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GEORGE SARTON AND THE HISTORY OF SCIENCE

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Sarton, who was born in 1884* in Belgium, came to the United States in 1915. He gave a few lectures and courses during his first years in America, and in 1918 he became associated with the Carnegie Institution of Washington. He had already founded Isis in 1912, while in Belgium, and although its publication was interrupted during the four years of World War I, it began to reappear in the postwar years when Sarton established himself in the United States. Following a meeting of the American Historical Association in Boston, in December 1923, David Eugene Smith, Lynn Thorndike, and a group of other members organized the American History of Science Society, incorporating it in January 1924. The History of Science Society was created for the specific purpose of furthering the study of the history of science, and to support Sarton's work and especially his journal Isis.[1]

The first years in the United States were not easy for Sarton, but when in 1918 he was appointed research associate of the Carnegie

* We have thus reached the centenary of Sarton's year of birth.

[1] *Isis*, vol. 6. 1924, pp. 4-8 ; *Isis*, vol. 7, 1925, p. 371; *Isis*, vol. 16, 1931, pp. 125-126; James B. Conant, "George Sarton and Harvard University", *Isis*, vol. 48, 1957, p. 302; Dorothy Stimson, *Sarton on the History of Science, Essays by George Sarton, Selected and Edited by Dorothy Stimson*, Harvard University Press, 1962, Preface, p. VI.

Institution of Washington, things started to rapidly change for the better. For this enabled him to devote himself to his studies without financial anxiety. After a short time he moved to Cambridge, Massachusetts, and was given a suite of rooms in the Widener Library, where he continued to work almost to the end of his life.[2]

A French article by Sarton, bearing the title "Une Encyclopédie Léonardesque", published in 1919 in *Raccolta Vinciana*, clearly shows the great importance Sarton attached to his association with the Carnegie Institution. There he writes:

"L' Institution Carnegie, jeune comme elle l'est, a déjà accompli sa haute œuvre civilisatrice dans des directions nombreuses et avec beaucoup de succès. ... L'an dernier elle a pris une initiative nouvelle qui lui fait le plus grand honneur; les *Trustees* ont décidé l'organisation de recherches systématiques sur l'histoire des sciences et m'ont nommé associé de l'institut (research associate) dans ce but précis. C'est là un commencement modeste, mais dont l'importance ne saurait être exagérée; qu'il me suffise de dire que la position qui a été créée pour moi par la *Carnegie Institution* et qui me permet de consacrer tout mon temps à l'étude désintéressée de l'histoire des sciences est autant que je sache unique au monde."

Here Sarton appends the following footnote: "Il existe une ou deux positions semblables en Allemagne pour l'histoire de la médecine et des mathématiques, mais non pour l'histoire des sciences."[3]

Sarton then broadly outlines his projected work. There are two major items listed here: 1) A substantial work on Leonardo and the science of his time, and, 2) The history of nineteenth century physics and its applications.[4]

Then he continues with the following words:

"Et d'abord, — pourquoi Léonardo a-t-il été choisi comme le sujet de notre première entreprise? C'est que la portée des études

[2] E. M. S., "Bibliographical Data on George Sarton", *Studies and Essays in the History of Science and Learning Offered in Homage to George Sarton*, ed. M. F. Ashley Montagu, Henry Schuman 1944, p. XII-XIII.

[3] George Sarton, "Une Encyclopédie Léonardesque", *Raccolta Vinciana*, fascicule 10, Milano 1919, pp. 235-236.

[4] *Ibid.*, p. 236. See also, "A Summing up" (Report to the Carnegie Institution of Washington, 1949), *Sarton on the History of Science*, ed. Dorothy Stimson, pp. 367-370.

historiques, auxquelles j'ai consacré ma vie, dépasse de beaucoup leurs résultats immédiats. Le but n'est pas seulement de connaître l'histoire des sciences, mais d'*humaniser* la science, c'est-à-dire de la rendre plus aimable et plus vivante, de la montrer en voie d'évolution et de progrès, de mettre en évidence à la fois son unité profonde et ses relations innombrables avec toutes les autres activités de notre vie. Or, comment ce but serait-il mieux atteint, comment serait-il possible de mieux faire comprendre à la fois aux savants et aux artistes cette synthèse et cette harmonie idéale que de la leur montrer déjà realisée dans la personnalité unique et grandiose de Léonardo, à la fois le plus grand artiste, le plus grand savant et peut-être le meilleur homme de son siècle?" [5]

Two pages later the text reads thus:

"D'ailleurs, je ne me propose pas seulement d'exposer les idées de Léonardo et de ses contemporains, mais je m'efforcerai de plus d'expliquer aussi complètement que possible leur genèse et leur évolution. Cela m'oblige à étudier plus profondément que je ne le désirerais, la philosophie médiévale chrétienne, arabe et juive, mais la récompence est grande. De même que Léonardo me permettra de démontrer d'une manière concrète l'unité de la science, il me permettra aussi de montrer sa *continuité.* Car, si original que soit son singulier génie, il n'en est pas moins profondément enraciné dans le passé. Léonardo n'est pas un accident isolé, un miracle, mais le fruit soudain et rare d'une longue évolution, jamais entièrement interrompue et qui, pour être en grande partie secrète, n'en est pas moins réelle." [6]

Speaking of Sarton, Dorothy Stimson writes:

" ... Thus his first scholarly love, Léonardo da Vinci, could not properly be studied until he knew what had gone before. Out of that search grew his many-volumed *Introduction to the History of Science* which after twenty years' labor he had to end fifty years before he had reached da Vinci." [7]

Two mutually related ideas on which Sarton insisted throughout his career were the ideas of "the unity of mankind" and "the unity

[5] *Ibid.*, p. 236.
[6] *Ibid.*, p. 238.
[7] Dorothy Stimson, *Sarton on the History of Science*, Preface, p. IX.

of science" or "the unity of knowledge". He must have felt entitled to a verdict on these points also because of his wide coverage of so many groups of people from all over the world in his *Introduction* volumes. And he dealt there with periods during which there was comparatively little cultural contact between those widely different geographical regions.

Early in his career, Sarton says:

" ... For one thing, science—at least that part of it which has already become classical—is the common thought of the whole world; it is the organized body of all the facts and theories from which almost all arbitrariness has been excluded, upon which enlightened people are unanimously agreed and which is placed temporarily beyond the range of discussion. The domain of classical science is the privileged domain of internationalism, for it is already the common patrimony of all men. Moreover, science constitutes the very axis of human advance and furnishes the very principle and the fundamental methods of social organization. ..." [8]

We also hear him speak in the following words:

"The history of science establishes the unity of science in at least two different ways. First, the progress of each science is dependent upon the progress of the others; this implies of course that the sciences are not independent, but interrelated in a number of ways, and that the interrelations are not accidental but organic. Second, the simultaneity of scientific discoveries made in different places and sometimes by means of different methods implies also an internal congruency. ..." [9]

Suchlike assertions by Sarton, of which he was not very sparing, have created quite widely the impression that he was much given to philosophizing. Such a generalization would be quite misleading, however, particularly with respect to certain aspects of his ideas. With respect to his words in his last quoted passage, e. g., I feel that Sarton never appreciably underscored the idea of unity of knowledge in the sense of close interrelations between various fields of knowledge; he perhaps referred to it partly for the sake of completeness. It is

[8] George Sarton "The New Humanism", *Isis*, vol. 6, 1924, p. 24.

[9] Sarton, *Introduction to the History of Science*, vol. 1, p. 31.

my impression that his references to it were only sporadic and that they were often superficial rather than substantial. But, in contrast to this, he did emphasize the idea that science oversteps national, linguistic, and religious boundaries, which occurs in the passage quoted from him to which footnote 8 has been appended.

Sarton, as we have seen, had planned to prepare a history of nineteenth century physics for the Carnegie Institution. At Harvard he gave a history of mathematics course which was called Mathematics 7 and was listed among mathematics courses, if I remember correctly. Moreover, James B. Conant writes: "And the scholarly training which Professor Sarton considered essential for a real scholar included 'A knowledge of the European languages, paleography, scholastic philosophy, political history, ecclesiastic history' as well as a basic training in one of the natural sciences." [10]

All this indicates that Sarton did not hesitate to take up different sciences separately. We also see that he considered it quite natural for historians of science to cultivate only one scientific field as that of their major interest. Yet he did not believe that the juxtaposition of courses on the histories of physics, chemistry, mathematics, and biology in different departments of a university could constitute instruction in the history of science anywhere close to an ideal state of affairs.

Conant says, "From Professor Sarton I learned, while I was a graduate student in chemistry, the difference between the history of *a* science (as exemplified by Chemistry 8) and the history of science. . . ." [11]

It is well known that Sarton had pet ideas such as the claim that the history of science should be accorded a place of major importance in history in general and that it should constitute a bridge between science and the humanities, or between science and humanism. He might dwell briefly on such ideas at the very beginnings of his courses, but then he would rarely refer to them again as the courses proceeded. More frequently he would call attention to unintention-

[10] James B. Conant, "George Sarton and Harvard University", *Isis*, vol. 48, 1957, p. 305.

[11] James B. Conant, "History in the Education of Scientists", *Harvard Library Bulletin*, vol. 14, 1960, p. 317.

al and accidental cooperation between scientists working in different countries whenever, as in the case of science in modern Europe, the subject matter dealt with served to throw light on many clear and interesting examples of such nature. But even then his remarks would be of the nature of brief asides.

At any rate, as far as I know, Sarton practically never took up these notions in purely conceptual lines in a systematic manner, he never wrote substantial monographs on these ideas or on the concepts they involved with a formal philosophical approach. For him the unity of man and the unity of scientific knowledge were practically obvious on a factual basis, on the basis of copious data pervading all parts of the history of science. Over and above such notions and such pet ideas he was interested in promoting and establishing on a firm footing the cultivation of the history of science. His main concern or objective was to establish the history of science as an independent academic discipline.

In 1930 he wrote," ... The intellectual élite is at present divided into two hostile groups, — which we might call for short the literary and the scientific, — who do not speak the same language nor think in the same way. If nothing is done, the gap separating them must necessarily increase, together with the steady and irresistable progress of science. ...

"I believe that the gap can be reduced considerably if there be enough good will on both sides, and that it will eventually be possible to bridge it. The main purpose of the movement which I initiated so many years ago and to which my life has ever since been devoted, is precisely to build that bridge and to educate men who will become the natural intermediaries between the two sides. Such men would be very few to begin with but they would slowly increase in number. ...

" ... However humanism may be defined, at least we shall agree I am sure that it should not harbor intolerance.

"Personally I would much prefer not to speak of humanism any more but to work quietly in my little corner preparing materials for the bridge to be built. ..." [12]

[12] George Sarton, *The History of Science and the New Humanism*, 1931, pp. 8-10.

I believe that the statement in the last sentence above is truly descriptive of Sarton's attitude and behavior in the years that followed.

Science historians had of course been in existence before, and a quite impressive literature of the history of science had come to exist. But its coming into being had been dependent largely on chance and personal taste. The best and the most outstanding historians of science had generally been trained as scientists, and they had later developed their interest in the history of their branch of science and gone into the field of the history of science. It was Sarton's objective to have a substantial group of people trained in universities as historians of science, just as historians, physicists, and psychologists were trained by receiving instruction in these particular fields respectively. I believe that this was Sarton's paramount and straightforward goal in contrast to his more fictitious or idealized plans to humanize science or to make historians shift their central interest to science. This at least was the more urgent matter, and once it was realized to a reasonable extent, it was Sarton's hope that, somehow the rest would probably take care of itself.

According to Sarton, those who were to be trained as historians of science should, for this purpose acquire sufficient knowledge in one branch of science at least and also in certain source languages. A historian of science, in Sarton's opinion, should become familiarized with the whole field of the history of science and should, in addition, go into two kinds of specialization: Vertical specialization in a branch of the history of science such as the history of mathematics, physics, or astronomy, extending vertically through all periods, and a horizontal field of specialization spreading over a certain civilization or culture at a certain era but encompassing as much as possible all branches of science and related intellectual fields. Examples of this would be Greek science, India, medieval Islam, or seventeenth century Western Europe. Strictly speaking, this second type of specialization is more easily feasible for earlier periods, of course.

However, Sarton was not dogmatic or overenthusiastic, although he was in reality unswerving, in this mode of training historians of science. He used to say that as the history of science is a youthful discipline, there are various methods and manners of approach for

the historians of science, and that this freedom, not infrequently, was of advantage to the field. For in this way it became possible for its representatives to complete each other and to make up for one another's shortcomings.

Although the *Introduction* volumes had to stop at the end of the fourteenth century, Sarton himself measured up quite well to the ideal he set up for a well-trained historian of science. For he was very well-versed indeed in European science in the sixteenth and the following centuries. The wide coverage of the courses he gave at Harvard as well as certain substantial articles of his give ample proof of this. Modern European science rather than the Middle Ages was, at least initially, Sarton's area of primary competance.

Sarton's ideal was, however, to have people draw their intellectual inspirations from the history of science. Historians of science, indeed, as he would have them, with their primary field of specialization in the history of science itself, would not be expected normally to impose upon the history of science notions more peculiar to other fields of endeavor and not so appropriate to science and its history. He wrote in one of his later works: "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." [13]

Initially, Sarton's plan for his university education was to study philosophy, and he started to do so. But before long he abandoned the subject "in disgust". [14] It is interesting to hear him speak about twenty years later, in 1919, in a passage quoted from him above to which our footnote 6 has been appended, of the necessity for him to go more deeply into the study of medieval philosophy than he would have liked to do.

These statements from his student days and from the beginning of his career are typical of Sarton also in the much mature phases of his life. He certainly had no aversion or dislike for philosophy, but it may be said, I believe, without hesitation, that he did not find the philosophical approach to questions very inviting and much

[13] Arnold Thackray and Robert K. Merton, "On Discipline Building : The Paradoxes of George Sarton", *Isis*, vol. 63, p. 483.

[14] I. Bernard Cohen, "George Sarton", *Isis*, vol. 48, p. 287.

preferred the more concrete and direct scientific ways of dealing with things. In *A History of Science, Ancient Science Through the Golden Age of Greece*, published in 1952, he writes, "We clearly realize that Plato is the typical and 'ideal' philosopher, whose knowledge or wisdom is supposed to come from above and to stoop like an eagle on the objects below. The knowledge of a metaphysician is complete to begin with and proceeds from heaven downward; the knowledge of the man of science, on the contrary, begins with homely things on the face of the earth, then soars slowly heavenward. The two points of view are fundamentally different." [15]

Sarton conducted a seminar in the history of science to which guest speakers such as Abbot Payson Usher, Arthur O. Lovejoy, Raymond Clare Archibald, Tenny L. Davis, Dirk J. Struick, and Robert S. Woodbury were invited at times as guest speakers. [16] One day when Lovejoy was guest speaker, after he had finished speaking, Sarton made a remark to the effect that in such fields as medieval science and Aristotelian physics the more properly or specifically scientific content or material should be detached from its philosophical context and accorded preferential treatment by the historians of science. Lovejoy expressed his disapproval not only in simple words but also by a distortion in his countenance and said that the complex of these ideas resembled delicate roots of a plant all tangled up at the bottom of a pot and that one could not possibly hope to succeed in clearing and sorting out a single root without breaking it to pieces. Sarton had no answer, but he took this remark in good part; he merely smiled at Lovejoy's impatience with his suggestion.

Sarton too, I believe, did not have in mind a thoroughgoing dichotomy. In speaking of Ibn Sînâ, e. g., he says, "The philosopher Ibn Sînâ, as in Aristotle, can never be separated from the man of science." [17] But Sarton, when speaking, used often short and to-the--point expressions and did not use elaborate and sophisticated sentences, and that is why he had perhaps gone somewhat beyond his real mark.

[15] *Op. cit.*, p. 431.

[16] See, *Isis*, vol. 26, 1936, pp. 154-155.

[17] Sarton, "Avicenna : Physician, Scientist and Philosopher", *Sarton on the History of Science*, ed. Dorothy Stimson, 1962, p. 69.

With all due respect for the fine-grained and exacting philosophical analyses of men like Lovejoy and Koyré, they were, I feel, to some degree different from Sarton's ideal of historians of science drawing their main and adequate inspiration from within the pale of the history of science, or science itself perhaps, or, at least. Koyré was undoubtedly a great historian of science, interested in a limited part of that vast field, who was a powerful source of inspiration for an important generation of science historians and one who did exemplary work.[18] But he seems to have looked down to a considerable extent on the importance of experiment in Galileo's work, and, together with Cassirer to have exaggerated Galileo's Platonism. More recent research seems, indeed, to indicate that the place of experiment in Galileo's work was of considerable moment and that the situation was not at all like that pictured by Koyré.[19]

W. H. Donahue writes, "In the nineteenth century he [Galileo] was commonly depicted as a champion of fact (as opposed to weightless theory), discovering natural laws by watching chandeliers swing and dropping objects from the Pisan *campanile*. Later, Alexandre Koyré showed us quite a different Galileo, a Platonist whose regard for theory was such that he scornfully rejected the need for empirical verification. Although this view gradually gained wide acceptance, in more recent years, and especially during the last decade, it has been shown to be a serious misrepresentation. Research by Thomas Settle and others has revealed the large extent to which Galileo relied upon experiment, and there is little evidence to suggest that Galileo believed in a Platonic mathematical archetype for the universe. The result has been an increasingly clear picture of what Ga-

[18] Arnold Thackray, "Making History", *Isis*, vol. 72, 1981, pp. 7, 8.

[19] See, Thomas B. Settle, "An Experiment in the History of Science", *Science*, 6 January 1961, No 3445, pp. 19-23; David C. Lindberg, "Galileo's Experiments on Falling Bodies", *Isis*, vol. 56, 1965, pp. 352-354 ; Stillman Drake, "Free Fall in Galileo's Dialogues", *Isis*, vol. 57, 1966, pp. 269-271; Stillman Drake, "Galileo's Experimental Confirmation of Horizontal Inertia : Unpublished Manuscripts (Galileo Gleanings XXII)", *Isis*, vol. 64, 1973, pp. 290-305; James MacLachlan, "A Test of an 'Imaginary' Experiment of Galileo's", *Isis*, vol. 64, 1973, pp. 374-379; Stillman Drake and James MacLachlan, "Galileo's Discovery of the Parabolic Trajectory", *Scientific American*, March 1975, pp. 102-110; Ronald Naylor, "Galileo : Real Experiment and Didactic Demonstration", *Isis*, vol. 67, 1976, pp. 398-419.

lileo was not, and much lively controversy as to the philosophical basis (if any) for his views." [20]

Richard S. Westfall's appraisal of the question reads as follows:

"The larger work . . . is infused with Drake's own interpretation of Galileo. Not everyone will accept it. Drake is well aware that he represents a minority position; a polemicist like his hero, he has drawn all his details together into a vigorous and frequently pungent exposition of the experimentalist view of Galileo: The enemy is Alexandre Koyré and his followers, who emphasize Galileo's debt to Platonic philosophy and question whether he ever performed experiments. As far as I am concerned Drake settles the issue once and for all. From the manuscripts he draws manifold evidence of experiments (among others, with inclined planes) that are beyond reasonable denial. One cannot avoid the conclusion that Koyré's insistence on thought experiments in Galileo was exaggerated, indeed greatly exaggerated. I speak, let me say, as one deeply influenced by Koyré's writings.

"At the same time, it appears to me that Drake is guilty of equal excess in attempting to paint a narrowly empirical Galileo as the model of the modern experimental scientist. It was the great virtue of Koyré's work to teach us that profound philosophic questions not to be settled by observations in the laboratory lay behind the shift in views that ushered in modern science. The fact that Galileo did in fact experiment in no way negates that point. . . ." [21]

There is much wisdom and discernment in these words. It seems to me that it may be rightfully claimed, nevertheless, that as a result of Koyré's distorted view of Galileo's methodology, in so far as recourse to experiment is concerned, a more adequately or judiciously balanced picture of Galileo's work may be claimed to be given by, e. g., E. Gerland in 1913, than by the pretentious monographs of Koyré, [22] written twenty six years later.

[20] W. H. Donahue, review of Stillman Drake's *Galileo Against the Philosophers . . .*, *Journal for the History of Astronomy*, vol. 10, 1979, p. 44.

[21] Richard S. Westfall, review of Drake's *Galileo at Work*, *Isis*, vol. 70, 1979, p. 275.

[22] See, Gerland's *Geschichte der Physik*.

It should be of interest in this connection that in the Royaumont Symposium on the Sixteenth Century Science held in 1953 Koyré refused to attach any importance to a remark made touching the fact that Walter Hermann Ryff had, in 1537, just one hundred years before the appearance of Galileo's Dialogue on Two New Sciences, spoken of the empirically established conclusion that the maximum range of a projectile corresponds to 45° angle of elevation of the gun barrel. He declined to concede that suchlike experiences of gunners could be of relevance, as ready experimental data, for Galileo in his work on the trajectory of projectiles.

As to Galileo's Platonism, I have already quoted a statement of W. H. Donahue which is relevant to this question. I shall merely make the following additional quotation from Ernest A. Moody, to show at least that Galileo's Platonism would seem to be a highly controversial matter:

"To wed sense to reason, and to tie reason to reality — this is an ideal that transcends the oppositions between Aristotelians and Platonists, and it was his devotion to this ideal of true science that enabled Galileo to earn full right to the title of the 'founder of modern mechanics.' "[23]

It may not be out of place to wonder whether Koyré as an example for such an issue would not constitute a type that would well-nigh defeat its own purpose, considering the fact that I am favorably disposed towards defending Sarton's viewpoint. In the present context, however, its value rests mainly in its constituting a caustic test for the cogency of Sarton's viewpoint, and I believe also that it serves to bring out certain fine points on which there seems to have been some misunderstandings. Moreover, as I have pointed out above, Sarton also expressed the belief that the greater degree of freedom available to those who cultivate the history of science did, at times, serve as an advantage to the growth of the history of science, as a new discipline.

Koyré had, I assume, a philosophical basic training which somehow made him look down on the empirical side of scientific

[23] Ernest A. Moody, "Galileo and Avempace", *Journal of the History of Ideas*, vol. 12, 1951, p. 422. See also, *ibid.*, pp. 163-183, 192-193, and Stillman Drake and W. H. Donahue, *op. cit.* (Donahue's review of Stillman Drake's *Galileo Against the Philosophers*), *Journal for the History of Astronomy*, vol. 10, 1979, pp. 44-47.

work. But a person with a scientific basic training in mathematics and with a mathematical type of predilection may well feel pretty much the same way. And mathematicians too are known to have been wont to split among themselves into different schools of thought. According to Charles Singer, it has been said that 'everyone is by nature a disciple either of Plato or of Aristotle'.[24] Cultural backgrounds of this nature too could possibly account for such variations of intellectual taste.

It should certainly not be unduly optimistic to think or hope that the history of science of the self-centered and self-reliant type as conceived by Sarton in particular can effectively help broaden the perspective or background against which such differences of value judgements of the philosophy of science may be compared or appraised. It should therefore be commendable to create circumstances conducive to form or evaluate such judgements through the intellectual atmosphere emerging from the facts of the history of science itself, as much as possible, rather than have scholars trained in other fields try to introduce or impose preformed ideas into or upon the history of science. For, to say the least, this will add a new dimension to our way of looking into such matters. The same should be valid of course, and perhaps with greater force, for other more stereotyped varieties of ideologies.

I should stress the fact, on the other hand, that I have absolutely no concrete evidence that Sarton actually considered Koyré not to conform to his ideal type of science historian. It is only my personal judgement or feeling that he did not quite conform to that ideal type. I happened to sit in at an executive committee meeting of the International Academy of the History of Science and the Union of the History and Philosophy of Science held in Jerusalem on the occasion of the 1953 International Congress of the History of Science, and I was impressed by the genial relations between Sarton and Koyré, as well as others who were present, such as Bodenheimer, Millás Vallicrosa, Laignel-Lavastine, Joseph Needham, and their much younger associate, René Taton.

[24] Charles Singer, *A Short History of Science to the Nineteenth Century*, Oxford 1941, p. 34.

I knew Laignel-Lavastine through his work, and I had come to get more closely acquainted with him during the Congress. There was an item on the agenda of that evening's meeting which required a bit of subtle handling, and, all of a sudden, Laignel-Lavastine, who was very close to me, cast an inquisitive glance upon me and asked about the why and wherefore of my presence there. It was explained that I was Sarton's guest and that I naturally had no right to vote. For a moment I was afraid I was going to be thrown out of the room I had entered through no fault of my own, but the matter was settled with the greatest of ease, and I was allowed to stay. This little incident helped me though to notice more clearly the concord that seemed to prevail among these senior members of the family of the historians of science.

I also see, that Sarton had Giorgio de Santillana review Koyré's Etudes Galiléennes and that he published this not as an ordinary review but as a main article, though the review is by no means a long one. [25]

Santillana rounds up his review with the following words:

"After following this careful investigation, one is apt to feel that in its very accuracy it does less than justice to a fundamental character of Galileo's thought. In that intricate web of doubts, tests, and qualifications, we should not lose perception of an essential physical insight and firmness which eventually proved more fruitful than Cartesian clarity. But if we thus risk losing sight of the wood because of the trees, it is not the author's fault; it is simply that he has done his job with painstaking exactness."

I. Bernard Cohen writes:

"In 1936, Harvard established the degree of Ph. D. in the history of science, and Sarton inaugurated his seminars. Under his direction, two candidates completed their doctorates, ... I suspect that the reason why there were not more professionl students was that the immensity of his task of editing *Isis* and *Osiris* of research and writing, and of lecturing and propagandizing for the new discipline left little energy for attracting and training students. Yet he must have had considerable pleasure in seeing his labors bear fruit all over the world,

[25] See, *Isis*, vol. 33, 1941, pp. 654-656.

in witnessing new journals and many books and articles in the history of science." [26]

When I first came to Harvard in the school year 1934-1935, there were two candidates for Ph. D. in the history of science, both working under the direction of Professor Sarton. One of them was Robert S. Woodbury who lectured on the history of technology in M.I.T. I do not remember the other gentleman's name. They did not continue their work for the doctorate, however. As I remember it, it was said that a committee for work toward Ph. D. in the history of science had been set up in 1932 and that such work had thus become possible at Harvard since that date. I find no reference to such an arrangement in *Isis*, and this seems very puzzling to me. Could this possibly indicate a disappointment of Sarton on the decision taken?

James B. Conant, Harvard's distinguished president, makes the following statements which seem to contain a clue, though somewhat vague, concerning this matter:

"George Sarton's official connection with Harvard University started in the fall of 1916 and continued until his retirement as professor emeritus in 1951. The first appointment as a lecturer for two years seems to have been one of those temporary arrangements incident to a world war and its dislocations. . . . Certainly the first arrangements that were made were quite special. Sarton received an appointment to the staff of the Carnegie Institution of Washington, as well as an appointment as lecturer at Harvard. The History of Science Society was founded for the explicit purpose of supporting *Isis*. In all these matters, Professor Lawrence J. Henderson played an active role.

"Henderson was one of a small group of younger men on whose judgement President Lowell relied, . . . Neither President Lowell nor Professor Henderson were unduly worried about academic formalities or organization. They did not let concern about the future interfere with their conviction that the one thing that really matters in a university is the ability and originality of the scholarly professors. And President Lowell was usually willing to take unorthodox steps in support of his convictions. . . .

[26] I. Bernard Cohen, "George Sarton", *Isis*, vol. 48, 1957, p. 296.

"In 1933, at Henderson's instigation, an attempt was made to work out an arrangement with the Carnegie Institution by which Sarton's appointment as annual lecturer would be transformed into a permanent professorship. But it was not until 1940 that this suggestion became a reality and Professor Sarton's relationship to both Harvard and the Carnegie Institution was put on a permanent unambiguous basis. That this was a step forward in the recognition by Harvard of the significance of the history of science and the acknowledgement of Sarton's eminence, there could be no doubt. Furthermore, the appointment of a standing committee on History and [of?] Science of the Faculty of Arts and Sciences a few years earlier had provided for the first time at Harvard an academic basis for both graduate work leading to a Ph. D. degree and an undergraduate field of concentration. But such steps in Professor Sarton's opinion fell far short of establishing his discipline on an adequate basis." [27]

Conant may be referring to the committee which I remember as having been set up in 1932, but he does not specify the date of its formation. At any rate, Conant explicitly refers to Sarton's dissatisfaction with the steps taken at Harvard in the way of establishing the history of science there as an independent academic discipline.

The following statements by Conant are also of interest from this standpoint. He says, "... This and similar proposals that Professor Sarton from time to time put forward had budgetary implications which prevented the administration from giving them serious consideration". And again, "The time was not ripe for the launching of a scheme of the magnitude which Sarton had in mind. For my own part, I felt that in the United States, unlike Europe, a new academic discipline must prove its value at the undergraduate level if it was to find adequate support for a graduate program. On this point I never could convince Professor Sarton. ..." [28]

I do not remember hearing Sarton say anything concerning this question. My experience, however, has led me to think that, under certain circumstances, instruction in the history of science

[27] James B. Conant, "George Sarton and Harvard University", *Isis*, vol. 48, 1957, pp. 302-303.

[28] *Ibid.*, p. 304.

George Sarton (1884–1956)
at the age of 68

could at times be thought of as associated more conveniently with students of relatively advanced level. For the history of science obviously has to rest upon some knowledge of basic sciences and an appreciation of the flavor that can be bestowed by history upon our judgement. If I am not mistaken, instruction in such fields as librarianship and education too, which need necessarily be built upon or superadded to knowledge already acquired in certain branches of learning, are generally planned as postgraduate teaching. Sarton may possibly have had such a scheme of instruction in mind for the training of historians of science.

Altogether, it seems that Sarton, as a pioneer in establishing the history of science as an independent academic discipline, had the feeling that he was not in possession of adequate means for duly carrying out his mission from the standpoint of instruction. But he surely must have felt that he was in a fine position so far as laying the foundation of this work as a scholar was concerned. Hence his words quoted above to the effect that he would prefer to work quietly in his "little corner preparing materials for the bridge to be built."

Arnold Thackray and Robert K. Merton write:

"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 library to do scholarly work that made many regular members of the Harvard Faculty appear somewhat like dull serfs enslaved to teaching and committee work." [29]

At Widener Library Sarton did not have to gain access to the stacks through the main entrance. He had a pass key to certain closed doors leading to the stacks through a staircase not far from his study. He took me to the stacks a couple of times through these closed doors

[29] Arnold Thackray and Robert K. Merton, "On Discipline Building : The Paradoxes of George Sarton", *Isis*, vol. 63, 1972, p. 480.

in order to consult certain books. He would grasp the rail of the balustrade with his hand and pull himself up so that he would run up the stairs and without consulting the cards he remembered the approximate place where the needed books were located and after a short search he would pick up the particular book needed. I do not know how often he could accomplish this feat. But undoubtedly he was very familiar with sections of Widener Library stacks which were of greatest interest to him. Moreover, I never saw anybody else have recourse to this method of getting at the needed books, and nor did I hear anyone speak of other persons using a similar procedure. I have the feeling that the method was perhaps unique with Sarton. And the privilege was undoubtedly very generous and invaluable for anyone who could put it into good use.

Speaking of Sarton, Lynn Thorndike says:

"Once he did think of starting an Institue for the History of Science, but I dissuaded him, pointing out that he was already turning out more for the history of science all by his lonesome in 185 Widener than he would be able to do, if he saddled himself with a directorship, a librarian, a secretary, an annual report, multifarious administration, and what not." [30]

There is a brief reference to such an institute in Conant's article referred to above. But it is difficult with just such limited information to venture any guess on the comparative weights instruction and research activities were to occupy in the institute Sarton had in mind.

Jonaton R. Cole and Harriet Zuckerman write:

"Unlike his own teacher, George Sarton, Merton had some success in recruiting students to the discipline [of the sociology of science]. In his concern to establish the history of science as a respectable scholarly enterprise, Sarton made demands on students so severe as to be self defeating. Not many learned the classical and oriental languages whose mastery, along with five or six major modern languages, Sarton deemed necessary. And still fewer obtained the equivalent of advanced degrees in both the physical and the biological sciences he also considered necessary for historians of science. He

[30] Lynn Thorndike, "Some Letters of George Sarton", *Isis*, vol. 48, 1957, p. 323.

also failed to develop a coherent formulation of principal problems in the field and a set of usable research techniques. Although Sarton developed a distinctive perspective on the history of science, it was not one that could be readily adopted by potential recruits. It is not surprising then that few historians of science count themselves among Sarton's students." [31]

Two of the earliest publications of Merton are closely related to the history of science. These are "Science, Technology and Society in Seventeenth Century England", published in *Osiris* (1938), and "The Course of Arabian Intellectual Development, 700–1300 A. D." (in collaboration with Sorokin), published in *Isis* (1935). He seems therefore to have come under Sarton's influence. The same is probably true of Henry Guerlac who was a Harvard junior fellow and who shifted from chemistry to the history of chemistry sometime about 1935. Doris Helman too came apparently under Sarton's influence. For she worked for her Master's Degree under him in Radcliffe. I myself was sent to America, in 1934, by the Turkish Ministry of Education to study the history of science specifically under Sarton. Henry Guerlac introduced the history of science as an independent academic discipline in Cornell, where F. K. Richtmyer, who was much interested in the history of his field, physics, was, I believe, dean. Here, Marie Boas Hall, a Radcliffe graduate, became one of the first graduate students in the history of science. Frederick G. Kilgour, a student of Sarton, and a classmate of mine in some of the history of science courses, contributed, from quite early years on, to the cultivation of the history of science at Yale, where John F. Fulton, professor of physiology and the history of medicine, who had become associated at some stage of his postgraduate work with Harvard and who was a staunch supporter of Sarton's aspirations, was anxious to promote work in the history of science. [32]

[31] Jonathan R. Cole and Harriet Zuckerman, "The Emergence of a Scientific Specialty", *The Idea of Social Structure, Papers in Honor of Robert K. Merton*, 1957, pp. 155-156.

[32] See, John F. Fulton, "On the Development of Science. VI. The Discovery of the Circulation", *The Yale Scientific Magazine Lectures, The Yale Scientific Magazine*, vol. 23, No. 6, March 1949; Chauncy D. Leake, "John Farquhar Fulton, 1899-1960", *Isis*, vol. 51, 1960, pp. 560-562.

I cannot be exhaustive in giving such examples. I am simply not equipped with the means for doing so. But Harvard itself was of course the outstanding and the most abvious example. Brilliant young representatives of the history of science such as Willy Hartner and Giorgio de Santillana joined the Harvard group of history of science instructors in and shortly after 1935, and they, in turn, formed new centers of work and instruction in the history of science.

President Conant of Harvard University spoke thus in February 1960:

"Henderson's great contribution to the history of science was in bringing George Sarton to Harvard. ... This is not the time or place for me to attempt even to summarize the history of Professor Sarton's long years at Harvard, his prodigious scholarship, his editorship of Isis and Osiris, his vain attempts during the depression years to persuade either 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 been long 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 academic scene." [33]

It seems to me that these words of Conant have much food for thought. Sarton's activity and efforts in the line of teaching and organizing instruction in the history of science, in general courses in the history of science in particular, in contrast to histories of special branches of science such as the history of mathematics or the history of chemistry, must have played a great part in establishing and spreading the history of science as an independent academic discipline. I believe, likewise, that Sarton's activity of carrying out simple teaching, year after year, and organizing such instruction of more or less elementary general as well as special undergraduate courses should receive much more emphasis than it has hitherto done, in contradistinction to the activity of organizing and guiding research for graduate students trained in fields other than the history of science,

[33] James B. Conant, "History in the Education of Scientists", *Harvard Library Bulletin*, vol. 14, number 3, 1960, p. 317.

whereas this latter aspect seems to have tended to draw more attention by the writers on the subject.

Concerning Millás Vallicrosa, Thomas F. Glick writes as follows:

"That Millás was able to launch the history of science in Spain, in addition to pursuing his Hebrew and Arabic studies and pedagogy, was in part a result of the example, stimulation, support, and encouragement that he received from George Sarton." [34] Joseph Needham too seems to have been influenced to some extent by Sarton and his Introduction to the History of Science. [35]

All in all, there seems to be little doubt that Sarton was eminently successful in exciting interest in the history of science and that he was clearly instrumental in the expansion of instruction and research in the new discipline which he had somehow, through thick and thin, managed to summon into existence. His personal participation in instruction at Harvard must be deemed significant too. It extended over many years, it was supplemented by similar work at Radcliffe, and it was commensurate to the conditions prevailing for the newly forming discipline. The history of science courses given by Sarton, Henderson, Hartner, Santillana, and Dana B. Durand were not underpopulated when I took them. Sarton's courses in 1937 and 1938 had, as I remember them, about fifty students each.

Aldo Mieli too brought out a first rate journal of the history of science, had pretty important publications, and organized the International Academy of the History of Science. [36] But he has never been deemed, so far as I know, to rival Sarton as a pioneer in establishing the new discipline. Neugebauer undoubtedly made great contribution to the spread and growth of the history of science. But he concentrated on the exact sciences with emphasis on Antiquity and the history of astronomy. He replaced Raymond Clare Archibald

[34] Thomas F. Glick, "José Maria Millás Vallicrosa (1897-1970) and the Founding of the History of Science in Spain", *Isis*, vol. 68, 1977, p. 277.

[35] Arnold Thackray and Robert K. Merton, *op. cit.*, p. 491.

[36] P. Sergescu, "Aldo Mieli (1879-1950)", Brochure No. 5 of l'Union Internationale d'Histoire des Sciences, 19 pages; Herbert Butterfield, "The History of Science and the Study of History", *Harvard Library Bulletin*, vol. 13, 1959, pp. 329-347.

at Brown University, as I recall from a talk by Archibald in Sarton's Seminar. Donald Fleming, who prepared his Ph. D. thesis under Sarton's distinguished student I. Bernard Cohen, was in Brown around and shortly after 1950. I believe he had been a student of Sarton as well. He was not working with Neugebauer's group, however, so far as I know. For Neugebauer's idea of the history of science, or the scope of his department or section at Brown, was of a somewhat restricted nature. This is reminiscent of research work referred to by Sarton in his footnote to the passage quoted above from his "Une Encyclopédie Léonardesque". We see Donald Fleming to have joined the Harvard staff some time later, as information given by Price for the academic year 1967-1968 indicates.[37]

The following words of Dorothy Stimson seem to summarize very well Sarton's position. She says:

"The encyclopedic range of his writings led the way to fresh and fertile fields for other scholars. His teaching trained younger people in his methods and his point of view. Most of all, his unremitting maintenance of the highest standards of scholarship, his whole-souled devotion to his self-imposed task, and his integrity are certain to keep his memory alive for years to come. It is largely owing to his efforts and influence that the spread of the history of science is steadily widening in this country."[38]

Sarton had a prodigious capacity for work, and he spread his ideas both by precept and example. Thanks to Isis, moreover, he was quite efficiently active in propagandizing for the new discipline. Arnold Thackray and Robert K. Merton have the following to say on this and other similar matters:

"... 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 history of a discipline. That work found its focus as well as its fullest expression in the monumental *Introduction to the History of Science*; we shall therefore pay particular attention to it. But, as will become apparent, the *Introduction* was only one of a

[37] Derek J. de Solla Price, "A Guide to Graduate Study and Research in the History of Science and Medicine", *Isis*, vol. 58, 1967, p. 389.

[38] Dorothy Stimson, "Dr. Sarton and the History of Science Society", *Isis*, vol. 48, 1957, p. 284.

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 orientation 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. . . ." [39]

There is a claim to the effect that Sarton was wont to indulge in thinking of general principles or matters pertaining to complex human affairs in terms of simple theorems or straightforward syllogisms and that he at times fell into contradictions or became involved in paradoxes. This reminds one of the question of the so-called many--valued logic, although the claim is not elaborated in any formal sense but rests solely on the method of exemplification. To me Sarton's falling into contradictions in dealing with clear and simple propositions is out of the question. It seems possible to me, however, that the observations made may more aptly be interpreted in a different manner, namely, to the effect that Sarton was not likely to fall into the fallacy of misplaced precision i.e., of trying to make unduly precise what is not easily possible to do so.

[39] Arnold Thackray and Robert K. Merton, "On Discipline Building : The Paradoxes of George Sarton", *Isis*, vol. 63, 1972, pp. 475-476. See also, A. Thackray and R. K. Merton, "Sarton", *Dictionary of Scientific Biography*, vol. 12, 1975, p. 109 and pp. 107-114.

I have quoted at the beginning of this article somewhat extensively from an early paper of Sarton. One reason for this was that toward the end of the passage quoted from that paper Sarton writes, "Léonardo n'est pas un accident isolé, un miracle, mais le fruit soudain et rare d'une longue évolution, jamais entièrement interrompue et qui, pour être en grande partie secrète, n'est pas moins réelle." This brings to mind Thomas S. Kuhn. I am not ready to go into the question at any length, but it seems to me that, although Sarton put much stress upon the historical continuity aspect of revolutionary changes, he would not feel that Kuhn's thesis would be irreconcilable with that of his own. For he would think that Kuhn's idea is reconcilable with the principle of historical continuity. And this he would think of explaining on the basis of minute details involved in each particular process, as he actually asserted at least in one other case and in some detail.[40]

Sarton was anxious to detect regularities and recurring patterns from among the facts made available through a detailed and objective study of the history of science. And although he never treated the subject systematically writing monographs devoted to such a kind of approach to the history of science but merely referred to considerations or observations of this nature in a casual manner in his writings, he may be said to have been, in a sense, more pretentious or at least more optimistic than Kuhn in this respect. This may be gathered, e.g., from Kuhn's following statement:

"A third factor in the formation of modern historiography of science has been a repeated insistence that the student of scientific development concern himself with positive knowledge as a whole and that general histories of science replace histories of special sciences. Traceable as a program to Bacon, and more particularly to Comte, that demand scarcely influenced scholarly performance before the beginning of this century, when it was forcefully reiterated by the universally venerated Paul Tannery and then put to practice in the monumental researches of George Sarton. Subsequent experience has suggested that the sciences are not, in fact, all of a piece and that even the superhuman erudition required for a general history

[40] *History of Science and the New Humanism*, 1931, pp. 36-37.

of science could scarcely tailor their joint evolution to a coherent narrative." [41]

Sarton attached quite an importance to the idea that the facts of the history of science are complex and he believed that this was due largely to the complexity and intricacy of the process of the growth of scientific knowledge itself. He dwelled at times on such examples as Auguste Comte's bold guess to the effect that as the celestial bodies could not be introduced into the laboratories their chemical compositions could never be determined and pointed out that this was belied through the birth of spectrum analysis only a few years after Comte's death. Again, he would frequently refer to the failure of great scientists to appreciate contributions closely related to their own epoch-making discoveries, such as Dalton's failure to appreciate the values of the giant contributions of Gay-Lussac and Avogadro to his own atomic theory. Sarton used to refer to this kind of occurrences as the great discoverers' being "blinded" by the magnitude of their own discoveries.

As I have said before, over and above certain pet ideas he had, Sarton's main concern was to establish the history of science as an independent academic discipline. Independent, especially in the sense that historians of science should have the chance and opportunity, through their special training, of forming and shaping their views concerning science and its place in human life and thought primarily on the basis of the facts to be gleaned from the history of science itself and should not therefore be overinclined to use the history of science for the support and defence of ideologies introduced and borrowed from fields outside of the history of science. For presumably this would make the history of science more useful as a contributing factor and constituent element of our sagacity in making value judgements in matters pertaining to intellectual culture and science itself. This is a very important concern, a cardinal matter for consideration. Yet Sarton thought of this scheme of training historians of science as one that should be predominent but not necessarily exclusive and one not stereotyped but preferably leaving room for variations and adaptations to special conditions and needs.

[41] Thomas S. Kuhn, *The Essential Tension*, 1977, s. 109.

Such lack of rigidness should, in my opinion, in no way be interpreted as indecision or vacillation, or as paradoxical. Sarton had very fine personality traits. He was extremely democratic and liberal, and, in my understanding, he was entirely free from superstitions such as racial or religious discriminations and other human weaknesses verging on bigotry and intolerance. He was also exemplary in his sincerity and earnestness.

After the start of World War II when it became certain that Isis could no more continue to be published in Belgium, Sarton got in touch with local printing presses in Boston. He was speaking with a representative of such a printer or publisher, and Dr. Alexander Pogo, who was in the contiguous room and could overhear the talks, was getting nervous fearing that the man was going to put over on Sarton certain unreasonable ideas and at times he made gestures of interfering with the conversation. Sarton, however, closed the door separating the suite of rooms and let the man speak to him in greater privacy. There was occasion to refer to Pogo's concern after the man had gone, and Sarton explained that although he appreciated Pogo's concern in the matter, he wished the man to be satisfied with the bargain and that, after all, as businessman, it was the man's duty to prove shrewder than Sarton as customer and to extract from him certain advantages in their deal and bargain.

As I have explained before, in his self-assigned calling as a pioneer for the promotion of the cultivation of the history of science and even in his main concern to establish the history of science as an independent academic discipline Sarton was not dogmatic or overenthusiastic although unswerving in his ideal mode of training historians of science. But all this was due to his broadmindedness and his unwillingness to unduly interfere in the affairs of others. And, moreover, in his ideal program or scheme for training historians of science he was realistic and reasonable; he was not trying to have his candidates for advanced degrees in the history of science accomplish the impossible as it is sometimes asserted, apparently with perfectly good intentions or simple credulity, even by otherwise well-informed circles.

As a discipline builder, Sarton may have had some exaggerated schemes in mind concerning the training of historians of science, before say 1936, or 1932, but in that case he must have toned

down his plans to somewhat more moderate dimensions when he officially began to put his ideas into practice. It is of course impossible to be specialized or well-competent in Chinese astronomy, Mesopotamian medicine, Greek mathematics, alchemy in medieval Islam, and nineteenth century physics, just as it is impossible for one and the same person to be a brain surgeon, a specialist on the diseases of the respiratory organs, and a pediatrician. This does not make it unreasonable though to think that the history of science should be an independent academic discipline and that science historians should be expected to have a rough acquaintance with the whole field of the history of science just as it actually is in the more or less parallel case of the field of medicine, or in mathematics, physics, psychology, literature, philosophy, or in any comprehensive field of study, for that matter.