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On Discipline Building: The Paradoxes of George Sarton

By Arnold Thackray* and Robert K. Merton**

I believe one can divide men into two principal categories: those who suffer the tormenting desire for unity, and those who do not.

—George Sarton, *aet.* 20¹

The fundamental purpose is to establish the history of science as an independent and organized discipline.

—George Sarton, *aet.* 36²

I

THE PROCESSES BY WHICH KNOWLEDGE is institutionalized have yet to be explored in any systematic way. It is easier to sense than to articulate those characteristic changes in cognitive structure and social function which any field of learning undergoes as its legitimacy is established. Yet certain features may be delineated. The most obvious is the set of shifts that a field of learning experiences as it changes from being a diffuse, unfocused area of inquiry, at best tangential to the true intellectual concerns of its occasional votaries, to being a conceptually discrete discipline, able to command its own tools, techniques, methodologies, intellectual orientations, and problematics. This creation of a *cognitive identity* is only one facet of the institutionalization of a field of learning. A parallel set of shifts, which most often occurs at a later period in time, revolves around the creation of a *professional identity* for the new enterprise. This latter set of shifts has normally provided the focus for discussions of the professionalization of science.³ However, the creation of a professional identity is only one part of the process by which a discipline or group of disciplines are institutionalized.

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* Department of History and Sociology of Science, University of Pennsylvania, Philadelphia, Pennsylvania 19104 and **Department of Sociology, Columbia University, New York, N.Y. 10027. The authors wish to thank I. Bernard Cohen, A. Rupert Hall, Marie Boas Hall, May Sarton, and all those other colleagues who offered helpful comments on a draft of this paper, and the National Science Foundation for partial support of the necessary research.

¹ May Sarton, *I Knew a Phoenix* (New York: W. W. Norton, 1959), pp. 40–41.

² George Sarton, "Introduction to the History and Philosophy of Science (Preliminary Note)," *Isis*, 1921, 4:23.

³ See Everett Mendelsohn, "The Emergence of Science as a Profession in Nineteenth-Century Europe," in Karl Hill, ed., *The Management of Scientists* (Boston:Beacon Press, 1964), pp. 3–48;

Joseph Ben-David, *The Scientist's Role in Society. A Comparative Study* (Englewood Cliffs, N.J.: Prentice-Hall, 1971); George H. Daniels, "The Process of Professionalization in American Science: The Emergent Period, 1820–1860," *Isis*, 1967, 58, 151–166; Harriet Zuckerman and Robert K. Merton, "Patterns of Evaluation in Science: Institutionalisation, Structure and Functions of the Referee System," *Minerva*, 1971, 9: 66–100. On the theory of institutionalization, see Talcott Parsons, *The Social System* (New York:Free Press, 1951), esp. Ch. II, and Shmuel N. Eisenstadt, *Essays on Comparative Institutions* (New York:Wiley, 1965), pp. 3–68. On the institutionalization of fields of learning, see David Riesman and Christopher Jencks, *The Academic Revolution* (New York:Doubleday, 1968), and Terry Clark, "Institutionalization of Innovations in Higher Education: Four Models," *Administrative Science Quarterly*, 1968, 13:1–25.



George Sarton, in 1928, with Lady Jane Grey.

In the Western world, universities have most often provided the appropriate context in which a professional identity might be built. Their need of teaching cadres and their tolerance of research have permitted the growth of regular career structures directly or indirectly dependent on the discipline. Prizes, research grants, specially equipped facilities, prestigious positions, and honorific appointments are obvious manifestations of a field of learning which has created a professional identity for itself. The existence of career-related opportunities and rewards gives new meaning to an ability to perform well at the intellectual tasks of the discipline. The growth of such socially patterned arrangements inevitably transforms the enterprise on deeper levels. The movement from "gentlemen" to "players" is not simply a matter of changes in organization or possible personal reward. It also encompasses a transformation in the images of the discipline and its social and cultural functions as variously perceived by its practitioners, its patrons, and its larger public. Thus while it may be useful for analytic purposes to distinguish the emergence of cognitive identity from that of professional identity, the two are inextricably interwoven in the daily practice of any fully institutionalized field of learning, whether in the sciences or the humanities.

To point to these sociological regularities is to seem in danger of attributing effortless inevitability to what is always a matter of human struggle and corresponding contingency. Discipline building, more especially in its early stages, is a matter of personal, sometimes heroic, endeavor by some one or few persons seized with the possibilities of an as-yet-unrecognized, unorganized area of knowledge. The cognitive identity which a discipline assumes is itself profoundly influenced by the private vision of its founders. Through subtle processes both of assimilation and rejection, the tools, orientations, and problematics which initially characterize the new field are conditioned by its founders' moral ends, metaphysical assumptions, and particular *Weltanschauungen*. If they also possess the ability to create jobs and to place disciples in them, their influence on the professional identity of the emergent discipline will still further strengthen their impact on its cognitive form. Familiar if relatively unexplored examples abound. The way in which the nascent eighteenth-century discipline of empirical natural philosophy everywhere bore Isaac Newton's imprint is one. Another, on a different level, may be found in Émile Durkheim's impact on the new science of sociology.⁴ That in each case later practitioners revolted against the cognitive identity thus created reinforces rather than destroys the point. A further example, closer to home, can be found in George Sarton's intimate and paradoxical relationship to the discipline of the history of science.

In this paper we shall seek to bring out some aspects of the discipline-building process, by an immediate, limited focus on the activities of George Sarton and his relationship to the history of science, especially within America. This focus allows a

⁴ See Frank Manuel, *A Portrait of Isaac Newton* (Cambridge:Harvard Univ. Press, 1968), Ch. 13; Arnold Thackray, "The Business of Experimental Philosophy": The Early Newtonian Group at the Royal Society," *Actes du XIIe Congrès International d'Histoire des Sciences* (Paris:Albert Blanchard, 1970-1971), Vol. IV, pp. 29-34; Robert Bierstedt, *Emile Durkheim* (London:Weidenfeld & Nicolson, 1966); and for quite another kind of impact, see John Peel, *Herbert Spencer: The Evolution of a Sociologist*

(London:Heinemann, 1971), Chs. 8, 9; Lewis A. Coser, *Masters of Sociological Thought* (New York: Harcourt, Brace, Jovanovich, 1971). See also Arnold Thackray, *John Dalton, Critical Assessments of His Life and Science* (Cambridge, Mass.:Harvard Univ. Press, 1972), Ch. 2; J. B. Morrell, "The Chemist Breeders: The Research Schools of Liebig and Thomas Thomson," *Ambix*, 1972, 19:1-46; and Russell McCormach's forewords to *Historical Studies in the Physical Sciences*, 1970, 1971 (2, 3).

better understanding of Sarton's achievement. It also casts light on a particular phase in the historiography of science itself. Our discussion will inevitably fall far short of that full study Sarton deserves, even as it endeavors to exploit the wealth of available sources⁵ and to move beyond the character of pious memoir.

The rich significance of Sarton's life is far from being exhausted by the approach adopted here. As example we could point to how his career also provides illustrations of the role of the emigré or outsider in catalyzing intellectual and social development. Indeed his insistence on the centrality of an area of inquiry that others found at best tangential, and certainly of minor consequence to American culture, makes him a man doubly marginal to his adopted society. On a different plane we could point to how the daunting mass of material open to any student of Sarton illustrates a wider truth about the cornucopia of documentation and the corresponding procedural problems of "contemporary" history. The absence of a corpus of analytical and historical studies (e.g., on Harvard University between 1914 and 1945) renders precarious our understanding of Sarton's cultural milieu and underscores the provisional nature of such an essay as this. Conversely, his career and the Sarton archives offer important insights to those who would attempt the necessary broader accounts. Tempting as such themes are, this essay will abstain and concentrate on the central aspect of Sarton's life: his work as key figure in the early history of a discipline. That work found its focus as well as its fullest expression in the monumental *Introduction to the History of Science*;⁶ we

⁵ George Sarton (1884–1956) was a prolific author and correspondent. The sources for the study of his life are thus considerable and as yet barely touched. The memorial issue of *Isis* (1957, 49) contains a number of illuminating essays by colleagues, pupils, and friends, together with a complete bibliography of his publications. The analysis of Sarton's life and achievement by Professor I. Bernard Cohen is especially valuable. There are many other obituary notices in scholarly journals. More immediately helpful are his daughter's delightful *Sketches for an Autobiography*, published in May Sarton, *I Knew a Phoenix*. She has also written "An Informal Portrait of George Sarton" (*Texas Quarterly*, Autumn 1962, pp. 101–112). Sarton's varied writings are most easily sampled in Dorothy Stimson, ed., *Sarton on the History of Science* (Cambridge, Mass.: Harvard Univ. Press, 1962).

Some of Sarton's correspondence now preserved in Belgium is published in Paul Van Oye, *George Sarton, de Mens en zijn Werk uit Brieven aan Vrienden en Kennissen* (Brussels: Paleis de Academiën, 1965). Most of the letters are in French or English and date from his youth. The commentary is in Flemish. Further letters of this period (in French) are published in Suzanne Delorme, "La Naissance d'*Isis*," *Actes du XI^e Congrès International d'Histoire des Sciences*, 6 vols. (Warsaw, 1967), Vol. II, pp. 223–232. Lynn Thorndike's contribution to the memorial issue of *Isis* also includes some of their correspondence over the years.

Much Sarton correspondence undoubtedly

exists in other private archives. For instance, there are over 100 letters in the possession of Professor C. Doris Hellman of New York, who took a Radcliffe M.A. degree under Sarton's direction in 1931. The two largest collections of his letters are, however, publicly available, if heretofore unused. Somewhere between 20,000 and 30,000 letters from 2,108 and to 788 correspondents are preserved and indexed in the Houghton Library of Harvard University. An unknown number of letters (perhaps 500 to 1,000) to and from its president are also available in an uncatalogued state in the Carnegie Institution of Washington. We are indebted to the presidents, librarians, and staff of each institution for permission to examine and quote from these papers, identified as SHL (Sarton papers, Houghton Library) and SCI (Sarton papers, Carnegie Institution). We also wish to thank the registrar of Harvard University for information on the enrollment in Sarton's courses. The number and nature of the courses themselves and their relation to other offerings in the university may easily be traced in the annual catalogues of Harvard College. Similarly, Sarton's work for and in relation to the Carnegie Institution of Washington over the years may be followed in his reports, published annually in the Institution's *Yearbook* from 1919 to 1949. A full life of Sarton, drawing on these and other sources, remains a challenge and a desideratum.

⁶ George Sarton, *Introduction to the History of Science*, 3 vols. in 5 parts (Baltimore: Williams and Wilkins Co., 1927–1948).

shall therefore pay particular attention to it. But, as will become apparent, the *Introduction* was only one of a great variety of enterprises that Sarton undertook in his capacity as discipline builder.

Exploiting the liberty available to a pioneer, Sarton enjoyed a multiplicity of roles in relation to his discipline and played them all with a characteristic lack of self-awareness. A major one was that of propagandist. His evangelizing on behalf of his chosen subject inevitably calls to mind the way Francis Bacon served as propagandist for the field of science itself. And, like Bacon, Sarton had his most enduring impact in this vital, though little-acknowledged capacity. Other roles were more central to his life and mission. With a discipline to be created, a world to be won, the provision of tools, techniques, methodologies, and intellectual orientations lay uppermost in his mind and at the forefront of his actions. A cognitive identity for his new discipline was the primary goal, his own pattern of work the self-exemplifying model of appropriate scholarship. Sarton was also well aware of the real, if less immediate, need for professional as well as cognitive identity. Without it, his field of learning could never be secure, let alone accepted as crucial to man's intellectual quest. Appropriate exhortations poured from his pen. The need for career positions and institutes for the history of science were matters to which he often returned. Once again, Sarton provided the self-exemplifying models.⁷ He "invented" for himself both a research institute and a full-time career when the discipline barely possessed a cognitive, let alone a professional, identity.

Yet Sarton's success was ambiguous at best. If through a lifetime of labor he provided tools, established standards, and gave a sense of cognitive form, the orientations and assumptions he offered affected the course of his discipline in a negative rather than a positive sense. The reasons for this are themselves instructive and point to other facets of his significance. George Sarton was above all a man of the nineteenth century. His cultural orientation was toward universal history. It was also toward those progressivist philosophies which found their basis in positive science and their end in the imminent and universal brotherhood of man. Like H. G. Wells, the Webbs, and George Bernard Shaw, Sarton was to find his *Weltanschauung* rendered largely irrelevant by events. However effective he was in providing propaganda, establishing standards, and fashioning tools, these could not of themselves serve in lieu of the theoretical orientations, metaphysical assumptions, and sense of problematics which necessarily underlie a discipline. And Sarton's particular tragedy was that he could not meaningfully communicate his own over-arching outlook, his worldview, to those who came of age during or after World War I. Without this, his ambitious, encompassing vision of the proper problematics and methodology made little sense. A study of the ways in which he succeeded and of those in which he failed thus illuminates both the subtle processes of discipline building and the paradoxical career of a remarkable man.

⁷ As this discussion testifies, the historical sociology of science presumably incorporates a variety of ideas and uniformities exemplified in its own behavior as an intellectual discipline. On self-exemplifying ideas, see Robert K. Merton,

On the Shoulders of Giants (New York:Free Press, 1965), *passim* as indexed and Robert K. Merton, "Insiders and Outsiders: A Chapter in the Sociology of Knowledge," *American Journal of Sociology*, 1972, 78:9-47, esp. 10-11.

II

George Alfred Léon Sarton was born in Ghent, Belgium, on August 31, 1884. He was the only child of Alfred Sarton, one of the chief engineers and directors of the Belgian State Railways. His mother died when he was a few months old, and George thus knew the prosperous but lonely upbringing of an isolated child, surrounded by servants. Eleanor Mabel Elwes of London, whom he married in Ghent in May 1911, had experienced a similar lonely childhood, after being boarded-out as her parents (her father was a civil and mining engineer) traveled abroad. Among other similarities in the backgrounds of the pair we may note that Mabel's father was a Fabian and agnostic, while George's was liberal, anti-clerical, and engaged in Masonic activity.

Secure in a setting "as bourgeois as a Balzac novel," George Sarton followed a course of schooling that normally led to the study of philosophy at the University of Ghent. He soon abandoned philosophy in disgust in favor of the natural sciences. He first studied chemistry (for which he won a gold medal), and crystallography, then mathematics. His 1911 D.Sc. dissertation on "Les principes de la mécanique de Newton" provides an early indication of the direction his interests were taking under the philosophical influence of Auguste Comte, Pierre Duhem, and Paul Tannery. A visit to London at this time led to the systematic exploration of Wells, Shaw, and the Fabians, whom Sarton experienced as a refreshing contrast to the "doctrinaire Marxism" he and his bourgeois friends youthfully espoused. Socialism rather than Communism thus came to seem the necessary and inevitable prelude to the final achievement of benevolent anarchism.⁸

Graduated and married in 1911, George Sarton found it necessary to choose a career. The small private income he enjoyed was not enough to sustain a family, while all the assets of his wife's father had been lost in speculative mining stocks. A 1910 note in his journal speaks of his intention to "become the pupil, if I prove worthy, of Henri Poincaré: the most intelligent man of our time," before trying to get a post at the university. He goes on, in a passage both prophetic and revealing, that "It is almost certain that I shall devote a great part of my life to the study of 'natural philosophy.' There is great work to be accomplished in that direction. And—from that point of view—*living* history, the passionate history of the physical and mathematical sciences is still to be written. Isn't that really what history is, the evolution of human *greatness*, as well as its weakness?"⁹ This liberal and characteristically Edwardian faith was to guide most of the remaining forty-six years of his life. The transition from a dawning conviction of the importance of a passionate history of the physical sciences to the systematic work of equipping a new discipline with tools and standards, and more especially the transition to secure employment in an as-yet-nonexistent profession, was to prove slow and complex.

Using the proceeds from the sale of his deceased father's wine cellar (the sale itself being a typically outrageous act of the confident and iconoclastic young man) Sarton soon bought a pleasant country house in Wondelgem (near Ghent). Here his only surviving child, May Sarton, was born in May 1912. At about this time George wrote a local politician, seeking his aid in obtaining a sinecure in the state bureaucracy,

⁸ Letters to Sarton from H. G. Wells and Sidney Webb are in SHL.

⁹ The quotations are from pp. 61, 63, and 64 of

May Sarton, *I Knew a Phoenix*, which is the best source for Sarton's youth.

“because I have decided to devote my life to the disinterested study of mathematical methodology and the history of science”—subjects that offered no prospect of paid support. A field of learning that had yet to find and to institutionalize its professional identity could be significantly cultivated only by the independently rich or by dependent scholars kept alive by sinecures, patronage, or distracting jobs. To buttress his claim, Sarton pointed out that he had already published nine articles and notes on subjects from Ladenburg to Lomonosoff and from the beginnings of the Royal Society to the bibliography of the history of science.¹⁰ His appeal went unrewarded, a response frequently imitated by other potential patrons whom Sarton importuned in subsequent years.

Soon after his marriage Sarton made the bold decision to found *Isis*, his “Revue consacrée à l’histoire de la science.” Displaying as so often that single-minded and disinterested opportunism which marks the actions of a man wholly convinced of his mission, he rounded up a distinguished editorial board. By September 1912 he had secured the patronage of Svante Arrhenius, Émile Durkheim, Sir Thomas Heath, Jacques Loeb, Wilhelm Ostwald, Henri Poincaré, Sir William Ramsay, and David Eugene Smith, among others—no mean achievement for a newly graduated twenty-eight-year-old. Sarton’s methodical placing of these names in categories shows he was already convinced that the history of science subsumed under its wider heading the histories of mathematics, technology, chemistry, medicine, biology, physics, and astronomy and required besides the expert advice of scientists, historians, sociologists, and historians of philosophy.¹¹ The methodical division of his field also displays Sarton’s passion for tidiness and classificatory order. This passion was to inform all his efforts and may in part explain his attraction to the work of Auguste Comte, just as it lay behind his particular admiration for Linnaeus among men of science.

The decision to found a journal was crucial. In retrospect we can see how *Isis* provided Sarton with the first of the *institutional* tools he needed, if a long-continued but still incoherent area of inquiry was to be transformed under his leadership into an articulated discipline, with agreed critical standards and a definite cognitive identity. However, Sarton himself conceived of *Isis* as having far wider aims. His over-arching vision and evangelical belief were announced to the world in a series of explanatory passages in the early numbers of the journal. Here he emphasized the (often contradictory) faith to which he was constantly to return. As he pointed out, it was *not* the chosen domain of activity that made *Isis* unique, but the fact that no other journal would systematically and holistically connect methodological, sociological, and philosophical perspectives with the purely historical and thus allow historical inquiry to attain “its full significance.” Even beyond this, such a history was itself nothing but a means, an indispensable instrument, necessary for achieving “the philosophy of science.” That, or more properly the acquiring of a better understanding of the nature of man, was the ultimate goal.

The preoccupation with history was designed to give not only the intellectual exhilaration of better understanding life, but also the corresponding power to “mieux prévoir.” The aim throughout was a work of synthesis, such as Comte and Spencer

¹⁰ George Sarton to “L’Administrateur-Inspecteur,” July 17, 1912; quoted in Van Oye, *George Sarton*, pp. 20–21.

¹¹ Delorme, “La Naissance d’*Isis*,” Vol. II, p. 227.

had attempted. Bibliography, documents, and historical facts were to be subordinated even at the price of protests from "quelques érudits, incapables de s'élever au-dessus de leur érudition." Any synthesis achieved would of course be partial and provisional, for science, like life, was only a perpetual new beginning. Evolution and successive approximation were the keys, the immediate goal a study of the whole past of science. This past would nevertheless find all its *real* significance only in the light of the fleeting present. Seen in this way, the history of science was the history of human thought and civilization in their highest forms and thus the base for true philosophy.¹²

In these statements we may perceive some reasons behind a further paradox in Sarton's career. From one perspective his major achievement is that of the discipline builder: providing a key journal, establishing an identity for a field, encouraging the formation of a discipline-based learned society with its potential for sanction and reward, locating and mobilizing scarce resources of men and money in pursuit of crucial scholarly objectives, seeking to furnish reference works, general surveys, advanced monographs, and teaching manuals. To create the necessary intellectual and organizational infrastructure for a coherent discipline was a task that demanded a lifetime of faith and devotion. Yet to Sarton himself such work was only preliminary and minor compared with achieving the "new humanism," the holistic and all-embracing synthesis, that would be based on a just appreciation of science in history. Ironically, it was the yearning for this synthesis which made his contributions to his own new discipline less than complete. Partly because his vision was so catholic he could not communicate to others that sense of either the problematics, or the conceptual and analytic *schema*, necessary if his chosen field were to become a coherent, fully articulated discipline with a clear cognitive identity. The paradox is acute. Ambitious for the total vision, it is yet for bibliography, documentation, and the establishing of historical standards and facts that Sarton is most often remembered.

While he would on occasion write in an avowedly pragmatic and relativist vein, it was the heritage from positivism, progressivism, and Utopian socialism that more often controlled Sarton's argument and guided his actions. Thus the man who penned the passages quoted earlier could also seriously and repeatedly present a "theorem on the history of science" which ran as follows:

Definition. Science is systematized positive knowledge, or what has been taken as such at different ages and in different places. *Theorem.* The acquisition and systematization of positive knowledge are the only human activities which are truly cumulative and progressive. *Corollary.* The history of science is the only history which can illustrate the progress of mankind.¹³

Or again, the scholar who until his last years largely devoted himself to critical bibliography could also say that "The quest for truth and beauty is indeed man's

¹² See "L'Histoire de la Science," *Isis*, 1913-1914, 1:3-46; and especially "Le but d'*Isis*," *Isis*, 1913-1914, 1:193-196. Further information on the fortunes of *Isis* may be gleaned from the archives of the History of Science Society, now on deposit at the Library of the American Philosophical Society, in Philadelphia. For instance, a Jan. 30, 1939, report of Sarton's shows his own personal subsidy of *Isis* and *Osiris* to have reached a record figure of \$1,759 the

previous year.

¹³ George Sarton, *The Study of the History of Science* (Cambridge, Mass.: Harvard Univ. Press, 1936), p. 5. Though rarely so forcefully stated, this theme runs through all Sarton's writing, as when in the *Introduction* (Vol. I, p. 4) he says that "the acquisition and systematization of positive knowledge is the only human activity which is truly cumulative and progressive."

glory. This is certainly the highest moral certainty which history allows. . . . History itself is of no concern to us. . . . To build up [the] future, to make it beautiful [is rather the aim]."¹⁴ Whatever the contradictions and gaps between words and deeds, the fundamental belief was that expressed in some words from the first volume of the *Introduction*: "The history of science is the history of mankind's unity, of its sublime purpose, of its gradual redemption."¹⁵

The years of Sarton's maturity, say from 1914 to 1949, were years cruel to any such faith. Yet Sarton did not change his modes of thought nor renounce his vision, even though that vision dimmed and faded under the remorseless pressure of experience and events. Indeed Sarton was for the most part curiously isolated from both the practical and the intellectual life of his time. True, World War I made him a refugee and destroyed his early secure world. Yet he never experienced the fury of war at first hand, unlike many of his generation in Europe. The privations born of civil dislocation threatened, interrupted, and transformed his personal life. Yet they could not grip or hold him, thanks to his determination, his energy, and his burning sense of mission. And all through the later years of the Depression and World War II he was to have a reasonably steady income, secure access to a major library, the environs of an academic town remote from the world's trouble centers, and a liberty to do scholarly work that made many regular members of the Harvard faculty appear somewhat like dull serfs enslaved to teaching and committee work.

If current political events did not often impinge on Sarton's private world, no more did the intellectual currents of his day. His thought rather owes most to nineteenth-century models: Auguste Comte, Herbert Spencer, and the Utopian aspects of Marxian and socialist thought.¹⁶ The emphatic claims for a materialist history of science that enlivened the academic world of the 1930s seem to have meant little to him. (He did later decry the "belief that the history of science ought to be explained primarily, if not exclusively, in social and economic terms." But, characteristically, he then chose not to engage the intellectual issues but to adopt an uneasy imagery in which social and economic influences are resisted by the authentic scientists and affect only the "job-holders."¹⁷) In similar fashion the intellectually radical messages of the sociologists and of Freud seem to have made little impact on Sarton's beliefs, even though Émile Durkheim and E. Waxweiler (the director of the Solvay Institute of Sociology in Brussels) were among the original patrons of *Isis*. This is not to say that Sarton failed to voice an occasional remark about the need for psycho-sociological studies. On one occasion he even claimed that "the history of science in the main amounts to psycho-sociological investigation," while on another he would casually refer to "my sociology of science."¹⁸ But his scholarly writings show little sense of what sociology and psychology were really about, little awareness that their methods and assumptions might offer fundamental challenges to his own progressivist faith. Instead, their insights were somehow to be effortlessly integrated into that total synthesis which

¹⁴ George Sarton, *The Life of Science* (New York: Henry Schuman, 1948), pp. 57-58; taken from his article "The History of Science," first published in *The Monist*, 1916, 26:321-365.

¹⁵ Sarton, *Introduction*, Vol. I, p. 32.

¹⁶ See, e.g., *The Life of Science* and "Herbert Spencer," *Isis*, 1920, 3:375-390. The changing nature of the European intellectual context may

be approached via H. Stuart Hughes, *Consciousness and Society* (New York: Knopf, 1958).

¹⁷ George Sarton, *A History of Science* (Cambridge, Mass.: Harvard Univ. Press, 1952), pp. xii-xiii.

¹⁸ Sarton, *Life of Science*, p. 51; *Horus: A Guide to the History of Science* (Waltham, Mass.: Chronica Botanica Co., 1952), p. 94, esp. n. 87.

remained the goal. Here as elsewhere, Sarton played the role of propagandist for certain attitudes and approaches, not that of conceptually equipped analyst and substantive contributor.

Sarton also had an ambivalent response to the intellectual ferment excited by questions about the relativity of knowledge. Influenced by relativist currents, he could write in 1916 that "the truths of today will perhaps be considered tomorrow, if not as complete mistakes, at least as very incomplete truths; and who knows whether the errors of yesterday will not be the approximate truths of tomorrow?" But, in apparent contradiction, he would also insist that the history of truth was of primary interest to the investigator. Similarly, he could write with a straight face some four decades later that "The history of science should not be used as an instrument to defend any kind of social or philosophic theory; it should be used *only for its own purpose*, to illustrate impartially the working of reason against unreason." It is thus not surprising that the vigorous debates on historical meaning, historical method, and the relativity of knowledge, which exercised such major historians as Charles Beard and Carl Becker, apparently meant little to Sarton, the inheritor of encyclopedic European ambitions and the convinced proponent of a form of history itself the key to a new humanism.¹⁹

The paradox is that while Sarton, the enthusiast for past intellectual life, was out of touch with intellectual debate in his own time, he was also an undoubted pioneer in forms of history that have only come into vogue long after he pursued them. This is true not only for the history of science itself, but more interestingly for deliberate synthetic and comparative history. Deluged by facts and aware that Western supremacy is slowly passing, it is the fashion today for scholars to call for comparative investigations that encompass different civilizations and to advocate studies that go beyond the analysis of facets of our experience to integrate and synthesize that knowledge.

Such comparison and integration were fundamental to Sarton's mission. From the beginning he insisted that "the history of science is an encyclopaedic discipline."²⁰ By this he meant it was by nature a discipline devoted to summation, comparison, and synthesis. Indeed his own interest in the history of science was "dominated . . . by a philosophical conception." As he admitted to a correspondent in 1927, "I am anxious to prove inductively the unity of knowledge and the unity of mankind."²¹ The immensity of the task did not daunt him. Rather it provided the rationale for a lifetime pursuit of difficult linguistic skills and wide-ranging historical and scientific knowledge. The immense commitment also served to discourage potential apprentices. The master did not offer the easily learnt and transferable technique on which disciplines and more especially "schools" have usually been built. Instead he set forth an attitude and a vision beyond all but the bravest hearts.

The hoped-for proof of the unity of knowledge and mankind never came. Good inductivist that he was, Sarton's insistence on first collecting and analyzing the facts so filled his life that it kept him from undertaking the synthesis for which he labored. This is scarcely surprising, since unlike the "advanced" historians of his day, he was not content unless his facts related not merely to Mediterranean civilization, but to

¹⁹ Sarton, *Life of Science*, p. 43, and *History of Science*, p. xiv. Our italics. On American thought at this time see Richard Hofstadter, *The Progressive Historians* (New York: Knopf, 1968).

²⁰ George Sarton, "The Teaching of the History of Science," *Isis*, 1921-1922, 4:245.

²¹ George Sarton to John Charles Merriam, Dec. 7, 1927, SHL.

contemporary developments in China, Japan, India, and wherever else he saw the gleam of "systematized positive knowledge." In this insistence upon world history he was truly a pioneer, as much in advance of contemporary thought as in other ways he was behind.

III

In the early days of *Isis* all these matters, of course, lay in an unknown and surely unforeseen future. But the events that would crack, erode, and in the end destroy the progressive, bourgeois Edwardian confidence of which George Sarton was such a supreme exemplar were already under way. The German invasion of Belgium in August 1914 immediately and dramatically rendered Sarton's private world precarious. It was at this moment that he chose to make a first approach to Robert S. Woodward, second president and successful organizer of the Carnegie Institution of Washington. As a fresh center of scientific research, created and endowed by a multi-millionaire, the Institution naturally attracted endless letters from impecunious propagandists of visionary schemes. Whoever opened its mail must have felt a weary familiarity as he read Sarton's letter announcing his intention "to carry further, under a more systematic and collective form, the lofty intellectual work undertaken of old by several philosophers and especially by Comte and Spencer." To this end Sarton sought financial assistance for *Isis*, which had only 125 subscribers (distributed through 25 countries). In keeping with his penchant for ambitious programs, Sarton went on to announce that he already planned two special series of *Isis*, one "devoted to the systematical study of different epochs, or of the intellectual work accomplished by different races," the other to "the historical and critical study of scientific problems." With almost prophetic foresight he then concluded that "if the Institution thought it necessary I would even be disposed to come and settle with my family in Washington, to there continue my work." The letter went unanswered.²²

The advancing German forces propelled Sarton to more vigorous action. Abandoning Wondelgem, his library, and his notes (which he buried in his garden), he fled to London with his family. Further unsolicited letters to the Carnegie Institution repeated the appeal for *Isis*, announced that he hoped to obtain an English position on the recommendation of Sir William Ramsay, and inquired about lectureships in American universities. Persistence was finally rewarded. A form letter of November 9 from Woodward regretted his inability to help. Undeterred, Sarton promptly enlisted the good offices of the historian of mathematics David Eugene Smith, who knew Woodward personally. Smith's appeal was equally unsuccessful.²³ Driven no doubt as much by desperation as by faith, Sarton nonetheless sailed for America to explore the possibilities, leaving his family in London.

By April 1915 he was in Washington. Still importuning Woodward by letter, he reported that he was "appointed as lecturer in the history of science at George

²² George Sarton to Robert S. Woodward, Sept. 26, 1914, SCI. Sarton sent a second copy of the letter on Oct. 3, 1914 (SCI, draft in SHL). On Woodward, see George Sarton, "Robert Simpson Woodward 1849-1924," *Isis*, 1925, 7: 112-114.

²³ Sarton to Woodward, Oct. 25, 1914; Woodward to Sarton, Nov. 9, 1914; David E. Smith to Woodward, Jan. 28, 1915, SCI. Woodward and Smith had been colleagues at Columbia University.

Washington University." As was to be the case more than once in the next few years, Sarton, long on the margin of academic institutions, was driven to define a makeshift and temporary arrangement as much more secure and impressive than it was. Such behavior is a characteristic response of scholars and scientists dislodged by historical circumstances from their affiliations to become refugees in search of an institutionally defined status and role.²⁴ Once again blending opportunism, idealism, and ambitious plans, Sarton was soon writing Woodward to outline plans for a multi-million-dollar American institute to be established in Belgium, after the war was over. He also took the occasion to report that "it is very likely that I shall leave this country next spring . . . to go to Japan and China." His aim was to gather the "documents and objects" necessary to overcome the great lack of knowledge of Eastern science. Would the Carnegie Institution support such an enterprise? Parenthetically Sarton remarked that he hoped to write a manual on the history of science, but not "before twenty or thirty years."²⁵

This letter finally aroused Woodward's interest. While turning down Sarton's project once again, he went so far as to reveal what Sarton probably already knew (via Smith), namely that the history of science "is a subject in which I have been profoundly interested for many years." Indeed Woodward had actually tried to persuade J. T. Merz to write a history of science "which might be for the present what Whewell's *History of the Inductive Sciences* was for its day."²⁶ If anything was needed to confirm Sarton's hope of Woodward's promise as possible patron for a determined and visionary scholar, this letter provided it. The correspondence continued, now with Sarton as secretary of the "Belgian Scholarship Committee." In this capacity he could draw on an impressive list of sponsors, including the presidents of Johns Hopkins, M.I.T., Princeton, and Stanford, the provost of Pennsylvania, and the young Franklin Delano Roosevelt.²⁷ The ground was thus well prepared for a final "last desperate appeal" letter from Sarton to Woodward in March 1918.

If the Carnegie Institution seemed a promising but unforthcoming source of support, the universities of America provided more immediate though limited sustenance. The history of science was actually well established as an activity, although far from being an intellectual discipline and almost unthought of as an actual profession, when Sarton landed in the United States. A 1915 review article in *Science* makes this plain. It details no fewer than 162 courses in the history of particular sciences as well as 14 general courses in the history of science, spread among 113 institutions.²⁸ Following a pattern widely familiar in the emergence of the separate natural sciences, a distinct area of inquiry thus already existed, but without agreed standards and techniques and largely peopled by "amateurs" whose primary allegiances lay in other fields. The slow emergence of such an area of inquiry and common discourse appears to be a necessary precondition to the formation of a discipline. Sarton's particular mission and genius

²⁴ Sarton to Woodward, Apr. 21, 1915, SCI. On the general point, see Donald Fleming and Bernard Bailyn, eds., *The Intellectual Migration* (Cambridge, Mass.: Harvard Univ. Press, 1969).

²⁵ Sarton to Woodward, Oct. 6, 1915, SCI.

²⁶ Woodward to Sarton, Oct. 9, 1915; original in SHL, carbon copy in SCI. J. T. Merz was the author of *A History of European Thought in the*

Nineteenth Century, 4 vols. (Edinburgh, 1903-1914). William Whewell's *History of the Inductive Sciences*, 3 vols. (3rd ed., London, 1857) was a well-known survey.

²⁷ See Sarton to Woodward, Nov. 2, 1915, and May 6, 1916, SCI.

²⁸ Frederick E. Brasch, "The Teaching of the History of Science," *Science*, 1915, 42:746-760.

were to catalyze the transformation. His contributions to the institutionalization of his chosen field, on both cognitive and professional planes, were to be crucial if less than complete.

Through the help of friends and acquaintances, Sarton soon managed to arrange a frenetic but sustaining round of guest lectures, seminars, and temporary appointments in American universities. One of the men responsible and one most involved in the promotion of the history of science as a new area of pedagogy was L. J. Henderson, a polymathic biological chemist and junior but influential member of the Harvard faculty. Since 1911 Henderson had himself been teaching a regular Harvard course on the history of science.²⁹ He was thus no doubt early aware of Sarton's program, of which he was to become such a willing supporter. As a member of the inner circle, he was in a position to advance the institutionalization of the field at Harvard and in the process to have a profound effect on Sarton's life. By May 3, 1916, Henderson was able to write his new friend with the glad news that "from several different sources we have been able to put together \$2,000 for your first year [at Harvard]. The second year is not fully arranged, but I have not much doubt that we shall be successful." On the strength of this encouraging letter, Sarton understandably wrote Woodward the optimistic interpretation that "I have been appointed lecturer on the history of science at Harvard University for two years. A new chair has been endowed for the purpose."³⁰ As it turned out, Sarton was first featured as "lecturer in philosophy," with the bulk of his teaching being listed under the auspices of the Department of Philosophy. And, partly because of the financial problems Harvard faced as a result of America's entry into World War I (but perhaps also partly because his courses did not draw many students), his two years of appointment were not extended.³¹

²⁹ L. J. Henderson (1878–1942) was only six years Sarton's senior. Successively involved in biochemistry, physical chemistry, physiology, and sociology, he was "one of a small group of younger men on whose judgment President Lowell relied, and whose imaginative wire-pulling more than once had a significant and highly beneficial influence on Harvard history," according to J. B. Conant in *Isis*, 1957, 48:302. Like Conant after him, Henderson had been a student in Chemistry 8, the course largely on "the historical development of chemical theory," which Theodore W. Richards taught. It was Richards' work which set the stage for Henderson and Sarton. Thus at Harvard, as elsewhere, the history of chemistry was godmother to the history of science. Henderson's course (History of Science 1) was regularly offered from 1911/1912 to 1917/1918, drawing enrollments of from 15 to 45 students. On Henderson's own tendency to holistic thinking, see John Parascandola, "Organismic and Holistic Concepts in the Thought of L. J. Henderson," *Journal of the History of Biology*, 1971, 4:63–118. For a selection of his writings, see L. J. Henderson, *On the Social System*, Bernard Barber, ed. (Chicago:Univ. of Chicago Press, 1970).

³⁰ L. J. Henderson to Sarton, May 3, 1916, SHL. Sarton to Woodward, May 6, 1916, SCI. As the article by Brasch ("Teaching of the History of Science") makes plain, Henderson was at this time actively promoting the history of science far beyond Harvard. Some years later he was also to be influential in the formation of the History of Science Society, of which he was first president.

³¹ In 1916/1917, Sarton taught Philosophy 5, "History of Physics in the 18th and 19th Centuries," with an enrollment of 9. In addition he taught a new second semester supplement to (Henderson's) History of Science 1, devoted to "Science and Civilization in the 15th and 16th Centuries," with an enrollment of 26. In 1917/1918 Henderson was not teaching, and Sarton drew only 12 students to his section of History of Science 1. The outbreak of war brought a sudden drastic drop in student enrollments, and thus in tuition fees, at Harvard as elsewhere. In the spring of 1918 the war and the financial problems were expected to last a considerable time—hence, in all probability, the ending of Sarton's appointment.

In desperation Sarton wrote once more to Woodward, on March 30, 1918. This time the Harvard appointment was described as "an artificial one anyhow—the necessary funds having been subscribed by Harvard men." In histrionic tone, but quite authentically, Sarton then went on to detail his labors. He had worked from 9 a.m. to 10 p.m. every day for those two years. He had delivered more than 250 lectures on subjects ranging from Babylonia to Henri Poincaré. It was all apparently of no avail. "Is there not room for one single man of my kind in this country?" he asked, adding that "I thought it was a matter of duty to knock at your door, before going under—temporarily." Woodward's reply was once again regretful but firm. Yet it did open the door a crack by indicating that "if such a relationship as you desire were to be established, it would require approval of our executive committee which does not meet again until April 18." Sarton promptly seized his opportunity. His reply asked that his request be laid before the executive committee. It also reported that he had already made his own direct approach to Andrew Dickson White, historian of the warfare of science with theology, Carnegie trustee, and member of the executive committee.³²

Following the necessary inquiries and the usual administrative delay, George Sarton was appointed research associate in the history of science for two years, from July 1, 1918. Thus began an association with the Carnegie Institution that lasted throughout Sarton's professional life (in fact, his Carnegie appointment appears to have been made permanent within the first year of his tenure).³³ Though employed on a full-time basis from Washington, Sarton remained in Cambridge, pleading the uncertainty of war and the great value of a study in Widener Library. And when the end of his Harvard appointment raised the prospect of eviction from Widener, Woodward's solicited intervention proved crucial in allowing him to remain unmolested.³⁴

The unexpected end of the war in November 1918 allowed Sarton to plan an expedition to Wondelgem to recover his library and notes, in the summer of 1919. Following this expedition he was supposed to settle in Washington. Instead, he quickly returned to Cambridge. There, through the good offices of Henderson, he once more secured a room in Widener Library, on the basis of an annual appointment as lecturer in the history of science. His later complaints about Harvard's exploitation of him may have been amply merited, but it should be noted that Henderson clearly pointed out—and Sarton was, under the difficult circumstances, then willing to agree—that "there is to be no payment for the lectures," the arrangement rather being "one of mutual benefit."³⁵

³² Sarton's "last desperate appeal letter," in SCI, is undated. The draft in SHL is dated Mar. 30, 1918. See also Woodward to Sarton, Apr. 3 and Apr. 8, 1918 (SCI) and Sarton to Woodward, Apr. 5, 1918 (SCI) and Apr. 10, 1918 (SHL). The connection between Sarton and Andrew Dickson White began with Sarton's 1912 invitation to White to be a patron of *Isis* and was strengthened by Sarton's 1916 visit to Cornell. White thus became a crucial ally, who in 1918 was willing to "present the case as forceably as I can to Dr. Woodward." See White to Sarton, May 12, 1912,

May 13, and June 8, 1916, Jan. 16, and Apr. 3, 1918, SHL.

³³ See Woodward to Sarton, June 5, 1918 (SHL); Woodward to Internal Revenue Service, May 23, 1919 (SCI).

³⁴ Sarton to Woodward, Aug. 25, and Oct. 20, 1918; Woodward to William C. Lane, Aug. 27, 1918, SCI.

³⁵ Henderson to Sarton, Feb. 26, 1920 (and also Feb. 9, 1920), SHL. See also Sarton to Woodward, Feb. 4, 1919, Jan. 7, and Mar. 20, 1920, etc., SCI.

IV

Reasonably secure in one of the world's great libraries, guaranteed an annual salary, and with no specific duties other than those he fashioned for himself, Sarton was at last free to develop his own mission and his own life style. It was against this background that the idea of the *Introduction to the History of Science* gradually developed and matured. Aside from *Isis*, Sarton's immediate plans were often vague and shifting during the precarious years between 1912 and 1920. His 1915 intention to sail for China and Japan has already been noted. By 1918, when he had been at Harvard twenty months, his interests had somewhat changed. In reply to Woodward's request for specific information on the work he was undertaking, Sarton mentioned a study of Leonardo da Vinci's scientific manuscripts, which would take "about six months" to finish, and a book on *The Teaching of the History of Science*, which would take a further six months. A little later he was confiding to Woodward that throughout his life he planned "to carry on simultaneously research on ancient science and on XIXth century science." This same letter also makes specific mention of "my book on nineteenth century physics."³⁶

The correspondence between Sarton and his new benefactor quickly ripened and deepened. By January 1919 he felt able to set out in confidence one of those ambitious over-arching programs which continually recur in his thinking. The plan was "to lay the foundation of an empirical philosophy of science, to evidence the unity of science." The means included *Isis*, an annual series of Studies in the History and Method of Science to be jointly edited with Charles Singer, a "General History of Science and Civilization, to be written on an extensive scale by a large group of scholars," a history of physics in the nineteenth century, a "complete account of the [ancient] beginnings of science," facsimile copies of scientific books and manuscripts, a Chinese encyclopedia, an exhibition of the progress of science, and a catalogue of scientific instruments down to 1900. In all this, the cooperation of Charles Singer was described as essential, and if he could be brought to Washington (where Sarton then still intended to go), the city would become an international center for the history of science.³⁷ It says much for Woodward that he took Sarton's ideas in good part, without exactly promising to underwrite them all.

The work on Leonardo gradually made Sarton aware of his own historical naïveté and lack of training. In common with all founders of disciplines, he was perforce an autodidact. His very anxiety to escape from dilettantism and to establish critical standards also slowed his work. As early as July 1918 he wrote in tones of mingled consternation and delight that "Leonardo was interested in almost everything [My book] will be in fact an encyclopedia of the positive knowledge attained at the end of the fifteenth century."³⁸ Soon he was seeking specialist help, underwritten by the Carnegie Institution. Professor McMurrich of Toronto was enlisted for the anatomical part, and Professor Siren of Stockholm was approached concerning a cooperative

³⁶ Woodward to Sarton, Apr. 8, 1918, SCI. Sarton to Woodward, Apr. 10, 1918 (SHL), and May 30, 1918 (SCI).

³⁷ Sarton to Woodward, Jan. 23, 1919, SHL. Charles Singer had at this time inaugurated a hopefully annual but actually short-lived series of

Studies in the History and Method of Science, 2 vols. (Oxford: Clarendon Press, 1917-1921). The second volume explicitly referred to the "joint publication . . . projected between this series and *Isis*."

³⁸ Sarton to Woodward, July 17, 1918, SCI.

catalogue of Leonardo's paintings and drawings. Reference to the study of Leonardo—his main concern—continues in Sarton's correspondence and annual reports for several more years, though the promised book was never to materialize.³⁹

Another reason for slow progress was Sarton's incurable tendency to project and begin several studies at once. His first annual report to the Carnegie Institution refers to the Leonardo studies, the accumulation of materials for a history of physics in the nineteenth century, "activity in behalf of the new humanism," and plans for a retrospective survey on the occasion of the twenty-fifth anniversary of the Carnegie Institution. Characteristically, this last would "consider the Institution not as an isolated unit, but as part of the scientific organization of the world."⁴⁰ As if these activities were not enough, early the next year he was planning a general history of science. The work was to be edited jointly with Charles Singer of Oxford, written by the scholars best qualified, and published in ten or twelve volumes over the next decade by the Oxford University Press.⁴¹ As late as 1934 Sarton was still projecting what by now had become *The Harvard History of Science* in eight illustrated volumes "comparable to the Cambridge Medieval History and other Cambridge and Oxford standard collections."⁴² All this was in addition to his published plans for a research institute in the history of science,⁴³ an incessant round of travel and public lecturing,⁴⁴ the detailed personal editing and directing of *Isis*, and, throughout, work on the *Introduction* itself. This vast labyrinth of labor left no time for a private social life. And only by heroic effort could Sarton keep track of his many commitments and produce that controlled order he loved and which his critical bibliographies exemplify.

Sarton's continual tendency to project programs in the history of science had ramified intellectual roots. For one thing, never having been trained for historical work, he long underestimated its difficulty and slow-moving character, until hard-won experience taught him otherwise. Then again he was burdened with a sense of how many different things wanted doing, and all quickly, if the history of science was to become an academically reputable subject. Journals, teaching manuals, standard histories, source books, and above all *critical* bibliographies of what already existed and was being produced: each was desperately needed and would, if necessary, be produced by

³⁹ Sarton to Woodward, Oct. 30, 1918, and Feb. 4, 1919, SCI. Sarton had chosen Leonardo as his subject when invited to give the Lowell Lectures in Boston (most probably at Henderson's instigation) in 1916. Leonardo continued to occupy him actively at least through 1919, when he visited archives in England (Windsor and the British Museum) and Florence. Despite this, he published only two popular studies on Leonardo (*Scribner's Magazine*, 1919, 65:531-540, and a 30-page booklet for the Metropolitan Museum of Art, in 1952). J. Playfair McMurrich, his "assistant," proved more determined in this particular respect and in 1929 published *Leonardo da Vinci: The Anatomist* (Baltimore:Williams and Wilkins Co., 265 pp.). In a preface to the work Sarton wrote that it would be "many more years" before he resumed his own Leonardo studies.

⁴⁰ *Carnegie Institution of Washington Yearbook*, 1919, 18:347-349.

⁴¹ Sarton to Woodward, Jan. 17, 1920, SCI.

⁴² Sarton to Conant, Dec. 1934, SHL. *The History of Science* which Sarton began in retirement and of which Harvard University Press published two volumes was a quite separate and much later project.

⁴³ See, e.g., George Sarton, "An Institute for the History of Science and Civilization," *Science*, 1917, 45:284-286; 46:399-402; "An Institute for the History of Science and Civilization," *Isis*, 1938, 28:7-17.

⁴⁴ Besides his extended 1919 visit to Belgium and Italy and his trip to the Middle East (1931/1932), Sarton spent a number of summers in Europe. In his early years in the U.S. he lectured briefly at many different institutions. Later he was to occupy named lectureships at universities from California to Pennsylvania, in Nebraska and Rhode Island.

Sarton himself. But beyond such reasons, Sarton embraced global projects because he passionately believed in the unity of knowledge, in the integrity of experience, and in the need for a holistic philosophy that embraced art and science. "The moral failure which the [First World] War implied" made this philosophy all the more urgent. Only what he came to call "the new humanism" could offer the necessary "mixing of the historical with the scientific spirit, or life with knowledge, or beauty with truth."⁴⁵ Specialization everywhere threatened such broader insight and reinforced Sarton's belief in "the necessity of synthetic or encyclopedic studies, to keep alive the pure spirit of science."⁴⁶

It was out of this complex background that the *Introduction* gradually took shape. The essential impulse came in 1919, when Sarton was able to return to Belgium and retrieve his own private library and notes. With these at last safely ensconced in Widener Library, his second annual report to the Carnegie Institution was able to re-introduce what he described as "an old design." It was "the writing of an introduction to the history and philosophy of science, a sort of compendium of the sources of information to which the student . . . may have to refer."⁴⁷ By January 1921 the project had grown greatly in scope. The work was now to be in two parts, dealing with the history and philosophy of science as a whole and the history and philosophy of the special sciences and their branches. The fundamental aim remained that of establishing the history of science as an independent discipline, with its own tools and methods.⁴⁸ The project continued to grow and develop in Sarton's mind. By 1927, with the appearance of the first volume of what was by then to be a multi-volume *Introduction to the History of Science*, Sarton envisaged the project as containing "A purely chronological survey . . . which will require seven or eight more volumes," "Surveys of different types of civilization, e.g. Jewish, Muslim, Chinese . . . [in] seven or eight volumes" and a "Survey of the evolution of special sciences . . . [in] some eight or nine volumes."⁴⁹ He was forty-three years of age, with ample time, he felt, to finish the first series down through the eighteenth century, write parts of the Semitic and Far Eastern volumes for the second, and the whole of the physical sciences volume for the third series.

Work on the *Introduction* progressed far more slowly than Sarton's chronic optimism allowed. As early as the summer of 1921 he reported that chapters dealing with successive centuries from the ninth B.C. to the sixth A.D. (inclusive) were ready for publication.⁵⁰ Four years later a personal note to John Charles Merriam (Woodward's successor as president of Carnegie), assured him that "My Introduction is reaching completion . . . I am feeling like a prisoner whose time is almost up." Volume I was to be ready in February 1926, Volume II toward the end of that year, and Volume III (reaching the close of the seventeenth century) a year later.⁵¹ Volume I was in fact published roughly when planned. Volume II, which only reached to 1300, did not appear till 1931. By that time, Sarton enjoyed the services of a research assistant, Dr.

⁴⁵ George Sarton, "War and Civilization," *Isis*, 1914-1919, 2:315-321 (quotations from pp. 317 and 319). See also George Sarton, "Le Nouvel Humanisme," *Scientia*, 1918, 23:161-175.

⁴⁶ Sarton to Woodward, Oct. 2, 1919, SCI.

⁴⁷ *Carnegie Institution of Washington Yearbook*, 1920, 19:384.

⁴⁸ Sarton, "Introduction to the History and Philosophy of Science (Preliminary Note)," *Isis*, 1921, 4: 23-31.

⁴⁹ *Introduction*, Vol. I, p. 34.

⁵⁰ *Carnegie Institution of Washington Yearbook*, 1921, 20:426.

⁵¹ Sarton to John Charles Merriam, June 1, 1925, SHL.

Alexander Pogo. It is just one more irony that Dr. Pogo was initially hired, and for twenty years was employed, to collect material for the volume on the sixteenth century—a volume destined never to appear.⁵² When Volume III finally appeared in 1948, it brought the work only to 1400. By that time even the long indomitable Sarton, aged sixty-four, was prepared to relinquish his plans. “Never again,” he wearily put it to a correspondent.⁵³

The public events of the 1930s and 1940s, together with the growing realization of the immensity of the task he had undertaken, served to erode Sarton’s beliefs that lay behind the *Introduction*. As he wrote many times, “the day of Munich was the nadir of my life.”⁵⁴ Another blow from a quite different direction was the failure of Harvard to support his plans for an institute.⁵⁵ The decision of the Carnegie Institution in 1941 “not to continue the work on the history of science after I am gone” was equally hard to accept.⁵⁶ His letters at this time speak of “the best part of my effort [being] doomed to futility because of the lack of cooperation” and of “my optimism . . . [being] at present as low as it can be if one wants to go on living.” In September 1945 Sarton even saw a “growing lack of interest in the humanities and the history of science.” He went on to admit how very tired and sad the events of the last decade had made him and to observe that with Volume III of the *Introduction* complete, after the redaction of his Harvard lectures “I shall be more than ready to vanish from this human (or inhuman) stage.”⁵⁷

George Sarton’s closing years were far from being as unrelievedly gloomy as these passages suggest. From the time of America’s involvement in World War II until his final retirement he enjoyed a rapidly growing popularity with Harvard students. Whereas in the years 1923/1924 to 1930/1931 he had averaged only twenty-six students in his annual lecture course, and from 1935/1936 to 1939/1940 only fifteen, in 1940/1941 he drew an enrollment of forty-five. By March 1943 he was writing J. B. Conant that “[though] for a time I felt . . . that my message would become entirely irrelevant . . . I have now the largest classes in my experience, 58 in Harvard plus 14 in Radcliffe, that is 72 regular students plus sundry unlisted ones.” From this time until his retirement in the summer of 1949 his survey course never drew fewer than sixty-one enrolled students, and averaged 147.⁵⁸ Thus Sarton, at last, found a regular undergraduate teaching role, if only when events made his message one that drew on faith rather than contemporary evidence, on the hope of a new generation rather than on the experience of his own.

Despite his latter-day popularity at Harvard, Sarton’s wish to develop a formal institute for the history of science remained unfulfilled. Conant had already shown his

⁵² *Carnegie Institution of Washington Yearbook*, 1929, 28:359. In fact, Dr. Pogo worked at a great variety of tasks, from correcting *Isis* proofs to pursuing his own research on Mayan astronomy.

⁵³ Sarton to Andries McLeod, July 7, 1947; quoted in Oye, *George Sarton*, p. 112.

⁵⁴ See, e.g., Sarton to McLeod, Jan. 13, 1947, *ibid.*, p. 110; Sarton to R. J. Forbes, July 30, 1945, *ibid.*, p. 124; see also Vol. III, p. 6 of the *Introduction*.

⁵⁵ See, e.g., Sarton to J. B. Conant, Dec. 2, 1936, and Conant to Sarton, Jan. 4, 1937, SHL.

⁵⁶ Sarton to Conant, June 23, 1941. See also Sarton to Henderson, May 19, 1941, SHL.

⁵⁷ Sarton to Conant, Aug. 4, 1941, and July 20, 1940, SHL; Sarton to Forbes, Sept. 18, 1945, quoted in Oye, *George Sarton*, p. 125.

⁵⁸ Enrollment figures for each of Sarton’s courses have been supplied by the registrar of Harvard University. Frequent references in the *Yearbook* of the Carnegie Institution and in Sarton’s correspondence fill out the picture. The quotation is from Sarton to Conant, Mar. 22, 1943, SHL. Sarton’s largest class (enrolled students plus auditors) was 350 in 1947.

sympathy for the man, if not his ambitious plans, in 1940. In his capacity as Harvard president he then arranged Sarton's transfer from lecturer on annual appointment to professor with tenure. Sarton still drew the major fraction of his salary from the Carnegie Institution, even after this new arrangement. Yet he was moved to write Conant that "I hope that the day may come when I would serve only Harvard, which appreciates the humanities, including the humanities of science, and not the Carnegie Institution, which considers them 'irrelevant.'" But the ever frugal Conant preferred to have Carnegie continue to pay.⁵⁹ After all, Sarton was, in the special calculus of Harvard, a marginal although illustrious man. In 1940 he had still to produce his first successful Ph.D. candidate, while his undergraduate courses remained small, and he almost completely avoided all committee service and routine academic administration. The difference in attitude of the research scholar and the budget-conscious university president is highlighted in Conant's politely negative response to one of Sarton's articles on an institute for the history of science:

I can sum up my point of view by saying that I feel Macaulay was necessary in the development of scholarly and reputable political history, although I understand that now he is not considered as being at all scholarly and hardly reputable. I feel the history of science badly needs a Macaulay, indeed several of them.⁶⁰

V

Sarton was manifestly no Macaulay, and his enduring monument lies not in narrative accounts that have shaped the thinking of a generation, nor in lectures and students. It lies rather in the creation of tools, standards, and critical self-awareness in a discipline and more especially in the massive, specialized, and scholarly text of the *Introduction to the History of Science*. From the appearance of the first volume to the publication of the third, some twenty-one years later, the *Introduction* enjoyed a uniform critical acclaim. It was and is quite literally unique, and scholars were quick to recognize its magisterial and idiosyncratic character. Florian Cajori, Charles Singer, Aldo Mieli, F. J. Holmyard, Julius Ruska, and Lynn Thorndike, among others, hurried to sing its praises. Journals from Japan to Copenhagen offered glowing appraisals in a dozen languages, and the *London Evening Standard* even carried a review by Arnold Bennett.

The only work in the history of science contemporaneous with Sarton's and remotely similar to it is of course Lynn Thorndike's *History of Magic and Experimental Science*.⁶¹ But the two works stand in sharp contrast, for Thorndike's interest in intellectual currents, strange lore, and recondite knowledge merely serves to emphasize the remorseless emphasis on bibliography and positive knowledge that underlies Sarton's

⁵⁹ Sarton to Conant, Mar. 4, 1940, SHL. Like the canny administrator he was, Conant actually *decreased* the fraction of Sarton's salary that Harvard paid, though granting him tenure (Conant to Sarton, Jan. 4, 1939, SHL). Conant's own admiration for Sarton dated from 1927 when, as an assistant professor of chemistry, he wrote, "your undertaking [the *Introduction*] brings most needed intellectual stimulation and refreshment" (Conant to Sarton, May 4, 1927, SHL).

⁶⁰ Conant to Sarton, Jan. 4, 1937, SHL. Conant's efforts to find that Macaulay may be seen in his bringing J. G. Crowther to Harvard for inspection in 1937; see J. G. Crowther, *Fifty Years with Science* (London:Barrie and Jenkins, 1970), pp. 168-182.

⁶¹ *History of Magic and Experimental Science*, 8 vols. (New York:Columbia Univ. Press, 1923-1958).

program for “establishing the history of science as an independent and organized discipline.” Again, Joseph Needham has from the mid-1950s been publishing that “Chinese encyclopedia” which Sarton early held to be necessary for the history of science. In the preface to the first volume of his *Science and Civilisation in China* (the very title has a Sartonian ring) Needham pays tribute to the “exhaustive” method of the *Introduction*. The very different organization of Needham’s survey of one culture highlights the uniqueness of Sarton’s stress on half-century time units and contemporaneous surveys across all disciplines, races, and cultures.⁶² Even the guidebooks to ancient and medieval history, such as Pauly-Wissowa, to which Sarton constantly refers, knew no such ambitious, ecumenical, and chronologically precise organizing *schema* as he employed.

If chronological division was one way of handling the problem of “all disciplines and all nations,” critical bibliography was “another essential basis” of the work: hence the deliberate cross-linking of information in the *Introduction* with that contained in *Isis* (and later *Osiris*). As Sarton himself expressed it in retrospective summary,

The materials contained in the *Introduction*, *Isis* and *Osiris* are integrated by means of thousands of cross references. Thus we may say that volume 1 [of the *Introduction*] was built on a foundation of 8 volumes; volume 2, on a foundation of 15; volume 3 on a foundation of 42 . . . the *Introduction* is the most elaborate work of its kind, and by far, in world literature. This statement can be made without falling under the suspicion of boasting, for it is objective, controllable, and obviously correct.⁶³

If this quotation reveals Sarton’s very real sense of his own accomplishment, it also shows another aspect of his thinking which deserves mention, namely his delight in numerical analysis.

Typical of how this interest expressed itself is the analysis of his work presented in his report to the Carnegie Institution in 1923. Observing that he had by then written notes dealing with the 1,248 most prominent men of science and scholarship down to the end of the thirteenth century, Sarton found that they could be divided into 362 in the ancient Greek world and Eastern Christendom, 373 in Western Christendom, 324 in Israel and Islam, and 189 in India, Central Asia, and the Far East. These ratios convincingly displayed the importance of the scientific heritage from outside classical antiquity. Indeed, a graphical analysis by time could give a qualitative picture of “the amount of scientific activity accomplished at different periods by different provinces of mankind.” (See his figure, reproduced here.) This resort to the use of graphs and a

⁶² The nature of Needham’s debt to Sarton, despite their different goals, is suggested in the “Bibliographical Notes” in the first volume of *Science and Civilisation in China* (Cambridge: Cambridge Univ. Press, 1954, p. 42):

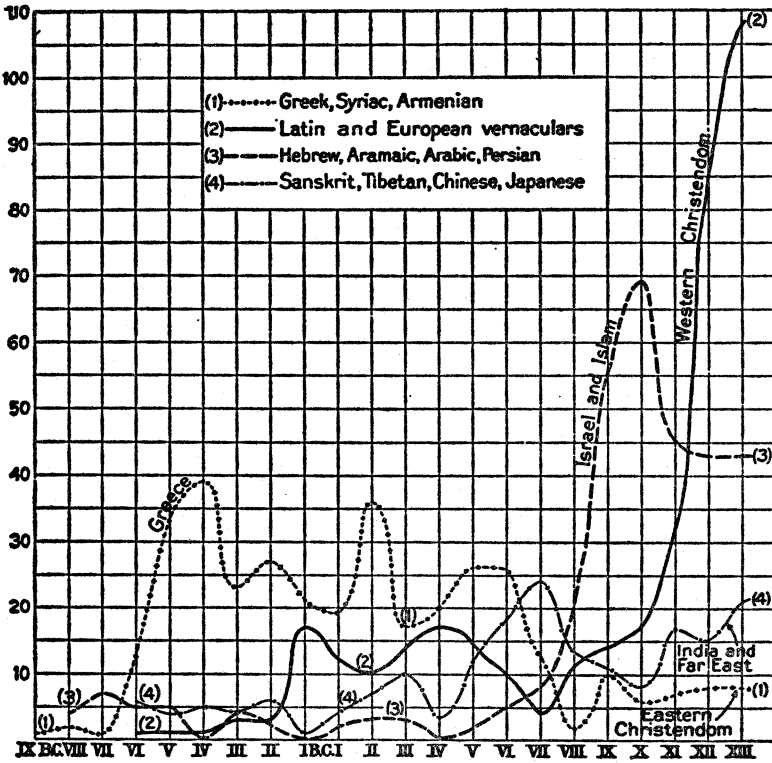
One of the great merits of Sarton’s five-volume *Introduction to the History of Science* is that, of all books on the subject, it for the first time included detailed mention of Chinese scientists and their work. It is also provided with an index of Chinese and Japanese names which gives their characters. But of course the encyclopaedic method there adopted does not set out to give a connected story for

the development of science in a specific part of the world, still less does it attempt to present (I dare not say that anyone could answer) the problem of why modern science and technology developed in Western Europe and not in Eastern Asia.

As early as 1927 Needham was writing to Sarton to congratulate him on the *Introduction*, to offer him two pages of corrections, and to seek his counsel on the history of embryology (on which Needham was then at work). See Needham to Sarton, Nov. 18, 1927, SHL.

⁶³ *Carnegie Institution of Washington Yearbook*, 1949, 48:243.

global form of argument was not then typical of Sarton; it is revealing that the passage in question is secreted in his annual report, not published in an article.⁶⁴ However, he routinely used numbers for various historical purposes.



Relative progress of thought among the people speaking: (1) Greek, Syriac, Armenian; (2) Latin and European vernaculars; (3) Hebrew, Aramaic, Arabic, Persian; (4) Sanskrit, Tibetan, Chinese, Japanese. (From Carnegie Institution of Washington Yearbook, 1923, 22:337.)

In his use of collective biographical statistics to reveal trends and information not obtainable by study of individuals, Sarton was—*en passant*—pioneering what has come to be called prosopography.⁶⁵ A more frequent variety of Sartonian arithmetic was the use of citation analysis as a means of establishing the extent and direction of

⁶⁴ Carnegie Institution of Washington Yearbook, 1923, 22:335–337. For a later separate development of similar techniques applied to Sarton’s *Introduction*, see P. A. Sorokin and R. K. Merton, “The Course of Arabian Intellectual Development, 700–1300 A.D. A Study in Method,” *Isis*, 1935, 22:516–524. It was characteristic of Sarton that he never mentioned his much

earlier forays in quantitative and graphic analysis when he enthusiastically decided that this little paper by Sorokin and Merton, based on data drawn from his own *Introduction*, should appear in *Isis*.

⁶⁵ Lawrence Stone, “Prosopography,” *Daedalus* (Proceedings of the American Academy of Arts and Sciences), 1971, 100:46–79.

intellectual influence.⁶⁶ Thus, for instance, Sarton remarks of Guy de Chauliac that "although he cites in all 88 authorities, Guy depends heavily on the writings of Galen (cited 890 times), Abū-l-Qāsin (175 times), Alī ibn 'Abbās (149 times) and Ibn Sīnā (661 times)."⁶⁷ This sort of analysis of course knows nothing of the single-minded rigor of the computerized citation indices which are now such an important (though dangerous) tool for the sociological analysis of contemporary science.⁶⁸ It does however again reveal Sarton's originality of mind, his determination in execution (all that manual counting), and his great desire to grasp the whole, by whatever methods came to hand.

The third and most characteristic brand of Sartonian arithmetic, that of quantitative content analysis, again shows the same concern to grasp and display the totality. Just as he would repeatedly quantify a book's citations to previous works as one way of establishing its intellectual heritage, so he would assay the structure of a work by measuring the number of pages devoted to each of its themes. A particularly apposite example is provided by Sarton's concluding examination of his own *Introduction*.⁶⁹ As this quantitative content analysis plainly shows, much of the *Introduction* is devoted to subjects that historians less holistically inclined than Sarton might hesitate to include within the scope of science. Thus 2.7 per cent is on law and sociology (*sic*), 8.5 on philology, 4.6 on translations, 9.1 on historiography, and 26.2 on religious, philosophical, and historical backgrounds: a total of 51.1 per cent, compared with 24.6 per cent on what are generally regarded as the natural sciences proper.

The results of his own analysis supply another reason why Sarton's work, while looming large in the broad field of ancient and medieval studies, has proved less central to the history of science than its author hoped. That the *Introduction* does not command the field of medieval science may be seen from some occasional statistics on citations—of the kind Sarton liked to use. A study of *Isis* for the year 1934 (when Vols. I and II of the *Introduction* were available) and 1969 shows sixteen separate references to the *Introduction* in the former year and not one in the latter. The journal *Speculum* also fails to show a single reference in 1969. Turning to recent influential monographs, Marshall Clagett's *Archimedes in the Middle Ages* has one solitary footnote to the *Introduction*, while Lynn White's *Medieval Technology and Social Change* ventures no more than six.⁷⁰

This comparative paucity of citation does not of course mean that scholars ignore the *Introduction* or find it less than fundamental for reference purposes. When the first volume appeared in 1927 Charles Singer came close to the mark in describing it as an "inspired dictionary"⁷¹—and, like other dictionaries, it is consulted rather than cited.

⁶⁶ For some remarks on Sarton's early use of citation analysis, see Merton, *On the Shoulders of Giants*, pp. 224, 227, and *Science, Technology and Society in Seventeenth Century England* (New York:Howard Fertig and Harper & Row, 1970), p. xii.

⁶⁷ *Introduction*, Vol. III, p. 1691.

⁶⁸ See, e.g., E. Garfield, "Citation Indexing for Studying Science," *Nature*, 1970, 227: 669–671; Jonathan Cole and Stephen Cole, "Measuring the Quality of Sociological Research: Problems in the Use of the *Science*

Citation Index," *American Sociologist*, 1971, 6:23–29.

⁶⁹ Vol. III, p. xiii. A rather different sort of example may be found in his 1936 *Study of the History of Science*, pp. 20–26.

⁷⁰ Marshall Clagett, *Archimedes in the Middle Ages* (Madison:Univ. of Wisconsin Press, 1964); Lynn White, jr., *Medieval Technology and Social Change* (Oxford:Oxford Univ. Press, 1962).

⁷¹ In the *English Historical Review*, 1929, 44: 294.

The analogy serves also to highlight Sarton's limited *intellectual* impact on the discipline he did so much to create. There are many reasons for this. His holistic concerns, his ambition for full comprehension, and his emphasis on the moral virtues of historical inquiry all run counter to the preference for depth, particularity of detail, and detached analysis that have increasingly characterized historical scholarship (not just the history of science) over the last half-century. The positivistic cast of his thought ("I have tried to name the people who were first to do this or that; to take the first step in the right direction . . .") and his belief in the uniqueness of science are antithetical to the idealistic, intuitionistic, and relativistic currents become so powerful in recent Western thought. Finally, his emphasis on historical approaches through biography and bibliography, necessary and useful as they are, could not capture the imagination of scholars or provide a powerful technique of analysis around which a research school could form.

To create a cognitive identity for a discipline requires more than the building of an infrastructure of journals, reference works, teaching texts, advanced monographs, and bibliographic aids. It also requires the formation of that sense of common orientation and purpose which both springs from and nourishes agreement on central problems and on the relevant techniques of conceptualization and analysis. Similarly, the creation of a professional identity is not guaranteed by the formation of learned societies and the production of extensive propaganda, necessary though these are. It also requires the recruitment of followers and students and more especially the creation of satisfactory career structures. These latter requirements depend on structural and cultural shifts within society at large and cannot be directly commanded or engineered.

Tools, texts, journals, and reference works are most often the labor of many hands. Problematics and techniques are more usually created by some one or few individuals—the discipline builders. Through personal effort of both an intellectual and social kind they define the central issues, create teachable and transmittable techniques, and recruit a school of followers and students. Justus Liebig, analytical routines and organic chemistry, or T. H. Morgan, fruit flies and genetics, provide obvious examples of this latter aspect of the creation of cognitive and professional identities of special disciplines. The paradox of George Sarton is that he almost single-handedly created the infrastructure of a new discipline yet failed to provide the readily identifiable intellectual orientations, problems, and techniques that would engage the interest of advanced students.

One ironical consequence was that Sarton thus opened the way for Alexandre Koyré to have a major impact on the first full-scale "generation" of American "professional" historians of science, when shifts in the larger society created a demand for university teachers of this new discipline. Equipped with a growing range of reference aids and a profound sense of the importance of their task, they could not find in Sarton's work (or elsewhere) coherent and finite problems and techniques. It was left to a philosopher, innocent of experimental science, committed to Plato, and hostile to all forms of positivism, to give an intellectual sense of direction to a discipline which had suddenly found its professional identity.⁷² It need scarcely be said that the thrust of

⁷² On this, see T. S. Kuhn, "The History of Science: Has Its Present Past a Future?" in D. L. Sills, ed., *International Encyclopedia of the Social Sciences* (New York: Macmillan, 1968) and Arnold Thackray, "Science: Has Its Present Past a Future?" in R. Steuwer, ed., *Historical and Philosophical Perspectives of Science* (Minneapolis: Univ. of Minnesota Press, 1970), pp. 112–127.

Koyré's work was antithetical to those holistic beliefs Sarton cherished but could not effectively communicate.

Sarton's influence on the discipline he labored so faithfully to create has thus been further obscured. Ironically, his monument is of its nature becoming progressively invisible to newcomers to the field. In founding a journal, in emphasizing critical bibliography, in essaying broad surveys, and above all in writing the *Introduction* he was creating elements required by the discipline, not methods to be emulated or finished products for display. Yet he did more than merely fashion tools. He also possessed, preached, and put into practice a range of insights that succeeding generations of scholars may reject but cannot safely ignore. Already younger historians of science, increasingly concerned with approaches that embrace a range of social and cultural factors in the development of science, may find themselves in surprised agreement with Sarton that "the task of the historian should not be restricted to the more technical aspects of scientific discovery."⁷³ His belief in the uniquely cumulative aspect of science may also be due for rediscovery. It will no doubt be considerably longer before scholars again attempt that holistic inquiry and feel that moral passion so typical of the man.

The limits of George Sarton's influence on the history of science reveal by default how the cognitive identity of a discipline is a matter of theoretical orientation and worldview as well as tools and techniques. His inability to engineer the careers and train the disciples who would create a professional identity for his subject also demonstrates how much this latter aspect of discipline building depends on factors beyond the control of any individual. The history of science is now a firmly institutionalized field of learning in America and elsewhere. At first glance it shows little trace of Sarton's influence. Yet he not only created and assembled the necessary building materials through heroic feats of labor, he also saw himself as—and he was—the first deliberate architect of the history of science as an independent and organized discipline. It is in some ways a fitting paradox that the edifice we enjoy is radically different from the one he envisaged and yet is fundamentally indebted to his plans.

⁷³ George Sarton, "Knowledge and Charity," *Isis*, 1922–1923, 5:5.

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[Footnotes]

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²⁰ **The Teaching of the History of Science**

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²⁹ **George Sarton and Harvard University**

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⁷¹ **Review: [Untitled]**

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