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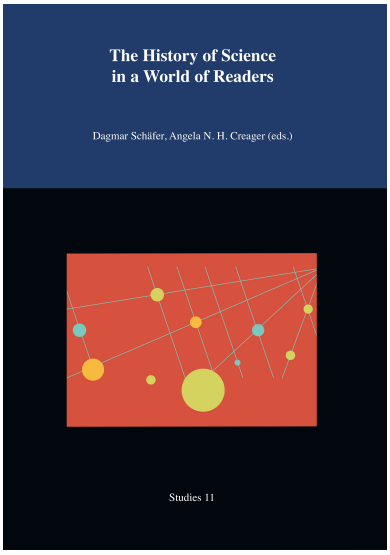
James A. Secord:

Knowledge in Transit



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Chapter 6
Knowledge in Transit

James A. Secord

What big questions and large-scale narratives give coherence to the history of science? From the late 1970s onward, the field has been transformed through a stress on practice and fresh perspectives from gender studies, the sociology of knowledge, and work on a greatly expanded range of practitioners and cultures. Yet these developments, although long overdue and clearly beneficial, have been accompanied by fragmentation and loss of direction. This essay suggests that the narrative frameworks used by historians of science need to come to terms with diversity by understanding science as a form of communication. The centrality of processes of movement, translation, and transmission is already emerging in studies of topics ranging from ethnographic encounters to the history of reading. Not only does this approach offer opportunities for crossing boundaries of nation, period, and discipline that are all too easily taken for granted; it also has the potential for creating a more effective dialogue with other historians and the wider public.

Halifax is just about the perfect place to hold this meeting. Not because of its eighteenth-century reputation as the wickedest town in North America, nor even because of the warm hospitality extended by our hosts, but, rather, because the city epitomizes so well the conference theme. As the site of several universities and a locus for exchange between different continents and traditions, Halifax is all about "circulating knowledge." The earliest evidence of migration to this region dates back over ten thousand years, and the Vikings probably visited about a thousand years ago. The area was explored by Europeans from the early seventeenth century, and the city itself was founded in the mid-eighteenth century. The first regular steam crossing of the Atlantic was inaugurated from Halifax in 1837, so that all news between Paris, London, and New York came through the town. Although the early Atlantic telegraphs went through Newfoundland, in 1925 Halifax became the anchor for all transatlantic messages. Today it is possible to travel easily from Europe to Halifax, precisely because the town remains a center for transportation and communication.

Conference themes, especially when the meeting is international and the participants are many, have a notoriously capacious quality, so that almost anyone can give a paper on almost any topic. "Circulating Knowledge" would seem at first glance to fit this bill. Historians of science may not agree about much, but I think they would all claim to share a concern with knowledge, and everyone recognizes in some way or other that it is not the sole property of individuals—that it "circulates." Indeed, every academic conference ever held could well be said to exemplify the theme of knowledge in circulation.

In this case, however, the theme does much more. It highlights an issue of real analytical significance—in fact, the central question for our field. How and why does 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? In

important respects, this involves issues of the social nature of knowledge, taking seriously the consequences of philosophical perspectives that are widely accepted by historians of science. In other ways, it is part of the history of education, ranging from the scribal schools of the ancient Near East to the modern university. In still others, it is a question of the passing on of tacit skill, as Harry Collins has stressed in his studies of attempts to transfer knowledge of how to make gravity-wave detectors. For students of literature and science, understanding the circulation of a variety of forms of writing has been a central concern. As Gillian Beer has said, this may involve conflict and transformation as much as mutual understanding and reconciliation.[1] In one of its most familiar aspects, the theme of circulation is represented by the history of book production and reading. In still others, it is part of the interaction between different cultures, as the blossoming of work on imperial encounters suggests.

That the spread of knowledge, its global ubiquity and circulation, should be a problem for the history of science is a great irony. For the positivist founders of the discipline assumed that this was the one issue they had cracked. Scientific knowledge spread because it was true; any failure of diffusion could be explained by resistance due to false beliefs and irrational commitments. Although this view would now have few supporters, historians have yet to take on board the full consequences of abandoning it. We have come to realize the centrality of knowledge in circulation—of science as a form of communication—only gradually and from diverse perspectives.

At the same time, the majority of the general frameworks and big pictures for history of science are worn-out inheritances from the origins of our discipline. The most persistent of these has been the concept of the Scientific Revolution. Criticized for positing a one-time shift toward modernity, it looked ripe for replacement a decade ago; but if anything it has expanded in significance, as areas as diverse as natural history and alchemy have been placed under its umbrella. The problems posed by the continued use of the concept of the Scientific Revolution, both for understanding the period in its own terms and for dealing with the surrounding centuries, have neither gone away nor been neutralized. It has, to an unfortunate degree, defined the task of charting long-term changes in knowledge as one of pinpointing comparable epistemological breaks, most notably the Chemical Revolution of the late eighteenth century and the so-called Second Scientific Revolution of the early nineteenth. Most of the narratives used to tell the stories of specific disciplines are looking even more threadbare, as their origins in partisan accounts written by victors in scientific debates are revealed. The Darwinian Revolution is a 1950s by-product of the modern evolutionary synthesis, given new life by the rise of sociobiology; the notion of "classical physics" and an Einsteinian revolution overturning it marks the triumph of relativity theory in the 1920s; the discovery of the DNA structure in 1953 is hailed as the founding moment for molecular biology, but only because of events in the 1960s.[2]

More seriously, revisionist accounts by historians of science tend to rely for their power on the continued dominance of older frameworks. We offer critiques rather than explanations or competing alternatives. My students sometimes complain that they have to learn everything twice: once to understand the old story, then again to learn why it is wrong.

[1] Collins (2004) and Beer (1996).

[2] On the problems of using the Scientific Revolution as an organizing concept see Jardine (1991). For the complications noted in the cases of the other so-called revolutions see Oldroyd (1984); Staley, "The Co-creation of Classical and Modern Physics," paper delivered at the BSHS/CSHPS/HSS meeting (2004) and de Chadarevian (2002).

We study "popular" science, "subaltern" or "indigenous" knowledge; but to varying degrees these categories are too easily framed through a contrast with an assumed story about elite Western knowledge.

There is a strongly felt need for replacements; and if these are to gain any currency outside specialist circles, they need to be simple and clear. Those of us who teach need them for our courses; those of us who write need them to combat what might be called the "Sobelization" of the history of science, with the subject being carved up into heroic sound-bites modeled on Dava Sobel's phenomenally successful *Longitude: The True Story of a Lone Genius Who Solved the Greatest Scientific Problem of His Time* (1995). We need unifying narratives and a sense of large connections, even if they are not the old-style Cinemascope stories offered in the confident days of the Cold War.[3]

Toward that end, I want to do three things in this talk. First, I will suggest that although an understanding of practices of communication, movement, and translation is becoming central to specific aspects of the way that the history of science is conceptualized, to go further we should think much more consistently about the problem, from the kinds of analytical resources we apply to the specific kinds of narratives we write. Second, I will argue that too much of our current work (my own included) has remained limited by unconceptualized geographical and disciplinary boundaries: we choose, as graduate students, to study eighteenth-century French natural philosophy or twentieth-century American physics, without knowing enough about what goes on around and just beyond the limits. And third, I will suggest that sustained attention to knowledge as communication can draw together a variety of approaches, while encompassing an understanding of the specialized, esoteric aspects of science that must remain central to what we do.

6.1 Knowledge as Practice

As will be evident to anyone who has looked over publishers' catalogues in recent years, historians of science have developed superb techniques for placing science in local settings of time and place. A standard model for historicizing science is to locate specific pieces of work in as tight a context as possible, binding them ineluctably to the conditions of their production. As it has usually been read, the classic work in this tradition is Steven Shapin and Simon Schaffer's *Leviathan and the Air-Pump* (1985),the most influential text in our field since Thomas Kuhn's *Structure of Scientific Revolutions* (1962).[4] In demonstrating the mutual relevance of disputes about the power of kings and the composition of cement, *Leviathan and the Air-Pump* revealed the founding moment of the new experimental philosophy as an outcome of specific circumstances in Restoration England, challenging those who assumed the value-free transcendence of science. My point is not that all historians subsequently adopted a local approach, nor even that the concerns of *Leviathan* necessarily tended in this direction. It is, rather, that such work became the focus of the key debates.

That an approach based on the close study of knowledge in context should have come to dominate controversies in our field is not surprising. The same thing was happening in the humanities much more generally, and especially in the history departments where most historians of science (at least in North America) actually teach. Microhistories modeled on

[3] On these issues see the essays in "The Big Picture," a special issue of the *British Journal for the History of Science* (1993). David Philip Miller discusses "The Sobel Effect" in Miller (2002); the work itself is Sobel (1995).

[4] Shapin and Schaffer (1985) and Kuhn (1962).

Clifford Geertz's anthropological notion of thick description became the foundation for the new cultural history. What Lawrence Stone called the "revival of narrative" has had particular appeal for historians of science, for it offers a way of bringing the transcendent power of science down to earth, locating it in specific times and places.[5] Moreover, these trends in general historical debate joined up with those within the developing field of sociology of knowledge, especially as practiced in the Edinburgh Science Studies Unit and then by Harry Collins and his students at Bath. Here specific passages of scientific activity provided "case studies" in the social character of the making of knowledge. The methods demanded an attention to circumstance and situation broadly compatible with what was being pursued in history. There were of course differences, including those of scale: at least some of the sociologically informed work tended to assume that the immediately relevant context was within the boundaries of relatively well-defined communities of practitioners—as in Trevor Pinch's *Confronting Nature: The Sociology of Solar-Neutrino Detection* (1986) and Andrew Pickering's *Constructing Quarks* (1984). Other works, notably Leviathan and the Air-Pump itself, looked to wider debates in politics and religion.[6]

As became clear in the mid 1980s, these approaches were converging toward a view that considered science as a practical activity, located in the routines of everyday life. Knowledge itself came to be seen as a form of practice. It is in this respect that the wider shifts associated with feminism and gender studies had their greatest (though often unacknowledged) shaping effect on the field. Science was, as Donna Haraway memorably put it, "situated knowledge."[7] The move to study practice has, in my view, been the single most significant transformation in our field during the past twenty years. It breached old boundaries between "internal" and "external" and opened up a view of science as a process, including inquiries into experiment, fieldwork, and theory making. Most fundamentally, it broke down old distinctions between words and things, between texts, books, instruments, and images.

It was in 1988, in the immediate wake of these upheavals, that the first of the joint British/North American gatherings was held in Manchester. For those old enough to have been there and young enough to remember it, that was a great meeting, offering for many whose framework had been defined by the intellectual parameters of the1970s Cambridge History of Science Series a huge sense of liberation and possibility. The meeting was notable on many accounts. For those studying the early modern period, it brought together a range of historians on both sides of the Atlantic who were interested in craft knowledge and the role of women. It revealed the range of work being carried out on twentieth- century science, especially in relation to military technology and popular audiences. And in some ways it was the high-water mark of integration between the sociology of knowledge and the history of science. Almost all the main historically inclined sociologists were there. And the meeting was also notable for a wonderful closing dinner in one of Manchester's best Chinese restaurants.[8]

Things have moved on in the past sixteen years, but I think it is fair to say that the core analytical issues that were under discussion at the Manchester meeting have remained central

[5]Geertz (1973) and Stone (1979).

[6]Pinch (1986); Pickering (1984). These controversies are apparent in Biagioli (1999), and esp. in Pickering (1992).

[7]Haraway (1988).

[8]The program is recorded in *Brit. J. Hist. Sci.* (1989, 502–512). Roy Porter (1990) discusses the contemporary state of the field.

to the field just about up to the present time. We can now see this exemplified (as indeed Kuhn himself would have predicted) in textbooks and works accessible to nonspecialists: the Science*Culture series edited by Steven Shapin for the University of Chicago Press, the innovative surveys published by Icon Books, and the amazingly useful (if frustratingly slow to appear) eight-volume *Cambridge History of Science* (2003–). The underlying questions involved are well summed up in Jan Golinski's *Making Natural Knowledge* (1998), which introduces historians to social theory and certain varieties of philosophy.[9]

I am not a big fan of labels, so the designations currently used for work being carried out in this tradition are worth examining. One, usually discussed in connecting history to sociology of knowledge, is "constructivist." This, in my view, tends to raise hackles unnecessarily and (after a certain point) tends not to do any work. As Margaret Jacob has said, "To speak about the social construction of science should be just another way of saying that people make science."[10]Probably the most widespread designations of the approach are "contextualist" and "cultural." These words, however, are now being so variously used that they scarcely have any meaning. As is suggested by the contents of the journal *Science in Context*, "context" can refer to anything from specific philosophical resources used in science to accounts of science in war and economic development. There is much to be said for this diversity, but there is not a good case for identifying it under a single rubric. Again, "contextual" starts to mean nothing much more than "historical." Many anthropologists would disown the term "culture" entirely. It has been important in history of science not so much for its analytical power, but as an identifying marker of an approach. From this perspective, if "science in context" is vague and implies unwanted distinctions between foreground and background, "science as culture" offers enticing possibilities of organic unity and integration. Science, understood through cultural history, can be seen as part of a distinct world of symbols, whose meaning is determined by a network of relations with other symbols. The danger, of course, is that such cultural systems are then seen as consistent, integrated, clearly bounded, and resistant to change. Moreover, the relation of cultural analysis to more traditional forms of social and economic history, with their emphasis on issues of access and power, can too easily be obscured. I suspect that the utility of "contextual" and "cultural" is pretty much exhausted, not only in history of science but in the humanities more generally.

If labels are useful in identifying emerging schools, they can also encourage new approaches to harden into orthodoxies. In this regard, the diversity and empirical grounding of most historical work has been a saving grace, especially compared with literary and cultural studies. But there are difficulties everyone has had to grapple with in practicing, reading, or challenging this form of history. One is the tendency to see the localizing of a piece of scientific work as a worthwhile end in itself. The difficulties of dealing with science as an object of inquiry have required attention to epistemological and ontological issues—a necessary ground-clearing that has been easy to mistake for actual history. The process of situating knowledge ends up as a conclusion rather than a method: the same implicit epistemological lesson, that knowledge is ineluctably local and variable, is hammered home again and again.[11] A second danger is that an emphasis on the local contexts of science can lead to parochial antiquarianism. We think we are making grand epistemological conquests, when in fact we are studying a few practitioners of a relatively esoteric activity, whose wider im-

[9]Lindberg and Numbers (2003–) and Golinski (1998).
[10]Jacob (1999, 115).
[11]Kohler (1999).

portance is assumed rather than demonstrated. The best work in our field is valued for its methodological sophistication and exploration of fresh topics, but it is often seen as being exceedingly narrow.

The final danger is that in focusing on locating the core aspects of scientific practices within broader situations, we may be depending too much on the willingness of other historians to take account of our work in general surveys. It certainly would be nice to think that we are showing how textbooks might include the history of science, beyond the ritual nod to Copernicus, Newton, and Darwin. Accounts of courtly patronage could include Galileo's telescope; histories of commercial culture in colonial New England could discuss the ways in which Benjamin Franklin's theory of electricity emerges from double-entry bookkeeping. Histories of postwar Britain could show how the rise of molecular biology depended on computers and other technologies whose development depended on the war. But in my experience, this kind of integration is happening primarily in those cases (and there are several distinguished ones) in which historians of science are involved as the coauthors of textbooks.[12] Moreover, the assimilation of history of science into general history, although highly desirable from many perspectives, is potentially at odds with an aim of creating big pictures focused on science itself.

So the field remains fragmented. The problems are, paradoxically, a by-product of the extraordinary success we have had in placing science in context, however that is defined. The more local and specific knowledge becomes, the harder it is to see how it travels. We have gained a breadth of connections and relations, but these are limited by the boundaries of a specific ethnographic field. The significance of this issue was, in fact, predicted well over a decade ago, in the justly celebrated paper in *Science in Context* by Adi Ophir and Steven Shapin entitled "The Place of Knowledge." In announcing a program that involved situating knowledge, they identified what they termed the "successor project" it generated:

> How is it, if knowledge is indeed local, that certain forms of it appear global in domain of application? Is the global—or even the widely distributed—character of, for example, much scientific and mathematical knowledge an illusion? If it is the case that some knowledge spreads from one context to many, how is that spread achieved, and what is the cause of its movement? Is its distribution a strong indication of its correspondence with reality, or is it properly read as reflecting the success of certain cultures in creating and spreading the very means and contexts of application? … Perhaps the days in which ideas floated free in the air are truly nearing an end. Perhaps, indeed, what we believed to be a heavenly place for knowledge we will come to see as the result of lateral movements between mundane places.[13]

Tellingly, this paragraph was the last in their paper —it raised the question but was not the heart of their argument. And it is telling that the main (and mostly beneficial) effect of Ophir and Shapin's intervention has been to spawn studies of science in a huge variety of places, from clubs and pubs to lecture halls and laboratories and playing fields. It has highlighted the significance of scientific architecture, encouraged studies of domestic spaces, and given

[12] Notable examples include Marvin Perry, Myrna Chase, James R. Jacob, Margaret C. Jacob, and Theodore H. Von Laue (2004) and Pauline Maier, Merritt Roe Smith, Alexander Keyssar, and Daniel J. Kevles (2002).
[13] Ophir and Shapin (1991, 16).

new life to studies of science in the city and the field.[14] It has, also, however, tended to legitimate the move toward local specificity—a trend that is seriously at odds with wider trends toward global and comparative history. The result is that we end up with a rich array of research that somehow adds up to less than the sum of its parts.

6.2 Literary Replication

I cannot promise you historiographical salvation, and even if it exists there is certainly more than one way to it. But I am sure that we need to think much more explicitly about the problem of the movement of local knowledge. Fortunately, as this conference shows, this involves not a new approach but developing a more explicit sense of some important current trends within the field.

There are lots of ways of tackling this issue, but we need first to recognize that the issues are fundamental, involving the need to rethink the way in which the program of the cultural history of science was originally set out. That agenda has, as will be clear from what I've said already, encouraged a view in which science is created locally but then, by other processes, is transferred outward toward more general contexts.[15] To escape this, we need to shift our focus and think about knowledge-making itself as a form of communicative action. There are good precedents for taking such a view. Many of the philosophical issues most debated by historians of science in recent years give interaction between agents a central role in epistemology. Questions of trust, testimony, and communitarian objectivity are simultaneously questions of how knowledge travels, to whom it is available, and how agreement is achieved. "As a shared form of knowledge," Scott Montgomery argues, "scientific understanding is inseparable from the written and spoken word. … Communicating is the doing of science."[16]

To do real historical work, this perspective needs to be not only explicit but also foundational. This means thinking always about every text, image, action, and object as the trace of an act of communication, with receivers, producers, and modes and conventions of transmission. It means eradicating the distinction between the making and the communicating of knowledge. It means thinking about statements as vectors with a direction and a medium and the possibility of response. The most important task is to make our understanding of science as a form of communication—which is a commonplace in the theoretical literature—really work within the narratives we write. This sounds simple, and in many ways historians of science have devoted a huge amount of attention to identifying the audiences for science and the rhetorical strategies used to reach them. Yet we still regularly write as though people read authors rather than books. We speak of reading Einstein, when what we really mean is reading an article of 1905 in the *Annalen der Physik* on the electrodynamics of moving bodies. We speak of the reception of Descartes, or (worse still) of an essence called "Cartesianism," when what we mean is the debates that took place after the publication of a series

[14] Much of this work is summarized in Livingstone (2004).

[15] Since the days of the "strong programme" in the 1970s, the transmission of knowledge has always had a place in studies of science, but it has often been a secondary one. Thus, when Barry Barnes first introduced English-speaking historians of science to the work of the German social theorist Jürgen Habermas, it was through interest theory rather than the ideas of communicative action that were actually more central to his thought.

[16] Montgomery (2002, 1). See the special issue on testimony in *Studies in History and Philosophy of Science* by Kusch and Lipton (2002a), see esp. (2002b) with bibliography. The two most influential historical works have been Shapin (1994) and Daston and Galison (1992).

of printed books. We write, moreover, as though the author speaks to us directly ("Einstein says," "Descartes says"), when we know perfectly well that what we are actually reading is a narrative voice aimed at a particular horizon of expectations.

These points have been a commonplace of critical theory in the humanities for decades, and there is much to be learned from Hans Robert Jauss, Wolfgang Iser, and other exponents of reader-response theory. However, we need to use this approach much more consistently than is usually done in literary and philosophical studies, which have tended to develop theory rather than explore its application. The issues are especially vital when scientific works are being examined, for these more than any others gain their power through a claim to objective transparency, so that authors appear to speak directly for nature. We cannot get to the core of the problem without reading our most traditional sources—words and images—a lot more closely than we usually do. As Jonathan Topham stressed recently in *Isis*, the study of practices related to printed works has lagged far behind those dealing with experiment and fieldwork. As historians, we are in a good position to combine careful readings of texts, images, and objects with the evidence, often fascinating and diverse, of actual readers.[17] Thus although many historians of science have referred to the brilliant discussion of techniques of literary persuasion in *Leviathan and the Air-Pump*, fewer have followed the authors further in this direction or explored the extensive literature on prose rhetoric and genre.

The point I am making is a semantic one, but not merely so, for its consequences involve profound assumptions about the politics of knowledge. Traditionally the consequences of eliding author, narrator, text, work, and readers have been avoided by analyzing situations in which the distance between these elements is relatively limited and subject to convention. We have tended to assume that the works we study are universally available to all relevant readers and that all those who read them have access to knowledge of the author's person. But this is also to assume a highly specific model of the community of practitioners, in which practices travel relatively freely and modes of communication are relatively transparent. Now, it has been recognized for a long time that this is rarely the case: every act of communication excludes as well as includes. Yet the approach most historians of science take to transmission has tended to be piecemeal, after the real work of explanation is done.

Part of the issue involves recognizing that history of science, even more than most historical fields, has focused on origins and producers. Even when we are not explicitly studying discovery and innovation, we are obsessed with novelty and the places in which novelty begins. The further we move away from sites of the production of new knowledge, the vaguer our descriptive categories tend to become. Rather than saying that an idea was "popular," a "best seller," or a "sensation," we need to analyze audiences and readerships closely and carefully, with the same awareness of cultural nuance we might bring to an account of life in the laboratory. Otherwise we are simply reproducing the notion that science passes from highly individualized sites of production to an undifferentiated mass public.

Take the literature on Michael Faraday, which exhibits all the features of current best practice in the field. We have wonderful discussions of the way in which Faraday developed his experiments for presentation on the stage of the lecture room. The significance of his work in relation to the politics of the Royal Institution and the role of his lecture demonstrations in establishing his career have been brilliantly studied.[18] But we know less about his auditors (other than that they were genteel) and their reasons for attending. What made

[17] Jauss (1982); Iser (1978); Topham (2004).
[18] Morus (1998) and Gooding and James (1985).

chemistry and natural philosophy fashionable? How and why did certain newspaper and periodical editors report the lectures, and which ones did not? In relation to Faraday's audience among fellow practitioners, much has been written on how he made his experimental arrangements convincing, but less on how he addressed his readers and the role of publishing in journals such as the *Philosophical Transactions* and the *Philosophical Magazine*. There is no discussion, in relation to Faraday's work, of where and how such periodicals could be read, how many copies were printed, and how they were made available in other countries.[19] By default, such publications become universal multipliers: they take us from Faraday's immediate context to an international knowledge of what he was doing. In consequence, we have only a rather vague idea of how Faraday's unparalleled reputation actually developed over time. Until more of the perspectives that have broadened our understanding of Faraday's experimental practice area applied to his immersion in the world of print—through communicative actions largely carried out by others—we unwittingly enhance his heroic status. Readers are led to picture a Faraday who was supremely good not only at things that might justify his title of genius (such as experimental skill and conceptual innovation) but also at things he was unable to do—such as making his name known to everyone in the land or traveling instantaneously between continents. Moreover, our stress on origins and producers has led to inadequate attention to the structures of time itself in the stories we tell. François Furet has argued that all narrative history is a succession of origin events, as any narrative is dominated by its end and beginning. In telling stories, we are inevitably drawn toward a teleology. I am far from thinking that we should cease to write narratives, but I would suggest that we need to stop using time unreflectively. What is needed is not less attention to time, but more: a history in which notions of time are not taken for granted. As the American historian Thomas Bender has said, "A history liberated from origins would … historicize the axis of time itself, emphasizing structure, transformation, and relations."[20]

One of the few works in history of science to discuss such issues is Martin Rudwick's *Great Devonian Controversy* (1985), which explicitly attends to the relation between different scales of time involved in a scientific controversy. In that case, the highly publicized meetings of the British Association for the Advancement of Science offered very different opportunities for debate than those presented by the fortnightly discussions at the Geological Society of London. The temporal sequencing of communication has also been prominent in the essays produced from the SciPer (Science in the Nineteenth-Century Periodical) project at the universities of Leeds and Sheffield. These bring out important questions about the periodicity of knowledge as presented to readers in dailies, weeklies, monthlies, quarterlies, and annuals. Serial reading offered ways of creating and reinforcing individual identity, religious faith, and social cohesion.[21]

So the first of my suggestions would be to think, at every point in our work, about science as a form of communicative action—to recognize that questions of "what" is being said can be answered only through a simultaneous understanding of "how," "where," "when,"

[19] An exception is a short article on the *Athenaeum* and the *Literary Gazette*: see James (2004). There are a number of studies that focus on the posthumous reputations of scientists such as Faraday, notably Cantor (1996). It is telling, however, that none of the literature I have read on Faraday's science cites Brock and Meadows (1984), the standard history of the firm that published and printed almost everything he wrote. On the market for Faraday's work the main source remains Berman (1978), though this can be updated by several of the essays in James (2002).

[20] Furet (1984, 69) and Bender (2002, 8).

[21] Rudwick (1985). For publications from the SciPer project see Henson et al. (2004); Cantor and Shuttleworth (2004); Cantor et al. (2004).

and "for whom." The "successor problem" identified by Ophir and Shapin needs to be part of the original formulation of what historians think they ought to be doing. It is not so much a question of seeing how knowledge transcends the local circumstances of its production but instead of seeing how every local situation has within it connections with and possibilities for interaction with other settings. If the slogan for much history of science in the past twenty years was "science in context," we could do a lot worse than to think now about "knowledge in transit."

6.3 Conventions of Circulation

At the time of the Manchester meeting, the one author whose works were really putting issues of the movement of knowledge on the agenda was Bruno Latour. His writings, especially on Louis Pasteur, proved exceptionally helpful in taking studies of scientific practice beyond the microsocial, embedding science in networks of translation and appropriation. In the end, however, Latour's conclusions have proved too ahistorical and too concerned with unstable hybrids to offer the practical resources historians need for interpreting the past. Concepts such as "centers of calculation," "immutable mobiles," and "obligatory passage points" proved to be better suited to thinking through the relation of single centers to a periphery (Pasteur's lab and French farmers) than for elucidating competing or multiple ones (Pasteur's lab and that of the German bacteriologist Robert Koch). Most fundamentally, historians of science have resisted Latour's call to give equal agency to nonhumans and humans. Giving agency to microbes and doors would seem to require recourse to the latest findings of biological and physical science, a move that goes against the most basic precept of the field as it has developed during the previous twenty years: the principle of symmetry in treating evenhandedly scientific findings that have proved true and those that have not.[22] Even so, Latour's writings have been of signal importance in stressing the need to examine knowledge as an activity occurring in time and space. Historians have adopted his emphasis on process, reception, and audiences; and they have done so in a way that has recognized the relative stability of many of the networks that Latour tended to believe were infinitely flexible. Not least, this has made the networks amenable to historical analysis.

Latour's work is thus only the most radical of several attempts to refocus the study of science around practices of entanglement, translation, and border crossing. Here we can identify part of the reason why the investigation of what Peter Galison has called "trading zones" has been so fruitful and why James R. Griesemer and Susan Leigh Star's work on boundary objects has been so widely cited.[23] Concentrating on sites of exchange is not enough, however, for these are often on the margins and involve practices developed for dealing with relative outsiders. It is in those fields where the study of contact zones has been combined with an understanding of relatively stable patterns of practice that we have begun to develop some of the most effective new big pictures. In this way, we are beginning to understand the generic regularities involved in the circulation of knowledge —and how these change according to time and circumstance.

The key to creating this history is our new understanding of scientific knowledge as practice. All evidence from the past is in the form of material things. This is (or, rather,

[22] Schaffer (1991) and Bloor (1999). Among Latour's influential works, see (1987, 1999b, 1999a).
[23] Galison (1997, 803–844) and Star and Griesemer (1989).

has become) obvious in the case of experimental instruments, natural history specimens, and three-dimensional models.[24] But it is equally true of pamphlets, drawings, journal articles, notebooks, diagrams, paintings, and engravings. Whether they study Newton's graffiti on Grantham schoolhouse or tape recordings of the discovery of pulsars, historians are inevitably chroniclers of the material world.[25] Robert Westman put it perfectly in his talk at this conference: "books and letters, not 'isms,' passed hands." It is in tracing the patterns of circulation of these "things-in-motion," as the anthropologist Arjun Appadurai has called them, that we can create a history that goes beyond particular instances. And because practices are often persistent and relatively stable, we are thus in a position to trace not just individual objects, but larger classes and genres of things. The new orientation thus offers the potential—as yet only partially realized—for histories that span long periods of time and different countries. It is a view that has already gone much further in transforming the histories of medicine and technology, cognate fields in which the material world has been harder to ignore.[26]

There are many resources to draw on for developing this approach. One of the best-established traditions of work is in art history, which since Michael Baxandall's *Painting and Experience in Fifteenth Century Italy* (1972) has dealt extensively with the transmission of material practices in both the making and the viewing of paintings. Pamela H. Smith's *The Body of the Artisan* (2004) brings these perspectives to bear in demonstrating the role of practical men in the transformation of knowledge in the sixteenth and seventeenth centuries. We can also see the significance of skill, training, and apprenticeship in Myles Jackson's work on Joseph von Fraunhofer and precision optics in Bavaria and England. Apprenticeship of a different kind is explored in Andrew Warwick's *Masters of Theory* (2003), which shows how mathematical physics in nineteenth-century Cambridge was transformed by coaching and examination. Transmission, innovation, and skill are bound together in pedagogy. As Warwick points out, education has received considerable attention from historians of science, but its potential for redrawing the larger contours of an understanding of knowledge as practice remains surprisingly underdeveloped.[27]

A related focus on material forms of knowledge transfer is available in work on the history of print and the sociology of the mass media. Roger Silverstone, Ien Ang, and other students of modern media consumption have been exceptionally helpful in opening up new questions about this field; some of the most suggestive studies of audiences, from which historians have much to learn, are based on empirical studies of television watching as an example of domestic technology in use.[28] Janice Radway's *Reading the Romance* (1984), an in-depth empirical study of a group of women romance readers in the midwestern United States, offers many insights into how to study a particular literary genre in relation to its readers. The greatest impact of studies of print has been in late medieval and early modern histories, notably the work of Ann Blair, Anthony Grafton, Adrian Johns, Nancy Siraisi,

[24]For examples see Gooding, Pinch, and Schaffer (1989); Jardine, Spary, and Secord (1996); de Chadarevian and Hopwood (2004).
[25]For examples see the essays in Gumbrecht and Pfeiffer (1994) and in Daston (2000).
[26]Robert Westman, "Circulating Theoretical Knowledge: Kepler and Galileo in the Years of Public Silence," paper delivered at the BSHS/CSHPS/HSS meeting (2004); Appadurai (1986, 5). For a stimulating example of how models from medical history can be used to create a "big picture" account of science see Pickstone (2000).
[27]Baxandall (1972); Smith (2004); Jackson (2000); Warwick (2003).
[28]Marris and Thornton (1999) offers a comprehensive anthology; Schiller (1996) is a helpful introduction to the main debates.

and others.[29] But related areas continue to receive less attention, especially the history of scientific periodicals, journalism, and book production after about 1850.

Why has it taken so long for the histories of education and communication—among the most promising avenues for developing a history of knowledge practices—to gain a significant position in academic debate? It a curious relic of disciplinary hierarchies that important aspects of these fields were for many years kept apart from the rest of the humanities. The history of primary and secondary education was usually taught in specialist teachers' colleges; the history of all but the most elite forms of publication was limited to journalism schools. Book history meant bibliography, which was taught primarily to librarians. These were vocational subjects, related to professional training, and although the work done was often of high quality, it remained low in status compared to scientific sociology, the history of ideas, and abstract philosophy. In art history, too, study of the material qualities of paintings was, for many years, seen as subsidiary to (and largely separate from) analyses of iconography and attribution. This situation is now changing, but it has taken an immense effort (and much administrative reorganization) to acknowledge the significance of these subjects. Even museums, which from the start played a founding institutional role, have only in the past two decades achieved a central position in defining the intellectual agenda.

Studies from these new directions, dealing with paper, parchment, ink, brass, steel, rubber, and glass, are grounded in the material world, and as such they are deeply rooted in ecological history. This relation has been brought out most explicitly in Robert Kohler's work, which examines the boundary between the laboratory and the field using tools modeled on those developed in William Cronon's *Nature's Metropolis* (1991) for understanding the relation between the city and the country.[30] As the writings of ecological historians show, attaining a global picture is not a question of transcending or erasing local practices but of giving more attention to practices of circulation on a wide variety of scales. Writing a global history of knowledge primarily as doctrine and ideology is probably impossible; writing a history of knowledge as circulating practices would not be easy, but at least it is possible to see how it might be done.

An approach grounded in communication opens up the possibility of integrating accounts of technical, specialist aspects of science with their wider uses. When Claude Bernard jotted down in his notebook his results on the physiological effects of curare, he was bridging what he was doing in the lab and what he would eventually report to the Académie des Sciences. The particulars of this bridging practice were so much taken for granted that they are unlikely to be made explicit; rather, they need to be teased out from the practical ways that specific passages from the notebooks were recycled for later use in publications. Even pencil jottings made in the laboratory were targeted toward potential audiences, and notebooks have conventions and a history of their own within a cycle of communication.[31] It is often thought, for example, that the history of the scientific book involves looking at publishers, binders, readers—anything but the actual words on the pages being produced. But this is simply not the case, or at least certainly should not be. Everyone knows Marshall

[29] Radway (1984); Blair (1997); Grafton (2001); Johns (1998). Also helpful are Frasca-Spada and Jardine (2000) and the discussion between Adrian Johns and Elisabeth Eisenstein in "How Revolutionary Was the Print Revolution?" (2002).

[30] Kohler (2002). See Cronon (1991) and also (1995).

[31] Grmek (1973); Holmes (1974, 1987).

McLuhan's famous slogan, "the medium is the message"; but it is simultaneously true that messages are the medium: they are what defines a communications technology.

So we need accounts of the generic development of the field notebook, the experimental register, the museum catalogue, and other documents of practice, as bridging studies moving between specific passages of technical work and their wider settings. It is amazing that we lack a good general history of the protocols and procedures for announcing a discovery in different periods. We know a lot about theories, but not nearly so much about theorizing as an act of communication. We have only a limited number of studies of scientific letter writing, note taking, habits of journal reading, technical drawing, close observation, lecture attendance, and lab talk. We have paid little attention to local attitudes toward different forums for publication and to specific practices for producing words and images in relation to education, textbooks, and translations. There are only a handful of accounts of the conventions of natural philosophical travel, scientific museum going, and the experience of attending conferences. Recent works, however, have begun to take border crossing as their main subject. Jean-Paul Gaudillière has shown how the travels of French biologists to the United States shaped the development of biomedicine in postwar France. "Rather than being simple transfers," he writes, "the transatlantic exchanges nurtured processes of adaptation, tinkering, and mobilization of outside resources for local purposes."[32]

As such studies suggest, the aim is not just to append accounts of some new aspects of science to our existing analyses. Part of the difficulty has been in thinking of communication as something that is involved in all aspects of science, not something that occurs only when scientists write for publication. Many historians of science will know Robert Darnton's communication circuit, which shows how a work passes through a cycle of production from author, to printers and publishers, to readers, and back to the author. However, this model, at least in its main outlines, is too focused on the production of printed materials to have had much impact outside the history of the book. Readers—surely of the greatest significance to most historians—play a role in the circuit primarily in terms of their feedback to the authors and the subsequent publication process. Unless carefully used, the communication circuit tends to produce accounts in which histories of publishers, printers, broadcasters, and so forth are inserted into an already-known story.[33] Adding a brief account of the publishing firm of Macmillan to a study of a laboratory group that regularly publishes in *Nature* is unlikely to be illuminating. What we need to know more about are patterns of circulation and use in the appropriate local settings.

Concentrating on conventions of circulation is especially important if we are not to end up just adding further particulars to a story already heavy with detail. It may seem challenging enough to explicate the contents of a particular piece of scientific writing or to characterize a passage of experimental activity—without the additional burden of explaining whom it was for, by what means it was communicated, and how it was received. To undertake a close investigation of the context of every statement would be insufferable. Writing a history based around changing practices for knowledge, however, is much more feasible. For all their faults and inconsistencies, we could do well to look to Raymond Williams's *The Long Revolution* (1961) and *Culture and Society* (1958), which examined the creation of literature in England in terms of changes in the audience and mechanisms of authorship,

[32] Gaudillière (2002, 413).

[33] This point is briefly developed in Secord (2000, 126). In his recent writings, Darnton's model is considerably more complex, though in practical terms it has remained centered in the world of publishing. See Darnton (2000).

reading, and publication. Or we could look again at Friedrich Kittler's *Discourse Networks 1800/1900* (1990), which (although difficult to read, at least in translation) shows what a history of writing in the machine age might look like.[34]

Perhaps the biggest challenge is creating a history that keeps the virtues of the local but operates at a unit of analysis larger than a single country. Much of the founding work in the social history of science in the 1970s was concerned with national styles in science: French, Scottish, Canadian, American, and so forth.[35] In identifying national "styles," historians challenged universalist notions of science, but they also tended to align their work with a certain kind of nationalism—an alliance made all the more potent by problems of language and the traditional association of history writing with the rise of the nation- state. As a result, it now appears to require a huge scholarly investment to move one's research outside the boundaries of a single country. There is, of course, nothing wrong with a geographical focus, as long as it not simply taken for granted—or, what is worse, assumed as a kind of global microcosm. This has notoriously been a problem with studies of Britain, where accounts of the origins of the Royal Society or the reception of Darwin's *Origin* are often taken as possessing an automatic international applicability.

A good example is the remarkable gulf between studies of science in Victorian Britain and the antebellum United States. For scholars of the seventeenth and eighteenth centuries, Atlantic history has been a commonplace for at least fifty years. But the situation is very different for the nineteenth century—although communication was during most of this period far better than it had been before. As a result of the widening of the Atlantic in the nineteenth century, we have two sophisticated bodies of secondary literature on two closely connected national cultures—but little cross-citation between those who study them. In part, this is because of general issues of exceptionalism in the writing of American history; in part, it is because of British parochialism and the long-standing dominance of literary scholarship within accounts of the Victorian period. But whatever its causes, the result is that some of the most relevant and best work in one field is simply not used in analyzing closely similar situations in the other. For example, the most revealing works on scientific fraud and hoaxing are not about English showmen, but about P. T. Barnum.[36] Yet, bizarrely, almost no one in Victorian history generally (let alone in studies of science) ever refers to these works. In effect, we have been even more nationalistic than the people we study.

One way of getting beyond national histories has been to undertake comparative studies. But as has often been pointed out, comparative work can all too often end up reaffirming national boundaries, as the nation becomes the standard unit of comparison. Volumes such as *The Scientific Revolution in National Context* and *The Comparative Reception of Relativity* have brought out the complexity and particularity of specific national situations, but they have done less toward creating a global picture.[37]If you want a history that truly does the job, the answer is not to invite one contributor to discuss each country separately but to find people willing to study different kinds of interactions, translations, and transformations.

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[34]Williams (1961,1958); Kittler (1990).

[35]For characteristically illuminating thoughts on the issue see Rosenberg (1970); Morrell (1974), and the editors' introduction to Levere and Jarrell (1974). There were of course many exceptions: notable ones dealing with transatlantic relations include Fleming and Bailyn (1969) and Rossiter (1975).

[36]Harris (1973); Reiss (2001); Cook (2001).

[37]Porter and Teich (1992) and Glick (1987).

More promising has been the outpouring of work on imperial and postcolonial science during the past decade. An early fascination with Latour's actor-network theory has given way to a fully historical understanding, often informed by anthropological perspectives, with divisions between center and periphery replaced by patterns of mutual interdependence. The consequences are clear in the new history of disease and germs, which goes beyond the laboratory to interpret the forging of bacteriology as part of the processes of imperial exchange. Most strikingly, accounts of standardization, measurement, and public exhibition have transformed the history of the physical sciences. The result has been a dramatically new picture of the origins of field theory, energy physics, and statistics in relation to telegraphy, economic development, and modern accounting practices.[38] These have been such exciting sites for research, I would argue, because they raise issues so clearly implicated in political struggles over global empires and industrial capitalism.

In situations where domination and conquest are less obvious, the significance of communication and acting at a distance has been easier to miss. This has certainly been the case in many local studies, whether by professional historians or not, in which scientists are shown to interact with those immediately around them, with other audiences and competing centers of practice remaining in the background. At the other extreme, the writers of general histories tend to imagine that modern scientific inquiry is the closest thing to a perfectly globalized system that we possess. This surely remains a dominant view among scientific practitioners and the public at large. International conferences, international journals, and international visitors are taken for granted, thus making fields such as nuclear physics or molecular biology appear at times to be without boundaries at all. Here the assumption that knowledge simply travels by itself seems easier to make, for the work that has gone into making this appear to be the case is so pervasive and institutionalized that it has become hard to see. Struggles for access and control, however, are always at stake in any form of communication: to make knowledge move is the most difficult form of power to achieve.

6.4 Conclusion

Historians have a tendency to neutralize fundamental challenges by creating new subdisciplines that allow their advocates room to work while minimizing their impact. They add sidecars to a vehicle that continues to travel in the old way toward the old destination. So I should stress that I am not recommending that historians of science pursue the creation of a separate discipline of the "history of the book" or of "print culture." At one level, book history has been concerned with publishers, editors, printers, and so forth, aspects of production that are important but need not occupy the attention of more than a minority of historians of science. Book history, in that sense, has been too narrowly about print to capture the full range of what historians of science ought to be interested in. If science really is an activity pursued by people, the study of communicative practices should be something that we all do all the time. So there are lessons to be learned from book history, just as there are from translation studies and accounts of the laboratory–field boundary; but the label is not really appropriate for the range of things that need to be done.

Similarly, I am not advocating the creation of a subfield within history of science devoted to the study of popular science. Indeed, at this stage it would be best if "popular

[38] Much of this work is conveniently surveyed in Nye (2003).

science" as a neutral descriptive term was abandoned. As a descriptive category, "popular science" and its cognates suffer from serious disadvantages. First, they have an exceptionally rich and multivocal history. Studying these meanings is eminently worthwhile, but it is hard to see how together they refer to a coherent entity. To dump Johann Amos Comenius's *Orbis sensualium pictus* (1658), Camille Flammarion's *Astronomie populaire* (1879), and Stephen Hawking's *Brief History of Time* (1988) in a single genre surely conceals more than it reveals. "Popular science" is not a thing that comes into being at a particular moment or period; it is not appropriately seen as an emergent category.[39] The second disadvantage is the diffusionist baggage that the term "popular science" has carried since the mid-nineteenth century. To label something unequivocally as popular science can be seen as tantamount to saying that it is "not science" or even a kind of pseudoscience parading as the real thing. Above all, it prejudges the boundary that Ludwik Fleck long ago identified between expert, esoteric knowledge and the exoteric knowledge found in textbooks and simplified redactions. In any historical study of science, that boundary ought to be a critical site for investigation.[40]

These are not easy times for history of science. In Roy Porter, Stephen Jay Gould, and Susan Abrams, we have lost too early some of the most effective public advocates for our field. As everybody knows, getting an academic book into print is much more difficult than it was four years ago, when Jan Golinski spoke so eloquently to the last three-societies meeting about historical narratives and the wider public.[41] These days even university presses seem reluctant to take on titles unless they promise abroad appeal. The real running in the past decade seems to have been made by journalists whose writings bring past science to a large general readership. Many of these works are excellent, but many also do little more than reinforce existing attitudes toward heroic genius, the inevitable progress of science, and the triumph of narrowly defined conceptions of national character. What these books do make clear is that there are large audiences for history of science, which a number of our colleagues have begun to reach with different and more challenging messages.

Perhaps this is just my own experience, but I think it is fair to say that the field of history of science, compared to any time since its founding in the 1950s, has experienced a loss of direction. I suspect this is because, as in other parts of the humanities, a certain kind of engagement with theoretical perspectives is coming to an end, and it is not clear what the replacement is to be. At Manchester, for example, all the main figures in sociology of knowledge gave papers; here we are largely on our own, with our links and collaborations more likely to be with general historians of the periods we study. It is no longer possible to look to Paris, Edinburgh, Bath, or even Cambridge fora unified, programmatic notion of what is to be done. That is probably a good thing, for the subject has always thrived on diversity, but it is also a challenge.

It is, of course, always possible that history of science will seamlessly merge into cultural history, philosophy, the natural sciences, or the fields on which it borders in science studies. Last year's president of the History of Science Society, John Servos, once pub-

[39]Comenius (1658); Flammarion (1879); Hawking (1988). For exploration of the rich history of "popular science" and its cognates, the special issue on "Science Popularization" (1994) remains a good starting point, as does Whitley (1985).

[40]On the diffusionist baggage borne by the term "popular science" see Secord (1994). For Fleck's boundary see Fleck (1979).

[41]Jan Golinski, "Tall Tales and Short Stories: Narrating the History of Science," available online at https://www.academia.edu/9271123/Tall_Tales_and_Short_Stories_Narrating_the_History_of_Science.

lished an essay in *Isis* on "a disciplinary program that failed" in physical chemistry.[42] In my more pessimistic moods as a graduate student, I sometimes wondered if the last article in the journal might be a similar obituary for the field I was just then entering. My sense these days is much more optimistic, if not always about jobs then certainly about the underlying intellectual enterprise. There are many indications that we are beginning to tackle, from a fundamentally historical perspective, knowledge not just as abstract doctrine but as communicative practice in a range of well-integrated and closely understood settings. My sense also is that this transformation is more advanced in some fields, such as imperial science and the sixteenth and seventeenth centuries. Moreover, there are encouraging signs of an appreciative audience for our work among general historians, historians of art and literature, and the public at large. Historians of science have been influential beyond their numerical strength in pursuing new topics, from the history of the book to the history of the body, in ways that have attracted interest throughout the humanities.

For this to continue, we need to grapple with the circulation of knowledge at the right scale. Here there really are abundant opportunities. It is only in the past few years that we have begun to realize just how constrained the frameworks for understanding the larger narratives of science really have been. But the great advantage now is one of perspective beyond that of the inherited stories. We have a way to move toward larger narratives made by historians of science and specifically tailored to serve historical purposes. The words Roy Porter quoted in 1975 from the geologist Charles Lyell, at the first history of science meeting I ever attended, are still to the point: "the charm of first discovery is our own, and as we explore this magnificent field of inquiry, the sentiment of a great historian … may continually be present to our minds, that 'he who calls what has vanished back again into being, enjoys a bliss like that of creating.' "[43]

2004 is the year of the transit of Venus, and this surely is a heavenly sign of the ascendancy of the forms of historical practice I have been discussing. The transit of Venus has never been primarily about discovery but, rather, about determining the basic astronomical unit, the distance from the Earth to the Sun; it thus underlines the significance in science of measurement, standardization, and ordinary practice. It is a local event—to be seen by specific observers in specific places—that has sparked national rivalry, global exploration, and wide inquiry. It is an event that has caused both astronomical observers and historians to think about time, from the scale of the personal equation of individual observers in seeing the notorious "black drop" to the scale of years and centuries when the transit recurs. At every stage the transit has underlined the integration of new forms of communication and how these have been transformed, from its observation by Jeremiah Horrocks in a Lancashire village in 1639 to its appearance in early June of this year, when I saw it both through the early Victorian Northumberland equatorial telescope at the Institute for Astronomy in Cambridge and on my laptop computer at home. Moreover, the transit has been a huge public success, not only for astronomical science but also for interest in its history. The transit of Venus will be visible again in eight years, just in time to herald what will be the seventh of these three-society meetings. I'm looking forward to seeing where historical studies of science are headed in that time.

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[42] Servos (1982).

[43] Porter (1976, 100). Lyell was quoting the pioneering German historian Barthold George Niebuhr, whose *History of Rome* had been translated into English in 1828.

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References

Appadurai, Arjun (1986). Introduction: Commodities and the Politics of Value. In: *The Social Life of Things: Commodities in Cultural Perspective*. Ed. by Lorraine Daston. Cambridge: Cambridge University Press, 3–63.

Baxandall, Michael (1972). *Painting and Experience in Fifteenth Century Italy: A Primer in the Social History of Pictorial Style*. Oxford: Clarendon Press.

Beer, Gillian (1996). Translation or Transformation? The Relations of Literature and Science. In: *Open Fields: Science in Cultural Encounter*. Oxford: Clarendon Press, 176–95.

Bender, Thomas (2002). Historians, the Nation, and the Plenitude of Narratives. In: *Rethinking American History in a Global Age*. Ed. by Thomas Bender. Berkeley and Los Angeles: University of California Press, 1–21.

Berman, Morris (1978). *Social Change and Scientific Organization: The Royal Institution, 1799–1844*. Ithaca, NY: Cornell University Press.

Biagioli, Mario, ed. (1999). *The Science Studies Reader*. New York: Routledge.

Blair, Ann (1997). *The Theater of Nature: Jean Bodin and Renaissance Science*. Princeton, NJ: Princeton University Press.

Bloor, David (1999). Anti-Latour. *Studies in History and Philosophy of Science* 30(1):81–112.

Brock, W.H. and A.J. Meadows (1984). *The Lamp of Learning: Two Centuries of Publishing at Taylor & Francis*. 2nd ed. London: Taylor & Francis.

Cantor, Geoffrey (1996). The Scientist as Hero: Public Images of Michael Faraday. In: *Telling Lives in Science: Essays on Scientific Biography*. Ed. by Michael Shortland and Richard Yeo. Cambridge: Cambridge University Press, 171–93.

Cantor, Geoffrey, Gowan Dawson, Graeme Gooday, Richard Noakes, Sally Shuttleworth, and Jonathan R. Topham, eds. (2004). *Science in the Nineteenth-Century Periodical: Reading the Magazine of Nature*. Cambridge: Cambridge University Press.

Cantor, Geoffrey and Sally Shuttleworth, eds. (2004). *Science Serialized: Representation of the Sciences in Nineteenth-Century Periodicals*. Cambridge: Cambridge University Press.

Collins, Harry (2004). *Gravity's Shadow. The Search for Gravitational Waves.* Chicago: University of Chicago Press.

Comenius, Johann Amos (1658). *Johann Amos Comenius*. Nuremburg: Michaelis Endteri.

Cook, James W. (2001). *The Arts of Deception: Playing with Fraud in the Age of Barnum*. Cambridge, MA: Harvard University Press.

Cronon, William (1991). *Nature's Metropolis: Chicago and the Great West*. New York: Norton.

———— ed. (1995). *Uncommon Ground: Toward Reinventing Nature*. New York: Norton.

Darnton, Robert (2000). An Early Information Society: News and the Media in Eighteenth-Century Paris. *American Historical Review* 105(1):1–35.

Daston, Lorraine, ed. (2000). *Biographies of Scientific Objects*. Chicago: University of Chicago Press.

Daston, Lorraine and Peter Galison (1992). The Image of Objectivity. *Representations* 40:81–128.

de Chadarevian, Soraya (2002). *Designs for Life: Molecular Biology after World War II*. Cambridge: Cambridge University Press.

de Chadarevian, Soraya and Nick Hopwood, eds. (2004). *Models: The Third Dimension of Science*. Stanford, CA: Stanford University Press.

Flammarion, Camille (1879). *Astronomie populaire: Description générale du ciel*. Paris: C. Marpon and E. Flammarion.

Fleck, Ludwik (1979). *Genesis and Development of a Scientific Fact*. Ed. by Thaddeus J. Trenn and Robert K. Merton. Chicago: University of Chicago Press.
Fleming, Donald and Bernard Bailyn, eds. (1969). *The Intellectual Migration: Europe and America, 1930–1960*. Cambridge, MA: Harvard University Press.
Frasca-Spada, Marina and Nick Jardine, eds. (2000). *Books and the Sciences in History*. Cambridge: Cambridge University Press.
Furet, François (1984). *In the Workshop of History*. Chicago: University of Chicago Press.
Galison, Peter (1997). *Image and Logic: A Material Culture of Microphysics*. Chicago: University of Chicago Press.
Gaudillière, Jean-Paul (2002). Paris-New York Roundtrip: Transatlantic Crossings and the Reconstruction of the Biological Sciences in Post-war France. *Studies in History and Philosophy of Biological and Biomedical Sciences* 33(3):398–417.
Geertz, Clifford (1973). *The Interpretation of Cultures*. New York: Basic Books.
Glick, Thomas F., ed. (1987). *The Comparative Reception of Relativity*. Dordrecht: Reidel.
Golinski, Jan (1998). *Making Natural Knowledge: Constructivism and the History of Science*. New York: Cambridge University Press.
Gooding, David C. and Frank A. J. L. James, eds. (1985). *Faraday Rediscovered: Essays on the Life and Work of Michael Faraday*. London: Macmillan.
Gooding, David C., Trevor Pinch, and Simon Schaffer, eds. (1989). *The Uses of Experiment: Studies in the Natural Sciences*. Cambridge: Cambridge University Press.
Grafton, Anthony (2001). *Cardano's Cosmos: The Worlds and Works of a Renaissance Astrologer*. Cambridge, MA: Harvard University Press.
Grmek, Mirko (1973). *Raisonnement experimental et recherches toxilogiques chez Claude Bernard*. Geneva: Droz.
Gumbrecht, Hans Ulricht and K. Ludwig Pfeiffer, eds. (1994). *Materialities of Communication*. Translated by William Whobrey. Stanford, CA: Stanford University Press.
Haraway, Donna J. (1988). Situated Knowledge: The Science Question in Feminism as a Site of Discourse on the Privilege of Partial Perspective. *Feminist Studies* 14(3):575–99.
Harris, Neil (1973). *Humbug: The Art of P. T. Barnum*. Chicago: Chicago University Press.
Hawking, Stephen (1988). *A Brief History of Time: From the Big Bang to Black Holes*. Toronto/New York: Bantam.
Henson, Louise, Geoffrey Cantor, Gowan Dawson, Richard Noakes, Sally Shuttleworth, and Jonathan R. Topham, eds. (2004). *Culture and Science in the Nineteenth-Century Media*. Hants: Aldershot.
Holmes, Frederic L. (1974). *Claude Bernard and Animal Chemistry: The Emergence of a Scientist*. Cambridge, MA: Harvard University Press.
———— (1987). Scientific Writing and Scientific Discovery. *Isis* 78(2):220–35.
Iser, Wolfgang (1978). *The Act of Reading: A Theory of Aesthetic Response*. Baltimore: Johns Hopkins University Press.
Jackson, Myles W. (2000). *Spectrum of Belief: Joseph von Fraunhofer and the Craft of Precision Optics*. Cambridge, MA: MIT Press.
Jacob, Margaret C. (1999). Science Studies after Social Construction: The Turn toward the Comparative and Global. In: *Beyond the Cultural Turn: New Directions in the Study of Society and Culture*. Ed. by Victoria E. Bonnell and Lynn Hunt. Berkeley and Los Angeles: University of California Press, 95–120.
James, Frank A. J. L. (2002). *The Common Purposes of Life: Science and Society at the Royal Institution of Great Britain*. Hants: Aldershot.
———— (2004). Reporting Royal Institution Lectures. In: *Science Serialized: Representation of the Sciences in Nineteenth- Century Periodicals*. Ed. by Geoffrey Cantor and Sally Shuttleworth. Cambridge: Cambridge University Press, 67–79.
Jardine, Nicholas (1991). Essay Review: Writing off the Scientific Revolution: Reappraisals of the Scientific Revolution. *Journal of the History of Astronomy* 22(4):311–8.
Jardine, Nick, E.C. Spary, and James A. Secord, eds. (1996). *Cultures of Natural History*. Cambridge: Cambridge University Press.
Jauss, Hans Robert (1982). *Toward an Aesthetic of Reception*. Translated by Timothy Bahti. Minneapolis: University of Minnesota Press.
Johns, Adrian (1998). *The Nature of the Book: Print and Knowledge in the Making*. Chicago: University of Chicago Press.
Johns, Adrian and Elisabeth Eisenstein (2002). How Revolutionary Was the Print Revolution? *American Historical Review* 107(1):84–6.
Kittler, Friedrich A. (1990). *Discourse Networks, 1800/1900*. Translated by Michael Metteer with Chris Cullens. Stanford, CA: Stanford University Press.
Kohler, Robert E. (1999). Review: The Constructivists' Tool Kit. *Isis* 90(2):329–31.

Kohler, Robert E. (2002). *Landscapes and Labscapes: Exploring the Lab–Field Border in Biology*. Chicago: University of Chicago Press.
Kuhn, Thomas S. (1962). *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.
Kusch, Martin and Peter Lipton (2002a). Testimony. *Special Issue of British Journal for the History of Science* 33(1).
———— (2002b). Testimony: A Primer. *Studies in History and Philosophy of Science* 33(2):209–17.
Latour, Bruno (1987). *Science in Action: How to Follow Scientists and Engineers Through Society*. Milton Keynes: Open University Press.
———— (1999a). *Pandora's Hope: Essays on the Reality of Science Studies*. Cambridge, MA: Harvard University Press.
———— (1999b). *The Pasteurization of France*. Cambridge, MA: Harvard University Press.
Levere, Trevor H. and Richard A. Jarrell, eds. (1974). *A Curious Field-Book: Science and Society in Canadian History*. Toronto: Oxford University Press.
Lindberg, David C. and Ronald L. Numbers, eds. (2003–). *The Cambridge History of Science*. 8 vols. Cambridge: Cambridge University Press.
Livingstone, David N. (2004). *Putting Science in Its Place: Geographies of Scientific Knowledge*. Chicago: University of Chicago Press.
Maier, Pauline, Merrit Roe Smith, Alexander Keyssar, and Daniel J. Kevles (2002). *Inventing America*. 2 vols. New York: Norton.
Marris, Paul and Sue Thornton, eds. (1999). *Media Studies: A Reader*. Edinburgh: Edinburgh University Press.
Miller, David Philip (2002). The 'Sobel Effect'. *Metascience* 11(2):185–200.
Montgomery, Scott L. (2002). *The Chicago Guide to Communicating Science*. Chicago: University of Chicago Press.
Morrell, J.B. (1974). Reflections on the History of Scottish Science. *History of Science* 12(2):81–94.
Morus, Iwan Rhys (1998). *Frankenstein's Children: Electricity, Exhibition, and Experiment in Early-Nineteenth-Century London*. Princeton, NJ: Princeton University Press.
Nye, Mary Jo (2003). *The Modern Physical and Mathematical Sciences*. Ed. by Mary Jo Nye, David C. Lindberg, and Ronald L. Numbers. Vol. 5. The Cambridge History of Science. Cambridge: Cambridge University Press.
Oldroyd, David R. (1984). How Did Darwin Arrive at His Theory: The Secondary Literature to 1982. *History of Science* 22(4):325–27.
Ophir, Adi and Steven Shapin (1991). The Place of Knowledge: A Methodological Survey. *Science in Context* 4(1): 3–21.
Perry, Marvin, Myrna Chase, James R. Jacob, Margaret C. Jacob, and Theodore H. Von Laue (2004). *Western Civilization: Ideas, Politics, and Society*. 7th ed. New York: Houghton Mifflin.
Pickering, Andrew (1984). *Constructing Quarks: A Sociological History of Particle Physics*. Chicago: University of Chicago Press.
———— ed. (1992). *Science as Practice and Culture*. Chicago: Chicago University Press.
Pickstone, John V. (2000). *Ways of Knowing: A New History of Science, Technology, and Medicine*. Chicago: University of Chicago Press.
Pinch, Trevor (1986). *Confronting Nature: The Sociology of Solar-Neutrino Detection*. Reidel.
Porter, Roy (1976). Charles Lyell and the Principles of the History of Geology. *British Journal for the History of Science* 9(2):91–103.
———— (1990). The History of Science and the History of Society. In: *Companion to the History of Modern Science*. Ed. by R.C. Olby, G.N. Cantor, J.R.R. Christie, and M.J.S. Hodge. London: Routledge, 32–46.
Porter, Roy and Mikuláš Teich, eds. (1992). *The Scientific Revolution in National Context*. Cambridge: Cambridge University Press.
Radway, Janice A. (1984). *Reading the Romance: Women, Patriarchy, and Popular Literature*. Chapel Hill: University of North Carolina Press.
Reiss, Benjamin (2001). *The Showman and the Slave: Race, Death, and Memory in Barnum's America*. Cambridge, MA: Harvard University Press.
Report of Council for the Year 1988–89 (1989). *British Journal for the History of Science* 22(4):495–512.
Rosenberg, Charles E. (1970). On Writing the History of American Science. In: *The State of American History Writing*. Ed. by Herbert J. Bass. Chicago: University of Chicago Press, 183–196.
Rossiter, Margaret W. (1975). *The Emergence of Agricultural Science: Justus Liebig and the Americans, 1840–1880*. New Haven, Conn: Yale University Press.
Rudwick, Martin (1985). *The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists*. Chicago: University of Chicago Press.

Schaffer, Simon (1991). The Eighteenth Brumaire of Bruno Latour. *Studies in History and Philosophy of Science* 22(1):175–92.
Schiller, Dan (1996). *Theorizing Communication: A History*. New York: Oxford University Press.
Secord, Anne (1994). Science in the Pub: Artisan Botanists in Early Nineteenth-Century Lancashire. *History of Science* 32:269–315.
Secord, James A. (2000). *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of Vestiges of the Natural History of Creation*. Chicago: University of Chicago Press.
Secord, James A. (guest ed.) (1993). The Big Picture. *Special Issue of the British Journal for the History of Science* 26(4).
Servos, John (1982). A Disciplinary Program That Failed: Wilder D. Bancroft and the Journal of Physical Chemistry, 1896–1933. *Isis* 73(2):207–32.
Shapin, Steven (1994). *A Social History of Truth: Civility and Science in Seventeenth-Century England*. Chicago: University of Chicago Press.
Shapin, Steven and Simon Schaffer (1985). *Leviathan and the Air Pump: Hobbes, Boyle, and the Experimental Life*. Princeton, NJ: Princeton University Press.
Smith, Pamela H. (2004). *The Body of the Artisan: Art and Experience in the Scientific Revolution*. Chicago: University of Chicago Press.
Sobel, Dava (1995). *Longitude: The True Story of a Lone Genius Who Solved the Greatest Scientific Problem of His Time*. New York: Walker.
Special Issue on "Science Popularization" (1994). *History of Science* 32(3):237–360.
Staley, Richard (Aug. 2004). "The Co-creation of Classical and Modern Physics". Paper delivered at BSHS/CSHPS/HSS meeting in Halifax.
Star, Susan Leigh and James R. Griesemer (1989). Institutional Ecology, 'Translations,' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907–1939. *Social Studies of Science* 19(3):387–420.
Stone, Lawrence (1979). The Revival of Narrative: Reflections on a New Old History. *Past and Present* 85:3–24.
Topham, Jonathan R. (2004). Scientific Readers: A View from the Industrial Age. *Isis* 95(431):431–442.
Warwick, Andrew (2003). *Masters of Theory: Cambridge and the Rise of Mathematical Physics*. Chicago: University of Chicago Press.
Westman, Robert (Aug. 2004). "Circulating Theoretical Knowledge: Kepler and Galileo in the Years of Public Silence". Paper delivered at BSHS/CSHPS/HSS meeting in Halifax.
Whitley, Richard (1985). Knowledge Producers and Knowledge Acquirers: Popularisation as a Relation between Scientific Fields and Their Production. In: *Expository Science: Forms and Functions of Popularisation*. Ed. by Terry Shinn and Richard Whitley. Dordrecht: Reidel, 3–28.
Williams, Raymond (1958). *Culture and Society, 1780–1950*. London: Chatto & Windus.
———— (1961). *The Long Revolution*. London: Chatto & Windus.