Science as a subject of history

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The 20th century saw great changes in natural science, and even greater changes in the historiography of science. Wilhelm Conrad Roentgen, Jacobus van't Hoff, and Emil von Behring, the Nobel science laureates of 1901, would probably find more that was familiar to them in today's practice of science than our noble ancestors Ernst Mach, Paul Tannery, and Antonio Favaro would in the sociological bent of many of the younger historians of science at work today. And reciprocally, I would guess that whereas all physicists today can identify Roentgen, few historians of science who are not French or specialists in classical antiquity can identify Tannery.

Ignorance of our own field and historiography may well be the main reason that, as José Lopez Piñero has argued with commendable warmth, we take rediscovered old truths as valuable novelties and accept as truths nonsense against which wider reading in our own subject would have protected us. Lopez Piñero points especially to the insularity of the sociologically oriented Anglo-American scholarship that now dominates our field -- and to the consequent loss, by the individual practitioner, of the international scope and disciplinary coverage typical of the group mobilized by Geroge Sarton to found *Isis* in 1912. To some extent the narrowing of the individual historian's scope has been countered by the expansion of the collective work of our disciplineto to include relations with technology, religion, literature, institutions, economics, politics, and so on; and, of course, the narrowing can be justified as the inevitable effect of the growth of knowledge.

Nonetheless, there has been real loss, most evident in the declining proportion of science in the history of science. Here are two telling indicators from Anglo-American practice. One is the course in the Scientific Revolution now given at the Open University in Great Britain; it has as its principal text a book of readings in social history, without an equation, diagram, or picture of any natural object to relieve the monotony. The other indicator is the program of the annual meetings of the (US) History of Science Society; well over half of the papers presented have nothing to do with "science" as understood by Mach, Tannery, Favaro, or Sarton.

My purpose today is not to lament the state of our discipline, which is fluorishing in many ways and places, and which stands poised to be of infinite use to humankind if only its potential as a collaborator in the teaching of natural science and up-to-date humanities can be realized. Rather, I shall review the development of the historiography of science during the 20th century to indicate some origins of the strengths we should build on and the weaknesses we should correct. A few simplifications and some magnificent generalizations will be required to delineate a century's developments in 45 minutes. I'll proceed by snapshots, three of them, catching our discipline at 50 year intervals beginning around 1900. Three-fold divisions usually work. This one does, especially well, since the snapshots map perfectly onto the three stages in the advancement of human thought discovered by Auguste Comte -- with the slight exception that our field went through his stages in inverse order.

To complete the symmetry, I've divided my talk into three parts. First, an adaptation of the Comtean stages to the historiography of science during the 20th century. Second, a description of the contributions, positive and negative, of the adapted stages. Third, a brief guess at where we might be headed, and what might be done to deflect our course.

1. Comte's three stages and the history of science

The academic study of the history of science shared the developmental traits of science during the 20th century. Around 1900, the majority of the best work was inspired by a positivistic understanding of the nature of science. Around 1950, supported by the success of science during World War II and the felt necessity of advancing basic science rapidly during the Cold War, the new American National Science Foundation invoked history as the source material for a true science of science. Towards the end of the century, disciplinary fragmentation and unfriendly interpretations of science derived from the humanities and opposed to military and industrial uses of science-based technologies inspired the diversity of approaches to the history of science that now energizes its academic study.

This process recalls the historiography of Auguste Comte, the fountain of the positivist philosophy, who laid down as law that the scientific thinking of the human race must progress through two preliminary stages before reaching the modest certainty of positivism. In the first Comtean stage, the theological, mankind, presumptuously seeking the causes of things, externalizes their agencies and moves from fetishism through polytheism to monotheism. This process sharpened the tools of thought but not the method of acquiring true knowledge, which, according to Comte, consists of observation and experiment. During the second, or metaphysical stage, the same impulse to reach the inaccessible ground of things predominated, but now with abstractions, like innate forces, rather than gods, as the final causes. The last phase of this second stage, completed during the Enlightenment, placed an abstract, omnicompetent Nature in place of the one God of the monotheistic phase of the preceding stage.

Metaphysical thinking was essential to the transition to positivism, since it abetted, though it did not accomplish, the elimination of anthropomorphic elements from scientific concepts. In the final or positive stage, the true objects of science, observed facts and the laws that bind them, take center stage. The hidden causes of things remain hidden. Comte cites as examplars of positivistic science Newton's law of gravitation and Fourier's law of heat conduction, both delivered without pronouncements about the ultimate nature of the phenomena considered. Not all sciences had reached the positive stage in Comte's time and, as he lamented, throwbacks to earlier ways occurred even in the advanced sciences. Avatars of earlier thinking can erupt at any time. Comtean stages are modes of thought, not historical periods.

From the few data presented so far, anyone acquainted with philosophical thought (as Comte would say) can see that the historiography of science in the 20th century followed the inverse of the law of stages. It began positivist, producing its own Comte in the Belgian polymath George Sarton, who took his crusade to reduce the history of civilization to the history of science to the United States during the first world war. The metaphysical stage arrived with the program of deducing the principles of scientific advance from a close study of its history. The prime though poisoned fruit of this project was T.S. Kuhn's *Structure of scientific revolutions* (1962), which fulfilled its duty in the manner that Comte taught, by planting the seeds that destroyed it.

The seeds germinated with the help of heavy manuring from French intellectuals, especially Michel Foucault, and also from British sociologists and feminist theorists. In the 1980s the theological stage was in full flower. Representatives of the several schools displayed to perfection the traits that Comte specified as characteristic of the first intellectual efforts of the human race: an "inclination toward the most insoluble questions, toward subjects most radically inaccessible to any decisive investigation." Another such characteristic discerned by Comte anticipated the arrival of constructivist historiography. During the theological stage, our thinking forefathers "assimilated all phenomena [and behavior] whatsoever to those we produce ourselves;" which renders perfectly the narcissism that now plagues our profesion. Let us regard this anticipation as a prophecy and, in the manner of scientists, accept its astonishing accuracy as a sufficient confirmation of the scheme of inverse stages.

Comte's law of stages is thus discovered to be more powerful than even he supposed. Like the laws of celestial mechanics, it runs equally well backward as forward. In astronomy the direction of rotation does not of itself indicate progress or regress. Perhaps Comte's law applies with similar indifference. Let us then turn to the questions whether and how the historiography of science advanced during the 20th century.

2. Comte´s stages reversed and the historiography of science

The positivist

However misguided it may appear to postmodern historians of science, the positivism of the late 19th century steered the first sustained development of our field. It mobilized resources from universities and states newly aware of the importance of science in international competetion. Among the enduring products of this collaboration were positivist histories of science, bibliographies and biographies of scientists, and standard editions and translations of classical texts. Let me offer you some examples from the exact sciences -- mathematics, astronomy, and the physical sciences -- in which the positivistic method worked best.

Handbücher. Positivist historians had a choice of at least two different paths. Either they could emphasize the amassing of facts, or they could dwell on the generalizations extracted from the pile. The exemplar of history by accretion was Ludwig Darmstädter's *Handbuch zur Geschichte der Naturwissenschaften und Technik*. It lists, in chronological order and at twenty to the page, "pioneering facts and fundamental results, and the individual steps...of the various discoveries" from caveman to kaiser. The increasing rate of accumulation jumped to the eye: the discoveries of 1600 occupy one page, of 1700, a page and a half; of 1800, two and a half; of 1900, twelve.

Positivistic historiography that emphasized general progress rather than accumulation of infinitesimals reserved a place for great men. They were the effective explorers, the true projectors, the indispensable pioneers. Thus Galileo, or perhaps Bacon, was the "Columbus of Science," and William Gilbert, "the Galileo of Magnetism," "the Father of Magnetic Philosophy." The exemplar of this sort of history is Friedrich Dannemann's four-volume *Die Naturwissenschaften in ihrer Entwicklung und in ihrem Zusammenhänge* (1910-13). We have "scientists" from Aristotle ("the founder of zoology") through

Galileo ("the inventor of physics") to Kirchhoff and Bunsen ("the creators of spectralanalysis").

Sarton judged Dannemann's history to be "the first satisfactory textbook dealing with the history of science as a whole," but deficient in principles (Dannemann ignored Comte) and overly partial to Germans. Nonetheless, Dannemann's approach obtained asylum in England in Abraham Wolf's *History of science, technology and philosophy in the 16th and 17th centuries* (1934). Wolf's book, together with its companion volume for the 18th century (1938), had an immense influence on English-speaking historians of science. Hence the German positivistic tradition in its most inclusive form was an important, though mediated, spur to the development of Anglo-American history of science just after World War II.

Sources. Dannemann drew much of his inspiration from Ostwald's *Klassiker der exakten Naturwissenschaften*, so much indeed that he regarded his work as a frame for their display. Wilhelm Ostwald, himself the author of a celebratory history of electrochemistry in the positivistic style, started the *Klassiker* in 1890. Over 100 volumes had been printed by 1900, 150 by 1905, 200, by 1923. The *Klassiker* had pale parallels in England, in the series issued by the Alembic Club (21 volumes, 1898-1933), and in France, in the Société française de physique's compendia of classical papers (9 vols., 1884-1914).

The misconception that there exist great men combined with the still greater error of nationalism to produce great monuments to and of scholarship. The French were first in this field of glory with the *Oeuvres* of Lavoisier (6 vols., 1862-93), Lagrange (14 vols., 1867-92), Laplace (14 vols., 1878-1912), Cauchy (26 vols., 1882-1970?), and Descartes (12 vols., 1897-1913). Their rivals across the Rhein responded with the works of Gauss(12 vols., 1863-1933) and Wilhelm Weber (6 vols., 1892-94). Smaller countries, with fewer heros to celebrate, produced even bigger editions. Thus Euler's *Opera omnia*, commissioned in 1909 at 43 volumes and raised to 72 in 1947, may reach 110 by the end of this decade. Huygens' *Oeuvres complètes* weighed in at 22 vols. (1880-1910), the "national edition" of Galileo's *Opere* at 20,1890-1910, and the *Opera omnia* of Tycho Brahe at 15 (1913-29).

The connection of this outpouring with nationalistic impulses appears frankly enough in Antonio Favaro's appeal to the patriotic sentiments of the newly unified Italy for money to support his new edition of Galileo's *Opere*. He pointed out that the French had stolen a march by publishing the works of Lagrange, who came from Torino. "Oh! How much better it would have been for us to have provided for our own glory by...publishing [his] work ourselves!...Let us proceed vigorously or...we will not be spared the supreme disgrace of seeing a complete edition of the writings of Galileo done by a foreigner."

The champions of exact thinking in antiquity lacked a modern homeland. They did not lack modern editors. All Europe shared the heritage of the ancient mathematicians, Euclid, Archimedes, Aristarchus, and Apollonius, who came forward in new Greek editions and/or vernacular translations. The twentieth century began well equipped with easily accessible texts of the ancient and modern masters reputed to be the inventors, as well as the exemplars, of the art of right thinking.

Bibliographies. As editor of the *Annalen der Physik* for over fifty years, Johann Christian Poggendorff made it his business to fend off articles afflicted with "speculation." The severe empiricist made an excellent bibliographer. The first two volumes of his bio-bibliography of the exact sciences, the product of fifteeen years' labor, covered the period from minus infinity to 1858; five years in printing (1858-63), it contained entries on 8501 "scientists" in 1526 pages. The enterprise caught the spirit of the day. It was continued by Poggendorff's disciples until 1912, when the Saxon Academy of Sciences took it over. The sixth "volume," which took the story from 1923 to 1931, occupies 2975 pages.

Grand and useful as it is, Poggendorff is not the only, or, for certain purposes, the best, bibliographical inheritance historians of the exact sciences received from the positivistic compilers of the 19th century. There is also the Royal Society of London's *Catalogue of scientific papers*, issued in 19 volumes in 4 series, reporting, by author, the titles of all the scientific papers published in all the scientific journals in all fields and all languages between 1800 and 1900. It exhibits the same pattern as Poggendorff in its growth. The seven volumes for 1884-1900 list 384, 478 papers by 68, 577 authors. Altogether, the 19th century produced over a million scientific papers.

Bibliographical compulsion made up much of the scholarly metabolism of the man who created the first thoroughly independent, enduring, general journal for the history of science. In December 1912, George Sarton, a recent graduate from the University of Ghent, distributed a prospectus for a new periodical to be called *Isis*. Its editorial office was in the garden of his home in Belgium. The following March the first number appeared, displaying on its cover the names of the distinguished *comité de patronage* that Sarton had collected to endorse his enterprise. The thirty-three patrons comprised distinguished scientists and leading contributors to the history of science. Most of the historians worked on the exact sciences, the leading sector of the field in fact as well as in Comtean theory.

Sarton's ambition for *Isis* recognized few boundaries. 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."

Metaphysical

In February 1955, midway through the decade that saw the Korean War, the testing of the super bomb, the launch of nuclear submarines, the spread of home fallout shelters, the communist witchhunts, and the beginning of the space age, thirty-three philosophers, sociologists, and historians of science met in Philadelphia at the invitation of the American Philosophical Society and the (US) National Science Foundation to discuss what their disciplines could do for science and what science -- represented by the Foundation -- could do for them. The answer to the second question did not demand much thought. The meeting recommended that the Foundation supply fellowships, research funds, money for international travel, and support to university departments. The supplicants argued that, with proper support, they would help the Foundation fulfill its mission to enhance the nation's science and security.

The spokesman for the history of science, I. Bernard Cohen of Harvard, said that he and his colleagues could help secure the progress of science by exposing the conditons under which it had fluorished or decayed. Unfortunately, Cohen and his colleagues knew nothing about those conditions in recent times. All the reigning experts in history of science worked on earlier periods. How could he justify spending the Foundation's money on the natural knowledge of the middle ages or the Renaissance? Cohen argued that historical investigation of all science, whatever its antiquity, should qualify for support because scentific advance had followed the same process for 2000 years and more. This metahistorical, indeed metaphysical, notion of universally valid laws and principles of progress undergirded the strategy for enlarging the resources and manpower of the the new American discipline of the history of science. Let us call the notion "uniformitarianism."

The uniformitarian metaphysic incorporated an intellectualist and, to use a term first popularized in 1952, "internalist," conception of science. The contrasting style, "externalist," was associated with socialism primarily through *The social function of science* (1939) by the British physicist J.D. Bernal and reports on the state of science by the British journalist J.G. Crowther. The very phrase that identified their subject, " the social relations of science," had a Marxist air; the pioneering work of the sociologists Robert K. Merton and Edgar Zilsel on the Scientific Revolution was guilty by association, and so avoided. The leaders of the newly vigorous history of science in the United States almost to the man, and to the lone woman too, fled Marxist-socialist-sociologizing externalism. That did them no damage when asking the NSF for support in the common cause against communism.

When Cohen addressed the NSF as the representative of the history of science, he was Assistant Professor (indeed the only assistant professor) in Harvard's program of history of science. He got this toe-hold on the academic ladder through a Cold War opportunity created by the president of the university, James B. Conant. Conant needed help in institutionalizing his program of teaching undergraduates the "tactics and strategy" of science. Since the majority of them did not know any science, the lessons in tactics and strategy would have to be taught, in the manner of the military, by case studies of old battles. No matter. Uniformitarianism guaranteed their relevance and continued applicability.

Cohen's closest reader was Thomas Kuhn, who acted as an assistant in Conant's course and accepted the uniformitarian principle that grounded the case-study method. With his famous discovery that Aristotle had not been writing bad Newtonian physics but good Greek philosophy, Kuhn saved the fundamental assumption of Conant's pedagogy: Although Aristotle's science was wrong, his reasons for believing it did not differ in kind from those a Galileo, Newton, or Einstein had for adhering to their scientific world views.

To pursue this insight, Kuhn sought an occurrence in the history of science of a conversion as deep as his own discovery that not all of Aristotle is nonsense. He found his case in the Copernican revolution. Uniformitarian metaphysics authorized its generalization to all of history. Thus arose *The structure of scientific revolutions.*

The key innovation of the book is not the revolutions but what Kuhn supposed to happen between them. That is "normal science" or "puzzle solving" conducted under a "paradigm" inclucated and certified by the "relevant scientific community." The community or great group replaced the great man as the agent of scientific change.

Research under a paradigm is rigid and autocratic: a scientific community can have one and only one paradigm at a time (if it allows more than one it is not scientific), and its leaders must reject all work in their domain that does not conform to their principles and practices. Moreover, only the scientific community has the right to judge the value and direction of the work within its competence. The inflexibility of Kuhn's concept of the paradigm inspired his influential if implausible doctrine of incommensurability -- no doubt the most metaphysical result of uniformitarian metaphysics.

Kuhn's authoritarian paradigm and custodial scientific community may have owed something to the dispute that raged in the United States over science policy when he took up service under Conant. The dispute centered on control of the policies of NSF. Should the scientists be in charge, and award grants only to the most meritorious among them as determined by peer review? Or should the Foundation be responsive and responsible to the people, and distribute its support with an eye to geographical balance and social utility? The paradigm as Kuhn's overly logical, overly rigid, solution to this political problem.

No big intellectual shift in Anglo-American historiography of science has been achieved without some input from awe-inspiring French intellectuals. Precisely the ingredient needed to consolidate the internalist inclination of the profession around 1950 was supplied by Alexandre Koyré, an historian of theology and philosophy stationed in the US during World War II. There he publicized his view that the Scientific Revolution consisted in the "geometrization of space and the dissolution of the Cosmos," in the replacement of the Aristotelian structured qualitative universe by an indifferent abstract Euclidean receptacle. These improvements made possible the invention of the law of inertia and the unified mathematical treatment of celestial and terrestrial motion. The Scientific Revolution did not depend upon mutations in instrumentation, experimentation, or technology, or on the discovery of the New World, but on a profound change in a few minds about the nature of space and time. According to Koyré, the common notion that Francis Bacon's propaganda for experimentation initiated the Scientific Revolution was a "joke, and a very bad one too." His teachings soon reached Harvard. Cohen vibrated to the "thrilling...revolutionary insurgency" of Koyré's historiography. Kuhn incorporated it into his courses.

Among the appealing features of Koyré's illumination to the young American historians of the exact sciences was its correspondence with the vision they shared with theoretical physicists of the revolution in physics that had occurred during the previous fifty years. Having witnessed the possibility and power of purely intellectual transformations of mathematics and mathematical physics, readers of 1950 were primed to accept Koyré's elucidation of the origins of modern science. Owing to uniformitarian metaphysics, Koyré's internalist account of the Scientific Revolution became a Kuhnian paradigm for the history of science as a whole.

Theological

In the 1980s, a proselytizing Anglo-American theology began to make conquests among newly fledged historians of science. It drew its inspiration and doctrine from many fountains: Kuhn's notion of scientific community; oracular utterances by Wittgenstein about "ways of life" and "language games;" Foucault's dark linkages between power and knowledge; and the strong program in the sociology of scientific knowledge associated with David Bloor. From Bloor the new church took its commandments, four in number, which enjoined the virtues of causality, impartiality, symmetry, and reflexivity. Causality requires believers to give causal, that is, sociological, explanations of scientific practice and belief; impartiality and symmetry prohibit taking sides in a scientific dispute or using truth, success, or rationality and their opposites to explain either the acceptance or the rejection of a theory; reflexivity obliges the faithful to explain their own practices in the same way they do those of any other science.

The old testament of the new religion, which became known as constructivist history, was Steven Shapin and Simon Schaffer's *Leviathan and the air pump.* It describes the "way of life" in Robert Boyle's work place or "elaboratory." This Eden was the site of development, and the center of diffusion, of the air pump, and also the delivery room for the birth of experimental science. The Edenic character of the place was secured by a pre-established harmony between "the experimental life" and the spirit of reconciliation and tolerance supposed to characterize the Stuart Restoration. Both the life and the spirit forbade discussion of politics and religion, limited discussion to subjects about which agreement could be reached, stuck to matters of fact, promoted industry, sobriety, good judgment, and balance, and opposed enthusiasts, dogmatists, radicals, and sinners.

Shapin and Schaffer develop these analogies between restoration politics and natural philosophy with the persistence and prolixity of two Boyles. According to them, Boyle wished to set aside a "social space" for the cultivation of experimental philosophy; and also to demonstrate to society at large how civic and religious dissent might be managed peacefully and productively. In fact, if he managed the one he would necessarily advance the other, for, as Shapin and Schaffer assert, in a now famous declaration, "solutions to the problem of knowledge are also solutions to the problem of social order."

Boyle secured the "experimental form of life" in his dispute-free zone by three "technologies." Only one of these technologies corresponds to ordinary English. It is material technology, that is, experimental apparatus, of which the air pump was the exemplar. In addition, Boyle had his prolix literary technology for describing experiments performed, witnesses present, and machines employed, and a social technology, or rules of engagement in philosophical debate. The rules gave preeminence to matters of fact and downgraded or excluded theories about the causes and principles of certified phenomena.

These "technologies" drew fire from the plentiful furnace of Thomas Hobbes, whom Shapin and Schaffer use as a detector of the aspects of Boyle's program offensive to contemporaries who differed from him politically. Hobbes pointed out that the material technology leaked; that the literary technology, at least in respect to the testimony of witnesses, had no force (certainty comes from authority, not from testimony, even of an "infinite number of grave and learned men"); and that the social technology misconstrued the nature of knowledge. Hobbes held that by making the matter of fact and not the underlying principle the main object of investigation Boyle forfeited all hope of arriving at the truth and gave no reliable way to exclude serious error.

Hobbes' dogmatism in natural philosophy was of a piece with his concept of the state. In philosophy, the force of reason, working from sure principles in the style of Euclidean geometry, must compel assent: "who is so stupid [he asked] as both to mistake in geometry, and also to persist in it, when another detects his error to him?" (The answer, incidentally, is Hobbes himself, who persisted in thinking that he had squared the circle and duplicated the cube long after better geometers had exposed his mistakes.) In the state, the king's authority should prevail over all dissent and dissenters in both civil and religious matters. Just here, Hobbes hallucinated, the particular matters of fact that Boyle claimed to establish presented a public menace. Loose talk about vacuums threatened the peace. The method of creating and certifying knowledge, and the problem of establishing social order, thus forced Boyle and Hobbes to sharply different conceptions of the relationship between organized religion and natural philosophy as well as to conflicting evaluations of the relations between knowledge and fact.

The parallel fails when applied to the Accademia del Cimento of Florence and the groups that coalesced into the Académie des Sciences of Paris. If plausible sociological reductions can be found for these cases, they would have the undesirable feature of deriving similar "forms of experimental life" from the absolutist regimes of the Grand Duke of Tuscany and the King of France, and from the more permissive parliamentary monarchy of England. To make the same point in time rather than space, the same solution to the problem of knowledge, that is, the experimental way of life, has done amazingly well in political regimes far different from those in which, according to Shapin and Schaffer, it came to be.

Shapin and Schaffer understood that a special vocabulary, or, rather, the use of ordinary words in special ways by initiates, would help believers to recognize one another and to proselytize further. In addition to special usage of ordinary words, like authority, discourse, gesture, local, negotiation, space, and technology, they make extensive use of puns to veil meaning and affect profundity. "Body," as in human, politic, and knowledge; "spirit," as in soul, angels, ghosts, and alcohol; "power," as in authority, prime mover, and affectations of matter, are manipulated to suggest connections never demonstrated and probably undemonstrable between knowledge, church, and state. Schaffer, who has been the cult leader through his extensive and brilliant if far-fetched articles and his teaching at Cambridge, has added an apocalyptic style to the "literary technology" of constructivism.

Constructivism appeals more to certain feelings than to conventional evidence and arguments. These feelings contain an element of antiscience, a bias in favor of the underdog, and a strong predisposition to believe in the more negative aspects of the equation between knowledge and power. Hence the younger constructivists place unquestioning faith in the writings of the church leaders and the research results of their colleagues. They do not practice the historian's method of example and counter-example to arrive at a balanced judgment; but rather that of the old theologians, who accepted the statements of the fathers and saints as so many blackboxes (to use one of their terms of art) to be combined and rearranged but not opened.

In exercising its way of life, the constructivist church has stressed and routinized an approach adumbrated in the early 1960s by Henry Guerlac and Robert Schofield, whose *Lunar Society of Birmingham* is now praised as a "milestone" for its accounting for "the emergence of new ideas by framing them in their social and personal milieus." The search for social components of scientific practice and belief is now standard in writing the history of science, and our discipline is the richer for it. But we need not oblige ourselves always to end the search for social causes successfully or always to couple what social components we do find to contests for political authority.

3. Trends

The constructivist church appears to be growing, though not, perhaps, by the accretion of many true believers. Newcomers' use of the special constructivist liturgy often does not suggest adherence to the full theology. The constructivist fascination with the local and parochial, and with the borderline and nascent, however, will persist; invocation of great men, scientific truth, and established fact will risk the censure of serving a master narrative, a creation myth, or (to say the worst) a realist epistemology. The same trend will continue to squeeze science out of history of science.

The replacement of science by sociology in much current practice of history of science is one reason that the Anglo-American section of our subject now enjoys greater public exposure than it ever did before. Professional writers have moved in to rephrase, simplify, and tart up our newly accessible scholarship. Kuhn's *Structure* was the most successful book of our metaphysical stage, Dava Sobel's *Longitude* the best-seller of the theological. Many other short books, emphasizing personalities and obscuring technicalities, also have done well in the market place. Interest in our field has coaxed other commercial publishers to bring out multi-volume histories of science. Five of the nine volumes of the *Storia della scienza* of the Istituto dell'Enciclopedia italiana have ben published during the last two years, and the first volume of a competitor of similar size, from Cambridge University press, is due out in December. To add to the pile, Oxford University Press will publish a fat *Companion to the history of modern science* next spring. Needless to say, the Cambridge series has been influenced the most by social history, the Italian series the least.

We will not return to the positivist stage, nor should we want to. Nonetheless, we should strive to arrest the eviction of science from its history. That will require the recruitment of more people trained in science. There are at least two likely sources of recruits. The first is pedagogy, the second biology. Yesterday I suggested ways in which the history of science could help in teaching science -- the substance, not the sociology, of science. People raised in a scientific discipline as well as in history of science would be needed to prepare the necessary materials and to help teachers learn how to use them. Training these prople will require professors of history of science who know some science.

As for biology, I have in mind a parallel to the conversion of young Ph.D.'s or almost Ph.D.'s in physics into history of science during the 1950s and 1960s. Some converts, like Kuhn, came to find the principle of scientific progress, others to find the reasons for their disenchantment with the practice of physics. The reasons -- the complexity and detail of the subject, the length and uncertainty of experiments and calculations, distress at being a cog in a big-science machine, and concern about the (mis)application of science -- may be building up in biology. The public and its leaders need information and a point of view for thinking about the social and ethical issues presented by the biological sciences. That might prompt universities to hire disenchanted young Ph.D. biologists as historians of science just as the Cold War stimulated them to engage young Ph.D. physicists. Owing to the professionalization of the discipline over the last fifty years, however, these mobile biologists will have to know much more history of science than their physicist predecessors to have a chance at a post in history.

To sum up: The main social-scientific issue of the positivist stage was scientism and the liquidation of the ancien regime; of the metaphysical stage, nuclear bombs and mutual assured destruction; of the theological stage, cloning, stem-cell research, and genetic engineering. The consequent preoccupations of history of science were, in the positivist stage, the acquisition of facts and laws; in the metaphysical, the discovery of the principles and conditions of scientific and technological advance; and in the theological, the sociological determination of knowledge. What could be clearer?