George Sarton: Episodic Recollections by an Unruly Apprentice

*By Robert K. Merton**

HALF A CENTURY HAS RACED AND STUMBLED BY since I first found myself, as a third-year graduate student in sociology at Harvard, daring to knock on the door of George Sarton's famed workshop-cum-study, Widener 185–189. The reason for taking this daunting step was clear: having elected to try my hand at a dissertation centered on sociologically interesting aspects of the efflorescence of science in seventeenth-century England—a kind of subject not exactly central to sociology back then—it did not seem unreasonable to seek guidance from the acknowledged world dean among historians of science.

Although Emerson Hall, which housed the Department of Sociology, was only a hundred paces from Widener, this was not a short journey. Traffic to the Sarton workshop by denizens, mature or immature, of the newfangled Department of Sociology faced formidable barriers. For one thing, the few graduate students who then had any knowledge of Sarton's scholarly existence took him to be a remote, austere, and awesome presence, so thoroughly dedicated to his scholarship as to be quite unapproachable by the likes of us. Thus do plausible but ill-founded beliefs develop into social realities through the mechanism of the self-fulfilling prophecy. Since this forbidding scholar was bound to be unapproachable, there was plainly small point in trying to approach him. And his subsequently having little to do with graduate students only went to show how inaccessible he actually was.

Buttressing this imputed barrier of personal inaccessibility were the authentic university barriers of departmental organization. The understaffed Department of Sociology, established just three years before, had enlarged its graduate program by reaching out to list research-and-reading courses in a great variety of departments: psychology and economics; government, religion, and philosophy; anthropology and social ethics amongst them. But nary a graduate course in the history of science. This for the best of reasons. Harvard had no autonomous department devoted to that undisciplined subject, nor, for that matter, had any other university. Still, in the preceding academic year, 1932–1933, I had managed to audit the sole lecture course in the field, entitled "History of Science

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1. History of the Physical and Biological Sciences." (As you may have begun to suspect, the title HS 1 was an unredeemed promissory note; there was no HS 2 back then.)

The first semester of the course was given by the biochemist and polymath of great note L. J. Henderson, later described by James Conant as "the first roving professor in Harvard." And rove he did. Not only had he instituted the course in the history of science two decades before, but, in that same year of 1932, he had also instituted his unique graduate "Seminary in Sociology" entitled "Pareto and Methods of Scientific Investigation." The plural "methods" rather than the more familiar and misleading singular, "the scientific method," also reflected a theme in the first semester of the history of science course as the "pink-whiskered" Henderson engaged in his typically forceful, magisterial exegesis of texts by Hippocrates, Galileo, and Harvey—this allowing him to expound his conception of the varieties of scientific inquiry. But as Conant confirms, Henderson, like Sarton, would have hooted at the then not uncommon notion that a grounding in the history of science served to sharpen one's capabilities as a scientific investigator.

The second semester of this lone course in the history of science was given by the lecturer, *Dr.* Sarton—decidedly not yet *Professor* Sarton; that title was only to come seven years later, when Sarton was fifty-six, and Conant, as Harvard's president, finally intervened to bring it about. Sarton differed greatly from Henderson in both the style and substance of his teaching. Warmly enthusiastic rather than coldly analytical—in a fashion that plainly irritated Henderson from time to time[1]—Sarton traced expanses of scientific development chiefly through the lives and accomplishments of what he took as prototypal figures in that development. (I gather from I. Bernard Cohen's recent account of that course as

[1] Henderson was impatient with Sarton's expression of sentiments, whether in the classroom or in his writings (to which he had, of course, much more access). Here is Henderson admonishing his colleague in truly Paretan style:

Notes on Applying the Word "Barbarous" to the
English System of Weights and Measures

1. Barbarous is a term that ordinarily arouses the feelings of those who hear or read it. For this reason, it introduces the play of the sentiments, not for everybody but for some people, whenever it is employed. . . .
2. Historical writing that does not introduce the play of the sentiments in this manner is a different thing from historical writing that does, and there are many historians and others who prefer not to arouse the sentiments of their leaders [readers] or to seem to express their own sentiments in their professional writings. Some of them are the very persons who feel [*sic*] that this preference has sound, logical foundation in the wide induction from experience that the introduction of the play of the sentiments into historical writing prevents or seriously interferes with that important task of the historian, the objective characterization of the sentiments and the play of the sentiments among the people whose deeds and words are the subject of the discussion.
3. It is my observation that a large proportion of the best historians at the present time have adopted the position just stated.
4. Therefore, they frequently express unfavorable opinions of some of the things you write because words like barbarous, which to them are danger signs, appear frequently in a certain class of your writings.
5. I have, as a result, found myself not infrequently in the position of defending your writings on the ground that if these words are excluded, no substantial change whatever is made in the meaning of what you write. Since I am very anxious that people should appreciate the value and importance of your work and since I find by experience that the use of such words frequently does interfere with that appreciation, I am disturbed when I encounter instances of the phenomenon in question.

—Henderson to Sarton, 24 Nov. 1936, Houghton Library, Harvard University it was a few years later that all this remained much the same.[2]) Looking back, one is inclined to say that if Henderson still dressed in Edwardian style, Sarton still thought in Edwardian style. Both were thoroughly engaging in their fashion; neither is now readily reproducible.

As a mere graduate student, I knew nothing, of course, about the grim vicissitudes Sarton was experiencing in the determined effort to supplement his own scholarship with institutional arrangements designed to advance the cause of the historiography of science. But here is Conant's retrospection on Sarton's incessant efforts at this time (when Conant was president of the university and a self-declared amateur in the history of science):

> This is not the time or place to summarize the history of Professor Sarton's long years at Harvard, his prodigious scholarship, his editorship of *Isis* and *Osiris*, and his vain attempt during the depression years to persuade Harvard or any other university to endow what he considered a minimal department of the history of science. That we are meeting here tonight with a teaching staff in the history of science at Harvard in active service, that a flourishing undergraduate and graduate field of study in history and science has long been characteristic of this University are some of the fruits of George Sarton's long uphill struggle to make the history of science an important part of the American scene.[3]

But this public statement does not fully reflect Conant's complex image of Sarton back in the 1930s, which evidently was, and long remained, ambivalent. That ambivalence was expressed in a letter written almost forty years later regarding the first biographical piece Arnold Thackray and I published about Sarton: "You are quite right in giving Henderson a key place in your story. I talked to him more than once about Sarton and he reported on his difficulties with this stubborn genius. Henderson often served as an intermediary. He understood how exorbitant were Sarton's demands. Your footnotes 29 and 30 are quite correct. My viewpoint was greatly influenced by Henderson."[4]

But enough about those hard times for George Sarton. In an obviously Tristram Shandy mode, where it takes more time to record life than to live it, I have left my youthful self in the fall of 1933 knocking on the door of that austere scholar's study in Widener, quite determined yet rather fearful of this first face-to-face audience with his august presence. (I say "august presence," for so it seemed to me at the time, although he was then still in his forties, just as I say "first audience" since I had not before had a private session with him, having attended his course only when I could escape from duties as a teaching-and-research assistant to the sociologist Pitirim Sorokin.) On that initial well-remembered occasion, the reputedly unapproachable scholar did not merely invite me into his "tiny book-lined study"; he positively *ushered* me in. Thus began my short, incompleat, and sometimes unruly apprenticeship, followed by an intermittent epistolary friendship that continued until his death in 1956. I

[2] I. Bernard Cohen, "A Harvard Education," *Isis*, 1984, *75*:13–20.

[3] James B. Conant, "History in the Education of Scientists," *Harvard Library Bulletin*, 1960, *14*:315–333, on p. 317.

[4] Conant to Merton, 12 Sept. 1973. "The story" refers to Arnold Thackray and Robert K. Merton, "On Discipline Building: The Paradoxes of George Sarton," *Isis*, 1972, *63*:473–495. See also John Murdoch, "George Sarton and the Formation of the History of Science," in *Belgium and Europe: Proceedings of the International Francqui-Colloquium* (Brussels/Ghent, 12–14 November 1981), pp. 123–138; and *Sarton, Science, and History: The Sarton Centennial Issue, Isis*, 1984, *75*:6–62.

began that first audition by telling of my plans for a dissertation already begun. I cannot say that he greeted those plans with conspicuous enthusiasm; instead he mildly suggested that so large a canvas as seventeenth-century English science might be a bit excessive for a novice. But he did not veto the idea. I should describe his response as, at best, ambivalent. Having registered his doubts, he then proceeded to tailor a research course to the needs of the first graduate student to have come to him from the social sciences since his arrival at Harvard some seventeen years before.

I now suspect that the unheralded appearance of a young sociologist-in-the-making may have reactivated his own youthful ecumenical vision of transcending disciplinary boundaries. Recall only his vision, full of innocence and hope, of the about-to-be-launched *Isis* as "at once the philosophical journal of the scientists and the scientific journal of the philosophers, the historical journal of the scientists and the scientific journal of the historians, the sociological journal of the scientists and the scientific journal of the sociologists."[5] As one notes, that daunting aspiration called not alone for a philosophy, history, and sociology of science but also for the sciences of philosophy, history, and sociology, all to find suitable expression in this variously ecumenical journal. That aspiration, it might be observed, was not much diminished by the circumstance that two years after its founding in 1912, *Isis* had acquired a world total of 125 subscribers. Of all that I had not the remotest idea when I venturously crossed the threshold of Widener 185, where worked the founder-editor of *Isis* and the author of the newly published monumental two volumes of an *Introduction to the History of Science,* which had managed to make its way from Homer through the thirteenth century in some 2,000 closely printed pages. Since, not quite incidentally, he was also a Harvard lecturer, I was there to ask that this composite personage break through all bureaucratic barriers to establish a research course for a neophyte sociologist.

Happily, Harvard was not in the hands of bureaucratic virtuosos and manifestly that special course was soon arranged; else I would not be thinking back on the devices this early master of the art and craft of the history of science invented to bring that maverick sociologist across academic boundaries into the then hardly institutionalized discipline of the history of science.

And now I undermine credibility by reporting that, during those many years—first as student and apprentice, then as journeyman and junior colleague, and finally as a properly certified scholar in my own right—I do not recall having been seriously irritated by this deeply committed, often impatient, and sometimes difficult scholar. Considering that he has been declared variously exasperating and downright abrasive by early colleagues and later students—I again need instance only his ambivalent advocates L. J. Henderson and James Conant and his student I. B. Cohen—it appears either that I simply lacked the same sensibility or the same range of close, continued interaction, or perhaps, that I have managed to repress, beyond all hope of retrieval except through the deployment of drastic psychoanalytic techniques, a deep underlying irritation that would evoke an intolerable conscious sense of guilt were it allowed to surface.

[5] George Sarton, "Histoire de la science," *Isis,* 1913, 1:3–46; on this and other aspects of her father's life as seen by a poet-and-novelist observer, turn to May Sarton, *I Knew A Phoenix* (New York: Norton, 1959).

I reject that last plausible hypothesis (be it noted without a betraying excess of protest). It simply doesn't wash.

There is yet another evident hypothesis: that in truth, George Sarton happened to treat me with friendly care, even with solicitude. This is somewhat more plausible. It has the further merit of being in accord not merely with possibly undependable memory traces but with personal documents. From them, the plain fact emerges that I liked and appreciated Sarton even when he was having at me for departures from the Comtean faith or, quite rightly, was reminding me of defections from norms governing the several roles of the scholar, such as my not getting reviews of books or referee reports in on time. Nor is it surprising that I should have remained attached to him, early and late in our evolving relationship. For as I have discovered only now in reliving the history of that relationship for this centenary moment, he had bound me to him—not with any such intent, I believe—by a flow of gifts, freely bestowed, which in their cumulative outcome may have affected my life and work in ways that have little or nothing to do with substantive doctrine or method of inquiry but much to do with discovering the pleasures and joys, as well as the nuisances and pains, of life as a scholar. I now see that he provided an accumulation of advantage,[6] thus leading me to incur a debt that called for a life of continuing work long after the insidious temptations of an easy retirement have been painlessly resisted.

Only now, decades after the events, have I come to recognize the patterned flow of the gifts, material and symbolic, which this ostensibly peripheral mentor bestowed upon me. And should I be exaggerating their import and consequences, as I may be doing in the first flush of their composite discovery, they remain nevertheless as I describe them. But if that large claim of the Sartonian largess is to persuade me, let alone you, they must not rest on vagrant memories—that is, memories without visible means of documentary support. For that reason, I shall draw upon fragments of the correspondence between us, as a basis for the rest of this episodic glimpse into George Sarton's mentorial style.

THE GIFTS

The first gift was his accepting a graduate student drawn from a department of learning in which he took no part. By intimation rather than in so many words, this was on condition that I did not threaten his "disciplined routine" of scholarship or require him to abate "the fury with which he set himself to work."[7] Having made that evident, he went on to provide me with a place in the large workshop adjacent to his small study, which I shared, to a degree, with his secretary, Frances Siegel, and his research associates, the formidable Dr. Alexander Pogo in the field of astronomy and the accommodating Dr. Mary Cath-

[6] The "accumulation of advantage" refers to "processes of individual self-selection and institutional social selection [which] interact to affect successive probabilities of access to the opportunity-structure"; Robert K. Merton, *The Sociology of Science: An Episodic Memoir* (Carbondale: Southern Illinois Univ. Press, 1979), p. 89. The pages following in that memoir provide an account of the process in the case of Thomas S. Kuhn; the following pages of this account testify to the accumulation of advantage I derived from my own apprenticeship to George Sarton. For the initial formulation of the concept, see Merton, "The Matthew Effect in Science," *Science*, 1968, 159: 56–63.

[7] May Sarton, "An Informal Portrait of George Sarton," *Texas Quarterly*, Autumn 1962, 101–112.

erine Welborn in medieval studies. That microenvironment itself constituted a second-order gift, for I learned many now-indeterminate things from that variegated pair of talented associates, albeit through a kind of cognitive osmosis rather than through formal training.

From the beginning, George Sarton did much to help set me on the path of scholarship. He proceeded methodically—he was methodical in most things—to transform me from a graduate student, struggling with preliminary work on a dissertation, into a tyro scholar addressing an international quasi community of scholars in print. This he did first by opening the pages of *Isis* to me. During the next few years, he accepted several articles of mine along with some two dozen signed reviews and another twenty or so entries for its annotated critical bibliographies.

In retrospect, I am persuaded that this initial run of scholarly experience served as both catalyst and exemplar. This I infer from finding that my first batch of published reviews, all eight of them, appeared in *Isis,* and that I soon went on to write a good many articles and reviews for other journals during that period. Moreover, had it not been for a publication schedule noticeably slowed by having *Isis* printed abroad—this by the St. Catherine Press in Bruges—the paper entitled "The Course of Arabian Intellectual Development, 700–1300 A.D.," which was written for *Isis* (in collaboration with Sorokin) might also have been my first article to appear in print. At any rate, I have a note from Sarton, dated just two months after he had admitted me to Widener 185 and addressed to me at the infirmary where I had completed the manuscript. In it, he writes: "I will try to come to see you before I leave" (this, for a needed restful cruise in the West Indies) and then appends the seemingly casual postscript: "Will be delighted to publish your paper in *Isis*."[8] Eighteen months later, it was in print.

The flow of gifts continued. Once equipped with a desk in the Sarton workshop, I was allowed to move freely through the fabulous bibliographic files, asked to serve as referee for the vanishingly few manuscripts with a sociological tinge, and enabled to read selectively in the galleys of the forthcoming contents of *Isis*—the latter a privileged access that would dramatically affect the oral examination on my dissertation. But months before that fateful occasion—indeed, before I had actually completed the dissertation—Sarton is writing me an altogether astonishing letter which reads in its entirety thus:

3510.08[9]

Dear Merton,

I have examined your thesis with great interest and have read much of it. I think it is an excellent piece of work and warmly congratulate you.

[8] I also have this note: "Dear Merton: I also received Dr. Sarton's letter of acceptance of the paper. What is the matter with you? And for how long you are in the Infirmary? Wishing you to be out of it as soon as possible. Cordially yours, P. Sorokin"

[9] In much of his correspondence and in his journal, Sarton preferred his own calendrical notation to the conventional ones. He saw no great need for the innovation; it was only another salute to rational order. His sequential notation moves steadily—one might say, inexorably—from the (tacit) largest calendrical unit of the century with its implied millennium, to the year, the month and finally, the day of the month. In this rationally ordered series, if one were compulsively minded to be ever more specific, one could move without breaking stride to the largely eponymous day of the week, then to the hour of the day, and so on. The notation was for personal, not historical, use: As he put it to me, he could safely omit the millennium and century since, having entered the twentieth century at age 16, he saw no danger of his staying on to enter the twenty-first. I soon became

The lack of a table of contents—including the chapters written & unwritten—makes it difficult to appreciate the symmetry of the whole structure.

From p. 267 on should in my opinion form a new chapter, Chapter X.

[In the event, it did. And then comes the paragraph with its climactic gift.]

The sincerity of my praise of your work will be best established by my readiness to publish it in *Osiris,* vol. 2 or 3, if H.U. or another agency is ready to share the financial burden and risk with me. This would be the cheapest mode of publication.

[In the event, neither Harvard nor any other agency shared the burden and the risk; my mentor himself provided the functional equivalent of a publishing grant from the nonexistent National Science Foundation. And then Sarton concludes the letter with a manifestly ambivalent judgment.]

should

The work might possibly be somewhat condensed, notably the religious part—though this might be difficult, as I found no trace of prolixity. [In the event, this part was not condensed in the published version.]

With kind regards & best wishes,
George Sarton

[And then, an afterthought expressing the lifelong Sartonian concern with indexes, about which I shall have more to say.]

In the case of publication an index should be added, but it might be compiled on the page proofs.

A few words about the magnitude of that gift. During my impromptu rather than regularly scheduled sessions with Sarton, he had made it clear that he preferred not to discuss my developing dissertation nor to see the manuscript until it was well-nigh complete. Instead, those sessions were largely given over to his telling an interested listener about the work life of a scholar with a defined mission: about the long frustrated yet continuing aspirations for an institute or a department of the history of science, about the problems of keeping *Isis* intellectually and financially solvent, about the slowly evolving work on the third huge volume of the *Introduction,* about the extraordinary array of requirements for the proper education of an encyclopaedic historian of science (an exceedingly demanding array which was much moderated in his later public statements on the subject), and so on. Thus that letter with its emphatic vote of confidence in my manuscript came without the least prior intimation that he had accepted in the event what he had understandably doubted in the intent as an excessively ambitious subject for a dissertation.

Nor, of course, was that gift wholly symbolic; it had a decidedly practical aspect. Like other newly minted Ph.D.s in those Depression years of the 1930s, I had pretty much assumed that the dissertation would not be published since it would see print only if I should subsidize publication (as I manifestly could not). Then came the Sarton offer, with its contingent-subsidy clause soon removed. I did not refuse that gift, either.

Even so, all this was only prologue to the dissertation defense a short while later. As sponsor and chairman, Pitirim Sorokin was hard put to piece together an appropriate examining committee for this out-of-phase dissertation. The mandatory three members of the understaffed Department of Sociology were Sorokin himself, whom I was assisting in writing the chapters dealing with sociological aspects of scientific discovery and technological invention in his four-

converted to his notation in certain contexts and take pleasure in observing some of my own students (and a few of their students in turn) adopting it as a friendly salute to the teacher of their teacher.

volume *Social and Cultural Dynamics;* the young instructor Talcott Parsons, still two years away from his masterwork, *The Structure of Social Action,* and with no public identity as a sociologist since he had published only two articles all told, which derived from *his* dissertation; and Carle C. Zimmerman, the rural sociologist Sorokin had brought with him from the University of Minnesota. The fourth member of the committee was George Sarton.

In an action composed of equal measures of deference and prudence, Sorokin invited Sarton to begin the examination. His first question struck me dumb: "Mr. Merton, will you tell us, please, who discovered the greater circulation of the blood?" In a matter of milliseconds, as I now reconstruct it, these anxious thoughts raced through my mind: "What *is* he up to? How can he possibly ask this elementary question? After all, he knows that Henderson[10] has put us through our paces on Harvey and he knows that I've been visiting seventeenth-century England for several years, and he *has* said kind things about the dissertation. How *could* he ask that question? What *is* he up to?"

Rendered utterly desperate by the thought that my mentor was sadistically subjecting me to some arcane test of competence, I launched on this approximate reply (though not, I suspect, with as orderly a syntax): "Of course, the greater circulation of the blood was discovered by William Harvey. Some claim that it was intimated in his lecture notes of 1616, although he didn't get around to publishing it until 1628, and some maintain that even his *De motu cordis* . . . "—and so on and so on, in the familiar textbook style.

Then came this desperate plunge into irrelevance: "But for historians of science, the recent excitement lies in the new confirmation, called for some years ago in your *Introduction,* that the thirteenth-century Arab physician Ibn al-Nafīs did indeed discover the lesser pulmonary circulation, long before its independent discovery first by Servetus and then by Columbo. It should be said, however, that he arrived at the lesser circulation, not through dissection, which was of course taboo in his culture, but on strictly theoretical grounds. Furthermore, . . ."

At this point, George Sarton literally rose to the occasion. With expressive disbelief and enthusiasm, he leapt to his feet, pounded the large library table round which sat the inquisitors and their innocent victim, and exclaimed: "How *could* you know of that confirming evidence? My old friend Max Meyerhof found several manuscripts of the Arabic text in Cairo and published them in a specialized German journal in the history of medicine which you would surely have no reason to read. Later, he sent me a condensed translation for *Isis* where it appears in a belatedly distributed issue. Tell me, how *do* you know of this new evidence?"

For a moment, I was anxious rather than triumphant. Would my examiners from Sociology think that this was all a put-up job between Sarton and myself? Nevertheless, I went on to explain: "But as you know, Dr. Sarton, when I'm at my desk in your Widener workshop, I make a point of reading certain articles in galleys and Meyerhof's happened to be one I read."

[10] L. J. Henderson, the author of the much esteemed book *Blood: A Study in General Physiology* and guiding light of the famed seminar on Pareto I had attended in its first year, could not be present at the examination. This inevitably invites the query, had he been there, would Sarton have begun the proceedings in the same way?

The rest of that *rite de passage* known as a doctoral examination was smooth and pleasurable sailing.[11] But to this day, I do not truly know what Sarton had in mind. Having been an examiner on scores of such occasions, I suspect that he introduced that elementary question simply to put me at ease. This imputed intent becomes the more plausible in light of the journal entry on his own doctoral examination. There he writes: "I passed my examination pitifully: the first ordeal toward the doctorate in physical and mathematical sciences. It made a very painful impression on my professors and will do me a lot of harm at the final examination."[12]

Whatever his intent, George Sarton had bestowed another, possibly inadvertent gift. For had he not mystified me by that opening question, I would not have had the occasion or the temerity to tell of Ibn al-Nafīs and thus to impress my professors by that display of new-found, distinctly limited, and altogether irrelevant erudition.[13]

Sarton's gifts of publishing the dissertation and getting me off to a grand though unearned start in the examination belong to the class of what the anthropological poet-ethicist Lewis Hyde describes as "threshold gifts." These, he notes, "mark the time of, or act as the actual agents of, individual transformation."[14] Almost as though he were acting out the concept, Sarton went on to adopt the explicit symbolic language of gift-giving, as he proceeded to mark and to facilitate my passage from apprentice to journeyman, my transformation from a graduate student into a junior member of the Harvard faculty. He proposed that I join a company which included the distinguished medieval historians Charles H. Haskins of Harvard, author of *Studies in the History of Medieval Science* (revised ed. 1927), and Lynn Thorndike of Columbia, well along on what would become his unique eight-volume work, *A History of Magic and Experimental Science* (1923–58), and, to go no further, the Yale neurophysiologist, bibliophile, and historian of medicine John F. Fulton, who had already published his magisterial bibliography of Boyle and would soon start work on his cele-

[11] I need hardly say that, in the absence of a tape-recording or even an entry in a nonexistent diary, these quotations from that ancient and anxious examination are merely approximate, not exact. That potentially traumatic but actually consummatory experience evidently lingered in subliminal memory. Only now do I come to recognize the context of the following passage-cum-note in my book *On The Shoulders of Giants* (New York: Harcourt Brace Jovanovich, [1965] 1985), p. 265: "[Sarton] writes also of that Bolognese jurist and equine anatomist of the sixteenth century, Carlo Ruini, that he was 'standing on the shoulders of Vesalius and others and applying their methods to the horse.' " The appended note reads: "I should perhaps add (after Sarton) that Ruini did NOT discover the greater circulation of the blood, despite that plaque to the contrary put up by the veterinary school of Bologna. You can discover the grounds for rejecting this mistaken claim to priority and the truncated simile in Sarton's *Appreciation of Ancient and Medieval Science during the Renaissance,* as published in 1955 by the University of Pennsylvania Press, p. 123."

[12] Quoted by May Sarton in *I Knew a Phoenix,* p. 61.

[13] Some thirty years later, I was to read a manuscript on the history of the lesser circulation by a new-found friend, André Cournand (who not long before had received the Nobel prize for his pioneering cardiopulmonary research), in which he concluded that Ibn al-Nafīs's rather cryptic remark about blood being aerated in the lungs hardly qualifies as a discovery of the pulmonary circulation: see André Cournand, "Air and Blood," in *Circulation of the Blood,* ed. Alfred P. Fishman and Dickinson W. Richards (New York: Oxford Univ. Press, 1964), pp. 3–70, esp. pp. 15–17. That the question remains moot can be seen from the counterinterpretation of the very same passage by Albert Z. Iskandar, "Ibn al-Nafīs," *Dictionary of Scientific Biography* (New York: Scribners, 1974), Vol. IX, pp. 603–606.

[14] Lewis Hyde, *The Gift: Imagination and the Erotic Life of Property* (New York: Random House, 1983), pp. 40–41.

brated biography of Harvey Cushing. But that is not how George Sarton phrased his invitation; here are his actual words:

3712.21

Dear Merton,

I have a fine proposition to make to you—as a Christmas present. Would you care to become associate editor of *Isis*, your domain being defined, e.g., [as] "social aspects of science"?—You would not be expected to do more for *Isis* than you have done thus far, but, I believe, this new title would be professionally helpful to you. Should you accept—as I hope you will—please send me as much of a "curriculum vitae" as you would like me to publish in *Isis*. See vol. 27, 330.

w.k.r.
George Sarton

A year or so later, there is another threshold gift. To my mind, a gift of great symbolic magnitude; to Sarton's mind, evidently one also designed to advance my role as an academic journeyman. Here, in truncated form, is the letter carefully addressed to *Dr*. R. K. Merton:

May 3, 1939.

Dear Merton,

The Fifth Congress of the Unity of Science will meet at Harvard University on Sept. 5–10, 1939. On one of these days not yet determined there will be a joint meeting of the International Institute for the Unity of Science and of the History of Science Society.

In my capacity as chairman of the program committee of that special meeting I am now writing to you. The idea is to have four items as follows [do note, once again, the company he asks me to keep]:

1. Prof. Werner Jaeger: Aristotle
2. Dr. De Lacy: Leibniz
3. Prof. G. de Santillana: The Encyclopaedists
4. : Comte

I much hope that you will accept to deal with the last item. This would give you a good opportunity of distinguishing yourself. . . . The matter being urgent I would be grateful if you would answer it promptly, and much hope that your answer will be Yes.

With kind regards,
George Sarton

But alas, as I was compelled to report, I could not answer yes. For at the time of the Congress, I would be taking up my new post as contingent chairman of the Department of Sociology at Tulane University in the remote and inviting city of New Orleans.[15] In long retrospect, I think it is perhaps just as well for the relationship between my erstwhile mentor and myself that I could not accept the invitation to speak my mind on Comte and his positivistic descendants.

AN UNRULY APPRENTICE

That reflection concerning Comte gives me pause. I must not give the impression that all was sweetness and light between that mentor and me, that he was ever

[15] Sarton soon wrote me there: "It is remarkable that the destruction of the [Huey] Long machine followed so closely your arrival in New Orleans. What are you going to do next?" And a year later in another moment of Flemish humor: "Congratulations for the New York appointment, which will bring you nearer to us, though it takes away my main reason for visiting New Orleans. May be I'll never get there now and it will be *your* fault!"

the benign, kindly spoken master and I ever the compliant apprentice. That was not the case. There were times, especially in his positivistic moments, when he was the exigent and angry master and I the brooding and unruly apprentice.

There was the time, for one instance, when I brought him, as a token gift in the asymmetrical reciprocities that mark the relationship of master and apprentice, an offprint of my first published paper—this appearing in a journal of sociology rather than, as I had hoped, in *Isis*. The gift was no doubt designed to intimate that my master's confidence in me was not entirely misplaced. Entitled "Recent French Sociology," it is as condensed and bibliographically crowded, if I may say so, as any entry in George Sarton's great *Introduction*. But in it I allude mockingly—not to say, flippantly and arrogantly—to "the enlightened Boojum of Positivism." My mentor did not take kindly to that facile (and Carrollesque) depiction. Still, this early episode led to little more than a symbolic rap on the knuckles. That was nothing at all to compare with my mentor's outrage, two years later, when I committed the cardinal sin of harshly criticizing Comtean positivism as set forth by F. S. Marvin (rather than criticizing that disagreeable man Comte himself, which would have been quite all right). That I should have done so as a guest lecturer at Sarton's invitation served only to compound the offense. That performance elicited this note:

3511.17

Dear Merton,

I think your talk was very good. Thanks.

I was sorry to detect in your character a streak of cannibalism. At least your ferocious treatment of Marvin suggested that. Here is an old man who has devoted his whole life to the defense of generous ideas—you dismiss his collection of essays as if they deserve no attention. He repeats himself. Of course, he does; every one who has an important message *must* repeat himself time after time, for he knows that most people will only begin to understand at the 1000th time.

w.k.r.
George Sarton

Evidently I had touched an exposed Comtean nerve. And yet Sarton eventually did forgive—he was not of that Lethean mind that would forget—my behavior. Three years later, as I have reported, he was inviting me to speak on Comte at the exceptional joint meeting of the Unity of Science group and the History of Science Society. Thinking back on that earlier episode, I am inclined to agree with the import of Sarton's plain-spoken judgment on the style if not the substance of what must have been a blustering assault rather than a closely argued criticism. Perhaps I had engaged in the naïve and nasty game of simply scoring points off the other (absent) fellow, thus seeking to exhibit my seeming intellectual powers; and then again, perhaps not. I like to think that even as a callow graduate student I was well out of that unappetizing game. More in point, I like to think that George Sarton's angry rebuke persuaded me once and for all that strong scholarly criticism need not be uncivil.

Along with learning from Sarton's response to the cardinal sin of pride (if not, I now say defensively, of sloth), I also learned from his response to venial sins in the scholarly life. I can tell here only a very few symptomatic episodes in the acquisition of craft skills and craft norms. From the start, Sarton had made it plain that it was good for a novice scholar to contribute his share to the common stock of knowledge through original research recorded in articles and monographs. But that was not enough. The role of the scholar called for more. One was obliged, for example, to do one's part in enlarging and facilitating the access of other scholars to the growing mass of knowledge claims by the writing of book reviews and bibliographic notes.

In this mode of scholarly work, Sarton himself was of course the incomparable exemplar. Deploying a typical piece of Sartonian arithmetic, an entry in his journal of 1952 estimates that, over a span of forty-one years, he had contributed about 100,000 notes to the critical bibliographies in *Isis*—those periodic, systematic, and annotated bibliographies which continue to this day. Sarton goes on to calculate: "I have written an average of *six* notes a day (holidays included). It is like the walking of 1000 miles in 1000 consecutive hours. To write six notes each day for a few days is nothing, but to do so without stop or weakness for 14,975 days is an achievement. It implies at least some constancy."[16] Nor did this calculus include the hundreds of his detailed reviews in the pages of *Isis*. Although he surely expected nothing of such magnitudes from others, he was concerned to set his novice on the right track. So it was during the half dozen or so years of my novitiate that I found myself writing, at a rather more restrained pace, some twenty articles and sixty-five reviews in various journals, along with those twenty entries for the critical bibliographies of *Isis*. Compared with the vast magnitudes sustained by my mentor, that seems little more than evidence of good intentions.

As editor-mentor, Sarton was also concerned to inculcate the norm of timeliness. After all, there were publishing deadlines to be met. Judging from his many handwritten notes to me on my reviews for *Isis,* he was generally satisfied with them on the counts of number, quality, and probity. But from time to time he was put off by my venial sin of procrastination. That sin he treated as a misdemeanor calling only for light, sometimes playful reproof. (That his sanctions were so gentle may help account for my recurrent attacks of procrastinitis over the years.) Thus, a note delivered to me at nearby Emerson Hall in 1937 consists simply of this temperate prod: "The 'scientist in action' [the title of a book by W. H. George] was sent to you last summer. What about the 'reviewer in action'?" That note is dated October 31st; the next note, postmarked the very next day, reads: "Many thanks for the very good review of George's book." Evidently I had not long remained tardy.

In contrast to such mild injunctions for timely action, Sarton expressed intense commitment to another craft norm: the preparation of an adequate index to a scholarly book was for him a sacred trust. So it was that, in his reviews, he would severely rebuke the negligent authors of books that lacked an index or sported one that was perfunctory and therefore largely functionless. Such authors were guilty of the moral dereliction of requiring serious readers who wanted to make limited, specific use of those books to engage in a drawn-out search through its pages and of requiring readers who vaguely remembered a salient passage in a book read some time before to reread much of the book in order to locate that passage. An index was for Sarton the instrumental expression of a technical norm and the symbolic expression of a moral norm, with the second supporting the first.

[16] Quoted in May Sarton, "An Informal Portrait of George Sarton" (cit. n. 7), p. 108.

Along with the exercise of moral suasion and public sanctions, Sarton provided a prototype of indexing in unexampled detail. The first volume of his *Introduction,* running to almost 800 pages of text, has an index of 52 double-columned pages. The second volume of 1,138 pages of text has an index of 110 pages, supplemented by a "meager" Greek index of three pages. But it is the third volume (in two parts), with its almost 2,000 pages of text devoted to "science and learning in the fourteenth century," which engaged Sarton's indexing energies to the full: the "General Index" in English, prefaced by "Introductory Remarks" on the compiling of the index, runs to 173 double-columned pages; the Greek index to another 8 pages; the "Chinese Index and Glossary to Volumes 1, 2, and 3" (prepared with the aid principally of J. R. Ware) requires another 40 pages and, finally, the Japanese index (aided by his Harvard colleagues, Serge Elisséeff and E. O. Reischauer) runs to 14 pages more. Altogether, an assemblage of indexes comprising 235 pages. (We who know of Sarton's deep interest in Muslim contributions to science and learning might be tempted to ask: "But where is the index of Muslim [Arabic] names?" An apt question with a ready answer: those names were faithfully transliterated and incorporated in the General Index.)

At that time I had been at Columbia University for some years and so knew nothing at first hand of Sarton's vast labors on this array of indexes. However, a letter from him, dated 4710.04, reported with great relief and a tinge of pride, that "I had to work here the *whole* summer, without let-up, to organize the index to my vol. 3. My secretary is typing it now, 1,000 pages!" That note to me is evidently condensed from an entry in his journal on his sixty-third birthday:

> 4708.31
>
> This birthday ended the hardest summer of my life—hard labor on the index to Vol. III. I began the preparation of the main index on 4707.07 and ended 4708.15. Greek Index 4707.12–18; Chinese Index 4708.15–26; Japanese Index 4707.27–29. The main index was ended and the Chinese one begun on The Assumption—the most memorable Assumption of my life next to the one in 1925 when Mabel, May and I were in Lourdes, in the Pyrenees.
>
> [He then sums up his deep-seated feelings about the intrinsic and symbolic meanings of an authentic index.] Hard as it was, the work was bearable because I thought of its usefulness, and because I realized that this was the last large (gigantic) index of my life.[17] An index is the nearest approach in the world of scholarship to charity in common life.[18]

Not long afterward, Joseph Needham is validating Sarton's article of faith:

> 2d January 1949
>
> Many thanks for the proof copy of the Chinese-Japanese index of your great work. I have bound it and it is in daily use on our table.[19]

In this occasion-induced retrospect, two thoughts about my own indexes come to mind. One is the startled recognition, delayed for almost half a century, that

[17] This turned out to be indeed the last gigantic index but scarcely the last substantial one. The index of Vol. I of Sarton's last book, *A History of Science* (Cambridge: Harvard Univ. Press, 1952, 1959) runs to 29 pages; the index of Vol. II, completed just before his death, to 26 pages.

[18] Quoted in May Sarton, "An Informal Portrait of George Sarton," pp. 109–110.

[19] Needham to Sarton, Sarton papers, Houghton Library, Harvard University.

the editor had allowed my dissertation to appear in the newly founded *Osiris* with only an index of names running to a mere nine pages. The exigencies of meeting a publication deadline had evidently prevailed. The other belated thought derives from a recent review of the vicennial edition (1985) of my book *On the Shoulders of Giants: A Shandean Postscript,* which calls attention to its idiosyncratic index in the form of an "Onomasticon or A Sort of Index" that goes on to identify the "persons and personages" mentioned in the book (with Shakespeare, for an example, being identified as an "inveterate plagiarist of 20th-century psychological knowledge"). Only now does it occur to me that this eccentric index may have been an unwitting salute to George Sarton's enduring insistence on the symbolic as well as utilitarian value of indexes. That supposition gains credibility from May Sarton's note evoked by that review:

April 27 [1985]

> Dear Bob,
> If the index is to charity of scholars—as G. S. always said!—a sense of humor in indexing is even more charitable. . . .

Reflecting on the time when George Sarton was socializing this apprentice into the scholarly role, I am put in mind of a rather more notable event in sociological scholarship. This was the advent in 1935 of the profoundly original and lately rediscovered monograph by the Polish bacteriologist and self-taught sociologist Ludwik Fleck, audaciously entitled *Genesis and Development of a Scientific Fact.*[20] Obscurely published in Switzerland, this pathbreaking but not then pathmaking book never was sent to *Isis* for review. Had it been, Sarton would probably have turned it over to me. I like to think that I would have been taken then with its striking sociological notions of "thought-style" and "thought-collective" just as I was, some four decades later, while coediting its English translation. After all, it was just about then that Sarton had proved willing to have me introduce the subject of the sociology of knowledge into the pages of *Isis* with an article centered on the work of Max Scheler, Karl Mannheim, Alexander von Schelting, and Max Weber, along with the first "history" of the subject, Ernest Grünwald's *Das Problem der Soziologie des Wissens.*[21] Sarton himself retained enough interest in this field of inquiry to ask, ten years later, "whether you would not consider preparing a second paper on the subject dealing with the newer publications." With distinct embarrassment, I had to report having recently published just such a piece on the sociology of knowledge elsewhere.[22]

I suspect but cannot truly say that my early experience with Sarton persuaded me that it was not enough to do one's own scholarly work. As will be recalled, he held that one was also obliged to facilitate the scholarship of others. At any

[20] Ludwik Fleck, *Genesis and Development of a Scientific Fact,* ed. Thaddeus J. Trenn and Robert K. Merton, trans. Fred Bradley and Thaddeus J. Trenn (Chicago: Univ. Chicago Press, 1979); first published in German, 1935.

[21] Robert K. Merton, "The Sociology of Knowledge," *Isis* 1937, *27*:493–503.

[22] That embarrassment was compounded by a like episode during the time of my apprenticeship. Dated in the Sartonian style, 3808.29, his note reaches me at my nearby Harvard office: "I have read your paper on 'Science and the social order' with great interest. It is very good. I regret it was not my privilege to publish it in *Isis.* [And then the probably unintended coals of fire] On the other hand I rejoice that you had an opportunity of reaching another audience. I'll quote a long extract from it in *Isis.*"

rate, I must confess that I had not his staying power in the publicly observable role of reviewer: some thirty years later, my published reviews had lessened to a trickle as my uninstitutionalized editorial labors on the manuscripts of colleagues, nearby and at a distance, continued to grow. I trust that I do not violate George Sarton's precepts on this score when I find myself asking whether Schopenhauer had it right in declaring that the chief sin against the Holy Ghost of the intellectual life is to put down one's own work in order to take up another's.

Just a few more words to elucidate the experience of having been Sarton's much-benefited and grateful but unruly apprentice. The unruly aspect derived chiefly from marked differences between master and apprentice in what have been described as thought-styles, thought-collectives, and reference groups. The mature Sarton I had come to know plainly retained his youthful vision of an encyclopaedic history of science. He had never abandoned the emphasis upon an ecumenical history that would transcend the Western world to take account of the Islamic world as of China, India, and Japan. And, to a degree, he had retained his early humanistic interest in sociological perspectives. At times, he could even write that "the history of science in the main amounts to psycho-social investigation" and casually refer to "my sociology of science."[23] However, he became increasingly ambivalent toward a sociology that had largely lost its early Comtean, progressivist moorings. He could not bring himself to keep in close touch with the work actually being done in the contemporary sociology and psychology and did not much like what he did see of it.

Mantled in learning and temperamentally averse to the explicit use of analytical paradigms, Sarton continued to prefer the descriptive history of science, in his hands supported by an extraordinary range of bibliographic correlates and underpinnings. He did not take to systematic conceptual schemes, let alone to metatheory, metamethod, or metaphysics. This attitude of mind was far removed from the sort of sociological framework adopted in my dissertation, which was designed to generate analytic problems to guide the search for apposite historical materials. It was even further removed from my later efforts to develop what I described as "paradigms," as in the 1945 "paradigm for the sociology of knowledge" (published in that paper preempting Sarton's request for such a piece). Nevertheless, Sarton accepted all this from me—early and late in our relationship—although it was alien to the procedural and substantive character of his own pioneering vision of a history of science interwoven with a sociology and perhaps even a philosophy of science. He did not so much as hint that an apprentice should be a disciple.

In this regard, George Sarton was poles apart from another Harvard mentor, the sociologist Pitirim Sorokin. The contrast was palpable. An official sponsor of my dissertation, Sorokin took it as something of a cognitive rejection—even a betrayal—of his developing doctrine of historical cycles of ideational, idealistic, and sensate cultures that, in 1937, would animate the four volumes of his *Social and Cultural Dynamics*. Indeed, in the strongest possible language, he paid me the supreme compliment of concluding that I was clearly as mistaken

[23] George Sarton, *The Life of Science* (New York: Henry Schuman, 1948), p. 51; Sarton, *Horus: A Guide to the History of Science* (Waltham, Mass.: Chronica Botanica, 1952), p. 94, n. 87; see Thackray and Merton, "On Discipline Building" (cit. n. 3), p. 40.

as Ernst Troeltsch and Max Weber had been before me. That sense of my having badly let him down persisted, if one may judge from his ambivalent inscription in the one-volume edition of the *Dynamics* published twenty years later. Couched in amiable hyperbole, it reads: "To my darned enemy and dearest friend, Robert—from Pitirim." Sarton could no more have taken my divergence from his ideas and style of thought as betrayal than he could have engaged in the other extravagance of "dearest friend." In the tranquility of retrospect, however, I must conclude that it took no great effort to steer through the dangerous waters between the Scylla of Sorokin's passionate cyclicalism and the Charybdis of Sarton's Comtean progressivism. One need not agree fully with either of them to learn—quite different things—from both of them.

But if Sarton did not require an apprentice to become a disciple devoutly adopting his substantive doctrines and commitments, he did exert other, sometimes unpremeditated, influence. At this moment, I am mindful especially of one instance which neither he nor I could foresee. In 1935—just at the time of that doctoral examination in which he took so commanding a part—he was publishing another in his series of scholarly queries that appeared in *Isis*. This one was "Query no. 53—'Standing on the shoulders of giants.' " In elucidating the query, he compactly summarizes what he knows about the history of "that saying" and, linking it to his abiding interest in the idea of scientific progress, goes on to write:

> There must be other examples of its occurrence and I would be grateful to the readers who would kindly point them out. Examples anterior to the twelfth century would be particularly precious. Ideas may be compared to seeds which are sometimes slow in germinating but never die. Are there no traces then from the first to the twelfth century of the Senecan conception of the cumulative and progressive nature of knowledge? Inasmuch as there was a Senecan tradition, and that Seneca was even believed to be a Christian, I would not be surprised if such traces were eventually discovered.[24]

Sarton and I never got to talk about this query, neither then nor since. But apparently the seed imbedded in his query continued to germinate for some twenty-five years until I found myself responding to a letter from the Harvard historian Bernard Bailyn. He had read a recent article of mine which quoted "the epigram Newton made his own": "If I have seen further, it is by standing on y^{e} sholders of giants." Bailyn remarked that Etienne Gilson and Ernest Lavisse had attributed it to Bernard of Chartres. His letter sparked a rather lengthy reply. Fueled by my often reiterated immersion in the pages of *Tristram Shandy* over the years, the reply ran to several hundred pages. Since it refers frequently to George Sarton and his originating query, it saddens me to recall that it was not written until two years after his death in 1956 and not published until 1965.

Only lately, while checking certain details of this fund of memories in the Sarton archives located in the Houghton Library of Harvard, do I learn that in the very year my mentor was publishing his belatedly remembered query in *Isis*, he was also giving a version, in anticipatory fashion, of what C. P. Snow would articulate, a quarter century later, as the gulf between the Two Cultures. In an

[24] George Sarton, "Query no. 53 . . . ," *Isis*, 1935, *24*:107–109.

effort to demonstrate that the history of science and learning is uniquely qualified to bridge the abyss between the humanities and the sciences, Sarton variously employed the standing-on-the-shoulders-of-giants composite of simile, metaphor, epigram, and parable. This he had done briefly in his Colver Lectures published in 1931[25] and, with sharper focus, a few years later in a letter addressed to Henry James, son of the psychologist and philosopher William James and the nephew identified as "Harry" in the letters of Henry James. Since the later Henry James was variously engaged in philanthropic enterprises—he was a trustee, for example, of the Rockefeller Institute for Medical Research and an overseer at Harvard—one surmises that the letter represents yet another of Sarton's efforts to enlist support for the still-uninstitutionalized history of science at Harvard.

May 17, 1935.

to Mr. Henry James
522 Fifth Avenue
New York, N.Y.

Dear Mr. James,

In answer to your request of yester-afternoon I take great pleasure in sending you a brief statement concerning the subject of my studies,—the "History of science and learning." This is not very easy, but I will do my best.

Let me remark to begin with that the subject has been unduly neglected, being either left out of the curriculum altogether or treated casually by incompetent teachers. This neglect is due to the fact that our intellectual elite is divided into two hostile groups which we may call the literary group and the scientific one: members of the first group are not interested in science or know too little of it to study its history; scientists are not historically minded, and many of them are not even educated. That is a vicious circle which we must break.

The only bridge between these two groups is provided by our studies. It is not enough to study science in order to obtain more and more "results", and to make more and more discoveries. To claim that would be as foolish as to claim that nobody has any business with art except the creative artists. Few of us are artists, but most of us are deeply interested in the history of art—and that is as it should be. In the same way we should study the history of science: this would reveal to us another aspect of humanity, and a wonderful one.

Man is immeasurably more interesting than other animals because he alone is able to create such intangible values as beauty, justice, truth. Is not the history of these creative activities the most interesting part of the history of man? The scientific activity is particularly interesting from the historical point of view because it is not simply creative but *cumulative*. Our artists are not greater than artists of the past, our saints are not better than those of the past, but our scientists are undoubtedly more knowing. Michel Angelo stands upon the shoulders of Phidias, but that does not make him any taller. On the other hand, Newton stands upon the shoulders of Galileo and because of that he can see further. . . .[26]

Previously unpublished, this letter can serve to close these centennial recollections by an erstwhile, somewhat unruly, and long indebted apprentice who, half a century later, has not yet the temerity to claim that he stands on the shoulders of his mentor, George Sarton.

[25] George Sarton, *The History of Science and the New Humanism* (New York: Henry Holt, 1931). It is this book which is dedicated to "My dearest friend, E. M. S. the Mother of those Strange Twins, May and Isis."

[26] George Sarton to Henry James, Sarton papers, Houghton Library, Harvard University.