CHAPTER ONE

# Historiography of the History of Science

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Over the past 35 years or so, the subject matter, people, places, and processes associated with history of science have grown vastly. Exaggerating only slightly for effect, an older predominant history of science might be captured by the image of a tree of scientific ideas rooted in the base of Western culture (perhaps extending downward earlier to ancient Egypt and Babylonia); the task of the historian of science was to trace the tree's growth and branching. Today a more fitting image would be of the history of science as a densely tangled bank of people and material things teeming with social, cultural, economic, and religious life, that covers the globe. The historian's task now is to tease out how certain forms of knowledge and practice within this mass of activity came to be understood as "science;" what has sustained science socially, culturally, and materially; and who has benefitted and who has suffered in its formation. What happened in the past did not change: what we expect professional historians of science to know and care about has.

The four parts of this volume—Roles, Places and Spaces, Communication, and Tools of Science—reflect broad analytical categories central to today's history of science. They cut across historical periods, geographical locations, and sciences to provide a common vocabulary that helps tie our far-flung history together. Rather than reproduce these categories in the present essay, I sketch out some of the historiographic trends that made it possible—even commonsensical—to use them to thematize contemporary history of science scholarship written in English.

I focus first on the social constructionist turn of the late 1970s and early 1980s, and its consequences for how we think about the nature of scientific knowledge and who is involved in its making. I then turn to the subsequent (re-)formulation of approaches to answering two fundamental questions in our field. One focuses on *making* scientific knowledge, asking "How is scientific knowledge constructed in a given context?" Historians' answers to this question since the early 1990s have become increasingly attentive to scientific practice, its settings and material culture. A second question focuses on *moving* scientific knowledge. As James Secord (2004, 655) put it, "How

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and why does [scientific] knowledge circulate? How does it cease to be the exclusive property of a single individual or group and become part of the taken-for-granted understanding of much wider groups of people?" Scholars working on this question have highlighted the tropes of communication and circulation, and indeed often question the very distinction between making and moving.

Recent history of science has been profoundly shaped by its historians' interactions with scholars from other disciplines across and between the social sciences and humanities. In these exchanges, historians of science have both given and received, but they have often shied away from direct theoretical statements in favor of a more empiricist style that integrates analytical insights into narrative structures. Within the broad themes of this essay, I highlight works that articulate or exemplify analytical approaches and conceptual tools that might be applicable to different places and periods. While these often originate from individual authors, I have been particularly struck by the importance of thematic journal issues and that most maligned of genres, the multi-authored edited volume. Thematic volumes are notoriously hard to get published, yet they can raise the visibility of an approach or topic well above the level of the individual article or even book, and give a sense for the significant conversations in which our community participates. The liveliness of these conversations is evidenced by the large number of collective works cited in the present essay—and also, of course, by this volume, which as a whole attests to the community-based nature of the history we make.

## Constructing Scientific Knowledge, Socially

Since the late 1970s, historians of science have gradually come to accept a predominantly social constructionist account that views the development of scientific knowledge as depending heavily on particulars of local circumstances, people, epistemes, and politics, and that doesn't necessarily drive ever closer toward a single truth. Although historians of science had long been interested in recovering earlier knowledge systems and the means by which they were transformed over time (e.g. Kuhn 2012), social constructionism offered new tools for doing so. The sociologists of the "Edinburgh School" and the "Bath School" developed many of these tools in the 1970s and early 1980s; despite differences in approach, they broadly articulated what was known as the "Strong Programme" of the social construction of scientific knowledge. (For retrospective analyses of the early situation, see Golinski 2005; Shapin and Schaffer 2011; Kim 2014; Soler et al. 2014).

The new sociologists of scientific knowledge participated in a broader postmodern rejection of our unmediated access to reality, often associated with other critiques of science's truth value. Michel Foucault (especially 1970, 1973) challenged historians to understand how the structures of knowledge, discourse, and institutions instantiated forms of power (the entire bundle called "epistemes") that were virtually invisible to those living inside their regimes. Since he offered no clues as to how one episteme turned into another, and little in the way of specific empirical evidence for his provocative claims, Foucault's work remained largely (if importantly) inspirational. From a different direction, feminist scientists would soon expand the purview of social constructionist criticism of science (Bleier 1984; Fausto-Sterling 1992). Uneasy with both the implications of radical social constructionism and the "all-seeing" stance represented in standard claims to objectivity, however, Sandra Harding (1986) and Donna Haraway (1988) developed, respectively, the crucial ideas of standpoint epistemology and "situated knowledges." Haraway (1988, 590) in particular advocated the "partial perspective," which lent the authority of agency to individuals previously without standing and demanded communal effort to arrive at shared reliable knowledge.

Such perspectives collectively challenged the received view of history of science in two fundamental ways. First, they demonstrated that scientific knowledge was *constructed* by human beings, not discovered in nature. Second, this process was not the work of individual minds but was ineluctably *social.* The implications for history were profound.

If knowledge of nature is made, not arrived at, then we should not expect that science will progress toward a pre-existing universal truth. One important implication is that the truth value of a claim in the past cannot be assessed by what we now believe to be true—an account of the success or failure of a scientific claim must be neutral with respect to that outcome. Evaluations of success must depend on other grounds—social, political, rhetorical—and both successes and failures must be treated similarly. In the 1980s cutting-edge historians of science adopted these principles of "neutrality" and "symmetry" (Bloor 1976), taking up the challenge of treating the outcomes of scientific controversies as determined not by the truth winning, but by social interactions.

The paradigmatic example of this sociological-historical approach is Steven Shapin and Simon Schaffer's *Leviathan and the Air-Pump* (1985). They interpreted the contest between Robert Boyle and Thomas Hobbes as not just over the existence and nature of the vacuum and its experimental proof, but over what sort of knowledge would be counted as scientific (or, more properly, "natural philosophical"), and what adjudged not. The very division between "science" and "non-science" was at stake, and the winner not only won the specific controversy but also the right to claim what kind of knowledge would be constituted as authoritative (experimental knowledge), who would be considered a natural philosopher in the future (Robert Boyle), and who would not (Thomas Hobbes).

Developing the commitment to neutrality with respect to the outcome of a controversy led Martin Rudwick to take a different tack. His *Great Devonian Controversy* (1985) experimented with a radically anti-teleological narrative of controversy, persuasion, and power that steadfastly resisted letting the reader know how this geological story came out until its end. It thereby called attention to the conventions of histories that anticipate the outcome, challenging readers to problematize the very structure of historical narrative and to recognize the contingency of the development of science.

Both books also forcefully showed the extent to which the construction of scientific knowledge was *social*, in the sense of involving many people (see also Smith 1998 on the collective "discovery" of the conservation of energy). The diversity of kinds of people included in this social reckoning has only expanded over time. If Michael Ruse was innovatively broad, in his 1979 *Darwinian Revolution: Science Red in Tooth and Claw*, for including over a dozen British male natural philosophers as the relevant community that helped to make the revolution in Darwin's name, its scope seems narrow today, when we see that revolution as preceding Darwin in many of its features (Desmond 1992; Secord 2000) and extending far into nineteenth-century British and European culture (e.g. Beer 1983; Glick and Engels 2008)—and indeed cultures worldwide (Pusey 1983; Elshakry 2013).

The key second claim of social constructionism, then, was that the development of science involved many people, doing many different kinds of things. As microsociological laboratory studies demonstrated the centrality of postdocs, graduate students, and technicians to making knowledge (Latour and Woolgar 1979), historians wondered, Who were the "invisible technicians" of the past (Shapin 1989; Hentschel 2007)? How were the social relations of knowledge production managed, and how did these change over time?

Feminist scholars observed that European women were in fact also involved in many aspects of making knowledge about nature, though only exceptionally afforded opportunities to "do science" in ways we easily recognize (Schiebinger 1989; Findlen 1993; Terrall 1995). Women participated in science as patrons and *salonnières*, as illustrators, as teachers of children, as popular writers (Shteir 1996), and as partners working with their scientific husbands (Pycior, Slack, and Abir-Am 1996) long before "careers" in science were generally available to women. As historians looked beyond European laboratories and the social structures that surrounded and sustained them, they found not only women but also men who helped make science in the field in these and other ways as well—as servants, collectors, and taxidermists; as translators, providers of local or indigenous knowledge, and other sorts of go-betweens; and as experimental subjects. (See Part I, "Roles," in this volume.) The peoplescape of contributors to science has grown accordingly.

As the kinds of people recognized as involved with science have diversified, the notion of the "scientist" itself has undergone new scrutiny, most prominently with the development of the idea of scientific *personae* (Daston and Sibum 2003). This concept simultaneously offers a theorized way to differentiate among kinds of scientists, describe certain collective patterns of scientific behavior, and offer an intermediate level of analysis between the individual and the institution. The "scientist as expert" has spawned a distinctive specialist literature as well (Lucier 2008; Broman 2012; Klein 2012). To be sure, more traditional biography has hardly disappeared from the history of science—indeed, four of the eleven winners of the History of Science Society's Pfizer Prize for best scholarly book between 2003 and 2013 were biographies (Terrall 2002; Browne 2003; Antognazza 2009; Schäfer 2013). Historians have also been inspired to revisit how scientific biographies themselves are constructed—by scientists (Otis 2007), by admirers (Rupke 2005), and by historians (Söderqvist 2007).

## Doing Scientific Things with Scientific Things: Practice and Materiality

Historians of science today do not write only about scientists and others producing and supporting science. They write about the *stuff* of science: about glassware, computers, fruit flies, oceans, books, diagrams, maps, models, and particle accelerators. They write about theory, too—but their goal is less often to elucidate how scientists derived their theories than to present a broader historical web of scientific and cultural practices that in turn are solidly embedded in the physical world. This rich material tapestry has been woven together from diverse strands: the social-constructionism-inspired turn to experimental practice; the formerly distinct scientific instrument tradition; attention to natural history collections and fieldwork; and interdisciplinary studies of material culture.[1]

The central feature, which gained heft from the social constructionism of the 1980s, has been the turn toward practice (Soler et al. 2014). Literary postmodernists of the period might declare with Derrida that all thought is discourse, and thus all products of thought were forms of text, amenable to deconstruction. Not so analysts of science. Shapin and Schaffer (1985, 25), for instance, bent far backward to call written arguments "literary technologies," which along with material and social technologies established scientifically legitimate "matters of fact" in the Scientific Revolution. To them, seeing science as constructed meant focusing attention on the physical, material means of that construction. Since the 1980s, broader trends have helped to keep historians' attention on the materiality of science. The digitization and virtualization of our academic and social world has wrought renewed appreciation for physical things, while at the same time, ever-increasing awareness of our dependence on a rapidly degrading nature has lent new urgency to that appreciation. We can no longer afford to attend primarily to theory.

Attention to materiality is not new to the history of science. An older Marxist tradition insisted on the central role of material and economic needs in shaping science (Bernal 1971). Separately, a long tradition studied historical scientific instruments; with its valuation of object-connoisseurship connected to art history and museum work, this was often treated as a sideline in the field. Then in the mid-1990s, scholars of material culture—mostly working in museums—made new claims for their importance to the study of history of science and technology (Lubar and Kingery 1993; Kingery 1996). Combined with the history of science's new focus on practice, this helped push instruments and other materials toward the center of the field (van Helden and Hankins 1994).

Analyses of the material nature of scientific practice have looked different as they intervened in different historical subspecialties. In early modern studies, for instance, such analyses have carried forward the theme of the "scholar–craftsman" union (Zilsel et al. 2000; Roberts, Schaffer, and Dear 2007; Long 2011); a similar concern with the relationship between abstract knowledge and craft knowhow has animated recent work on ancient and non-Western understandings of nature (e.g. Robson 2008; Schäfer 2011). In the history of modern physics, the study of experimental practice challenged the historiographic dominance of theoretical physics. As Peter Galison (1997) has argued, developments in theoretical and experimental physics have not been yoked together; tracing the history of experimental physics, its instruments and material practices, yields new historical narratives that change our picture of "physics"—even challenging its unity as a science.

In the history of twentieth-century experimental life sciences, attention to practice and material culture led to new ways of thinking about the unique tools for investigating living processes (Clarke and Fujimura 1992). Robert Kohler's iconic *Lords of the Fly* (1994) analyzed the Morgan school of *Drosophila* geneticists, showing how the organisms themselves began to drive the systems of investigation (and indeed, the entire "moral economy" of the school) and analyzing how the scientists responded. Subsequent scholarship further refined analyses of knowledge-making systems involving people, model organisms and organic materials, and experimental set-ups in the life sciences (e.g., Rheinberger 1997; Creager 2002; Landecker 2007).

Historical studies of experimental practice, then, have shared a focus on the use of instruments and experimental systems that extend our senses and manipulate nature to tease out its processes, their underlying structures, and, ultimately, their laws. Historians of natural history have attended to quite different aspects of material practice, including not only the life and work of scientists in "the field" (Kuklick and Kohler 1996; Vetter 2011) as they searched for natural objects and materials, but also the practices of collection and preservation, and the organization of specimens into ordered collections (Heesen and Spary 2002; Endersby 2008; Johnson 2012). Here, the history of science has intersected with the history of museums and collections, and with the broader material culture perspective that museums have promulgated (Nyhart 2009; Alberti 2011; Poliquin 2012).

Such approaches have drawn attention to the spatial dimensions of scientific practice—another aspect of its materiality closely intertwined with social organization (Finnegan 2008). Modern scientific activity typically takes place in recognized kinds of venues: observatories, laboratories, museums, and "the field" are perhaps the four most prominent categories (see Part II, "Places and Spaces," this volume). Each of these has evolved over time and developed characteristic forms of social organization and practices, though historians have repeatedly noted how permeable and variable these sites are (e.g., Gooday 2008). This focus may be understood as part of a broader interdisciplinary "spatial turn" visible recently across the humanities and social sciences (e.g., Warf and Arias 2008). Geographers have offered taxonomies of scientific spaces and places that draw useful distinctions (such as that between particular locations in the world—Brazil, say—and kinds of places—such as "the tropics"), and have called attention to important differences in the scales at which spatial analysis of science may be undertaken (see esp. Livingstone and Withers 2011). Spatial and geographical language—referring to actual places, kinds of places, and metaphors of place and mapping—now provides a prominent vocabulary and mode of analysis among historians of science.

## Moving Knowledge Around: Communication and Circulation

A long-accepted tenet of the social constructionist history of science is that scientific knowledge begins locally. If this is the case, then how does it spread? Over the last three decades historians have pursued this fundamental question in many directions, and the analysis of the ways in which people, ideas, and artifacts travel and communicate to move science around has yielded an especially rich set of intellectual tools.

The communicative practices within and surrounding science are central to its spread, and writing is the practice historians have studied longest and most deeply. For decades, if not centuries, historians of science have analyzed texts. In the 1980s rhetoricians joined them to examine anew both the persuasive strategies of scientists and the forms of scientific publication, especially the scientific article (e.g., Bazerman 1988; Dear 1991; Gross, Harmon, and Reidy 2002). Unpublished (if not always private) forms have also received scrutiny, especially as they reflect the broader social structures in which they were embedded, such as the correspondence network or the archive (Hunter 1998; van Miert 2013).

Beyond its rhetorical dimensions, the historical study of science communication has been transformed by the dramatic expansion and increasingly sophisticated historiography of "popular science" (often conflated with "science popularization"). An older, diffusionist model tended to treat popular science as a watered-down version of "real" science, popularizers as lesser lights who lacked the chops to do their own research, and readers as a passive audience. This has given way to a perspective in which both writers for the general public and that public itself are treated as active cultural interpreters and knowledge-makers worthy of study (Cooter and Pumfrey 1994). James Secord (2000) has shown just how far one can take this approach, with his classic study *Victorian Sensation*, which treats Robert Chambers' 1844 *Vestiges of the Natural History of Creation* as a remarkably fluid text: he shows how its many editions developed in conversation with its critics, while also illuminating localized styles and cultures of reading. More recently, Topham (2009) has suggested considering science popularization more seriously as an actor's category, while Daum (2009) has proposed a broader historiographic transformation that would consider popular science as part of a larger notion of public knowledge.

Daum has rightly criticized the existing historiography of popular science for its parochial focus on nineteenth-century Britain—a trend reinforced by the large number of literary scholars of Victorian culture who have reached out to meet historians of popular science, especially (though not exclusively) via a mutual interest in the genre of the general periodical (e.g., Cantor et al. 1994; Cantor and Shuttleworth 2004; Lightman 2007). It is refreshing, therefore, to see innovative analyses of popular science being developed for new contexts such as the twentieth-century Soviet Union and China, where the relationships among public science, the state, and forms of identity have been both fraught and different from British-inflected Western assumptions (Andrews 2003; Schmalzer 2008; Fan 2012a).

Communication has a material history, too, explored powerfully through its print culture. Historians of science have come to view books, atlases and encyclopedias, journals, and popular magazines not just as vehicles of scientific information but also as objects whose physical attributes offer important historical clues to the authors, artists, engravers, printers, publishers, and patrons who contributed to making the printed scientific work (and thus further expand the cast of characters involved in producing science). The material object also provides clues to which sorts of readers might have had access to it and where, how they might have read it, and indeed the broader culture of reading of which the work was a part. As the technologies and economics of printing and publication have changed, so, too, have the associated cultures of print (Johns 1998; Secord 2000; Apple, Downey, and Vaughan 2012).

Historical analysis of scientific communication extends beyond the study of writing. The history of "non-verbal communication in science" (Mazzolini 1993) has become increasingly broad and varied, and its analyses now often combine with those of other forms of scientific communication, analyzed within the overlapping interdisciplinary fields of visual, print, and material culture of science (Fyfe and Lightman 2007; Hopwood, Schaffer, and Secord 2010; Jardine and Fay 2013; Messbarger 2013; Hopwood 2015; cf. Topper and Holloway 1980). These non-verbal aspects have even become fully integrated into topics once judged exclusively philosophical, as demonstrated by Daston and Galison's *Objectivity* (2007). As the present volume illustrates, the study of science's communicative practices also encompasses in-person forms of transmission such as lectures and demonstrations, distance media like radio and television, and a host of visual and material forms that often blur the already soft lines among the technical, the didactic, and the popular.

Although the distinction between "making" and "moving" knowledge has some utility, a considerable body of literature demonstrates its superficiality. Historians and anthropologists have long recognized that scientific knowledge changes when moving from one place to another; thus, moving knowledge means, at the very least, re-making it in some ways. Older rubrics for this process included *knowledge transfer*, *reception*, and (following an older sociological tradition) *diffusion* (Dolby 1977). All these earlier terms placed the primary agency on a source understood to be scientific, which is then differentially adopted by recipient cultures. It is now appreciated how inadequate this perspective is: there is always more knowledge-making going on at the "receiving" end.

The analysis of linguistic translation is an obvious way in to understanding problems of cultural translation and transfer, tracking what remains more or less the same and what is transformed when ideas are brought into new cultural environments. Such analyses challenge the longstanding assumption that scientific knowledge is merely transposed in linguistic translation, and not transformed at all (Elshakry 2008). The nitty-gritty details of translation indicate some of the cultural challenges. What was the German professor–translator H. G. Bronn, Europe's highest paleontological authority, to make of Darwin's pigeon breeds, with their impossible names, and Darwin's easy assumption that these would help win over his audience to evolution (Gliboff 2008)? How much more was transformed beyond language in the centuries-long projects of translating Greek texts into Arabic (and commenting on them), which produced new documents that themselves served as the sources subsequently translated into Latin in medieval Europe and the Mediterranean! While later cast as the "rediscovery" of an ancient Classical heritage that was merely routed through the ancient Near East, scholars have shown how misleading this story is—how it ignores the power, autonomy, and creative contributions by the many cultures of western Asia and the Near East to what we call "science," and the many transformations accompanying translation (Montgomery 2000; Iqbal 2012). Textual translation was further complicated when the writing systems, visual culture, and technologies of text production differed (Fu 2012).

The complex relationship between moving and making scientific knowledge goes beyond the alterations undergone in transit. Analysts of science have argued in different ways that the movement of knowledge itself has been essential to making it scientific. One argument, focused especially on laboratory knowledge, goes roughly like this: for something to be true, it must be true in more than one place; hence the importance of replicating results. Drawing on this logic, historians and sociologists have examined how scientists have worked to recreate "the same" conditions and techniques in different places in order to render the laboratory a "placeless place" in which scientists might successfully replicate results and thus create empirically based assent (Gieryn 2002; Kohler 2002 and sources cited therein). Here, science is simultaneously made and moved by homogenizing and spreading its techniques and environments.

Another perspective has focused on how certain kinds of objects and information—in the sociologist Bruno Latour's (1987) term, "immutable and combinable mobiles"—have been extracted from "elsewhere" and brought together in specific "centers of calculation." At these centers—typically Western, metropolitan, and more powerful than the diffuse locations from which the objects come—scientists do the work that would yield the knowledge called "scientific." Such historical attention to the forging of scientific knowledge through the centralized accumulation, organizing, analysis, and classification of objects and information has increased along with attention to the natural-historical sciences, and more broadly, with what Lorraine Daston has called the "Sciences of the Archives" (see http://www.mpiwg-berlin.mpg.de/en/research/projects/DeptII_Daston-SciencesOfTheArchives).

A third, increasingly prominent, approach has participated in broader historiographic trends of studying the global movements of people, things, and ideas. Much of this work has gone under a general framework of (Western) science and (European) empire. It has highlighted the mutual accommodations made among Western scientists (especially naturalists), commercial interests, Christian missions, and expansionist states, as well as appropriations of indigenous materials and knowledge (e.g. Schiebinger 2004; Schiebinger and Swann 2004; Delbourgo and Dew 2007; Bleichmar et al. 2009; Mitman and Erickson 2010).

In one of the most ambitious of these accounts, Harold Cook (2005) has argued that the Scientific Revolution itself should be located in the constellation of values encouraged by the early modern Dutch commercial empire, which valorized an interest in detail and "matters of fact" that served both the global commerce undertaken by Dutch traders and, as it turns out, science. In Cook's picture, the knowledge that came to be considered scientific emerged from global interactions of people, organisms, and things that filtered back to Europe through circuitous and often contingent networks. In this view, "science" is not made in one place and then spread to another, nor is it located primarily in the organization of bits of information into complex systems at the metropole by leading knowledge producers. Rather, it is the historical product of many different people who themselves contributed, not always voluntarily, to a culture that valued things, their description, and the making of scientific meaning around them.

This sort of account has often been connected with the term "circulation" (e.g., Raj 2007; Terrall and Raj 2010; Lightman, McOuat, and Stewart 2013). This term has been used to emphasize the agency of those formerly considered merely passive instruments in the spread of scientific knowledge (either as receivers or as those whose local knowledge was appropriated), opening up analytical space to acknowledge their interests and their creative, knowledge-generating work. Such analyses have highlighted reciprocal interactions among historical actors, sometimes involving "go-betweens" (Schaffer et al. 2009), often at sites where "trading zones" (Galison 1997) existed or hybrid knowledge cultures persisted (Kohler 2002; Gómez 2013).

In conjunction with a global perspective, the circulation metaphor does important work: it displaces the unidirectionality of older center–periphery models centered on western Europe and the US, and flattens the status difference these models imply, elevating the status of non-Western contributors to Western knowledge and also the non-Western cultures and knowledge systems themselves. It also offers a new big-picture framework under which to unite a plethora of local studies. Because science has for so long been considered an exclusive product of the West, this is a salutary development.

Yet this vocabulary of global "circulation" and "flows" of knowledge has generated criticism from scholars such as Warwick Anderson, who has somewhat sardonically dubbed it the "hydraulic turn" (2014, 375). Fan (2012b, 252) has articulated the concern: "The image of circulation tends to impose too much unity, uniformity, and directionality on what was complex, multidirectional, and messy. . . . [It also] doesn't encourage a critical analysis of, say, power relations in science." Fan, Anderson, and others would prefer more attention to specific sites of resistance and stories of conflict, to remind us that, in historically specific situations, those "flows" may meet significant "blockages" worthy of our attention.

## Scaling History of Science

The world covered by historians of science is bigger, more densely populated, and more complex than it once was. How shall we manage this multileveled intellectual terrain? How can we avoid getting lost in its lush vegetation? As we have seen, current high-visibility scholarship seeks to bind the local and the global through tropes of motion, bypassing well-worn social categories, such as the state and civic institutions, that operate at intermediate levels. Following people and objects around, as they travel the globe, allows the historian to collapse low and high levels of resolution into a single story, which is very appealing. Yet the broad range of intermediate levels of analysis should not be forgotten (Kohler and Olesko 2012). Attending to scales of analysis may in fact help us negotiate the tensions over global circulation mentioned above: a high-level focus on broad patterns tends to gloss over non-hegemonic voices, while lower levels of specificity bring them out. (See Misa 2009 for a similar analysis in history of technology.) Moreover, intermediate levels are crucial for tackling other leading questions not addressed in this essay, such as the comparative history of demarcation, which asks "How has science calved off historically from other activities into its own cultural field?" "How has such demarcation been supported socially and economically?" and "How has it been maintained (or not) in the face of contestation?"

As historians, we must attend to temporal scale as well. Localized stories often take place at the scale of a human lifespan or less, while questions about periodization remain a staple of mid-range temporal analysis. Scholarship on science and history extending temporal scales of analysis to yet broader expanses is emerging around us, drawing on archaeology, anthropology, and environmental history (Robin and Steffen 2007; Robson 2008; Safier 2010). It remains to be seen whether this scalar challenge is one historians of science are willing to take up, and if so, how.

The landscape of the history of science is one we simultaneously inhabit and cultivate: as both science and our broader cultural concerns continue to change, so, too, will the history of science. But the fundamental shift that has taken place since the late 1970s appears to be permanent. Historians of science now treat science as something that has been produced historically and contingently, not arrived at through an increasing recognition of truths. It has emerged instead through the cultivation of particular values that have sustained the investigation of the material world around us, in different directions at different times and places. People undertaking the activities we call "science" have created cultural space for themselves by advancing and taking advantage of new institutions and communicative forms; these in turn have been sustained by the commitments and livelihoods of many people who are not themselves "scientists."

Indeed, the science we depict is deeply embedded in its surrounding culture (even when scientists and spokesmen for science have argued otherwise)—yet that culture itself is typically not closed, but instead engaged in constant exchange with other cultures, feeding the wellsprings of scientific innovation, power, and conflict.

All of this makes the history of science a buzzing, dynamic field of action. Whether we examine it from close up, deep inside the tangle, from a mid-range that resolves certain actors and structures while leaving others fuzzy, or from a more distant view focused on large-scale patterns, our intellectual challenge is to explore diverse narrative and explanatory paths through this terrain. Our practical challenge is to illuminate these paths using all the tools we have available—academic monographs and articles, exhibitions, living history reconstructions and performances, films, podcasts, and the sweep of possibilities offered by new media—to invite others, not always historians of science, to come along with us.

## *Endnote*

1 New attention to bodies in feminist and gender studies and the history of medicine forms a parallel topic that is unfortunately beyond the scope of this essay.

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