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The History of Knowledge and the Future of Knowledge Societies

Sven Dupré and Geert Somsen

Summary: The new field of the history of knowledge is often presented as a mere expansion of the history of science. We argue that it has a greater ambition. The re-definition of the historiographical domain of the history of knowledge urges us to ask new questions about the boundaries, hierarchies, and mutual constitution of different types of knowledge as well as the role and assessment of failure and ignorance in making knowledge. These issues have pertinence in the current climate where expertise is increasingly questioned and authority seems to lose its ground. Illustrated with examples from recent historiography of the sixteenth to twentieth centuries, we indicate some fruitful new avenues for research in the history of knowledge. Taken together, we hope that they will show that the history of knowledge could build the expertise required by the challenges of twenty-first century knowledge societies, just like the history of science, throughout its development as a discipline in the twentieth century, responded to the demands posed by science and society.

Keywords: artisanal knowledge, history of knowledge, history of science, learned societies

Introduction[1]

The new field of the history of knowledge is often presented as a mere expansion of the history of science. It has been portrayed as nothing more than making explicit the broadening of the scope of the history of science, which the discipline has undergone anyway since the turn to practice in the 1970s. On that reading, the scope of the history of knowledge is essentially the same as that of the new his-

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tory of science, which now also includes, without much contention, for example, non-Western and artisanal knowledge practices. It is noteworthy that even proponents of the history of knowledge point out that the history of science has been leading the way. In terms of the future prospects of the discipline of the history of knowledge, Peter Burke envisions a global approach (concentrating on "knowledge on the move," "encounters, clashes, translations and hybridizations"), a social approach (which would entail "a history of knowledge from below" and "everyday knowledges, including the tacit, bodily knowledges … not only in crafts such as metal-working but also in fields as diverse as diplomacy, trade, connoisseurship, management and sport"), and a concern with the long term (focusing on the use of language and the invention of writing systems, for example).[2] Yet, while anthropologists, archaeologists, and global historians have much to say on these approaches, it is fair to say that they nevertheless emerged from within the history of science ("as has so often happened," according to Burke). Lorraine Daston observed that the history of knowledge is currently used for two different research agendas: one that focuses on "forms of knowledge that have historically been denigrated as substandard" (such as the skills of artisans), the other on the history of learning and the humanities, which is primarily practiced by highly educated elites and has little in common with the activities of craftsmen.[3] The only thing these two research programs seem to share is that they are not about science, and thus, the history of knowledge is defined only in a negative way.

We do not disagree with the broadening of the scope of history of science; in fact, we welcome the expansion of the field of the history of science to the humanities, to science in non-Western contexts and to vernacular knowledge, and acknowledge that the history of knowledge is made possible and should build upon this recent work in the field of the history of science.[4] However, we argue that the carving out of the new field of the history of knowledge serves more ambitious aims than mere expansion. More precisely, we claim that these aims *should* be more ambitious in direct response to the challenges posed to us by present-day knowledge societies in which expertise, whether scientific or not, is widely contested, and in which the value of different forms of knowledge is fluid. The redefinition of the historiographical domain of the history of knowledge allows and urges us to ask new questions concerning the boundaries, hierarchies, and mutual constitution of different types of knowledge as well as to the role and assessment of failure and ignorance in making knowledge. These issues have pertinence in the current climate where expertise is relentlessly questioned and authority often seems to lose its ground. As a recent exchange between Martin Mulsow and Lorraine Daston shows, the history of knowledge, primarily defined as being about epistemic hierarchies and their dynamics, can offer insights into these current societal challenges.[5]

In this paper, we will indicate some fruitful new avenues for research in the history of knowledge illustrated by examples from the sixteenth to twentieth centu-

[2] Burke 2015, on 102–103.
[3] Daston 2017, on 143.
[4] For the consequences of this expansion for the history of early modern science, see Smith 2009.
[5] Mulsow and Daston 2019.

ries. Taken together, we hope that they will show that the history of knowledge could build the expertise required by the challenges of the twenty-first century. In itself, such responsiveness is not new. In fact, the discipline of the history of science has always developed in response to the demands posed by science and society, ever since its emergence in the early twentieth century. We illustrate this in the next section, showing how over this period, the field evolved from a context of *belle époque* positivism via post-war scientism to Cold War modernism and, finally, 1970s relativism—always in reaction to changing societal circumstances and environments.

History of Science in Context

History of science was never established (or sustained) for its "intrinsic value." From its start as an academic field, it received support because it seemed valuable for specific purposes and because it seemed to offer an answer to issues of the time. If anyone was aware of this, it was the field's most tireless organizer in the early twentieth century, George Sarton. Sarton believed that history of science should exist as an autonomous discipline, worthy in and of itself, yet he knew he had to show what this existence was *for* if he was ever going to make this possible. Throughout his career he publicly pronounced the field's aims, calling attention to various *raisons-d'être*: History of science could help "humanize" the sciences by showing their track record as a human achievement. It could show the "unity of mankind" by science's demonstrable universality. And it could reveal the "secret history" of steady civilizational progress that regular historians failed to see in their focus on war and conquest, bloodshed and power grab.[6]

All these reasons pointed to a general cultural value that appealed to the liberal positivist milieu of Sarton's *belle époque* Belgium.[7] But when he moved to the US in 1915, he entered a wholly different context where his arguments no longer had the same traction. In fact, despite his success running *Isis* and setting up the History of Science Society, Sarton received remarkably little material support for his enterprise. He owed his position at Harvard to the Carnegie Institution of Washington whose director liked his broad view of science (that he missed among the scientists that he normally funded).[8] But the university itself was quite uncommitted, paid no salary, and provided no chair—only a windowless space in the Widener Library and a small annual fee for teaching an undergraduate course.[9] Sarton made waves, but his vision of the field remained unrealised for most of his career.

The first steps toward academic institutionalization only came a generation later, and again not for the field's intrinsic value, but for its educational function. James Bryant Conant, former overseer of the Manhattan Project and long-time

[6] Sarton gave these three reasons in his manifesto "The New Humanism" (1924): 9–42, on 16, 24 ff., 33, based on the French original of 1918. Elsewhere he made still different appeals, the earliest promising to discover the "laws of [scientific] development" by recording the pace of discovery over time: "L'Histoire de la Science", *Revue Générale de la Science* 23 (1912): 93–94, on 94.

[7] Wils, 2005.

[8] See Sarton's correspondence with this director, Robert Simpson Woodward, between 1916 and 1919. Harvard University, Houghton Library, Ms Am 1803, 2045 and 762.

[9] Cohen 1956; Conant 1956.

Harvard president (as well as Sarton student) made history of science the core of the general science education programme.[10] *All* students, especially those *not* majoring in the natural sciences, *must* acquire an understanding of what science is and what it "can and cannot accomplish."[11] And they should learn this via "case histories," historical vignettes showing aspects of the scientific method. In Conant's hands, the field suddenly acquired a central position on the post-war campus, and this was possible because it catered to his widely shared conviction that science was what defined the modern world, and that in order to understand that world, and take a leading position in it, one had to grasp how science functioned. History of science could provide that grasp.

Elsewhere, leading historians of science also managed to institutionalize their field but usually through a different approach. Alexandre Koyré in Paris and Herbert Butterfield in Cambridge, UK, sought to understand modernity not through (essentially timeless) case studies but by locating it in a single historical moment, a shift in mentality, an "intellectual 'mutation' of which modern physical science is at once the expression and the fruit."[12] This Big Bang, of course, was the Scientific Revolution, a concept that they launched and made the focus of their newly professionalized field. But what sustained that field and its professionalization was the same conviction that Conant had catered to: that the modern world was made by science, and that history of science was the key to grasping this.

What made the latter approach prevail, however, was another asset: its stress on the intellectual character of the history of science. This stood in contrast to a conception of science as a problem-solver, an instrument serving the practical needs of society. Such a view had been promoted by Marxist science historians like Boris Hessen and J. D. Bernal who used historical arguments to advocate Soviet-style scientific planning. This had had appeal during the Great Depression and the war years, but its communist connotations quickly made it suspect during the Cold War. As Anna Mayer has shown, the appointment of the first history of science professor at Cambridge followed precisely this path: promoted by Marxist scientists in the 1930s, the chair finally fell to an intellectual historian after the war. History of science now served anti-communist ends.[13]

For the span of their generation, then, the first professional historians of science taught that science was a particular way of thinking, born out of the Scientific Revolution, laying the foundations of the modern world. But this modernity was strictly of a Western kind. Charles Gillispie openly worried about what would happen if science's "instruments" (read: the atomic bomb) would fall into the hands of "men not of the West."[14] And Henry Kissinger concluded that nations who had missed the "impact of Newtonian thinking" were geopolitically second-rate.[15] The field's master-narrative hence fit a Cold War perspective with science as a unique mentality, marking the difference between West and East, developed and undeveloped.

[10] Hamlin 2016.
[11] Conant 1957, on vii; Jewett 2012, on 256 ff.
[12] Koyré 1989, on 120.
[13] Mayer 2000.
[14] Gillispie 1960, on 9.
[15] Kissinger 1966, on 528.

This perspective was never without its critics, but in the course of the 1960s and 70s doubts started to dominate. Thomas Kuhn, teaching on Conant's case-history programme, began to wonder whether Western science was really so different from Soviet indoctrination.[16] Scholars around Joseph Needham gave a Marxist interpretation to the rise of Western science itself. And more and more students became critical of the notion of science as pure thought, tending to what vaguely became known as a "social history of science." Something like it was institutionalized in the University of Pennsylvania's History and Sociology of Science programme (starting in 1970), in the new *Social Studies of Science* journal (1975), and for instance in the *maatschappelijke aspecten* movement in the Netherlands.[17] These new sensibilities found their most systematic and critical elaboration in the field of Sociology of Scientific Knowledge (SSK) launched in Britain.

What sustained this shift was not an anti-science sentiment but a growing aversion to viewing science as singular, exalted, and rarefied. Much of the student revolt of 1968 and after was directed against the "ivory tower" (even though such academic isolation rarely existed in reality) and claimed that scientists had a "social responsibility".[18] In history of science the new buzzwords were "practice" and "context," shifting focus to what scientists did (often manually) and how they related to their world.[19] Such "lowering the tone" had the effect of relativizing the power of expertise, even if not denying it—one of SSK's founders, Harry Collins, would later call for *more* trust in experts.[20] But, as his fellow-founder David Bloor pointed out, the new approach was like biblical criticism: it did not necessarily undermine belief in science but its sacred aura.[21]

Over the course of the twentieth century, then, history of science evolved in response to changing circumstances: from *belle époque* positivism via post-war scientism to Cold War modernism and, finally, 1970s relativism. Today, we live in different times again. There is no need to reject our field's last shift—the focus on practice and context and subsequent developments have made it as rich as it is now. But we have to acknowledge that the circumstances in which we operate have markedly changed. Instead of a sacralization of science we find its regular (if qualified) denouncement; instead of the veneration of expertise we find it being incessantly questioned. This does not mean that we want to return to the views of our field of the Cold War period. Nor should it be our task to defend science.[22] Rather, we suggest that the new fluidity of cognitive authority raises questions as to the hierarchy of knowledge and its historical development. How have different kinds of knowledge defined each other, been valued, contrasted, and ranked? How has the epistemological map changed? What accounts for these shifts and their (temporary) stabilization? In other words: What does a history of knowledge look like?

[16] Reisch 2012; Reisch 2019.
[17] Rip and Boeker 1975.
[18] Shapin 2012.
[19] Golinski 1990.
[20] Shapin 2010; Collins and Evans 2007.
[21] Bloor 1991, on 183–185.
[22] For discussions on the same topic within STS, see a series of articles in *Social Studies of Science* 47 (2017), on 3–6 and 4 (2017) on 580–586, 587–592, and 593–599.

In the following two sections, respectively focusing on early modern and modern history, we show the potential of such questions, tapping our own expertise and research as well as emerging historiographies in the history of science and related fields. We focus on such categories as ignorance, failure, pseudoscience, and discipline because they allow us to raise questions around the boundaries, hierarchies, and mutual constitution of different types of knowledge which have pertinence in our present-day societies, where expertise and authority are relentlessly questioned.

Failure and Ignorance

Given the current fluidity of cognitive authority, one important point of focus of attention for the history of knowledge is the role and assessment of failure and ignorance in the production of knowledge. Failure and ignorance point to less highly valued forms of knowledge as well as weaknesses in ways of knowing. Yet, historians of knowledge would argue they are inherent to processes of knowledge-making, even if it concerns types of knowledge which, historically speaking, at one point in time and in one place, were considered to be at the top of the ever-shifting hierarchy. The history of knowledge has emergent historiographies of precarity and ignorance in the early modern period to build upon. Martin Mulsow has argued for the importance of the category of precarious knowledge; he has shown how the recognition of the precarity of knowledge leads to questions about the security and loss of knowledge as well as to research on strategies of preservation.[23] Likewise, he has pointed to the significance of strategies of how to deal with not knowing, be it developed by a ruler at the early modern court in Gotha in relation to investments in alchemical transmutation, or during imperialist and colonial venturing into unknown distant lands.[24] A history of knowledge pays as much attention to the "dark side of knowledge"; it explicitly deals with historical typologies of ignorance and historical strategies of dealing with unknowns.[25]

Here we will briefly discuss failure, which has recently received quite a bit of attention (and media coverage) although, or perhaps just because, contemporary science and society are obsessed with success. Johannes Haushofer, Princeton professor of psychology, published his CV of failures, and other scholars have followed him in this to show how important failure has been to their careers and processes of learning. Helsingborg, Sweden, is home to a Museum of Failure, which houses a collection of more than a hundred failed products from some of the world's best-known companies. A museum like this one wants to convey that failure is essential to innovation and business success, a conviction which is shared by the Maastricht University Institute of Brilliant Failures.[26] In the history of science and technology failure plays a rather minor role. Graeme Gooday has aptly observed that in the history of technology, failure has typically been used to cate-

[23] Mulsow 2012.
[24] Mulsow and Daston 2019, on 168–169.
[25] Zwierlein 2016.
[26] Iske 2018.

gorize "pathological" technologies that clearly demarcate them from successes.[27] Nevertheless, a historiography of failure, and related concepts and words such as "error" and "mistake", is emerging. The building of a historical typology of failure in the early modern period is part of this ongoing endeavour. It could demarcate errors from mistakes, for example, along lines of distinction between thought and action, or between the absence or presence of rules or norms, while recognizing at the same time that, historically, the distinction between error and mistake is fluid and that different word categories were used in various ways.[28] A history of knowledge can pay attention to error and failure if it is written from the perspective of epistemic virtues. Klara Vanek has shown, for example, how avoidance of error was tied to the definition of the *bonus chirurgus* in the early modern period and how this was linked to debates about the value of anatomical, surgical, and medical knowledge as well as epistemic hierarchies of hand and mind.[29] Another approach, perhaps even more relevant in light of current debates, might be to historicize the epistemic value of failure. One of us has traced elsewhere the rise of a "poetics of failure" in the early modern sciences.[30]

Such an approach to the history of knowledge can take recourse from much older historiographies in other disciplines, which regain currency, or, like some of Michael Polanyi's notions, have never been absent from the history of science and technology since the 1970s. Another historiography, that of craft theory, treats failure as a mundane occurrence in technological design. In his theory on the nature of design and craft, woodworker and professor at the Royal College of Art, David Pye argues that design cannot be failure-free.[31] A technology cannot meet all requirements, and is always based upon a compromise between design requirements to be fulfilled in order to create an ideal object. According to Pye, such a compromise is always a sort of failure. Failure is then unavoidably ubiquitous in all design and technology. Even if it were possible for a technology to succeed at any moment, later users would come with different requirements, again making failure inevitable. Different from Pye, Michael Polanyi sees failure as an inevitable step towards success. In connection to "rules of skill and connoisseurship which comprise important technical processes," Polanyi speaks of "the usual process of unconscious trial and error by which we feel our way to success and may continue to improve on our success without specifiably knowing how we do it."[32] Historically speaking, early modern artisans knew that one could only learn by doing, and that this meant making mistakes. It is this epistemic value of error and failure which the French potter Bernard Palissy and so many others voiced when expressing scepticism about the didactic value of their own writings. Famously, Palissy made an allegoric figure called "Practice" reluctant to tell "Theory" the secret of white enamel. Palissy had "Practice" say that this was not a refusal for economic

[27] Gooday 1998.
[28] A selection which deals with the issue of typology: Rigolot 2004; Neumaier 2010; Gadebusch Bondio and Paravicini Bagliani 2012.
[29] Vanek 2014.
[30] The next paragraphs summarize an argument more fully developed in Epple, Müller, and Warner, forthcoming.
[31] Pye 1978, on 70.
[32] Polanyi 2005, on 65.

reasons, but simply because words were an ineffective way to learn a craft. "Even if I used a thousand reams of paper to write down all the accidents that have happened to me in learning this art," Practice says, "you must be assured that, however good a brain you may have, you will still make a thousand mistakes, which cannot be learned from writings, and even if you had them in writing you wouldn't believe them until practice has given you a thousand afflictions."[33] Only long and sustained experience, including the making of mistakes inherent to the apprenticeship, leads to the acquisition of knowledge.

However, while artisans recognized the epistemic value of failure, this did not necessarily translate to all genres of artisanal writings. In fact, a poetics of failure characterizes those artisanal writings which we might call "manuals", in the sense that they claim to serve learning a craft, whether it be surgery or goldsmithing, in opposition to (most famously) the *Encyclopédie*, in which Diderot followed a logic of representation in his description of the arts, and which was not intended to be used in the context of instruction. One example of such a manual is the eighteenth-century *Guidebook for Upcoming Gold- and Silversmiths* (1721) by the Dutch silversmith Willem van Laer. Van Laer's guide presents itself as a sort of structured curriculum for the apprentice, although it is in no way intended to replace, but rather to complement, hands-on education on the workshop floor.[34] Van Laer wrote down his instructions and descriptions of techniques in ways suggesting alternative histories of his own failures. A typical pattern is that van Laer suggests that a certain way of proceeding would fail and the result could be potentially disastrous. One example is his suggestion that, without the preparation of the Brussels sand to make the mould, the cast will be undesirably "rough". In his book, failures are ubiquitous, and he regularly includes extensive trouble-shooting sections, for example in his discussion of soldering. As a master silversmith, van Laer describes failures only to suggest how to correct them, but it is clear that the failures he describes are based upon his own workshop experience.

At the beginning of the seventeenth century, the epistemic value of failure became recognized in the world of scholarship, from the mathematical sciences to natural history. The dissatisfaction with the ways of writing down knowledge hiding the imperfection of the process of knowledge production, in conjunction with a belief in the open-endedness of processes of knowledge-making, became widely shared. Johannes Kepler's poetics of science,[35] for instance, is in fact a poetics of failure. In several of his books in the broader field of mathematics, Kepler presented his knowledge as a narrative of the historical development of his own paths of inquiry. One example is his presentation in the "Paralipomena" of his investigation of the measure of refraction (or what came to be known as Snell's law) taken from his *Optics*. Kepler's narrative of his research into refraction consists of three approaches which he tried out and, in the end, all failed to different degrees. Kepler started with the data which he had received from the measurements of atmospheric refraction by Tycho Brahe and Christoph Rothmann and the tables of refraction which he gathered from medieval optics. This approach failed, he tells

[33] Palissy, 1957.
[34] Hagendijk 2018.
[35] Hallyn 1990.

his reader, and characterizing his first strategy of discovery of the measure of refraction as "an almost blind plan of enquiry," he switched gear and moved on to a second method. His second path of investigation was fuelled by analogies between refraction and reflection, which however productive, also failed in delivering the measure of refraction that Kepler had hoped for. He moved on to his third path of investigation in which he thought through his considerations of the causes of refraction. This third way allowed Kepler to discover a constant relation between angles of incidence and angles of refraction, which only held for angles smaller than thirty degrees, and thus also fell short of his objective of the discovery of the measure of refraction.

In sum, Kepler's historical account of his paths of investigation, characterizing them as failures, was a strategy to cope with the imperfections of knowledge-making, opening it up for correction and improvement. Kepler recognized the significance of learning from failure. One could argue that it was the value of failure which the likes of Kepler, developing new ways of knowing in the sciences at the beginning of the seventeenth century, adopted from the world of artisans. It is telling that recent work on medical ethics—on how to deal with errors in medical practice—explicitly harks back to the work of Albrecht Dürer as a source of inspiration and a model of the recognition of the imperfections of knowledge and the ideal of openness.[36] It thus seems that artisanal knowledge remains a source for the recognition of the epistemic value of failure to this day, in the same way that it was around the year 1600. As such, a history of knowledge, even in the early modern period, gives significance to current concerns in science and society about the value of various ways of knowing.

Late-Modern Cartographies of Knowledge

While history of knowledge sheds new light on early-modern knowledge-making, and its pertinence for current practices, its approach is no less fruitful for later periods. Some recent historiography (advertently or not) sustains that view. What sets the period from the nineteenth century onward apart from earlier eras is that the cartography of knowledge was more firmly institutionalized in systems of disciplines and professions. But this does not mean that such arrangements were fixed—in fact they kept changing, and were never all-inclusive, uncontested, or without tension. Epistemic hierarchies became more entrenched yet continued to shift.

Perhaps this is most easily visible in the outer boundaries of science, which were once seen as set or taken for granted.[37] But claims of what is and what is not science have always been subject to debate. Thomas Gieryn has called this contestation "boundary work" and his own case studies are vintage history of knowledge.[38] One thing they show is how ambiguous scientific border control could be.

[36] Gadebusch Bondio 2012, on 295–296.

[37] For example, universalizing histories of science, such as Sarton's or Needham's, had to fix what was and what was not science. Their "many rivers, one sea" picture of contributions to the global stock of knowledge hinged on sharp delineations of what could count as such a contribution.

[38] Gieryn 1999.

In his defence of professional science, for example, John Tyndall stressed its empirical character against theology and its theoretical nature versus the mechanical arts.[39] Tyndall's flip-flopping suggests that demarcating was less about defining science than about identifying its 'Others'. Other work reveals how permeable the outer boundary could be (see for example a recent set of studies on the surprisingly manifold relations of the sciences to the humanities).[40] Nor did only binary juxtapositions exist. Mid-twentieth century philosophers of science, for instance, claimed it was *their* task to distinguish science from non-science, creating a triangle of disciplinary relations. History of knowledge invites us to ask what was at stake in such rearrangements, and why they happened when they did.

Recently, Michael Gordin has enriched the study of boundary work by analysing and historicizing the phenomenon of "pseudoscience".[41] Always a contested category (no pseudoscientist ever identified as such), its meaning has often closely followed science's self-understanding. As Gordin's case of Immanuel Velikovsky's controversial cosmography shows, in its attempts at recognition such would-be science tends to take on the trappings of the real thing: falsifiability, peer review, naturalistic explanations, whatever counts at the moment. But precisely because of this imitation, pseudoscience can never be presumed identifiable on the basis of its own distinguishing characteristics.[42] Pseudoscience is not an alternative to regular science, it is its shadow, following it, resembling it, and never disappearing.

Boundary-drawing of science versus other categories of knowledge is not only about professional relationships, it can also have geopolitical implications. In an eye-opening study, Marwa Elshakry investigated where the notion of science as Western has come from.[43] Not merely from Cold War Anglo-Saxon historians, she argues, but to a large extent also from scholars "not of the West." In nineteenth-century Egypt and China, she shows, bits of knowledge and news of discovery from Europe were readily absorbed into local traditions of scholarship without much distinction. The Chinese term *gezhixue* included both, as did the Arabic *'ilm.* Only with rising reservations about the blessings of the European imports, and Spenglerian notions of a decline of the West, did a juxtaposition of "traditional" to "modern," and of "Arab" and "Chinese" to "Western," science emerge, this "Western science" being considered at the same time local in origin and universal in aspiration. Elshakry adds to her analysis that she could not have made it within the confines of an unproblematized history of science precisely because the definition of its object itself is at stake. But a history of knowledge per-

[39] Gieryn 1999, on 37–64.
[40] Krämer 2018.
[41] Gordin 2012.
[42] Steven Shapin has suggested one potential identifier: trying too hard. "Beware of hyperscience. It can be a sign that something isn't kosher. A rule of thumb for sound inference has always been that if it looks like a duck, swims like a duck and quacks like a duck, then it probably is a duck. But there's a corollary: if it struts around the barnyard loudly protesting that it's a duck, that it possesses the very essence of duckness, that it's more authentically a duck than all those other orange-billed, web-footed, swimming fowl, then you've got a right to be suspicious: this duck may be a quack." See Shapin 2012, on 38.
[43] Elshakry 2010.

spective allows her to ask the kinds of questions where "science" is but one of several categories of varying meaning and translatability.

A history of knowledge does not need to limit itself to the delineation of "science," however. Maps of knowledge have many borders, provinces, and regions, in and out of science, and changing over time. Specialization has naturally increased the number of fiefdoms, but mergers have also occurred, such as solid-state physics (combining existing branches in an interdisciplinary way) and evolutionary biology (adding an umbrella). Moreover, some fields of knowledge have dwindled or disappeared altogether (think of phrenology or colloid chemistry), some have become "service disciplines" (statistics, nursing), and still other branches are of a not fully scientific status (physiotherapy, car mechanics). There is nothing fixed about these cartographical hierarchies (nurses used to *aspire* to the status of *doctores medicinae*, but simply lost this battle), at least for the time being. Historicizing the dynamics of such interdisciplinary relations is precisely what a history of knowledge can do.

Such a project can very fruitfully borrow from the sociology of professions, particularly approaches developed since the 1980s by Andrew Abbott and subsequent scholars.[44] Abbott sees professions in permanent competition for what he calls "jurisdiction" over certain tasks, that is, the right to perform them. Only surgeons can legally perform operations, for instance, yet ophthalmologists have come to share their monopoly on eye measurement with opticians. In Abbott's frame, battles over jurisdiction are fought out in different arenas (public media, the courtroom, the work floor), they employ different strategies (such as reducing weather forecasting to physics), and they lead to various settlement options. Abbott's turf wars are not all-or-nothing. They can also lead to cooperation, division of territory, subordination, or diarchy. This richness makes his approach extremely fruitful for historians of knowledge, particularly those working on the system of disciplines that governs modern maps of knowledge.

Another angle on epistemic hierarchies and their changing institutionalization involves looking at bricks and mortar. Already in the 1980s Sophie Forgan pointed out what the architecture of learned societies can reveal about their external relations. In Victorian Britain, a neoclassical design was often chosen to confer authority and permanence, while a Tudor style invoked openness to working class participation.[45] In contemporaneous Dutch contexts, neo-Renaissance was the style of the learned liberal elites, whereas neogothic signalled attempts to reconnect science with the (newly emancipated) Catholic traditions of knowledge.[46] But the system of disciplines perhaps finds its most direct mapping in the lay-out of university campuses. Lorraine Daston has recently suggested to take the centrality of, say, the Institute of Philology seriously, or to consider the relative distance between buildings for history and natural history.[47] Several scholars in Germany and the Netherlands are now looking into campuses as maps of knowledge set in stone creating a topographical history of knowledge.[48]

[44] Abbott 1988.
[45] Sophie Forgan has written an entire oeuvre on the subject. The piece cited here is Forgan 1986.
[46] Weerdenburg 1994.
[47] Daston 2017, on 146–147.
[48] Forthcoming work by Fabian Krämer, Ab Flipse and Abel Streefland.

Finally, history of knowledge does not need to be confined to epistemic cartography. It can equally fruitfully ask what constitutes knowledge in the first place and historicize that question. In a recent book Alex Csiszar does exactly that, seeking answers, fascinatingly, in the history of scientific publishing.[49] Today the standard form of a legitimate claim to knowledge is the scientific research article. Discoveries may also be communicated by email, press release, or PowerPoint presentation, but none of these settles both claim and author as firmly as a journal publication. Yet this wasn't always so. Before the 1830s, it was academy proceedings and book-length treatises that carried this weight, while journals were part of the loud and unruly world of the popular press. When this format preference changed, so did the location of epistemic authority, as well as conceptions of authorship and scientific productivity. All came together in the changing definitions of the basic building blocks of knowledge. Hence, Csiszar's work can very well be read as a history of knowledge, and a timely one at that. As the author is well aware, questions of format, control, and authority are highly topical again now that subscription journals are losing control and scientific news and data are increasingly shared across new platforms. Such fluidity of epistemic categories calls for a history of knowledge.

Conclusion

History of knowledge does not just expand the boundaries of history of science, it investigates those very boundaries, between science and other forms of knowledge, and between different forms of science and different forms of other knowledge, in all possible combinations. Moreover, it asks questions about basic epistemic categories, their interrelations and mutual constitution, again both within and outside of science. What is a contribution to knowledge? What is a failed attempt at such? How is such failure evaluated? How does knowledge relate to ignorance? History of knowledge historicizes such questions and asks why particular answers were given at particular times. At the same time, history of knowledge responds to current concerns, just as history of science has done throughout the twentieth century. In today's climate of fluid cognitive authority, contested expertise, and changing forms of knowledge production and communication, history of knowledge can provide a deeper understanding. It is the field for the twenty-first century. It is history that matters to the future of knowledge societies.

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[49] Csiszar 2018.

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