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CRITIQUES AND CONTENTIONS

Dismantling Boundaries in Science and Technology Studies

By Peter Dear* and Sheila Jasanoff**

ABSTRACT

The boundaries between the history of science and science and technology studies (STS) can be misleadingly drawn, to the detriment of both fields. This essay stresses their commonalities and potential for valuable synergy. The evolution of the two fields has been characterized by lively interchange and boundary crossing, with leading scholars functioning easily on both sides of the past/present divide. Disciplines, it is argued, are best regarded as training grounds for asking particular kinds of questions, using particular clusters of methods. Viewed in this way, history of science and STS are notable for their shared approaches to disciplining. The essay concludes with a concrete example—regulatory science—showing how a topic such as this can be productively studied with methods that contradict any alleged disciplinary divide between historical and contemporary studies of science.

THE NIGHT, THEY SAY, is always darkest before the dawn. If that is so, the future of science and technology studies (STS) should be bright indeed, because the confusion surrounding the field, and in particular its relationship to the history of science, has now reached levels of almost impenetrable gloom. In a recent article in *Critical Inquiry*, Lorraine Daston makes fun of the unrequited love that scholars of science and technology studies (STS) allegedly feel for the history of science, as they chase after their elusive quarry with the giddy adoration of Shakespeare's deluded heroes and heroines in *A Midsummer Night's Dream*.[1] Beneath the banter, Daston purveys a divisive and, in our

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[1] Lorraine Daston, "Science Studies and the History of Science," *Critical Inquiry*, 2009, *35*:798–813.

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view, profoundly misleading message on many levels: about the histories of the history of science and STS, the nature of disciplines, and, not least, the actual current relations between established modes of studying science and technology.

The source of discontent, Daston suggests, has to do with disciplinarity, understood in her very particular sense as a function of the object of inquiry and the methods that investigate it. The history of science, she argues, has gained in disciplinary maturity over the last twenty years, largely by clothing itself in the enabling mantle of (cultural) history.[2] That choice lends definition to both object and method. Daston asserts that history of science, in the manner of its newly acknowledged parent discipline, continually problematizes its subject matter, no longer taking science for granted but instead asking what science is or was in different periods; the resulting investigations carefully tease apart the varied meanings of scientific practice in different temporal settings. Methodologically, too, the history of science has become more disciplined, she claims, by rigorously mastering the tools of the historian's craft—as revealed in elaborate footnotes, the well-carpentered joints of the historical edifice. STS, by contrast, has remained in Daston's account stuck in interdisciplinarity, derivative and undisciplined, slavishly borrowing its craftwork from other, more productive fields, and constrained in its imagination by accepting on faith the view that science is simply whatever scientists say it is.

These are weighty charges, turning on firmly drawn lines of demarcation, and Daston brings in heavy authorities to shore up her rhetorical boundary walls. On the STS front, she quotes a widely circulated (and in its way deeply misrepresented) "recantation" by Bruno Latour, in 2004, to the effect that "fortunately (yes, fortunately), one after the other we witnessed that the black boxes of science remained closed and that it was rather the tools [of science studies] that lay in the dust of our workshop, disjointed and broken."[3] On the side of the historians, she approvingly quotes, from Mario Biagioli's introduction to his 1999 *Science Studies Reader*, an apparent claim that science studies has not been forced to define its subject matter, because the latter comes "prepackaged" and is a "socially delineated object" no matter how you look at it: "As a result, science studies tends not to ask what science is but rather how science works."[4]

One could hardly ask for deeper confusion regarding what should be a fairly straightforward business. After all, most fields have managed to constitute their objects of study, and even to redefine them significantly over time, without getting lost in conundrums about their changing ontologies. Anthropology investigates (*inter alia*) human culture under any of its definitions, sociology studies society, political science reflects on the arts of governing, and philosophy grapples with the foundations of thought. Indeed, corresponding to almost every recognized discipline in the humanities and social sciences there is an area, albeit loosely defined, of large-scale, organized human activity that the field

[2] While Daston does not explicitly specify cultural history, her text is concerned primarily, and in our view much too narrowly, with cultural history as a model for the historiography of science.

[3] Bruno Latour, "Why Has Critique Run out of Steam? From Matters of Fact to Matters of Concern," *Crit. Inq.*, 2004, *30*:225–248, on p. 242. A closer reading of his text shows not an abandonment of constructivism in relation to scientific facts but, rather, the extension of that method to encompass the wider realities that Latour here calls "matters of concern." One can ask how new these insights are or take issue with Latour's definition of "critique." One cannot reasonably read the article, in the way Daston does, as an admission that there is little to be learned from science studies.

[4] Daston, "Science Studies and the History of Science" (cit. n. 1), p. 807, quoting Mario Biagioli, "Introduction: Science Studies and Its Disciplinary Predicament," in *The Science Studies Reader*, ed. Biagioli (New York: Routledge, 1999), pp. xi–xiii, on p. xii. (This is something of a misreading of Biagioli, but we are concerned here with Daston's own argument.)

claims as its own proper turf. Upon that turf, all questions relating to the nature, quality, purposes, techniques, uses, and abuses of the activity in question—past and present—are considered fair game. And this enterprise always includes interrogation and problematization of the discipline's topical category itself. Thus art historians and art critics have been just as preoccupied with questions such as "Is it art?" and "What makes good art?" as they are with the appearance of perspective in Western painting, the authenticity of a Rembrandt portrait, the social origins of the Arts and Crafts movement, or the political valences of cubism. Many radical shifts in the ways artists see the world have come about by virtue of changes in the material and social means of making art objects, such as the advent of photography and the moving image—and even genetic engineering.[5] Art historians take all these shifts on board as part of their intellectual turf without feeling threatened in their disciplinary identity—in effect problematizing quite unproblematically.

Why should things be any different for scholars who wish to study science and technology? For many people in both the history of science and STS the answer would be that they are not and never have been—at least not since the likes of Ludwik Fleck, Thomas Kuhn, and David Bloor made clear that science is much more than the bloodless realm of logical empiricism and first principles, that people preexist their knowledge of the world (just as much as the world preexists the knowledges of people), that materiality matters in the making and proving of scientific truths, and that both the sciences and the dynamics of scientific and technological practice are fertile ground for social, political, and ethical analysis.

The resulting body of scholarship has moved from infancy to adolescence by asking several shared questions about science and technology. Their character as social practices has been at the very center of the field of inquiry, regardless of the original "home disciplines" of contributors to the project and regardless, too, of the historical period on which they focused their inquiry. Robert Merton, the great midcentury exponent of American sociology of science, addressed the specialness of science by laying out its (as he thought) distinctive normative foundations: seeking to explain the origins of modern science as a social institution, Merton saw no methodological barriers in turning to seventeenth-century England. Kuhn, both philosopher and historian, offered an account of how "normal science" works in order to illuminate what happens at moments of radical change in the theoretical apparatus of science. Steven Shapin and Simon Schaffer demonstrated in their seminal work on Boyle and Hobbes how experimentalism was a political and social as well as an epistemic achievement in Restoration England. Evelyn Fox Keller historicized the same period with a feminist edge, arguing that gendered categories were built into the scientific project from its inception, defining both what science is and how it works. Latour, with inimitable wit and passion, queried and dissolved the boundary between making and "applying" science, popularizing the Heideggerian term "technoscience" and highlighting the role of the material and the mundane in the work of knowledge making; instructively, one of his frequently cited works in the genre of actor-network theory features the nineteenth-century practical science of Louis Pasteur. Ian Hacking has combined philosophical analysis with cultural history to probe categories of the human sciences and the putative differences between human and natural kinds.

[5] The Chicago-based artist Eduardo Kac created a stir with his glowing "green fluorescent protein" (GFP) bunny, a white rabbit genetically fitted out with modified fluorescence genes from jellyfish to make its eyes and skin glow under ultraviolet light. See Kac's website, http://www.ekac.org/, including specific pages of documentation on the GFP bunny, http://www.ekac.org/gfpbunny.html.

Donna Haraway—biologist, historian, and philosopher—taught a generation to see the life sciences as a rough-and-tumble space in which ideology, gender, and institutional power shape the very constructs with which scientists populate and represent the natural world.[6]

These scholars are all key figures in the emergence of science and technology studies (the field's proper name, despite its frequent reduction of convenience to "science studies"), as well as canonical authors in most history of science reading lists. To box them into neat compartments labeled, on the one hand, "what science is," for historians, and, on the other, "how science works," for social analysts, would do them, and their *oeuvre*, a grave disservice. More, it would ignore one of the major insights of science studies, and of social analysis more generally, over the past half-century or so: to study how something *works* is to study what it *is*. The nature of a thing is best got at, this view holds, as a function of how people go about with it, in it, around it, how they get a grip on it, make sense of it, use it, or bestow meaning on it. And, of course, of how people demarcate that thing from other things that could be confused with it. To put the question in the language common to all students of science and technology: When are they drawing boundaries around science or technology, and thereby making or reinforcing them?[7] Relatedly: Why do they seem to be constructing those particular demarcation lines? Who is served and in what ways by the demarcations? When should we, as thoughtful inhabitants of the social world, be content with the location of particular boundaries? That objects, theories, and systems of thought in effect acquire lives through the human practices that incorporate them is a cornerstone of Daston's own work, deeply informed by that of some of her most prominent STS colleagues.[8]

If the attempt to demarcate STS from history of science conspicuously fails when measured against actual scholarly practice, then are there better ways to think about the relationship between them? This essay offers the merest sketch of an answer, but in a manner that we hope will begin to clear away the murk, not just for practitioners falling under one or the other label, but for those interested in disciplinarity and interdisciplinarity more broadly. We look first at the history of the history of science itself and at the role of STS in that history. Turning from a preoccupation with what history of science and STS *are* to the more generative question of how they *work*, we then look at academic training as a mode of discipline building, importantly extending, in our view, Daston's emphasis on topic and method as the defining elements of disciplines. Finally, we return to one tacit feature of Daston's disciplinary boundary drawing—the separation of past from present in studies of science and technology—and ask if this barrier is worth sustaining in the light of recent scholarship.

[6] Robert K. Merton, *Science, Technology, and Society in Seventeenth-Century England* (1938; New York: Fertig, 1970); Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago: Univ. Chicago Press, 1970); Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton, N.J.: Princeton Univ. Press, 1985); Evelyn Fox Keller, "Baconian Science: A Hermaphroditic Birth," *Philosophical Forum*, 1980, *12*:299–308; Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge, Mass.: Harvard Univ. Press, 1987), pp. 174–175; Latour, *The Pasteurization of France* (Cambridge, Mass.: Harvard Univ. Press, 1988); Ian Hacking, *The Social Construction of What?* (Cambridge, Mass.: Harvard Univ. Press, 1999); Hacking, *Rewriting the Soul: Multiple Personality and the Sciences of Memory* (Princeton, N.J.: Princeton Univ. Press, 1995); and Donna J. Haraway, *Simians, Cyborgs, and Women: The Reinvention of Nature* (New York: Routledge, 1991).

[7] Thomas Gieryn, *Cultural Boundaries of Science: Credibility on the Line* (Chicago: Univ. Chicago Press, 1999).

[8] See, e.g., Lorraine Daston, ed., *Biographies of Scientific Objects* (Chicago: Univ. Chicago Press, 2000).

AN INFORMED FOLK HISTORY OF THE HISTORY OF SCIENCE AND STS

There are many ways of describing what happened to the history of science since its emergence as a recognized academic specialty in the mid-twentieth century: technical achievements were increasingly embedded in social and cultural histories; practice and performance received greater attention, through accounts of proof, credibility, and experiment; interest grew in how science is materialized through instrumentation and technics and in how it relates to other social institutions, such as law or religion. All these moves have broadened and enriched its investigative palette and, consequentially for our purposes, thickened the links between history of science and STS.

Academic studies of science and technology undertaken prior to the twentieth century tended largely to focus on historical narratives of conceptual change. Beyond academia there were, to be sure, occasional influential writers who argued that sociopolitical matters were either impediments to or facilitators of successful technical and scientific endeavors and who studied the implications of science and technology for society and politics.[9] But the dominant genre of "science studies" was the history of scientific ideas, whether William Whewell's *History of the Inductive Sciences* (1837), the historically narrated progress reports published by the French Académie des Sciences, or, later, the histories of astronomy by Agnes Clerke and the philosophico-historical studies of Pierre Duhem and Ernst Mach.[10] This emphasis on historical narration reflected a fundamentally intellectualist understanding of the field's subject matter: science was about increasing knowledge of the world, a process of the discovery of natural truths; its history could therefore be chronicled as conceptual advances over time. This necessarily progressive history aligned well with a demarcation between science as an endeavor in natural philosophy and the messier, less linear development of practical or "applied" science.[11]

That distinction, however, has come to be seen as a phenomenon deserving of analysis in its own right. Separating knowing from doing is itself an artful accomplishment, not easily captured by following ideas and their evolution. The creation of the twin categories "science" and "technology," and others functionally similar to them, involved much work in establishing the appearance of a fundamental difference between the two. Understanding the nature of that work often called for a fusion of the historical and sociological imaginations. An exemplary investigation of this kind is a 1976 article by Steven Shapin and Barry Barnes, "Head and Hand: Rhetorical Resources in British Pedagogical Writing, 1770–1850," which showed the justification of hierarchical social arrangements achieved by the head/hand dichotomy. The authors quote an article of 1826 concerning the inadvisability of scientific education for the working classes: "It may easily be shown that practice and theory seldom unite in the same individual; that the occupation of the practitioner requires all his time and thoughts to fulfill the wishes of eye or hand: whilst

[9] See Charles Babbage, *Reflections on the Decline of Science in England* (London, 1830); and Frederick [Friedrich] Engels, *The Condition of the Working-Class in England in 1844* (London, 1892) (the German original was published in 1845), on the social effects of new industrial machinery, as notable examples.

[10] E.g., Agnes M. Clerke, *A Popular History of Astronomy during the Nineteenth Century* (Edinburgh, 1886); Pierre Duhem, *Les origines de la statique*, 2 vols. (Paris, 1905–1906); and Ernst Mach, *Die Mechanik in ihrer Entwicklung* (Leipzig, 1883).

[11] For a survey and confirmation of this point see Rachel Laudan, "Histories of the Sciences and Their Uses: A Review to 1913," *History of Science*, 1993, *31*:1–34. See also Peter Dear, "What Is the History of Science the History *Of*? Early Modern Roots of the Ideology of Modern Science," *Isis*, 2005, *96*:390–406; and Dear, "Towards a Genealogy of Modern Science," in *The Mindful Hand: Inquiry and Invention from the Late Renaissance to Early Industrialisation*, ed. Lissa Roberts, Simon Schaffer, and Dear (Amsterdam: Koninklijke Nederlandse Akademie van Wetenschappen, 2007), pp. 431–441.

the theorist reasons with himself, *and throws himself on his mind*. Theoretical excellence must have reason for its soil, which mechanics [i.e., manual workers] have not."[12] Such arguments, commonplace not only in nineteenth-century Britain, show how much was ignored or lost in traditions of writing the history of science purely as an intellectual endeavor of natural philosophy.

In the concluding chapter of David Kaiser's 2005 edited collection *Pedagogy and the Practice of Science*, Kaiser and Andrew Warwick borrow from Foucauldian as well as Kuhnian perspectives to observe that a pedagogical history of modern science and technology "posits training as a general mechanism for the active production of knowing individuals that recognizes no natural distinction between the mind and the body, nor, by implication, between theory and practice." Warwick's own study of Cambridge physics focuses on theoretical training along exactly these lines—even so-called theoretical training involves no such distinction as being *necessary*.[13] For the historian, the real interest, and the real payoff, in looking at dichotomies between knowing and doing lies in the details of the story, whereby what has been involved in the historical creation of such taken-for-granted categories can be clearly exposed. For the social analyst, whose purposes often converge with those of social historians, such stories resonate with wider issues of class, race, gender, and power in ways that are of equal interest whether situated in the present or in the past.

Anglophone discussions of science as a topic of investigation from perspectives not just intellectualist but also social, political, economic, and cultural increasingly attained academic respectability in the first half of the twentieth century. These had long been seen as important practical matters for scientists, engineers, and policy makers, including jurists,[14] but they found their way into historical literature on science in the 1920s and 1930s in large part through Marxist and Marxist-inflected interventions. Thus, from the famous jolt provided in 1931 by Boris Hessen's "Social and Economic Roots of Newton's *Principia*" at that year's "Science at the Crossroads" conference, studies by Joseph Needham, J. D. Bernal, and, after World War II, Stephen Mason and others presented a new model for the understanding of science, technology, and their history.[15]

The 1950s and 1960s witnessed a marked growth, particularly in the United States, of

[12] Steven Shapin and Barry Barnes, "Head and Hand: Rhetorical Resources in British Pedagogical Writing, 1770–1850," *Oxford Review of Education*, 1976, *2*:231–254, on p. 232. The active creation of difference, for whatever reasons, between domains claimed to be categorically distinct means that the term "technoscience," which collapses the boundary on largely theoretical grounds, can be misleading in both historical and contemporary studies. See Latour, *Science in Action* (cit. n. 6); and Donna Haraway's discussions of technoscience in *Modest_Witness@Second_Millenium.FemaleMan©_Meets_OncoMouse™* (New York: Routledge, 1997).

[13] Andrew Warwick and David Kaiser, "Conclusion: Kuhn, Foucault, and the Power of Pedagogy," in *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives*, ed. Kaiser (Cambridge, Mass.: MIT Press, 2005), pp. 393–409, on p. 403; and Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago: Univ. Chicago Press, 2003).

[14] By the late eighteenth century, e.g., English common law courts were deeply engaged in determining what constitutes legitimate technical expertise and how to recognize it: Tal Golan, *Laws of Men and Laws of Nature: The History of Scientific Expert Testimony in England and America* (Cambridge, Mass.: Harvard Univ. Press, 2004).

[15] Boris Hessen, "The Social and Economic Roots of Newton's *Principia*," in *Science at the Crossroads*, ed. N. I. Bukharin (1931; rpt., London: Cass, 1971), pp. 151–212. See also Anna-K. Mayer, "Setting up a Discipline: Conflicting Agendas of the Cambridge History of Science Committee, 1936–1950," *Studies in History and Philosophy of Science*, 2000, *31A*:665–689; Mayer, "Setting up a Discipline, II: British History of Science and 'The End of Ideology,'" *ibid.*, 2004, *35A*:41–72; Vidar Ennebak, "Lilley Revisited; or, Science and Society in the Twentieth Century," *British Journal for the History of Science*, 2009, *42*:563–593; Michael Aaron Dennis, "Historiography of Science: An American Perspective," in *Science in the Twentieth Century*, ed. John Krige and Dominique Pestre (Amsterdam: Harwood Academic, 1997), pp. 1–26; and Steven Shapin, "Discipline and

studies on the politics and policy of science by Vannevar Bush, James B. Conant, Don K. Price, Harvey Brooks, and other practitioners. The approach of these American scholars was, of course, quite different from the state-directed conceptions of science and technology promoted by Marxist-inspired analysts such as Bernal: in light of Daston's argument, it is notable that in this period it was often those most knowledgeable about how science and politics work together in practice—usually scientifically or technically trained advisors to the state—who were considered most competent to comment on the nature of science in relation to policy. As a result, their actors' conceptions of how science works became naturalized into dominant accounts of the nature of science, especially in its interactions with politics. In casting a colder analytic eye on how science works in such domains, STS analysts have therefore also questioned taken-for-granted notions of what science is.

The establishment in the 1960s of the first academic programs in "Science, Technology, and Society" (STS) at such universities as Stanford, Cornell, Harvard, and MIT followed directly from that period's newly prominent political concerns, as well as from social movements against war, nuclear power, and environmental pollution. Emblematized by Rachel Carson's 1962 *Silent Spring*, American critiques tended to focus more on technology than on science and more on the (corrupting) military and industrial influences on science than on the role of the state itself.[16] Today, we can historicize some of these efforts as themselves implicated in a long-running project of creating and maintaining a value-free domain of science, separate from the tainting effects of corporate power and special interests, which is one of the durable markers of U.S. political culture and public reason.[17]

Meanwhile, following an apparently separate track, university programs and departments of the history of science or, more frequently, history and philosophy of science (HPS) were formed during the 1950s and 1960s in the United States as well as in Britain. The common association of the history of science with the philosophy of science in this period illustrates the persistence of the intellectualist perception of science as "natural philosophy" in the enterprise of the history of science itself. James B. Conant, an important figure in postwar U.S. science policy and hence deeply immersed in the politics of science, nonetheless promoted the study of the history of science at Harvard primarily in the mode of teaching science's *intellectual* practices. Such choices reflect the practical significance of Merton's contemporary demarcation that held separate the intellectual ("internal") and social components of science—and of the comparable division between science and technology.

It is an appropriate irony, therefore, and one central to the eventual emergence in the 1980s of the field of "science studies," that Thomas Kuhn was a direct product of Conant's historiographical ambitions. *The Structure of Scientific Revolutions* (1962), appearing in the same year as Carson's *Silent Spring*, offered the work of scientific communities as a crucial backdrop to an intellectual history of science. Kuhn expanded that reorientation in the 1970 "Postscript" to his book's second edition, suggesting that a sociological study of

Bounding: The History and Sociology of Science as Seen through the Externalism–Internalism Debate," *Hist. Sci.*, 1992, *30*:333–369.

[16] For more detail see Sheila Jasanoff, "A Field of Its Own: The Emergence of Science and Technology Studies," in *Oxford Handbook of Interdisciplinarity*, ed. Robert Frodeman, Julie Thompson Klein, and Carl Mitcham (Oxford: Oxford Univ. Press, 2010), pp. 191–205. Jerome R. Ravetz's important *Scientific Knowledge and Its Social Problems* (Oxford: Clarendon, 1971) may also be located in this context.

[17] On the interplay of epistemic and political authority in American political culture see, e.g., Sheila Jasanoff, *Designs on Nature: Science and Democracy in Europe and the United States* (Cambridge, Mass.: Harvard Univ. Press, 2005).

disciplinary communities ought to precede the study of the conceptual elements of their scientific work.[18] The second edition of *Structure* served as an impetus, or catalyst, for the developments that created the "sociology of scientific knowledge" (SSK), especially in the work of the British sociologists Barry Barnes at Edinburgh and Harry Collins at Bath and the Edinburgh philosopher–turned–social scientist David Bloor. Certainly, other intellectual resources besides Kuhn also played important roles, as with Bloor's and Collins's sociological readings of the later Wittgenstein and Bloor's interest in Durkheim, Mannheim, Mauss, and the work of the British anthropologist Mary Douglas. Edinburgh University's Science Studies Unit, founded by David Edge in 1964, became in the 1970s a beacon for a particular kind of sociological study of science, one that insisted—going well beyond Kuhn's sociological hints—that intellectual content is always and altogether amenable to sociological explication. Historical work was represented by various members of the Edinburgh school, notably Steven Shapin, in the enterprise of the so-called Strong Programme of SSK.[19]

Prior to the 1980s, the academic study of science still largely hewed to two distinct tracks, the history of science (or HPS, sometimes with an added "T" for "technology") and STS. The separation was justified by several institutional contingencies within and outside academia: the focus on the intellectual conception of science by HPS that maintained a separation between "science" and "technology"; the professional reinscription of that boundary by the History of Science Society (founded in 1924) and the Society for the History of Technology, formed as a splinter group in 1958 in recognition of the largely intellectualist orientation of the HSS; and, not least, varying social and political circumstances that imbued contextual studies of scientific practice with greater or lesser urgency across Western countries. The newly self-identified U.S. academic specialty of science, technology, and society joined hands with European science studies to form the Society for Social Studies of Science (4S) at Cornell in 1975 (the absence of the word "technology" in its title marks its connections to the HPS tradition but has since prompted much self-reflection by 4S members). David Edge, of Edinburgh's Science Studies Unit and the longtime editor of the new journal that soon became *Social Studies of Science*, was a founding member of the 4S, as was Bruno Latour; therefore 1975 can be designated as marking the birth of the field called "science and technology studies" (as then-president of the 4S Sheila Jasanoff noted in her 1999 talk at the History of Science Society's seventy-fifth anniversary meeting).[20]

This story necessarily smoothes over many of its own imperfections. The University of Pennsylvania's Department of History and Sociology of Science became known in the 1970s as a haven of the social history of science and medicine, self-consciously violating

[18] Kuhn, *Structure of Scientific Revolutions* (cit. n. 6), p. 176. Barry Barnes's *T. S. Kuhn and Social Science* (New York: Columbia Univ. Press, 1982) notably developed this theme.

[19] The philosopher Mary Hesse at Cambridge alternatively dubbed it "the strong thesis" in the sociology of knowledge, where "strong" means that sociological explanation is taken to be capable of accounting for almost everything, rather than being merely supplementary ("weak"): Mary Hesse, "The Strong Thesis of Sociology of Science," in *Revolutions and Reconstructions in the Philosophy of Science* (Bloomington: Indiana Univ. Press, 1980), pp. 29–60. Cf. the implication of Daston, "Science Studies and the History of Science" (cit. n. 1), p. 801.

[20] Sheila Jasanoff, "Reconstructing the Past, Constructing the Present: Can Science Studies and the History of Science Live Happily Ever After?" *Social Studies of Science*, 2000, *30*:621–631, on p. 622. In 2000, the 4S Council officially elected not to change the name of the society, which currently describes itself on its website (http://www.4sonline.org/society.htm) as follows: "Society for Social Studies of Science is the oldest and largest scholarly association devoted to understanding science *and* technology. While as many of us study technology as science, we continue to use our original name, or simply '4S.'"

the usual intellectualist norms of the history of science; many historians of science had by then begun to pay increased attention to the structures of scientific groups and communities, as Kuhn had prescribed.[21] But beginning in the early 1980s, "science and technology studies," standing on a tripod of the history of science and technology, "science studies" in the Edinburgh vein, and the more explicitly critical "science, technology, and society," had begun to stride out into the world as an autonomous formation. Historians trained in the dominant tradition of HPS noticed that there were other ways of engaging with scientific ideas than those bearing the imprimatur of philosophy. The approaches promoted by Bloor, Collins, Barnes, and Shapin in Britain, under the general banner of the "sociology of scientific knowledge," provided one alternative methodological structure within which to study scientific ideas; so too did Bruno Latour and Steve Woolgar's *Laboratory Life* (1979), an anthropological provocation concerning contemporary scientific work. The watershed may be taken as the appearance in late 1985 of Steven Shapin and Simon Schaffer's *Leviathan and the Air-Pump*.[22] One of its authors (Shapin, trained in the Penn program) soon thereafter joined the newly founded, and notably named, Science Studies Program at the University of California, San Diego (established in 1989); still later, exemplifying the practical fluidity of boundaries, he moved to the Department of the History of Science at Harvard.

Other intellectual resources that historians of science indebted to "science studies" in the 1980s began to appropriate were the work of Michel Foucault, further work by Latour, and the revival (thanks to an English translation in 1979) of Ludwik Fleck's 1935 *Genesis and Development of a Scientific Fact*.[23] Marxist perspectives continued to inform the work of historians of technology such as David Noble (as they also inform more recent research on global technological transformations). Important feminist work on science was produced around the same period, sometimes drawing on themes from SSK, as well as new studies investigating the political aspects of science and technology from standpoints in democratic theory.[24]

[21] For landmark historical work that began to focus on scientific communities see, among others, Maurice P. Crosland, *The Society of Arcueil: A View of French Science at the Time of Napoleon I* (Cambridge, Mass.: Harvard Univ. Press, 1967); Gerald L. Geison, *Michael Foster and the Cambridge School of Physiology* (Princeton, N.J.: Princeton Univ. Press, 1978); and Jack Morrell and Arnold Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science* (Oxford: Clarendon, 1981).

[22] Bruno Latour and Steve Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (London/Beverly Hills, Calif.: Sage, 1979); and Shapin and Schaffer, *Leviathan and the Air-Pump* (cit. n. 6). Also worthy of note is Barry Barnes and Steven Shapin, eds., *Natural Order: Historical Studies of Scientific Culture* (London/Beverly Hills, Calif.: Sage, 1979).

[23] Latour, *Science in Action* (cit. n. 6); and Ludwik Fleck, *Genesis and Development of a Scientific Fact*, trans. Thaddeus J. Trenn (Chicago: Univ. Chicago Press, 1979). For work reflecting the influence of Foucault see Stephen J. Cross, "John Hunter, the Animal Oeconomy, and Late Eighteenth-Century Physiological Discourse," *Studies in History of Biology*, 1981, *5*:1–110 (cf. Michel Foucault, *The Birth of the Clinic: An Archaeology of Medical Perception*, trans. A. M. Sheridan Smith [New York: Pantheon, 1973]); Simon Schaffer, "Herschel in Bedlam: Natural History and Stellar Astronomy," *Brit. J. Hist. Sci.*, 1980, *13*:211–239 (cf. Foucault, *The Order of Things: An Archaeology of the Human Sciences* [London: Tavistock, 1970]); and Schaffer, "Astronomers Mark Time: Discipline and the Personal Equation," *Science in Context*, 1988, *2*:115–145 (cf. Foucault, *Discipline and Punish: The Birth of the Prison*, trans. Alan Sheridan [New York: Pantheon, 1977]).

[24] For work from Marxist perspectives see David F. Noble, *America by Design: Science, Technology, and the Rise of Corporate Capitalism* (New York: Knopf, 1977); Noble, *Forces of Production: A Social History of Industrial Automation* (New York: Knopf, 1984); and Kaushik Sunder Rajan, *Biocapital: The Constitution of Post-Genomic Life* (Durham, N.C.: Duke Univ. Press, 2006). For examples of feminist work on science see Evelyn Fox Keller, *Reflections on Gender and Science* (New Haven, Conn.: Yale Univ. Press, 1985); Anne Fausto-Sterling, *Myths of Gender: Biological Theories about Women and Men* (New York: Basic, 1985); Sandra G. Harding, *The Science Question in Feminism* (Ithaca, N.Y.: Cornell Univ. Press, 1986); Londa Schiebinger, *The Mind Has No Sex? Women in the Origins of Modern Science* (Cambridge, Mass.: Harvard Univ. Press,

The particular intercontinental flow of scholarship that began with the formation of the 4S, and was further consolidated with the migration of European or European-trained scholars (Latour, Pinch, Rudwick, and Shapin, among others) to the United States, coincided with an important amalgamation of perspectives. No longer could American STS ignore the opening of epistemic, and later technological, black boxes, a process that had been under way in Europe for decades. At the same time, European science studies was necessarily drawn into closer conversation with U.S. scholarship that focused more intensively on the political backdrop of scientific and technical controversies—state institutions, economic interests, social movements, inclusion and exclusion of social groups, policy agendas, and the law.[25]

The 1980s witnessed, therefore, an efflorescence of activity in the history of science and technology and in the field of science, technology, and society. By the start of the 1990s, STS in America was increasingly, and somewhat confusingly, also called "science and technology studies"—another "STS" distinguished from the first by a concern, often historical, with the nature of scientific knowledge itself and a methodological focus on studies of scientific practice. In the case of the new department formed at Cornell in 1991, use of the acronym "S&TS" was an attempt, perhaps quixotic and prompted by institutional particularities, to resolve this confusion.[26] But there were by the turn of the century a good many academic enterprises aimed at understanding science and technology through their everyday workings, both internally and in relation to other social actors and contexts. While programs and departments, along with professional societies and journals, increasingly abounded, the question of how many of these various enterprises—if any at all—represented "disciplines" remained cheerfully open ended. What was clear, though, was the futility of pigeonholing any of the major figures writing on science and technology into boxes exclusively labeled with traditional disciplinary markers.

DISCIPLINES AND ACADEMIC TRAINING

The babel of voices making up the study of science and technology evokes in the well-trained scholar a desire to classify and order. Historians of science are sufficiently self-conscious as to recognize "history of science" as a category of its own, largely owing to the possession of a historiographical tradition, a specialist society, and—not trivially—a number of well-established centers of graduate training commanding earmarked university resources. But what is at stake in dubbing the history of science, or an associated domain called "science and technology studies," a "discipline"? And what might be at stake in denying that designation? A brief history of discipline formation suggests some answers.

1989); and Noble, *A World without Women: The Christian Clerical Culture of Western Science* (New York: Knopf, 1992). Work that looks at the political aspects of science and technology from standpoints in democratic theory includes Langdon Winner, *The Whale and the Reactor: A Search for Limits in an Age of High Technology* (Chicago: Univ. Chicago Press, 1986); Yaron Ezrahi, *The Descent of Icarus: Science and the Transformation of Contemporary Democracy* (Cambridge, Mass.: Harvard Univ. Press, 1990); and Philip Kitcher, *Science, Truth, and Democracy* (New York: Oxford Univ. Press, 2001).

[25] Dorothy Nelkin, a prominent figure in American STS in the 1980s and beyond, pioneered a genre of controversy studies that did not probe scientific claims or practices but instead situated controversies within their specific social contexts. See, e.g., Dorothy Nelkin, *Controversy: Politics of Technical Decisions*, 3rd ed. (London/Beverly Hills, Calif.: Sage, 1979).

[26] Cornell was one of the few major research universities to have initiated programs in both HPS and STS (science, technology, and society). The new rubric S&TS was meant to signal that the department was not simply engaged in the macrosocial and external analysis of science and technology.

Modern academic disciplines are usually, and convincingly, traced to the reformed Faculties of Arts at nineteenth-century German universities. Inevitably, these sociocognitive structures acquired an aura of self-evidence, as the "natural" way to divide up scholarly subject matter. Subsequent reactions against such disciplinary divisions have long taken the form not of outright rejection of the reified disciplines but, instead, either of their multiplication ("new" disciplines) or, particularly in recent decades, of their combination or hybridization ("interdisciplinarity" or "multidisciplinarity").[27] The curricular and bureaucratic convenience of thus maintaining "traditional" disciplines as an unchanging discursive core while simultaneously deploying rhetorics and practices of innovation has evidently proved attractive. For those in the social sciences, advantages lay in the classic perks of disciplinary identity: space (always at a premium); resources for laboratories, books, and other supports; the right to admit and train graduate students; and, particularly in U.S. liberal arts universities, access to the currency of undergraduate teaching, which in turn buys faculty positions and funding for graduate students. Perhaps, however, if one were starting from scratch today in designing an institutional envelope for studies of science and technology, one might prefer a single-payer approach in which the history of science is seen as an essential component of, not a competitor with, other fruitful directions in science studies.

Disciplines surely serve important functions beyond acting as pipelines for resources and recognition. Much as Kuhn stressed the value of a paradigm's narrowness for enabling depth of research by maximizing those things that everyone agrees upon, so an effective academic discipline can create profound and specialized knowledge, with deeply considered and enforceable criteria of validity.[28] Kaiser and Warwick, drawing, as noted, on Kuhn and Foucault (one might also add Pierre Bourdieu), rightly argue that for the sciences themselves the pedagogical component of a discipline is crucial. This is not because everything that scholars or scientists do once their formal training is complete follows rigidly from an initial "programming"—although there are often close stylistic parallels between subsequent professional research and prior training—but because pedagogical activity continues to play a significant role throughout most research scholars' and scientists' careers: pedagogy is not a one-way street.[29] In this sense, the pedagogical features of a field, both formal and informal, can serve as a convenient window onto its (temporary) coherence as a domain of intellectual, social, and material practices.

Perhaps such domains of authorized pedagogy are what talk of disciplines really means to designate. If so, then disciplines cannot and should not be identified by mere reference to traditional categories that once fulfilled that definition but may no longer comfortably do so. For disciplines change, with regard to both their self-understood missions and their methods and cultures of social reproduction. Indeed, such a view appears to inform

[27] One thinks of Warren Weaver in the 1930s: Robert E. Kohler, *Partners in Science: Foundations and Natural Scientists, 1900–1945* (Chicago: Univ. Chicago Press, 1991). Among much relevant literature tracing modern academic disciplines to German Faculties of Arts see Rudolf Stichweh, *Zur Entstehung des modernen Systems wissenschaftlicher Disziplinen: Physik in Deutschland, 1740–1890* (Frankfurt am Main: Suhrkamp, 1984). See also Stichweh, "The Sociology of Scientific Disciplines: On the Genesis and Stability of the Disciplinary Structure of Modern Science," *Sci. Context*, 1992, *5*:3–15; and William Clark, *Academic Charisma and the Origins of the Research University* (Chicago: Univ. Chicago Press, 2006). Joseph Ben-David, *The Scientist's Role in Society: A Comparative Study*, 2nd ed. (Chicago: Univ. Chicago Press, 1984), Ch. 7, is a now-standard overview for the sciences.

[28] Kuhn, *Structure of Scientific Revolutions* (cit. n. 6), p. 164; cf. Shapin, "Discipline and Bounding" (cit. n. 15).

[29] See esp. Warwick, *Masters of Theory* (cit. n. 13); see also Suman Seth, *Crafting the Quantum: Arnold Sommerfeld and the Practice of Theory, 1890–1926* (Cambridge, Mass.: MIT Press, 2010).

Daston's classification of scholarly studies concerning science and technology when she represents the history of science as having finally achieved an adoptive disciplinary status by supposedly submitting to the hegemony of "history."[30] Yet it would be quite easy to argue that "history," for all its firm instantiation in university departments and monolithic professional groups like the American Historical Association, is in reality a quite motley collection of scholarly specialties, distinguished by such considerations as period, geographical region, topic, and methodological approach. By comparison, as we will suggest in the next section, a generic field of science and technology studies, with the history of science and technology at its center, ought by now to be seen as potentially more coherent than the supposedly more disciplined field of history.

Many historians of science, and not only early modernists, have turned toward cultural history over the last two decades, as Daston rightly suggests. This is not to say, however, that they have apprenticed themselves to those cultural historians who live in history departments—any more than cultural historians had previously simply deferred to cultural anthropologists. Daston's assertion that "science studies" (by which she evidently means the agenda of SSK and of empirical laboratory studies, as distinct from "science, technology, and society") "transformed" the history of science in the 1980s may appear in retrospect to describe what happened, but it was not generally felt to be so among historians of science in the United States at the time (the influential books cited in Daston's note 11 all date from that decade, and all came from the United Kingdom).[31] The only "transformation" evident by around 1990 involved a minority of historians—those who had taken SSK seriously—pressing and being pressed by a majority who distrusted, even repudiated, it as overly theoretical and insufficiently historical.

When Daston quotes Charles Rosenberg's 1988 question as to "whether the history of science is a coherent discipline or just a collection of scholars aggregated by the accidents of history and the accretion of a common historiography," she does so to suggest that, since that time, "historians of science have become disciplined, and the discipline to which they have submitted themselves is history [meaning here cultural history]."[32] Such a claim appears both to dismiss the generation of scholars in the 1950s who founded a professional field of the history of science, together with university departments and programs to instantiate it, as well as to consign (on Daston's own account) the period of *rapprochement* with science studies in the 1980s to the field's disciplinary prehistory.

In fact, the use of cultural history as a model for professional historical studies *tout court* tends to gloss over not only varieties of style in historical scholarship but also important issues concerning the relationship between aspects of the past and the scholar's interested position in the here and now. Historians sometimes like to allude to L. P. Hartley's famous line about the past being a foreign country, and the sentiment conjures up a view of the historical past as a repository of anthropologically relativized domains

[30] Daston, "Science Studies and the History of Science" (cit. n. 1), pp. 808–809.

[31] *Ibid.*, pp. 808–809, 803. Thus Arnold Thackray, ed., *Constructing Knowledge in the History of Science* (*Osiris*, 1995, N.S., *10*), already shows considerably more awareness of SSK and related work than was the case at the History of Science Society conference in 1991 from which it derived (the unexplicated "constructivist" allusion in the title was also new). But it is striking that, as late as 2000, so signal a book as Shapin and Schaffer's 1985 *Leviathan and the Air-Pump* (cit. n. 6), and much historical work by, especially, Shapin on the same period, still had little or no evident impact on the contributions to Margaret J. Osler, ed., *Rethinking the Scientific Revolution* (Cambridge: Cambridge Univ. Press, 2000). The situation was markedly different in the United Kingdom, where a lot of venturesome historical work was brewing in the 1980s and later, especially at Cambridge.

[32] Daston, "Science Studies and the History of Science," p. 808.

having no more to do with "us" than E. E. Evans-Pritchard's famous Azande oracle. Indeed, thoroughgoing historicism (in its usual contemporary sense) suggests that differences in basic categories of understanding and action render people living in past worlds wholly other than ourselves, not to be explicated in our anachronistic terms. They must be understood, we say, "in their own terms," as early modern courtiers or natural philosophers or Victorian "men of science," rather than as modern scientists. All this is well and good, and it has become very familiar in the history of science.[33] But fears of anachronism, or of loosely defined "whiggishness," while crucial to creating sensitive and insightful historical studies, cannot and do not adequately define what historians of science do. There remains the issue of what kinds of questions, originating from what foundations and subject to what social or material constraints, drive historians of science in general, and here there is much, if not complete, overlap with drives within contemporary STS.

The issue of the relationship between the historian's present and history's past was intelligently discussed over twenty years ago by Adrian Wilson and Trevor Ashplant, who dubbed the cardinal sin "present-centredness": the inappropriate use of present-day conceptual categories in making sense of the past. Since then, others have written of the fact that it is strictly impossible, and often undesirable, to avoid using our own categories in understanding history. Some form of translation is always needed; we have our own things to say about what came before us.[34] The anthropological strangeness of the various pasts produced by the practices of cultural history is an immensely valuable intellectual therapy, especially in the study of the pasts of science. But history is also about understanding the continuities and processes of change that connect the past to the present. After all, we look to the past chiefly to answer questions and address concerns that arise in the present.

Quentin Skinner, in influential work on the conduct of intellectual history, addressed precisely this problem in a famous article of 1969. While his advocacy of a "speech act" approach to the history of ideas was widely noticed (and helped give rise to Steven Shapin's use in 1980 of the term "contextualism" for the history of science), another aspect of Skinner's discussion has been less remarked: his acknowledgment that "we are . . . committed to accepting *some* criteria and rules of usage such that certain performances can be correctly instanced, and others excluded, as examples of a given activity. Otherwise we should eventually have no means—let alone justification—for delineating and speaking, say, of the histories of ethical or political thinking as being histories of recognizable activities at all."[35] For the student of science and technology, Skinner's observation nicely captures the

[33] See Peter Dear, "Cultural History of Science: An Overview with Reflections," *Science, Technology, and Human Values*, 1995, *20*:150–170; and Dominique Pestre, "Pour une histoire sociale et culturelle des sciences: Nouvelles définitions, nouveaux objets, nouvelles pratiques," *Annales*, 1995, *50*:487–522. In the present context, it is ironic that much of the literature surveyed in these articles was greatly indebted to SSK.

[34] Adrian Wilson and Trevor Ashplant, "Whig History and Present-Centred History," *Historical Journal*, 1988, *31*:1–16; and Ashplant and Wilson, "Present-Centred History and the Problem of Historical Knowledge," *ibid.*, pp. 253–274. On the impossibility of avoiding our own categories in understanding history see, among others, Nick Jardine, "Uses and Abuses of Anachronism in the History of the Sciences," *Hist. Sci.*, 2000, *38*:251–270; and Jardine, "Etics and Emics (Not to Mention Anemics and Emetics) in the History of the Sciences," *ibid.*, 2004, *42*:261–278. For extensive references on "presentism" see Oscar Moro-Abadia, "Thinking about 'Presentism' from a Historian's Perspective: Herbert Butterfield and Hélène Metzger," *ibid.*, 2009, *47*:57–77.

[35] Quentin Skinner, "Meaning and Understanding in the History of Ideas," *History and Theory*, 1969, *8*:3–53, on p. 6 (a revised version of this essay appears in Skinner, *Visions of Politics*, 3 vols. [Cambridge: Cambridge Univ. Press, 2002], Vol. 1, pp. 57–89); and Steven Shapin, "Social Uses of Science," in *The Ferment of Knowledge: Studies in the Historiography of Eighteenth-Century Science*, ed. George S. Rousseau and Roy Porter (Cambridge: Cambridge Univ. Press, 1980), pp. 93–139.

unstable knife edge on which one always teeters in asking "what science is," and was, and "how science works," and worked. Those considerations are simply inseparable if we are to speak of our subject meaningfully to others in our own present-day communities.

Scholarly disciplines, as these observations underscore, are not natural and eternal divisions of universal knowledge. As a consequence, the idea of *inter*disciplinarity that Daston attributes to STS itself loses any intellectually serious meaning. Its chief value appears to lie in its temporally delimited, bureaucratic convenience for academic administrators: promoting collaboration "between disciplines" is much easier than reconstituting departments, especially at times of intellectual ferment or financial constraint. But setting convenience aside, in the study of science and technology (as with the study of any subject matter), all analytical and methodological techniques, and empirical resources, ought in principle to be available. Work performed, and sources used, by self-styled historians, or anthropologists, or sociologists, or lawyers, or literary scholars—or scientists themselves—can always be purloined without apology or permission by scholars able to argue for their suitability. All "disciplines" are in this sense "interdisciplinary," unless they have frozen into dogmatic bodies of faith.

PAST AND PRESENT IN STS

We have already noted that object-centered fields of study—including most fields in the humanities and social sciences—rarely divide themselves internally along temporal lines. Literary studies, for instance, may separate themselves into language-based subfields, with the sometimes oddly positioned discipline of comparative literature trying to bridge the divisions, but no English department tries to split its coverage of the English literary tradition into past and present. Modernism may be taught by someone other than the professors who tackle Chaucer, the Romantic poets, or the Victorian novel, and there are tensions over the sorts of boundaries that should separate Americanists from students of English literature written in Britain or its former colonies, but no literary scholar seems to think that meaningful *disciplinary* lines can (or should) be drawn between, say, the nineteenth century and the twentieth or between literature of the post–Cold War era and the literature that went before it.

History as a scholarly specialty concerns itself with difference, change, and temporality rather than with some special quality of the past *qua* past. And in the academic landscape of the present day, such issues should be integral to any inquiry into human cultures and practices. Temporality as an epistemic marker between such inquiries makes little sense in itself, not least because everything we know about has already happened, whether yesterday, last year, or several centuries ago. In this sense all knowledge is historical knowledge. The passage of time and the marks it leaves on concept and practice are themselves part of what a social scientist or humanist investigates. After all, what makes the present "the present" is of concern to all who wish to understand humanity from any disciplinary standpoint, and to learn from the past is essential to the pursuit of that understanding.

STS embraces as its field of investigation knowledge and knowledge making, including the wider ramifications of producing various kinds of authoritative knowledge (science writ large), embodying them in objects and material systems (artifacts, instruments, and industries), and seeing how the resulting "things," epistemic and otherwise, play their parts in such activities as law, policy, politics, social organization, religion, aesthetic

culture, the economy, and ethics.[36] Within this expansive domain, key problems include emergence (and *non*emergence), stability, contestation, and disappearance—all dynamic processes, with the passage of time built in. If Latour is right in proclaiming the dust-to-dust end of a certain kind of skeptical science studies, then that loss of status is itself a phenomenon we should try to understand historically, even if the supposed rise and fall of that tradition is almost entirely encapsulated within the past thirty years. But if, as would be our contention, Latour's declaration is based on an overly narrow and particular construction of critical practices in and around science studies, then that claim, too, deserves investigation with all the tools we have for explicating the uses of knowledge for social and political purposes, including the active making and unmaking of scholarly disciplines. In short, even what we say or enact about our own relatively new field(s) is at one and the same time epistemic, social, and temporally situated. It belongs at once to science studies and to history.

But let us look outward, away from our own disciplinary configurations, to a broader picture of what STS scholars study. History, we would argue, is always already part of the STS project in three important ways: the objects of inquiry (if they are worth studying at all) have "historical ontologies";[37] as phenomena in time, they engage with wider problems of historical understanding; and studying them, whether in the past or in the present, often involves methods of primary research and contextual reconstruction that are part of any disciplined historian's methodological toolkit.

A case may do more to clarify the argument than any attempt at negotiating abstract boundaries: regulatory science, the kind of science that regulatory agencies rely on in seeking to protect human health, safety, or the environment, as well as to serve social goals such as nondiscrimination or environmental justice. Regulatory science carries attributes of practice and reception that distinguish it in interesting ways from day-to-day scientific practice at the laboratory bench. Unlike most "normal science," regulatory science may be conducted in the glare of public scrutiny (as in the case of climate assessment reports produced by the Intergovernmental Panel on Climate Change);[38] it may be deliberated in the U.S. Congress, reviewed by the White House, or challenged in the Supreme Court if it threatens powerful economic interests; it may attract the attention of investors and venture capitalists (as with clinical trials for blockbuster drugs), of investigative journalists (sniffing out fraud, for instance), or of lawyers representing plaintiffs in malpractice or product-liability lawsuits. These features all ensure that regulatory science takes place in a political space. Inevitably, its analysts must also be students of politics.

At the same time, like any other area of scientific activity, regulatory science brims with

[36] Peter Dear, "Science Studies as Epistemography," in *The One Culture? A Conversation about Science*, ed. Jay A. Labinger and Harry Collins (Chicago: Univ. Chicago Press, 2001), pp. 128–141.

[37] The term is from Ian Hacking, *Historical Ontology* (Cambridge, Mass.: Harvard Univ. Press, 2002). For examples concerning material objects see Daston, ed., *Biographies of Scientific Objects* (cit. n. 8); and Lorraine Daston, ed., *Things That Talk: Object Lessons from Art and Science* (New York: Zone, 2004).

[38] In late 2009, when this essay was being written, climate science was engulfed in an episode promptly dubbed "Climategate," in analogy to the Watergate scandal, that was triggered by the hacked disclosure of numerous e-mails at the University of East Anglia, a leading center for U.K. climate research. The e-mails illustrated the human dynamics of scientific controversy and consensus that STS scholars have so frequently documented. Yet the public display of scientists showing "interests" ran sufficiently counter to the still-dominant Mertonian understanding (or ideology) of science as a detached, disinterested activity that many commentators and observers were appalled—or else found it in their interest to appear to be so. For brief accounts see Andrew C. Revkin, "Hacked E-Mail Is New Fodder for Climate Dispute," *New York Times*, 20 Nov. 2009; and Stephen J. Dubner, "ClimateGate: The Very Ugly Side of Climate Science," *ibid.*, 23 Nov. 2009.

scientific experts, knowledge claims, (inter)disciplinary controversies, negotiations and closures, proofs and demonstrations, and findings that carry weight. Regulatory science also gives rise to its own materializations, in the forms of models, test systems, measuring and monitoring devices, and the like. The records of regulatory science, moreover, are legion: massive agency dockets, published and unpublished scientific studies, advisory committee proceedings, newspaper reports, and possibly legal depositions, briefs, and judicial opinions. Making sense of such ideas, technologies, and documentary traces is part of any STS scholar's methodological training, whether or not those objects are "historical." Though regulatory science by that name is a product of the very recent past, to study it productively requires minds steeped in both historical and sociopolitical sensibilities.[39] It makes little sense to draw our disciplinary boundaries, and consequent pedagogical strategies, with constraints that prevent such minds from reaching disciplined maturity.

The student of science and technology, no matter what the topical and temporal focus, always requires an awareness of knowledge making and application as social activities, concerned with standards and criteria of excellence, embedded in an economy of credibility, both using and enabling new forms of materiality, and, most crucially, participating in an ongoing historical process. Our aim as students of science and technology should be to foster a more inclusive professional culture that respects well-tested modes of inquiry, and rejects ill-conceived ones, from any corner of our field. From that standpoint, the apparent plea to return to a more intellectualist analytical mode found in the last sentence of Daston's article in *Critical Inquiry* ("Philosophy, anyone?") marks a sad retreat.[40] It denies some fifty years of precisely the sort of synthetic vision of what science is and how it works that Daston advocates earlier in her final paragraph and that STS scholars of all methodological inclinations have been energetically pursuing for many years. Invoking tendentious disciplinary distinctions to exclude any of those concerns from a purified "discipline" does no one any good—neither the cause of scholarship nor the wider public goods of information and criticism that universities aim to serve.

[39] Sheila Jasanoff, *The Fifth Branch: Science Advisers as Policymakers* (Cambridge, Mass.: Harvard Univ. Press, 1990).

[40] Daston, "Science Studies and the History of Science" (cit. n. 1), p. 813.