moral proclamation:

The hardest possible case remains the Self: the disregarded agent of representation.

The call to find ways of interrogating Self is the fairly unsurprising conclusion to a critique of science with epistemologically radical pretensions. It is, after all, just the latest step in the long historical progression of de-centering—when displaced by Copernicus, Self found refuge in science; when displaced by relativism, Self once again found refuge in the analyst. (p. 109)

Woolgar would allow no resting place, and thus his critique presumes to continue the decentering of self in search of a new moral order. He would no less than redefine the knowing self, as well as recast the Kantian and neo-Kantian projects of distinguishing the noumena from phenomena through representation, all for the purpose of overturning the moral order. Is this the hegemony of science in our culture and its attendant ethos of objectivity, rationality, proof? Unfortunately, Woolgar neither specifies his telos nor declares to what end his program is directed. In another epoch, he would have been called an anarchist.

3. The armies joined in the science wars are in full array. It is commonplace among science studies scholars that “the history, philosophy, and sociology of science should not be entrusted to practicing scientists” (Lynch 1993, 268). Although scientists offer crucial self-portraits and eye-witness accounts, these are not generally regarded as sufficiently sophisticated or detached to achieve an analysis of discovery, theory formation, methodology, scientific practice, and so on. These latter efforts require a reflexive stance, a perspectival “distance.” Yet, we have seen that the putative distance, the moat, if you will, separating historian and scientist may be easily crossed, and scientists of a positive bent regard some critics as fostering their own ideological agenda upon a normative science. Indeed, when the walls of the laboratory are breached, hand-to-hand combat ensues. Integrative efforts are long past due, for the discussion on both sides has been dominated by rhetorical hyperbole; for example, “There is no goddess, Truth, of whom academics and researchers can regard themselves as priests or devotees” (Heal 1987-88, 108). The defense is just as impassioned: “It is downright indecent for one who denigrates the importance or denies the possibility of honest inquiry to make his living as an academic” (Haack 1996, 60). And to take the strident position of banishing what Bunge (1996) calls “cultural garbage” (p. 110) propounded by academic “slobs” (p. 96) and their collective “gangs” (p. 97) can hardly be regarded as either an enlightened or even remotely effective strategy. Taking the sole proprietorship on honesty cannot foster discussion, but then, who’s listening?

4. Few would dispute post-Kuhnian claims regarding the intercontextualization of science and its supporting culture, but these critiques have generated heated debate when the arguments have followed a theoretical continuum that appears to conclude in radical deconstruction, whose end point leaves science reduced to politics. The ire of scientists (and here I am referring to those who regard science as a normative enterprise) targets the assertion that theoretical formulations are heavily determined by ideological orientations, whether political or sexual, and those who would regard science as no more than a rhetorical enterprise, where persuasion is used to over-

whelm the opposition. On this view, the pursuit of knowledge seems to command interest only as a process in which scientists are regarded as pitted against one another in an "agonist field," locked into a constant trial of rhetorical strength. The sociological question, How is truth erected or arrived at? has on one hand been interpreted as a circumscribed description of the pathways governing scientific discourse; on the other hand, the interpretations of such practice have led "defenders" of science to decry what they perceive as a relativist assault on the scientific enterprise. They would hardly deny critical investigation of how science is successful, but their orientation to studies of the methodology of science would be governed by such questions as How and why does the use of experiment allow us to know the natural world? or similarly framed epistemological interrogations such as What kind of world allows science to explore nature experimentally? The scientific realist thus remains steadfast to the object of his investigation, the natural world, from a particular vantage point; but this perspective accounts for only part of his divergence from his critic (for extended discussion, see Tauber 1997).

## References

- Ashmore, Malcolm. 1989. *The reflexive thesis: Wrioting sociology of scientific knowledge*. Chicago: University of Chicago Press.
- Baigrie, Brian S. 1995. Scientific practice: The view from the tabletop. In *Scientific practice*, edited by Jed Z. Buchwald, 87-122. Chicago: University of Chicago Press.
- Barnes, Barry, David Bloor, and John Henry. 1996. *Scientific knowledge: A sociological analysis*. Chicago: University of Chicago Press.
- Bhaskar, Roy. [1975] 1997. *A realist theory of science*. London: Verso.
- Brettler, Marc Zvi. 1995. *The creation of history in ancient Israel*. London: Routledge.
- Bunge, Mario. 1996. In praise of intolerance to charlatanism in academia. In *The flight from science and reason*, vol. 775, edited by Paul R. Gross, Norman Levitt, and Martin W. Lewis, 96-115. New York: New York Academy of Sciences.
- Carr, E. H. 1961. *What is history?* New York: Vintage Books.
- Collingwood, R. G. 1946. *The idea of history*. Oxford, UK: Oxford University Press.
- Feyerabend, Paul. 1978. *Science in a free society*. London: NLB.
- Garfinkel, Harold. 1977. When is phenomenology sociological? A panel discussion with J. O'Neill, G. Psathas, E. Rose, E. Tiryakian, H. Wagner, and D. L. Wieder. *Annals of Phenomenological Sociology* 2:1-40.
- Haack, Susan. 1996. Concern for truth: What it means, why it matters. In *The flight from science and reason*, vol. 775, edited by Paul R. Gross, Norman Levitt, and Martin W. Lewis, 57-63. New York: New York Academy of Sciences.
- Hamilton, Peter. 1988. Editor's foreword. In *Science: The very idea*, edited by Steve Woolgar, 7-8. Chichester: Ellis Harwood; London: Tavistock.
- Harrison, E. 1987. Whigs, prigs, and historians of science. *Nature* 329:213-14.
- Heal, Jane. 1987-88. The disinterested search for truth. *Proceedings of the Aristotelian Society* 88:97-108.
- Jenkins, Keith. 1991. *Re-thinking history*. London: Routledge.
- Kragh, Helge. 1987. *An introduction to the historiography of science*. Cambridge, UK: Cambridge University Press.
- Kuhn, Thomas S. 1962. *The structure of scientific revolutions*. Chicago: University of Chicago Press.

- . 1970. *The structure of scientific revolutions*. 2nd ed. Chicago: University of Chicago Press.
- . 1977. *The essential tension*. Chicago: University of Chicago Press.
- Latour, Bruno, and Steve Woolgar. 1979. *Laboratory life: The construction of scientific facts*. Princeton, NJ: Princeton University Press.
- Laudan, Larry. 1996. *Beyond positivism and relativism: Theory, method, and evidence*. New York: Westview Press.
- Lynch, Michael. 1993. *Scientific practice and ordinary action: Ethnomethodology and social studies of science*. Cambridge, UK: Cambridge University Press.
- Podolsky, Scott H., and Alfred I. Tauber. 1997. *The generation of diversity: Clonal selection theory and the molecularization of immunology*. Cambridge, MA: Harvard University Press.
- Snow, C. P. 1964. *The two cultures and a second look*. Cambridge, UK: Cambridge University Press.
- Söderqvist, Thomas. 1996. Existential projects and existential choice in science: Science biography as an edifying genre. In *Telling lives in science: Essays on scientific biography*, edited by Michael Shortland and Richard Yeo, 45-84. Cambridge, UK: Cambridge University Press.
- Tauber, Alfred I. 1994. *The immune self: Theory or metaphor?* New York: Cambridge University Press.
- . 1997. Introduction. In *Science and the quest for reality*, edited by A. I. Tauber, 1-49. New York: New York University Press.
- Tauber, Alfred I., and Leon Chernyak. 1991. *Metchnikoff and the origins of immunology*. New York: Oxford University Press.
- Woolgar, Steve. 1988. *Science: The very idea*. Chichester: Ellis Harwood; London: Tavistock.

—Alfred I. Tauber  
*Boston University*