A Professional Life in the History of Science*
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Transactions of the American Philosophical Society; 2007; 96, Academic Research Library
pg. IX

INTRODUCTION

A Professional Life in the History of Science*

It was with some compunction that I acceded to the flattering invitation from Donald Yerxa, editor of *Historically Speaking,* to write of a professional life in the field of my specialty. Reluctance was the greater in that I had already given an account of that career in *Isis* on the occasion of the 75th anniversary of the History of Science Society in 1999.[1] In all probability, however, there is little if any overlap between subscribers to *Isis* and those to *Historically Speaking.* That such should be the case is one of the situations discussed. Anyone who consults the earlier essay will find that it turns on personal and institutional factors. I tried not to repeat myself more than was necessary to make what follows intelligible, and ventured instead to offer some reflections on the context of my work in relation to the development of the historiography of science.

First of all, a word about the subject. The generation to which I have the good fortune to belong is commonly said to have founded the history of science as a professional field of scholarship in the years after World War II. Marshall Clagett, I. Bernard Cohen, Henry Guerlac, Erwin Hiebert, Alistair Crombie, Giorgio di Santillana, Rupert and Marie Hall, Georges Canguilhem, René Taton, Thomas S. Kuhn—those are among the notable names. Having majored in some branch of science as undergraduates or the equivalent, and gone on to graduate school before or just after the war, all of us had somehow developed a strong ancillary taste for history. We came out of service of one sort or another in 1945, dazzled like everyone else by Hiroshima, the Manhattan Project, sonar, radar, penicillin, and so on. Independently of each other, or largely so, we each harbored a sense

* Reprinted from *Historically Speaking: The Bulletin of the Historical Society,* V:3 (January 2004), pp. 2–6.

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that science, even like art, literature, or philosophy, must have had a history, the study of which might lead to a better appreciation of its own inwardness as well as its place in the development of civilization.

With a few stellar exceptions, the history of science until that time was the province either of philosophers—Condorcet, Comte, Whewell, Duhem, Mach—each adducing exemplary material in service to their respective epistemologies, or of elderly scientists writing the histories of their science, or sometimes all science, in order to occupy their retirement. Though not written in accordance with historical standards, neither of these bodies of literature is to be ignored. The one is always suggestive and sometimes informative, the other often informative, almost always technically reliable, and rarely of much interpretative significance. Of the two notable scholars who flourished in the 1920s and 1930s, George Sarton was a prophet and scholarly bibliographer rather than a historian, while E. L. Thorndike was a devoted, learned antiquarian riding his hobby horse of magic and experimental science through the library of the Vatican. Though much and rightly respected, neither found a following. Nor did E. J. Dijksterhuis, whose *The Mechanization of the World Picture* (1950) is a classic that will always repay study.

Anticipations of a fully historical history of science appeared in the work of Hélène Metzger on 18th-century chemistry and Anneliese Maier on medieval science. Herbert Butterfield's *The Origins of Modern Science, 1300–1800* (1950) was a godsend both in itself and in that it was one of the few things one could expect undergraduates to read. The same was true of Carl Becker's *Heavenly City of the 18th-Century Philosophers* (1932), a supremely literate essay which (unfortunately in my view) has fallen into disfavor among students of the Enlightenment, and also of Arthur O. Lovejoy's *The Great Chain of Being* (1936), a founding work in the modern historiography of ideas. Two ancillary masterpieces, one from the side of sociology, the other from philosophy, were still more inspirational in exhibiting respectively the social and the intellectual interest that the history of science may hold, namely Robert K. Merton's path breaking *Science, Technology, and Society in Seventeenth-Century England* (1938) and Alexandre Koyré's superb *Études Galiléennes* (1939).

I had read none of these works when, safely out of the army in graduate school at Harvard in 1946–47, I thought to find a thesis subject in what to me was the *terra incognita* of the history of science. My scientific and military backgrounds were respectively in chemistry and a 4.2-inch chemical mortar battalion, but I had taken almost all my electives in history as an undergraduate at Wesleyan, graduating in 1940. The emphasis in the excellent department there was on English history, and my instinct was to

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look to Britain for a subject, rather than to chemistry. I'm not sure I even knew that there had been a chemical revolution centering on the work of Lavoisier. Darwin was the obvious link between science and intellectual history, but, such was my naiveté, it hardly seemed possible that anything new could be said about the theory of evolution, about science and religion, or about social Darwinism, and I elected to look into the background. That turned out to be in geology, whence my first book, *Genesis and Geology: A Study in the Relations of Scientific Thought, Natural Theology, and Social Opinion in Great Britain, 1790–1850* (1951). It has been in print ever since. Harvard University Press saw fit to put it in a new suit of clothes and reissue it in 1996. A foreword by a scholar of the next generation, Nicolaas Rupke, analyzes the way in which it came to mark a new departure in the historiography of science. He credits me with a novel methodology, first, in consulting, not only the original scientific texts, but the general periodical literature of the time; and second in telling not merely of technical discovery, but of the way in which varying religious views of geologists entered into the formation of their theories, and also the way in which the climate of social opinion entered into the discourse of theology as well as science.

I had no notion of anything of the sort. So far as I was aware, my thesis was a new departure for me, but not for a subject of which I was quite ignorant. Nothing was farther from my thoughts than methodology, something fit for Marxists and sociologists. All that we students of history were taught to do was to go look at the sources, all of them. Perhaps it was lucky that I had never taken a course in geology. Though formally trained in science, I wrote my thesis as someone being trained in history. Had I written it as a scientist, it would have been a chronicle of discovery, a sequence of correct theories displacing incorrect theories, the context being the state of knowledge about the earth in the author's time.

This is not to say that persons trained in a science cannot convert their approach so as to treat its development by historical standards. There are distinguished instances in later years. But I am not among them. Nor is it to deny that it is an advantage, if not quite a necessity, for historians of science to have had scientific training. The reasons are not so much technical as psychological. Except for contemporary or highly mathematical topics, one can always inform oneself about the technicalities, as I was able to do with respect to early 19th-century geology. But it is difficult though not impossible—again there are distinguished instances—to appreciate what it is to know something scientifically without having experienced it.

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The department of history at Princeton offered me a job in 1947. Harvard granted me the Ph.D. in 1949, and *Genesis and Geology* appeared to almost inaudible acclaim in 1951. There was no question of my teaching history of science at the outset, and I was quite unprepared to propose any such thing. The curriculum there had the advantage for neophyte faculty that they did not have the labor of preparing courses, and instead led freshman classes and preceptorial discussion groups in the courses taught by senior faculty, whatever the subject. Thus one learned a lot of history while having time to develop one's knowledge and scholarship. When as an assistant professor I had a course of my own, it was modern English history. Only in 1956 did I feel ready to offer history of science. In the interval, I had been able to read all the titles mentioned above and many others. I was informed about courses being offered by Henry Guerlac at Cornell, by Marshall Clagett and Robert Stauffer at Wisconsin, and by Bernard Cohen and others under James B. Conant's leadership in the General Education Program at Harvard. Equally important, and in a personal way more so, I had come to know Alexandre Koyré, who spent half the year annually at the Institute for Advanced Study from 1956 until 1962.

The opportunity to offer an undergraduate course in the history of science opened with the inauguration in the curriculum of an interdisciplinary humanities program. The senior faculty responsible accepted my proposal for a course on the history of scientific ideas from Galileo to Einstein. The notion was to present something that might contribute to the liberal education of students of science and engineering while opening to students in the liberal arts an awareness of the place of science in modern history. Enrollment was nothing of a mass movement, but the undergraduates who did participate in discussion of the material throughout the next three years helped me form a sense of the themes that made for viability. I was thus able to develop the lectures into a book, *The Edge of Objectivity, an Essay in the History of Scientific Ideas* (1960).

The time must have been ripe. That book has been translated into half a dozen languages, beginning with Japanese and ending with Greek. In 1990 Princeton University Press issued a second edition, which is still in print. The preface consists of a review of the thematics of the literature in the intervening thirty years. On its first appearance I had ventured to express the hope that my book might contribute to the development of a professional approach to the history of science.

It would have been more seemly to recognize that *The Edge of Objectivity* was an early instance of such a movement already under way at the hands, largely, of the colleagues mentioned above in the second paragraph. Professional graduate study in history of science was then avail-

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able only at Wisconsin, Cornell, and Harvard. My book was well enough received that Princeton thereupon agreed to my complementing undergraduate instruction with a graduate program that required additional staff.

In point of content, our attention, like that of colleagues elsewhere, was on the ways in which study of nature reciprocally formed and was formed by the world pictures of classical antiquity, the Middle Ages, the Renaissance, the Enlightenment, and modern times. In point of context, the tendency was to look to philosophy in antiquity, to theology in the Middle Ages, to art and humanism in the Renaissance, to secularism and literature in the Enlightenment, and to industrialization and military technology in modern times. With respect to science itself, the seminal transitions were what attracted scholarship: the Scientific Revolution, mechanization, the Chemical Revolution, the Industrial Revolution, Darwinian evolution. Chronologically, the center of gravity tended to be the 17th century. Other than Darwinism, much else in the 19th century and almost everything in the 20th—relativity, quantum mechanics, and genetics—awaited scrutiny. The narrative line throughout followed the route taken by the creation and transformation of scientific ideas and theories. We wrote, in a word, intellectual history of technicalities with important philosophical overtones. If social, economic, or political awareness crept in, it was around the edges.

The publication of the *Dictionary of Scientific Biography* (1970–1980) affords more objective evidence that a fledgling profession had come into existence by the 1960s, when its preparation began under my direction. The initiative came, not from a historian of science, but from the publisher, Charles Scribner, Jr., who had made a hobby of the history of science since his wartime service in cryptography. Soon after *The Edge of Objectivity* appeared, he asked whether I thought a series of books on the history of science would be viable. I had to say that most of the series known to me started off with one good book by the initiator, and then tailed off into mediocrity since few leading scholars were ever willing to write books on commission. Scribner agreed. His firm was publisher of the *Dictionary of American Biography*, however, and he then had the idea that something of the sort might be feasible in history of science. That, I thought, might work. One could probably persuade first-rate scholars to write, not whole books, but authoritative articles about figures known to them from their own studies.

What had not occurred either to Charles Scribner or myself was that preparation of the *Dictionary of National Biography* and later the *Dictionary of American Biography* had come about at a comparable stage in the

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formation of a professional discipline of historiography in Britain and the United States respectively. Such, quite serendipitously, proved to be the case with the *Dictionary of Scientific Biography (DSB)*. The quality of the board of editors, of the advisory committee, and of the thousand and more contributors whom it proved possible to enlist from every country with a scientific tradition other than mainland China, then incommunicado, not to mention a large grant from the National Science Foundation and sponsorship by the American Council of Learned Societies—all that succeeded, not only in the main purpose of eliciting over 5,000 articles in sixteen quarto volumes, but also in the unforeseen effect of drawing into a sense of common purpose practitioners dispersed among a miscellany of universities, institutes, national societies, and diverse academies throughout the world.

The *DSB* reflects the time in which it was conceived and composed in another way. The emphasis by design is on the content of the science created—one did not then say constructed—by the men and the few women who are subjects of the articles. The instructions requested authors to keep personal biography and extra-scientific context to the minimum required in order to explicate how the work was possible and wherein it contributed to the development of positive scientific knowledge. It is fair to say that the *DSB* was brought into being by a generation of scholars and scientists who, whatever their other differences, believed in the overall beneficence of science, as by and large did public opinion generally.

The climate of opinion changed amid the seismic shifts in cultural attitudes in the late 1960s and early 1970s. Amid the manifold, largely academic, rebellions of those years, authority became suspect everywhere, including the authority of science. In consequence what had been marginal became central, and social history became the approach of choice in historiography generally, and notably so in history of science. That development bore out a prediction by Robert Merton, to the effect that sociology of science would flourish only if and when the role of science in society should be perceived as problematic.

So it has proved. In consequence, historians of science who came to the forefront in the generation currently in its prime have tended to see sociology, and to a degree anthropology, rather than philosophy as the disciplines with which to link arms. The merit of the approach is not to establish the truism that science is a social and cultural product. No one ever doubted it. But with a few exceptions, the earlier generation never undertook much in the way of analysis of context. We produced little comparable to the fine-grained accounts that distinguish current work by recapturing the actuality of experiment; the life of a laboratory; the labor of

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field work in natural history and geology: the recalcitrance of instruments; the differences between what scientists say and what they do; the role of research schools; the place of patronage; the occasional cheating; the interplay of professional rivalries, of personal loyalties and hostilities, of institutional standing, of public reputations, of social position, of gender, race, material interest, ambition, shame, guilt, deceit, honor, pride. The practice of scientific research is currently shown to exhibit, in short, the springs of action that make people tick in all walks of life.

All that is to the good. At the same time, the emphasis on the practice, rather than the content, of science may entail certain drawbacks. Current authors often seem to lose interest in science once it is made. Phenomena for which it is difficult to seek any sociological dimension, say the return of Halley's comet, the law of falling bodies, or the fissionability of Uranium 235, are little scrutinized for themselves. What matters is the way they became known. In consequence, or perhaps because of that approach, the fit, if any, with nature is often taken to be ancillary at best, while analysis of the quality of the science under consideration is left aside.

Looking back at my career in the course of writing this essay, I realize that its development might be seen as a set of responses to what was happening in the historiography of science at large. If so, I was a fish in the stream under the impression that the choices were my own. Apart from the *DSB*, an organizational and editorial job, my most considerable effort has been directed toward the material covered in two books, *Science and Polity in France at the End of the Old Regime* (1980) and its sequel, *Science and Polity in France, the Revolutionary and Napoleonic Years* (2004). They are really volumes I and II of a single work. The former is being reissued with the latter, but I did not want to call it Volume I since it could have stood on its own feet if its author had fallen off his in the interval.

That research started, not in response to changing fashion in the historiography of science, but much earlier in consequence of teaching preceptorial discussion groups in Robert Palmer's course on the French Revolution during the academic year of 1951–52. That was the best undergraduate course, including any of my own, in which I have ever participated. *Genesis and Geology* had just appeared. I had begun to feel (no doubt wrongly) that English history, important though it is, held few surprises. It occurred to me that something must have happened to science during the French Revolution, as many things clearly did in this country amid the major events of the last century. The Guggenheim Foundation agreed, and its generosity allowed my wife and me to spend the academic

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year 1954–55 in Paris, where we have been for part of almost every year until the above work was completed.

That halcyon year was my introduction to archival research. It was clear ahead of time—and this was the attraction of the problem—that the period of French scientific preeminence in the world coincided with that in which political and military events centering in France were a turning point in modern history. The question was: what did these sets of developments have to do with each other? In the process of working that out amid the minutiae of the documents and the magnitude of all that happened in both domains, I came to feel that what I shall call the public history of science may better be elucidated through the medium of events, institutions, and practices than through abstract configurations of ideas and culture. What the relations of science and politics were I shall leave to readers of the books and not attempt to summarize here. Suffice it to say that they turned on the process of modernization in both areas and on the orientation toward the future that is always characteristic of science and was then radically characteristic of politics.

My career, such as it is, has unfolded not in accordance with some agenda, but as a set of responses to a series of lucky accidents—being a historian by nature who happened to study chemistry and mathematics, taking up Charles Scribner's idea for the *DSB,* precepting in Palmer's course on the French Revolution. Personal rather than professional encounters made possible two of the four books that are spin-offs from the research on French science. During our many sojourns in France, my wife and I chanced to meet descendants of two distinguished families, the Carnots and the Montgolfiers. Lazare Carnot has been known to historians only as the "Organizer of Victory" during the revolutionary wars. So he was, but he spent only six years in government during a long life, most of which was occupied with highly original work, not fully appreciated at the time, in mathematics and physics.

Learning of my interest in that aspect of his life, current members of the family arranged for me to spend a summer going through Carnot's papers, which no one had ever seen, in the house in Burgundy where he was born. The result was *Lazare Carnot, Savant* (1971), to which book my esteemed colleague A. P. Youschkevitch of the Soviet Academy of Sciences contributed a chapter. That was another lucky break. He was the only other historian of science who had ever taken an interest in Carnot. In the midst of a discussion about Russian collaboration in the *DSB,* I mentioned a hint in papers I had seen that Carnot had submitted an early draft of his book on the foundations of the calculus to a prize competition set by the Prussian Academy of Sciences. On his way back to Moscow he searched

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its archives in East Berlin, found it, and contributed a chapter analyzing Carnot's approach.

I knew, of course, that hot-air balloons are called *montgolfières* after the brothers Joseph and Etienne, who invented them in 1783. On meeting Charles de Montgolfier at a wedding reception, I asked whether he was descended from the big balloon. Sure enough, collaterally at least, and since I expressed interest, he invited us to visit in the country house in Annonay, where his ancestors were in the paper business. There he showed me designs, sketches, correspondence, all scattered among drawers and attics in his and his cousins' houses. Thence *The Montgolfier Brothers and the Invention of Aviation, with a Word on the Importance of Ballooning for the Science of Heat and the Art of Building Railroads* (1983). I give the full title (though aeronautics would have been more accurate than aviation) since it suggests, that even like Carnot's work in mechanics, Joseph de Montgolfier's further inventions (which to him were more important than the balloon), along with those of his nephew Marc Seguin, belong to the pre-history of the physics of work and energy.

Two other publications were happenstance in different ways. Firestone Library in Princeton University is fortunate to possess a rare deluxe printing of the *Description de l'Égypte,* this one having been presented by Napoleon to the king of Prussia and bought at auction in 1865 from an impoverished descendant of a Prussian courtier by Ralph Prime of the class of 1843, later one of the founding trustees of the Metropolitan Museum in New York. It had been clear from the outset that a chapter on the scientific component of Bonaparte's Egyptian expedition would be important in my book. While studying the gorgeous plates, I bethought me that a former student who had just started an architectural publishing business might be interested to see them. He turned over a few pages, and said, "Wow, can we do that?" It had never occurred to me to reproduce them, and that was the origin of *Monuments of Egypt, the Napoleonic Edition,* 2 vols. (Princeton Architectural Press, 1987), which I edited in collaboration with Michel Dewachter, an Egyptologist then with the Collège de France.

In like manner, *Pierre-Simon Laplace, a Life in Exact Science* (1997) emerged from an earlier publication, in this case the *DSB.* I had never intended to write a book about Laplace, who lies on the frontier of my ability to follow mathematical reasoning other than qualitatively. Unfortunately, or perhaps fortunately, two colleagues who had successively undertaken to contribute the article on Laplace failed one after the other to keep their commitments. *Faute de mieux* Laplace devolved upon the editor as default author. I worked on him for a year, harder than I have on anything else, and with the collaboration of Robert Fox and Ivor Grattan-

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Guinness for particular topics, produced a lengthy article, of which the subsequent book is a revision and enlargement.

Thus, exposure to archives and the close-in research required for these books, as well as editing the articles, many of them very technical, in the *DSB*—these were the experiences that led me to think that limiting one's attention largely to the history of scientific ideas and theories was like following the tips of icebergs, except that the history of science is anything but a frigid subject matter. One might perhaps consider that my individual development exemplifies Auguste Comte's dictum to the effect that, just as every discipline passes through theological and metaphysical stages before becoming positive, so every person is a theologian in infancy, a metaphysician in youth, and a physicist on reaching maturity.

However that may be, the discipline of the history of science has reached maturity. The first meeting of the History of Science Society I attended in 1952 comprised thirty or forty persons, for few of whom was the subject a livelihood. The most recent numbered upwards of 600, the great majority of whom are professional scholars in the discipline. The Society has an endowment and an office with an executive officer. A hundred or more books and collections are reviewed in every issue of the quarterly *Isis*. All that spells success. In only two ways do I feel some slight twinge of regret or disappointment, the first with respect to science and the second with history.

The perception of science as socially problematic in the 1970s and 1980s stemmed in some degree, though by no means entirely, from widespread feelings of anti-scientism in academic and literary circles. In consequence, science studies, whether sociological, political, historical, or a mixture, are often perceived by scientists as hostile enterprises. The most obvious complaint is that critics with no technical qualifications to understand the subjects they discuss are violating the precincts of science. The accusation is nonetheless damaging for being usually, though not always, incorrect or irrelevant or both. The second-order concern among scientists is that the image of science is thus tarnished at a time of weakened political support and stringent restrictions on funding. But the sense of offense goes deeper. While willing to agree that questions of power and advantage are factors both in the macro- and micro-politics of science, scientists resent any implication that their work serves no purpose larger than their own, that they are not in the last analysis investigators of the nature of things, that objectivity is an illusion and rationality a sham. There is the counter-cultural *casus belli* of what journalists have called the science wars.

There was, as well as I can recall, no sense of resentment or hostility to

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the history of science during the time when our discipline was getting into its stride. On the contrary. We met with every encouragement, institutional and moral, on the part of scientific colleagues. We needed it. I doubt that the discipline could have matured in the face of their enmity and contempt. I do not think that any discipline can flourish in a healthy manner in a mood of hostility to its subject matter. Not that one would argue that prudential reasons should lead historians, or social scientists generally, to refrain from critical and even skeptical scrutiny of the objects of their studies. Still, if we are to recreate the past, the essential matter is to see the subject whole. To set out to see through it is to turn the creatures one studies into specimens. By and large, however, I feel optimistic and think the tide of anti-scientism, if that is what it was, has turned. Much of the work of recent years engages science and scientists on their own terms as well as on the author's.

The slight disappointment has to do with history. It was our hope at the outset, even our expectation, that the historical profession would come to accord the role of science in history a place comparable to that of politics, economics, religion, diplomacy, or warfare. Science after all has been a factor shaping history no less powerfully than have those other sectors. That has not happened. A few departments of history—Princeton's among them—do offer undergraduate and graduate work in the field. But at many, and perhaps most institutions, the subject is taught, if at all, in a separate department or under the aegis of a science and technology studies program. Nor are writings in the history of science as widely read as are those in the conventional fields. The best known, unfortunately in my view, are those written in a more or less iconoclastic vein. Perhaps the barrier is psychological. There may be a fundamental divide between temperaments drawn to history and those drawn to science. At Princeton more of our undergraduate students are majoring in science, engineering, and pre-medical programs than in history or literature. The famous, or infamous, two cultures problem may well be real. Still, we work in hopes that it may be abated.

Note

1. "Apologia pro Vita Sua," *Isis*, 90 Supplement (1999): S84–S94.

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