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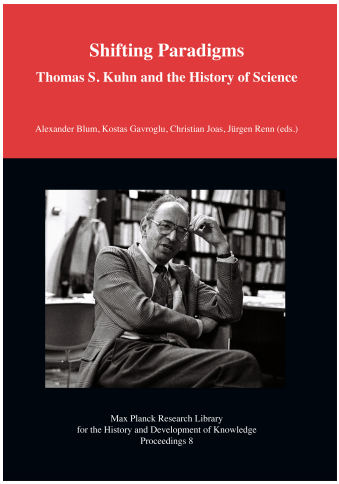
Where to Start?



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Where to Start?

John L. Heilbron

I want to thank the organizers for their generosity and their courage in asking me to open our useful and timely workshop. Not wanting to abuse the opportunity, I'll begin by asserting a proposition to which, as I suppose from your presence here, you all assent. Here it is: A better knowledge of the history of our discipline can help to resolve the identity crises that periodically afflict us and, perhaps, help us also to specify what, if anything, people who consider themselves historians of science have in common. Even a fuzzy specification can have its practical uses in suggesting curricula and defending territory within the institutions that support our work.[1]

History of Science and the Science of History

At first glance the task seems futile. Consider only the breadth of subjects slated for discussion at our roundtables and the proliferation of sub-fields reviewed in the *Isis* critical bibliographies. There are at least two signs, however, that point to a more hopeful prognosis. For one, the great expansion of our field, as measured by the number of entries in the *Isis Critical Bibliographies*, may have stabilized. After a big drop owing to changes in editors and editorial policy around 2000, they are tending towards, and perhaps will not exceed, their average in the 1990s.[2] The second hopeful sign is the selection of topics for the roundtables to begin tomorrow. Most of these topics are of the form "Science and X," where X equals science, philosophy, material culture, Eurocentrism, institutions and Thomas Kuhn. We do not have a provision for X = history. I take this omission as an indication that the organizers know that the history of science *is* history.

I believe that that was Kuhn's position too although his usual status as anguished outsider made him feel keenly the resistance of some general historians to our admission to their number. He attributed their resistance to the natural dislike

[1] The following text is a slight amplification of the opening talk at the Workshop, "Towards a History of the History of Science: 50 Years since *Structure*," organized by the Max Planck Institute for the History of Science, 17 October 2012. I am grateful to the editors for allowing me to retain the informal character of the original presentation.

[2] Entries remained flat at around 3000 between 1970 and 1985, and increased by 40%, to 4200 on average, in the 1990s.

of mathematics by people fond of history and to the persistence among them of a belief in a method that advanced science without any interesting intervention by human beings. Since he thought that the lessons of *Structure* had made this belief untenable, he regarded those who clung to it much as the old positivist historians had the Simplicios of earlier times. They were only a passing irritation, however, since eventually they would go the way of all Simplicios opposed to progressive paradigms. The two-culture problem, however, the antipathy of historians in general to science whatever its methods, was a far more serious problem. "In my depressed moments, I sometimes fear that the history of science may yet be that problem's victim." Kuhn meant that swelling our ranks with recruits who devoted themselves to external history would kill the true history of science while papering over the chasm between the cultures (Kuhn 1977, 160–161). This expression of foreboding dates from 1971. The history of our discipline that we are to construct will help us judge how far, if at all, Kuhn's bleak forecast has been realized.

Meanwhile, let us be content to know that history of science is history. It is not an inter-discipline, nor, I hope, an interim discipline. It has no special or preferred tie to philosophy, theology, sociology or political economy, although, as historians, some of us require some knowledge of one or more of them; as, indeed, we also do of art, literature, music, everyday life, in short, anything and everything that enables us to reconstruct the history of humankind's struggle to grasp, adapt to, and manipulate the natural world. We need not be overly concerned to draw boundaries among our sub-specialties or between history of science and general history. What should concern us is the scientific side of our business, by which I do not mean the sciences we study, but our standards of historical investigation and writing—the level of argument and evidence, and the control of technique, bibliography and languages, expected by and from professional historians.

If you grant this reasonable position, it follows that the historiography in which we should try to locate our own is the development of history as a science. The question whether or how far history can be considered a science is an old one. History itself gives the answer. Considered as a body of knowledge accumulated and upgraded by continually improving technique and ever-widening coverage, modern history is as much a science as modern physics. The two were begotten in the same scientific revolution and turned in parallel from reliance on ancient authorities to authentic documents. At the time that natural science learned to make instruments and experiments, history took up with charters, coins, medals, seals and inscriptions. Newton's *Principia* and Jean Mabillon's *De re diplomatica* were coeval—which does not mean equally bad. During the eighteenth and nineteenth centuries, the standards of evidence, reporting, testing and teaching rose

rapidly in both the historical and natural sciences, and sometimes, as in the invention of the seminar and the institute, and in the study of meteorology, metrology, chronology and geography, they borrowed fruitfully from one another.

At the beginning of the twentieth century natural scientists and historians unselfconsciously referred to their endeavors in the same terms. As an example, I offer you two quotations, one from a physicist, the other from a historian, each a leader in his field. It is not easy to guess which is which:

1. "It seems probable that most of the grand underlying principles of [our science] have been firmly established and that further advances are to be sought chiefly in the rigorous application of those principles to all the phenomena which come under our notice."
2. "Ultimate [science] we cannot have in this generation, but […] all information is [now] within reach, and every problem has become capable of solution."

The first quotation comes from A. A. Michelson's speech at the dedication of the physical laboratories of the University of Chicago in 1894. The second comes from Lord Acton's report of 1896 on the status of *The Cambridge Modern History*, of which he was editor.[3] Acton's claim that history belongs among the sciences, with its echo of the practice of his master Leopold Ranke, was by no means unique in England (Lord Acton 1960, 26, 32–34). Everyone in Oxford remembers the conclusion of J. B. Bury's address at his inauguration as Regius Professor in 1904: "[history] is simply a science, no less and no more."[4]

Let us agree that history is some sort of science and history of science some sort of history. Then the question that brings us together, the question how our field has developed during the last half-century or so, should be related to the development of general history over the period. We should not be narcissistic or provincial in our efforts to define our field or faddist in our ideas about its core subjects and problems. It may be that we can learn something about answering our questions from the general historians and friendly philosophers who have been discussing and refining them for 400 or 500 years.

The *terminus a quo*

The subject of our meeting—the development of our field since *Structure*—does not make a perfect period for the historiographer. A better start date would be the years around 1900. We still depend on the work of the scientist-historians of that time and some of our major projects follow their lead. Consider only

[3] A. A. Michelson, quoted by Rescher (1978, 33), and Lord Acton, quoted by Carr (1961, 3).
[4] Quoted by Burrow (2007, 205).

the edition of Galileo's *Opere* by Antonio Favaro, published in 20 folio volumes between 1890 and 1910, which, together with the many special studies he spun from it, continues to support scholarship on the period of the Scientific Revolution. Favaro's approach remains alive in such major enterprises as the exemplary ongoing letter-press edition of Einstein's papers and correspondence. Although it has proceeded at a more deliberate pace than Favaro's, and with greater resources and a larger staff, it has not outdone him. Beginning our account of our field around 1900 would emphasize this essential strand of our heritage and allow us to appreciate its continuation into the new electronic environment. Other sorts of achievements of the old scientist-historians, like the preparation and annotation of Ostwald's *Klassiker der exakten Wissenschaften*, which came out at the rate of ten a year in the 1890s, and the decipherment of Babylonian mathematical texts, which gave the history of exact science a higher antiquity than the Greeks, suggest the range of their contributions to our historiography.

Commencement around 1900 would also allow us to evaluate better how much our conception of our field, its limits and problems, owed and owes to scientists. The division of our discipline into sub-specialties still follows too closely the organization of knowledge current in 1900. Pierre Duhem's explorations of scholastic thought about what looks like questions in classical physics remain influential in accounts of the process that created modern science. The positivist line, represented around 1900 by Ernst Mach's *Mechanics and its Development* and the award of the first chair in history of science at the Collège de France to Comte's followers, combined with Belgian internationalism to create the institutional father of modern history of science, George Sarton. Sarton's establishment of *Isis* just before World War I with the endorsement of several eminent scientist-historians would make a convenient end of the initial period of our historiography. The journal was to make possible the writing of a "truly complete and synthetic" manual of the history of science; to help in the creation of textbooks in science arranged historically; to "contribute to a knowledge of humanity […] and study the means of increasing its intellectual output;" and to "refound, on the deepest and finest historical and scientific bases, the work of Comte." Oh, and also to contribute to world peace and prosperity through the critical study of science, "the only [domain of thought] universally shared" (Sarton 1913, 43, 45).

Another eligible *terminus a quo* is 1930. In contrast with the fin de siècle, when an Acton and a Michelson, a historian and a physicist, could describe their fields in much the same terms and scientists could turn historian without changing their positivist underwear, historians of science of the later period responded to the wider historiographic trends of the depression-ridden 1930s. The decade began with Herbert Butterfield's contribution to general historical methodological in his *Whig Interpretation of History* and with Boris Hessen's disclosure of a Soviet

approach to history of science, which inspired less crude versions by leftist British historian-scientists. At the same time, in quite a different direction, Otto Neurath and other logical positivists championed the idea of a unified science in which history would have a place—when it learned to express itself in the language of physics.[5]

Two new journals with distinctive programs in history of science made their appearance in the decade. *Annals of Science*, which aimed to "illuminate new aspects of political and social history" and to demonstrate that "all Science, all Natural Philosophy, is as purely human a production as Art or Literature, and is equally precious," began life in 1936 under the effective editorship of one of the world's few full-time lecturers in history of science, Douglas McKie of University College, London.[6] *Annals* specialized in the period since the Renaissance and carried the best of the production of the scientist-historians. *The Journal of the History of Ideas* first appeared on New Year's Day 1940. Its editor, Arthur Lovejoy, opened it by decrying departmentalization in the study of the history of ideas, the fad of the social construction of knowledge, faddism in general, and the lowering of standards of research and reasoning incurred by attributing irrational motives too freely to historical actors. This was the Lovejoy whose *Great Chain of Being* (1936) set a pattern for histories of unitary scientific ideas like Max Jammer's *Concepts of Space* (1954), *Concepts of Force* (1957) and *Concepts of Matter* (1961). Among much else of central interest to our field, Lovejoy's journal of ideas carried the entirely opposed but equally brilliant treatments of the Scientific Revolution by Edgar Zilsel and Alexandre Koyré. Finally, Koyré's peculiarly influential *Etudes galiléennes* dates from 1939.[7]

To stay with my theme of the relationship between general history and the history of science, I'll say a few more words on Butterfield and whiggism. He condemned it utterly. It is anathema, "the source of all sins and sophistries in history, starting with […] anachronism." We must not impose present notions on the past and we must not judge historical actors on how closely their behavior and ideas resembled ours (Butterfield 1957, 31–32, 97–98). That is about all most of us know about whig history. But Butterfield was too good a historian to leave it there. He added that no matter how hard you try, you will not avoid whiggism, it is an occupational disease. It is the inevitable consequence of the abridgments that transform note cards into analytic history, and of any narrative that has a beginning and foreseeable end. Because of its progressive character, science lends itself particularly well to whig history.

[5] Cf. Carnap (1959, 165–166).

[6] Knight (1998), p. 156 quoting Harcourt Brown, p. 158 quoting McKie.

[7] Lovejoy (1940, 4–6, 15–19, 21); Stoffel (2000, 39–40); Wiener and Noland (1957, 147–175, 219–280).

Butterfield later tried to show how to mitigate the problem in his account of what used to be the touchstone tableau of our discipline, the Scientific Revolution of the seventeenth, or maybe the sixteenth and seventeenth, centuries. He advised a longer period, 1300 to 1800, and called his book, which dates from 1949, *The Origins of Modern Science*. In it he emphasized the need to attend to the losers, to deal sympathetically with outmoded systems of thought, to keep constantly in mind that historical actors differed from us. But in specifying his task as the identification of "the particular intellectual knots that had to be untied at a given conjuncture," he in effect took his present as the measure of losers and winners, of those who tied knots and those who loosened them. After all the knots were cut or unraveled, there came a revolution that, in Butterfield's ringing words, "outshines everything since the rise of Christianity, reduces the Renaissance and the Reformation to the rank of mere episodes," and rises unique as "the real origin of the modern world and the modern mentality" (Butterfield 1957, vii–viii). Butterfield's performance was impressive. He knew his material, argued cogently, understood the risk of presentism, and yet, wiggle as he would, was whiggish.

The so-called "social turn" in the history of science has the merit of attacking the more obvious forms of whiggism in narrative but often at the expense of abridgments that admit the subtler sorts. The restriction famously intoned, by the authors of *Leviathan and the Air Pump*, that "solutions to the problem of knowledge are solutions to the problem of social order," seems a transparent translation of our concerns about the place of science in government, industry and the military into motives for the behavior of historical actors who had no desire or means to make their contributions to knowledge of any use beyond their own amusement. Perhaps a more gaping abridgment in the same work is the extravagant synecdoche of taking Hobbes as the leader and also the only member of a group who shared his paradigms of science and power.

Returning to the benchmark 1930s, I find in the history of historiography by the notorious Harry Elmer Barnes, Germanophile professor of European history at Columbia University in New York, an unexpectedly balanced view of the relationship of history of science to general history. Writing in 1937, he was eager to enroll our subject among the other new recruits – the histories of art, economics, literature, social institutions and general culture that, in his typically robust formulation, had made the previous fifty years "the most important [period of] historical writing of all time." Barnes reported regretfully that his colleagues had not yet given history of science "favorable or fruitful attention." They soon would have to do so, he warned, if they were to remain faithful to their commitment to tell the full story of modern life. "A generation hence, it may well occupy as much of their attention as the history of constitution-making" (Barnes 1962,

x, 298, 300 (all quotes), 302–308, 331–342). This proved a good forecast if only because historians lost interest in constitution-making.

History of science was just readying itself for promotion to a historical science in 1937. A year earlier the first professor of the history of science at Harvard, who had been waiting in the wings for 20 years, made his appearance stage center. This was Sarton. He published his inaugural address in *Isis* to serve as a milestone against which the progress of the history of science could be measured at other inaugurations to which he confidently looked forward. The main ingredients in his milestone were the rocks he threw at scientists who wrote incompetently on the history of their disciplines. He insisted that scientist-historians must meet standards of accuracy and objectivity, and deploy research techniques, no less demanding than those in force in the natural sciences. Scientists who wrote history (this is Sarton's opinion, not necessarily mine) abandon their standards and relax their rigor from the very first word. The result is worse than useless, since it diminishes the history of science for everyone (Sarton 1936, 3, 11, 16–18).

Sarton's *bêtes-noires* were whig scientists who lacked the historical science, that is, the bibliographical and research techniques, to do more than wrench the most obvious nuggets from the vast mine whence diligent diggers have been quarrying positive knowledge for millennia. These unscientific scientist-historians worked under what I'll call the old historiography or paradigm—in perfect correspondence with Kuhn's usage in *Structure*. Since scientist-historians were in effect the only practitioners of the history of science in existence when Sarton founded *Isis* in 1912, he had asked the best of them, including Favaro and Ostwald, to stand as its godfathers. Now, 25 years later, from the heaven and haven of a Harvard professorship, he declared that they stood in the way of progress. This was primarily a caricature devised for turf wars; Kuhn too was to find it useful; but a historiographer of our field who begins in 1900 would not entertain it for a minute.

To drive out the amateurs, Sarton proposed the establishment of an Institute for the History of Science. Its immediate objective was to produce a few standard works that would raise the level of scholarship so high that dilettante scientists who wrote their histories "with a complete lack of scholarly integrity" would have no serious reader. Behind this barrier, the Institute's staff would take on the preparation of massive and authoritative accounts of "the whole of objective and verifiable knowledge." Arranged hierarchically like the fathers of Bacon's Solomon's House, the staff, all of them humanists, would devote themselves to the study of "the most precious common good of mankind."

This good was the positive systematized knowledge that constituted science. While cleansing his stables, Sarton by no means abandoned the underlying assumption of the old scientist historians who had made their home there: in his

view, the history of science should be devoted to the origins of secure natural knowledge, of facts and the laws that connect them, with no admixture of metaphysics. The material requirements of this non-metaphysical operation were considerable. Sarton's Institute would need an endowment large enough to pursue its investigations in peace, productivity and prosperity in the manner, Sarton suggested, of the Bollandists, who had been writing their stories of the Saints, as free from hagiography as science is from metaphysics, for over 400 years (Sarton 1948, 170–171, 173, 1938, 7–8).

Enter *Structure*

The view of science as systematized positive knowledge was defended most vigorously around the middle of the last century by the logical positivists. One of their main projects was an *International Encyclopedia of Unified Science*. Its second volume, on social science, carried a long essay on the structure of scientific revolutions. The essay's main purpose was to bring what its author called the "new historiography of science" to bear on the philosophy of science, that is, to destroy the foundations of the logical positivism that had initiated the *Encyclopedia*. In so far as it undercut the epistemology of the old historiography, *Structure* made common cause with Sarton's project of expelling amateur scientist-historians from the fold, and freed historians still trapped by the old paradigm that regarded current science as the inevitable product of the dispassionate, logical, unprejudiced, objective human mind. When Kuhn wrote *Structure*, the old paradigm had not yet surrendered to the new; and, like Sarton soliciting endorsements for the infant *Isis*, Kuhn had to seek much of the historical information he needed for his work of destruction from people whose histories he hoped to render obsolete.

By the new history, or new paradigm in the history of science, Kuhn meant the intellectualized approach of Koyré, in which ideas beget ideas immaculately and the historian teases out the knotted evolution of intellectual pedigrees with sympathetic understanding of the intellectual world in which they developed. The new paradigm won the adherence or endorsement of the new leaders of the history of science in the United States—I. B. Cohen, Sam Westfall, Henry Guerlac, Marie Boas, and, of course, Kuhn. But only he stayed true to it. Kuhn believed that his particular strength as a historian was the ability to get inside others' minds, read them, and report back confidently on what he found there. Few of us can or perhaps even wish to practice the disciplined necromancy needed to crawl around in the heads of the dead. Kuhn was perhaps the only student of *Structure* to gain from it the inspiration to compose so severe and narrow a book as *Blackbody Theory and the Quantum Discontinuity, 1894–1912*. Almost everybody who rushed

into the vacuum *Structure* created by evicting philosophers had some social construction to push.

Although Kuhn deplored this unintended result, the advent of the constructivists had the important merit of accelerating the integration of history of science with general history. Koyré's accounts of immaculate conceptions, however useful in distinguishing among ideas, needed incarnation in time and space, in the social circumstances, programs and ambitions of those whose thoughts he analyzed so subtly. No doubt, the wider contextualization brought by the social turn reduced attention to the scientific ideas and constructs that Kuhn took to be the defining subject matter of the history of science. He worried that a sort of Gresham's law would take hold and the bad coin of constructivism drive out the good money of intellectual development. He would not be happy to read in the latest general work on historiography that "the Kuhnian model helped bring about a different kind of history fixed less on the detailed explication of past scientific ideas and more so on their social and cultural contexts" (Woolf 2011, 471). This correct judgment should not be read to mean that the history of science has dissolved into social history. I think that our historiography will show that there is still plenty of the good old coin around.

One way to keep the good money in circulation, to escape the degradation Kuhn deplored, is to brave the criticism of scientists. Just as general historians, especially of modern times, must endure the criticism of informed outsiders, so historians of science have the opportunity of exposing to scientists their reconstructions of episodes about which the scientists think they know something. Sometimes their interventions are salutary. An instructive example is the squabble in the late 1990s over a permanent exhibition of the place of modern science in the United States mounted at the Smithsonian Institution's Museum of American History. Its curators decided to emphasize applied science and especially its deleterious effects on the environment. Pesticides, pollution and weaponry occupied more space than the great discoveries that the scientific societies who paid for the exhibition thought appropriate. The scientists were correct in their criticism if not in their methods. For in their quite appropriate determination to avoid hagiography and include the wider ramifications of science, the curators had lost their balance and left out or downplayed science as most scientists had experienced it.

I take this story as a warning that the autonomy we may achieve by driving scientists, philosophers and other naturally interested people from our historiography of science comes with a risk. The ease of playing tricks on the dead increases with our distance from the time in which our victims lived. There are no professional societies except our own to protect the experiences and self-conceptions of historical actors in the remote past from obliteration by historians too eager to

impose their own views or too lazy to go beyond them. We have a responsibility to the historical actors we create.

This consideration brings me back to the program of Sarton's unfulfilled Institute. The research tools he called for in 1937 have been created in numbers and to standards that he could not have imagined. The *Isis* bibliographies have doubled their size, from 2000 at the time of his death in 1956, to 4000 last year. The *DSB* and *New DSB* have answered his call for a biographical dictionary. He did not foresee archival projects like those mounted in quantum physics and molecular biology in the 1960s or the energetic collecting of the papers of scientists by universities, professional organizations and learned societies. The resources devoured by letterpress editions of the works of Bohr, Darwin, Einstein, Henry, Lichtenberg, and so on would have astonished him. Then of course there is the incomparable research tool of the web and the scanned documents to which it leads of which none of us had an inkling 20 years ago.

Sarton had higher goals than creating the instruments to make a science of the history of science and obtaining for it a dignified place in general history. He wanted to incorporate everything of any value in general history into the history of science. It may be, as he claimed, that the history of science is the history of civilization. Before undertaking to conquer civilization in general, however, we should be clearer than we are about the advantages of such a takeover to other civilized folk. So, again, what *is* our subject matter?

If the current issue of the *British Journal for the History of Science* is any guide, we haven't the slightest idea. The issue is devoted to "transnational science." There is nothing obviously wrong with that. But what is the science transnationalized? We learn from the editors that "science is constructed as a universal and international phenomenon" and that "the production of scientific knowledge should be understood as the result of a struggle between alternative networks competing for durability" (Turchetti, Herran, and Boudia 2012, 331). These assertions are either empty or scary. If science is a phenomenon, how does it differ from moonshine or a talking dog? If scientific knowledge is the result of a struggle between great networks competing to sustain themselves, how does it differ from market share? What is science? Here is the answer given in the conclusion to the collection on transnational science. "'Science' is something that is constantly being deconstructed and redefined, or, more accurately, dissolved" (Pestre 2012, 426).

Let us hope that we may recrystallize our identity through an account of the development of our profession—an account that meets our standards as scientific historians and that does not cause sympathetic bystanders to laugh. I trust that it will disclose that science is not a phenomenon, although it deals with phenomena,

and that it is not a market share, although we may hope to retain and even enhance ours among the many divisions of history.

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