

GEORGE SARTON AND THE HISTORY OF SCIENCE

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Professor George Sarton was a fervent advocate of the thesis of a quite wide scope that scientific activity constitutes a common bond for the whole of humanity as the most reliable type of knowledge created by the human brain; as the basic element responsible for the progressive nature of culture and civilization: and as an endeavor responsible for the amelioration of human life. He consequently was strongly convinced of the importance science must have had in the making of the destiny of mankind, and he was engaged in a concerted effort to persuade, the intellectual elite, of the great significance and relevance of the history of science in our effort to grasp the major factors responsible for the phenomenal growth of civilization and as a constituent element bringing to the fore the inexorable force of history. It was a great ideal for him, consequently, to establish the history of science securely in the universities as an independent academic discipline -with this difference, however, that even if his ideas advanced in behalf of the role of science should be reduced in certain respects, the plea for the importance of the history of science as an academic discipline would still retain its validity and cogency.

These ideas and thoughts are seen to be closely associated with the intellectual culture sector of general culture and civilization; i.e., with the sector of culture that is based upon, or closely tied up with, science, and, more generally, with relatively sound and reliable knowledge. Intellectual culture is of course a subject of major interest for our Atatürk Culture Center, and the history of science which has rapidly grown as an academic discipline in the universities of the Western World during the last half century is a very convenient avenue of approach for gaining acquaintance with science as a perennial human endeavor. It deals with science in the making, thus revealing its dynamic aspects in particular; and it constitutes a convenient way of avoiding the difficulty of gaining a sound scientific culture through arduous efforts involving the mastery of narrow fields of

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specialization. The history of science is therefore conducive in multifarious ways to developing and promoting a broad view and outlook of science, its methods, and the scientific attitude.

We should entertain no doubt indeed that as an academic discipline the history of science should be a very welcome new item superadded to the more classical university curriculum. This usefulness could be conceived from the vantage point of a substantial culture-building process and especially as an efficient way of securing a sound and critical assessment and evaluation of science as a human activity.

From this standpoint in particular the history of science is to be advocated as an invaluable contribution to our intellectual culture and enlightenment. Moreover, it is very important that the history of science should be conceived and instituted or organized as an independent academic discipline if it is to constitute a really new and significant contribution to our culture and enlightenment. Indeed, it should undoubtedly be very useful to form views concerning the nature of science with the help of judgements acquired with the help of data and impressions gained or gleaned from within the pale of the history of science itself, as it is in fact already being done to some extent nowadays. For thus the history of science will be able to add a new dimension to our way of looking into matters pertaining to epistemology and the philosophy of science, or other matters pertaining to questions related to science.

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

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.

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

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.

quelle 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 réalisée 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 récompense est grande. De même que Léonardo me permettra de démontrer d'une manière concrète l'unité 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 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

⁵ *Ibid.*, p. 236.

⁶ *Ibid.*, p. 238.

⁷ Dorothy Stimson, *Sarton on the History of Science*, Preface, p. IX.

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 in 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 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 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

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

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

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 unintentional 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

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.

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 irresistible 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

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 change 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.

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

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 competence.

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 maturer 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 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 tangl-

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.

¹⁵ *Op. cit.*, p. 431.

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

ed 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.

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, 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

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

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 Discovery of the Law of Free Fall", *Scientific American*, May 1973, pp. 85-92; 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. David K. Hill, "A Note on a Galilean Worksheet", *Isis*, vol. 70, 1979, pp. 269-270; David K. Hill, "Galileo's Work on 116 v; A New Analysis", *Isis*, vol. 77, 1986, pp. 283-291; Ronald H. Naylor, "Galileo's Method of Analysis and Synthesis", *Isis*, vol. 81, 1990, pp. 695-707.

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 Galileo 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

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.

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 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 given by, e.g., E. Gerland in 1913, than by the pretentious monographs of Koyré,²² written twenty six years latter.

Koyré Writes:

"Indeed, an experiment —as Galileo so beautifully has expressed it— being a question put before nature, it is perfectly clear that the activity which results in the asking of this question is a function of the elaboration of the language in which it is formulated. Experimentation is a teleological process of which the goal is determined by theory. The "activism" of modern science, so well noticed —*scientia activa, operativa*— and so deeply misinterpreted by Bacon is only the counterpart of its theoretic development.

"It is well known with what extreme ingenuity, being unable to perform direct measurements, Galileo substitutes for the free fall the motion on an inclined plane on one hand, and that of the pendulum on the other. It is only justice to recognize his immense merit and genial insight, which are not diminished by the fact that they are based on two wrong assumptions. But it is justice too to turn our attention to the amazing and pitiful poverty of the experimental means at his disposal.

"A bronze ball rolling in a "smooth and polished" wooden groove! A vessel of water with a small hole through which it runs out and which one collects in a small glass in order to weigh it afterwards and thus measure the times of descent (the Roman waterclock, that of Ctesebius, had been already a much better instrument): what an accumulation of sources of error and inexactitude!

"It is obvious that the Galilean experiments are completely worthless. The very perfection of their results is a rigorous proof of their incorrec-tion (sic)."²³

Sarton's convictions concerning scientific method were in tune with the more traditional and classical views. They may be described as in con-

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

²³ Alexandre Koyre, "An Experiment in Measurement", *Proceedings of the American Philosophical Society*, vol. 97, Number 2, 1953, pp. 222, 224.

formity with the beliefs and feelings concerning scientific spirit and procedures of research as practiced, or, at least, idealized by men of science themselves. He would therefore not be expected to adhere to such extreme ideas as being, e.g., against mathematics as a key to understanding nature, or looking askance at attaching paramount importance to experimentation or careful observation. For him these the pillars on which the glorious edifice of science and scientific thought had to rest.

Stillman Drake, writing in 1973 speaks of some previously unknown notes of Galileo and says: "This unpublished material includes at least one group of notes which cannot satisfactorily be accounted for except as representing a series of experiments designed to test a fundamental assumption, which led to a new, important discovery. In these documents empirical data are given numerically, comparisons are made with calculated values derived from theory, a source of discrepancy from still another expected result is noted, a new experiment is designed to eliminate this, and further empirical data are recorded. The last-named data, although proving to be beyond Galileo's powers of mathematical analysis at the time, when subjected to modern analysis turn out to be remarkably precise. If this does not represent the experimental prowess in its fully modern sense, it is hard to imagine what standards historians require to be met.

"The discovery of these notes confirms the opinions of earlier historians. They read only Galileo's published works, but did so without a preconceived notion of continuity in the history of ideas. The opinion of our more sophisticated colleagues has its role support in philosophical interpretations that fit with preconceived views of orderly long-term scientific developments. ..."²⁴

It is of course a widely known fact, on the other hand, that Galileo's trail-blazing and diligent work in the field of telescope astronomy constitutes undeniably clear evidence to the fact that he attached great importance to the empirical foundations of scientific knowledge.²⁵

²⁴ Stillman Drake, "Galileo's Experimental Confirmation of Horizontal Inertia: Unpublished Manuscripts," *Isis*, vol. 64, 1973, p. 292.

²⁵ A brand new item of evidence for this may be mentioned as Galileo's accurate observations of Neptune 234 years before it was identified as a planet. The following statement is made in this connection: "The reliability of Galileo's observations makes his sightings of Neptune much more than a historical curiosity. His observations call into question the accuracy of the modern calculated orbit of Neptune." See, Stillman Drake and Charles T. Kowal, "Galileo's Sighting of Neptune," *Scientific American*, December 1980, pp. 52-59.

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 the relevancy, to the issue in hand, of Galileo's Platonism would seem to be a 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.'"²⁶

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 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

²⁶ 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.

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".²⁷ 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 prespective 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.

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

²⁷ Charles Singer, *A Short History of Science to the Nineteenth Century*, Oxford 1941, p. 34.

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 concern 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.²⁸

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 professionally 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, in witnessing new journals and many books and articles in the history of science."²⁹

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

²⁸ See, *Isis*, vol. 33, 1941, pp. 654-656.

²⁹ I. Bernard Cohen, "George Sarton", *Isis*, vol. 48, 1957, p. 296.

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 judgment 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. ...

"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."³⁰

³⁰ James B. Conant, "George Sarton and Harvard University", *Isis*, vol. 48, 1957, pp. 302-303.

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. ..."³¹

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 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

³¹ *Ibid.*, p. 304.

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.³²

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 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 Institute 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.”³³

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 ven-

³² Arnold Thackray and Robert K. Merton, “On Discipline Building: The Paradoxes of George Sarton”, *Isis*, vol. 63, 1972, p. 480.

³³ Lynn Thorndike, “Some Letters of George Sarton”, *Isis*, vol. 48, 1957, p. 323.

ture 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 sociology or 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 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.”³⁴

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 came under the influence of George Sarton, as we shall presently see.

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. Marie Boas Hall says that Henry Guerlac was a biochemist, obtained his master’s degree in 1933, and was elected to a junior fellowship at Harvard, and that shortly after this he turned to history, in the study of which he was influenced by L.J. Henderson rather than directly by George Sarton.³⁵ 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.

³⁴ Jonathan R. Cole and Harriet Zuckerman, “The Emergence of a Scientific”, *The Idea of Social Structure, Papers in Honor of Robert K. Merton*, 1957, pp. 155-156.

³⁵ Marie Boas Hall, “Eloges, Henry Guerlac 10 June 1910-29 May 1985”, *Isis*, vol. 77, 1986, pp. 504-506.

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,³⁶ 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.³⁷

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 obvious 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."³⁸

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 instruc-

³⁶ See, F. K. Richtmyer, *In Introduction to Modern Physics*, McGraw Hill, 1934, pp. 1-80.

³⁷ 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.

³⁸ James B. Conant, "History in the Education of Scientists", *Harvard Library Bulletin*, vol. 14, number 3, 1960, p. 317.

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

Robert K. Merton speaks of how he met professor Sarton for the first time in a personal interview. It was in the fall of 1933 that he knocked on the door of Sarton's room in the Widener Library. He had audited a course of his somewhat irregularly and he had heard of his reputation as a remote and austere person, a person dedicated to his own scholarship and difficult to gain access to. Merton was then a third year graduate student. He writes:

“On that initial well-remembered occasion, the reputedly unapproachable scholar did not merely invite me into his “tiny book-lined study”; he positively ushered me in. Thus began my short, imcomplete, and sometimes unruly apprenticeship, followed by an intermittent epistolary friendship that continued until his death in 1956. I began that first audition by telling of my plans for a dissertation already begun. I can not say that he greeted those plans with conspicuous enthusiasm; instead he mildly suggested that so large a canvas as seventeenth-century English science might be a bit excessive for a novice. But he did not veto the idea. I should describe his response as, at best, ambivalent. Having registered his doubts, he then proceeded to tailor a research course to the needs of the first graduate student to have come to him from the social sciences since his arrival at Harvard some seventeen years before.

“I now suspect that the unheralded appearance of a young sociologist-in-the-making may have reactivated his own youthful ecumenical vision of transcending disciplinary boundaries. ... Since, not quite incidentally, he was also a Harvard lecturer, I was there to ask that this composite personage break through all bureaucratic barriers to establish a research course for a neophyte sociologist.

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

“There is yet another evident hypothesis: that in truth, George Sarton happened to treat me with friendly care, even with solicitude. This is somewhat more plausible. It has the further merit of being in accord not merely with possibly undependable memory traces but with personal documents. ... Nor is it surprising that I should have remained attached to him, early and late in our evolving relationship. For as I have discovered only now in reliving the history of that relationship for this centenary moment, he had bound me to him —not with any such intent, I believe— by a flow of gifts, freely bestowed, which in their cumulative outcome may have affected my life and work in ways that have little or nothing to do with substantive doctrine or method of inquiry but much to do with discovering the pleasures and joys, as well as the nuisances and pains, of life as a scholar. I now see that he provided an accumulation of advantage, thus leading me to incur a debt that called for a life of continuing work long after the insidious temptations of an easy retirement have been painlessly resisted.

“Only now, decades after the events, have I come to recognize the at-
tended flow of the gifts material and symbolic, which this ostensibly
peripheral mentor bestowed upon me, and should I be exaggerating their
import and consequences, as I may be doing in the first flush of their com-
posite discovery, they remain nevertheless as I describe them.”³⁹

Bernd Dibner writes: “There are three named rooms in the Burndy Library: the Leonardo Room, the Faraday Room and the George Sarton Room. They are intended to represent to visitors the library’s major areas of interest. The Sarton Room breathes the spirit of the old-timers who helped Uncle George in his mission to foster the history of science as an intellectual discipline. Photographs and other pictorial matter relating to Sarton appear on the walls, his publications fill the bookcases, and memorabilia are exhibited in a large display cabinet. The memorabilia include offprints from among the more than 300 papers —spanning the

³⁹ Robert K. Merton, “Recollections and Reflections. George Sarton: Episodic Recollections by an Unruly Apprentice,” *Isis*, vol. 76, 1985, pp. 470-474.

breadth of human knowledge— that he published after his association with the Carnegie Institution and while at Harvard university. The offprints on display bear his inscriptions to friends and correspondents.”⁴⁰

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.”⁴¹ Joseph Needham too seems to have been influenced to some extent by Sarton and his *Introduction to the History of Science*.⁴²

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.⁴³ 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 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

⁴⁰ Bern Dibner, “Sarton Letters at the Burndy Library,” *Isis*, vol. 75, 1984, p. 45—49.

⁴¹ 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.

⁴² Arnold Thackray and Robert K. Merton, *op. cit.*, p. 491.

⁴³ 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.

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.⁴⁴

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."⁴⁵

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 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 propagand-

⁴⁴ 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.

⁴⁵ Dorothy Stimson, "Dr. Sarton and the History of Science Society", *Isis*, vol. 48, 1957, p. 284.

dist. 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.

...⁴⁶

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.

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

⁴⁶ 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.

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.⁴⁷

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 of science could scarcely tailor their joint evolution to a coherent narrative."⁴⁸

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 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

⁴⁷ *History of Science and the New Humanism*, 1931, pp. 36-37.

⁴⁸ Thomas S. Kuhn, *The Essential Tension*, 1977, s. 109.

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.

Georges Gusdorf speaks of Sarton and the background of his ideas at some length. I shall quote from him here the relevant passage in full:

LE NOUVEL HUMANISME EN HISTOIRE DES SCIENCES: GEORGE SARTON

La disparition de Tannery n'a pas sonné la glas de l'histoire des sciences. Elle semble plutôt consacrer l'avènement d'une nouvelle situation épistémologique; désormais il existe une discipline consciente d'elle-même, équipée d'institutions permanentes, sociétés et congrès, qui regroupent des savants spécialisés et disposent de publications périodiques. Néanmoins cette forme d'histoire n'a pas réussi à trouver dans les structures diverses de l'enseignement officiel une place à la mesure de ses aspirations. C'est pourquoi les défenseurs de l'histoire des sciences ont été amenés à revendiquer en faveur de leur domaine propre, dont ils s'efforcent de montrer qu'il représente une position-clef de la culture, injustement méconnue. Cette polémique persistante alimente la réflexion sur la nature et la fonction de la discipline qui nous intéresse.

Le point de départ de la réclamation semble le fait que, la culture contemporaine étant foncièrement historique, l'histoire des sciences se trouve néanmoins réduite à la portion congrue. Une injustice persistante semble frapper un domaine qui mériterait un traitement privilégié. Car l'histoire des sciences ne présente pas seulement un intérêt rétrospectif; elle a une valeur de constante actualité, dans la mesure où elle figure une expression décisive du devenir de l'humanité. Alors que les phases et rythmes de développement de l'art, de la politique ou de la religion évoquent des vicissitudes sans conclusion, le devenir de la science paraît évidemment orienté dans le sens d'une amélioration constante, chaque génération maintenant les acquisitions des générations précédentes, qu'elle enrichit de ses propres découvertes.

Le théoricien belge Quételet, libre disciple d'Auguste Comte, écrivait en 1835: "Il me semble qu'il n'y a de véritablement progressif que la science, et je prends ce mot dans sa plus grande extension. Toutes les facultés de l'homme qui ne sont point fondées sur la science sont essentiellement stationnaires et leurs lois de développement sont constantes. ... Chacune

d'elles ne subit de variation que dans la grandeur de son maximum, qui dépend du développement qu'a pris la science. Le développement de la science donnerait donc la mesure du développement de l'humanité."⁴⁹ A la même époque, Whewell soulignait de son côté le caractère additif du progrès scientifique, les vérités anciennes n'étant pas démenties par les acquisitions nouvelles, mais intégrées et maintenues: "Les vérités acquises d'abord ne sont pas chassées, mais généralisées, et l'histoire de chaque science, qui peut apparaître comme une suite de révolutions, est en réalité une série de développements."⁵⁰

Ces affirmations ont été reprises et généralisées par George Sarton, qui fut en Belgique d'abord, puis aux Etats-Unis, le plus inlassable défenseur de l'histoire des sciences pendant la première moitié du vingtième siècle. Sarton a donné le nom de *Nouvel Humanisme* à une sagesse qu'il a tirée de la prépondérance reconnue à l'activité scientifique parmi tous les comportements humains.

Seuls en effet le savant et l'artiste apparaissent, dans la vie sociale, comme des créateurs de valeurs nouvelles. La production des biens économiques, la poursuite de projets politiques, matériels ou militaires ne sont pour l'humanité que des moyens et non des fins. Les guerres, les révolutions, les spéculations économiques ne constituent, comme les tremblements de terre, que des événements incontrôlables, sans portée rationnelle. Les savants, au contraire, apportent leur contribution désintéressée au développement de l'humanité dans son ensemble. "L'histoire générale est, en gros, une histoire des passions, passions de certains individus ou de groupes plus étendus;"⁵¹ au contraire, "le progrès humain est essentiellement fonction du développement de la connaissance positive;"⁵² "des hommes dépourvus de connaissance scientifique ne permettent absolument pas d'expliquer le progrès de l'humanité."⁵³

Dans cette perspective, l'histoire de la science apparaît comme le "squelette de l'histoire de la civilisation. ... Que notre intérêt soit premièrement philosophique ou sociologique, dès que nous comprenons que notre connaissance de la nature et de l'homme ne peut être complète ni suf-

⁴⁹ A. Quetelet, *Sur l'homme et le Développement de ses Facultés ou Essai de Physique Sociale*, Paris, 1835, t. II, p. 280-281.

⁵⁰ W. Whewell, *History of the Inductive Sciences*, London 1837, t. I, p. 10.

⁵¹ George Sarton, *The New Humanism*, extrait d'*Isis*, vol. VI, 1924; Bruxelles, Weissenbruch 1924, p. 32.

⁵² *Ibid.*, p. 9.

⁵³ p. 33.

fisante à moins de combiner l'information historique et l'information scientifique, l'histoire de la science devient pour ainsi dire la clef de voûte de toute la structure de l'éducation."⁵⁴ La primauté de l'histoire des sciences par rapport à l'histoire de l'art ou à l'histoire des religions, elles aussi créatrices, se justifie par le fait que seule l'activité scientifique est progressive et cumulative.⁵⁵ D'ailleurs la formation littéraire ou morale demeure insuffisante, dans la mesure où elle ne met en oeuvre que des demi-vérités des opinions toujours contestables. "C'est seulement au laboratoire —parce que la vie réelle est beaucoup trop complexe— que l'on peut apprendre la signification réelle de la vérité."⁵⁶

Le nouvel humanisme de Sarton apparaît ainsi comme une reprise du positivisme, mais teinté d'idéalisme anglo-saxon et de physicalisme à la manière de l'école de Vienne. La vérité scientifique serait le lieu privilégié pour le rassemblement de tous les esprits humains, en dehors de tous les antagonismes politiques, des conflits de race et de religion. "Si chacun, estime Sarton, plaçait l'amour de la vérité et de la justice au-dessus même de l'amour de sa famille et de son pays, il n'y aurait aucune raison de craindre la désintégration de l'humanité au cours d'une nouvelle guerre civile."⁵⁷ Cette affirmation peut paraître naïve; elle montre bien que le nouvel humanisme est une nouvelle version de ce qu'on appelait, au début du XX^e siècle, la religion de la science. Il existe en effet une corrélation entre l'unité de la Science et l'unité de l'humanité. "Le progrès de la science n'est pas dû aux efforts isolés d'un seul homme, mais aux efforts combinés de tous les hommes. ... L'unité du savoir et l'unité de l'humanité ne sont que deux aspects d'une seule grande vérité."⁵⁸ Une Providence immanente peut seule assurer la convergence, ou plutôt l'harmonie préétablie, entre les recherches et travaux de tous ceux qui, à travers le monde, se consacrent à l'avancement du règne de la vérité scientifique, prototype de toutes les autres vérités. L'oecuménisme incontestable et incontesté de la connaissance scientifique représente bien une réussite exceptionnelle, qui autorise Sarton à affirmer: "L'arbre de la science symbolise le génie et la gloire de l'humanité dans son ensemble."⁵⁹

⁵⁴ p. 28.

⁵⁵ p. 31.

⁵⁶ p. 21-22.

⁵⁷ p. 27.

⁵⁸ p. 11.

⁵⁹ Sarton, *The History of Science and the Problems of Today*, Elihu Root Lectures, Washington 1936, p. 17.

Cette catholicité exceptionnelle de la science a été soulignée par un autre théoricien de l'histoire des sciences, l'italien Enriques. "La possibilité de l'entendement réciproque, affirme-t-il, implique l'identité de la raison humaine. C'est sur cette supposition que se fonde toute science communicable."⁶⁰ L'évolution du savoir positif dans le passé et son perfectionnement dans le présent attestent qu'il s'agit là de "l'école la plus vaste où s'est formée l'humanité". La connaissance ici ne doit pas être passivement reçue; elle s'offre comme "une conquête que chacun doit faire ou refaire de son propre effort, et que nous pouvons tout au moins inciter quelqu'un à refaire en notre compagnie. Cela reçoit sa signification propre par la foi en l'unité de la raison humanine."⁶¹

Ainsi, dans cette première moitié du XX^e siècle, où l'Occident apparaît déchiré par ses contradictions, le nouvel humanisme croit trouver dans l'histoire des sciences une espérance de remplacement. Les valeurs religieuses, les valeurs morales, les valeurs esthétiques ou techniques n'ont pu fournir les éléments d'une entente entre les puissances déchirées par le conflit des nationalismes. Sarton, Enriques, comme, en France, Léon Brunschvicg, estiment que le travail scientifique, parce qu'il met en oeuvre les vertus d'objectivité et d'universalité, peut fournir les bases d'une entente nationale et internationale. Si la Société des Nations politiques n'a pu réduire à la raison les égoïsmes nationaux, on peut et on doit faire confiance à une Société des Nations scientifiques. Comme le dit Enriques, "ce n'est pas en s'isolant dans une attitude de stérilité intellectuelle, mais c'est au contraire en essayant de pénétrer la pensée d'autrui que chaque peuple acquiert la conscience de soi-même et c'est à mesure qu'il dilate son être et s'universalise, qu'il atteint véritablement la plus haute expression de son génie particulier."⁶² Alternativement maîtres et élèves les uns des autres, dans le passé et dans le présent, tous les peuples de la terre feront à l'école de la science l'apprentissage de la solidarité internationale.

Le nouvel humanisme du XX^e siècle prenait ainsi le relais du positivisme d'Auguste Comte. Si l'on avait dû renoncer au schéma simpliste de la loi des Trois États, on croyait avoir trouvé dans l'histoire des sciences une nouvelle Histoire Sainte, dont l'enseignement exemplaire s'imposerait aux intelligences et aux sensibilités. Or il en a été du nouvel humanisme comme du positivisme. Auguste Comte s'est trouvé obligé, pour assurer

⁶⁰ Federigo Enriques, *Signification de l'Histoire de la Pensée scientifique*, Hermann 1934, p. 13-14.

⁶¹ *Ibid.*, p. 15.

⁶² *Ibid.*, p. 66.

le triomphe du positivisme, de faire appel à une nouvelle religion dont il se croyait le Messie. De même, le néohumanisme, qui prétendait fonder toutes les valeurs sur la valeur de la science, a dû reconnaître, sous la contrainte de l'évidence, que la science n'était pas une valeur indépendante. L'histoire des sciences n'explique pas le passé de la culture; elle ne contient pas la clef de son avenir, parce qu'elle est un aspect de la culture, solidaire de tous les autres aspects.

ÉCHEC DE L'HUMANISME SCIENTIFIQUE

Sans mettre en cause la bonne volonté de Sarton, ni ses bonnes intentions, on doit relever dans la formule du nouvel humanisme l'influence d'une idéologie aussi inconsistante que généreuse. Le progrès de la connaissance scientifique est censé assurer à lui seul le progrès de l'humanité; l'unité de la science doit suffire à rassembler l'espèce humaine dans une commune espérance. Ce symbole de la nouvelle foi s'est perpétué de Comte à Sarton et Enriques, en passant par Renan, Taine et Berthelot. Malheureusement les affrontements tragiques du XX^e siècle suffisent à attester que la science, si elle contribue à faire le bon côté de l'histoire, à l'occasion, oeuvre aussi non moins généreusement pour le mauvais. La science ne détermine pas l'histoire; elle influe sur l'histoire par l'intermédiaire des autorités humaines, qui remettent en jeu ses significations. Au lieu de s'imposer comme une fin en soi, la connaissance scientifique est généralement utilisée comme un moyen.

On relève dans les exposés de Sarton un certain nombre d'incohérences qui, seules, lui permettent de faire prévaloir ses thèses. Il affirme par exemple que le but véritable de l'humanité est la création de nouvelles valeurs intellectuelles, "le dévoilement graduel et le déploiement de l'harmonie de la nature, le développement et l'organisation de ce que nous appelons art et science. ... Les artistes et les savants sont, à mes yeux, les véritables createurs, les gardiens des idéaux humains; ce sont eux qui accomplissent les destinées de notre race et justifient son existence."⁶³ L'artiste et le savant se trouvent donc d'abord à égalité; puis le savant prend l'avantage sur l'artiste, à cause du caractère progressif de son savoir, de telle sorte que la science seule devient, comme nous l'avons vu, la clef de voûte de la culture. "La science constitue à proprement parler l'axe du progrès humain; elle fournit les principes mêmes et les méthodes de l'organisation sociale"; elle nous conduit lentement mais sûrement à la "fraternité universelle."⁶⁴

⁶³ Sarton, *The New Humanism*, *op. cit.*, p. 13.

⁶⁴ *Ibid.*, p. 24.

Mais ces affirmations enthousiastes s'accompagnent, chez Sarton, de certaines réserves. Il note, par exemple: "La science seule ne peut pas donner un sens à notre vie. La science, réduite à elle-même, n'est pas la culture, bien qu'elle en soit une part essentielle."⁶⁵ Ces affirmations, quelque peu postérieures à l'essai sur le *Nouvel Humanisme*, reflètent, sinon un certain désenchantement, du moins un retour au bon sens. L'avancement de la science, au lieu de contribuer "d'une manière presque automatique"⁶⁶ au progrès de l'humanité, devient un facteur indépendant dont la valeur n'est nullement incontestable. "Aussi longtemps que la science est considérée uniquement d'un point de vue technique et utilitaire, il n'y a guère en elle de valeur culturelle."⁶⁷ Il y a donc un bon et un mauvais usage de la science: "La science doit être humanisée, affirme maintenant Sarton; ce qui signifie entre autres qu'on ne doit pas lui permettre de se déchaîner aveuglément. Elle doit constituer une partie intégrante de notre culture, une partie qui doit demeurer subordonnée à l'ensemble."⁶⁸

Autrement dit, il apparaît que la science peut devenir folle. On ne peut donc plus lui vouer une confiance aveugle, et, dans la mesure même où elle doit être soumise à un contrôle qui la situe à sa place dans la totalité dont elle dépend, il est clair qu'elle ne saurait constituer le nouveau Verbe incarné qui assurerait providentiellement l'unité et l'harmonie de l'humanité. Puisque la science doit être réduite à la raison et à la sagesse, la science n'est plus le fondement de toute raison et de toute sagesse. Finalement, Sarton en vient à distinguer deux niveaux, ou deux usages, de la connaissance scientifique, en laquelle on peut rechercher soit une valeur utilitaire et technique, soit une signification supérieure, de nature contemplative. "Le but principal de la recherche scientifique, écrit-il, n'est pas d'aider l'humanité dans le domaine de la science ordinaire (*ordinary science*), mais de rendre plus aisée et plus complète la contemplation de la Vérité. Ceci implique une profonde conversion de l'esprit, qui ne peut être acquise qu'au prix d'une discipline longue et rigoureuse."⁶⁹

Ainsi la science et l'histoire des sciences, qui paraissaient d'abord, en elles-mêmes, messagères d'espoir, ne peuvent conserver cette signification que moyennant une ascèse préalable à une conversion de l'intelligence. Sarton, qui se présentait comme l'héritier du positivisme, en vient à pro-

⁶⁵ *The History of Science and the Problems of Today*, Washington 1936, p. 19.

⁶⁶ *The New Humanism*, p. 24.

⁶⁷ *The History of Science and the Problems of Today*, p. 24.

⁶⁸ *Ibid.*

⁶⁹ *Ibid.*, p. 27.

noncer, selon sa propre expression, “un plaidoyer pour un idéalisme sans compromis, dont notre époque, plus que toute autre, a profondément besoin.”⁷⁰ L’humanisme scientifique, le nouvel humanisme peut fournir les éléments de cet idéalisme “ou du moins certains d’entre eux.”⁷¹ On ne saurait mieux dire que, loin de proposer un humanisme préfabriqué, l’activité scientifique appelle, pour sa propre régulation, un humanisme préalable, qui aura dû être acquis par d’autres moyens. La science ne saurait être invoquée comme une fin en soi; l’humanité y trouve tout au plus des “éléments” pour la compréhension de son destin; ces éléments, ces moyens, il lui appartient de les utiliser pour le pire ou pour le meilleur.

Il est clair à cet égard que l’expérience du XXe siècle est une expérience d’échec. L’inflation scientifique a fourni aux politiciens des moyens de puissance sans cesse accrus, et des armes de destruction massive, dont on sait qu’elles remettent en cause l’existence même de la planète Terre. L’espérance du messianisme scientifique s’est heurtée au démenti tragique de l’histoire. Il n’y a pas d’humanisme scientifique, pour autant du moins que l’on entend par là un accomplissement harmonieux de l’être humain. La science ne représente qu’un aspect, qu’une possibilité parmi les autres; si on la considère comme une fin en soi, elle a tendance à prendre en quelque sorte la tangente par rapport à la réalité humaine. Le déséquilibre intellectuel ainsi créé s’accroît encore par l’effet des applications techniques de la connaissance scientifique, qui détruisent les équilibres traditionnels de la vie, sans fournir pour autant un équilibre de remplacement. Lorsque Sartre lui-même affirme que “la science doit être humanisée”, il reconnaît que la question fondamentale des valeurs humaines se pose en dehors de la science, et doit être résolue indépendamment de sa compétence.

Quelques formules de Jaspers prennent ici tout leur sens: “La connaissance scientifique des choses n’est pas la connaissance de l’être. Car toute science est particulière; elle concerne des objets et des aspects déterminés; elle ne concerne pas l’être lui-même. La connaissance scientifique ne peut pas fournir des buts pour la vie. ... La science ne peut pas donner de réponse à la question de son propre sens. L’exigence de science repose sur une volonté originaire de connaissance, dont la justification ne peut être donnée scientifiquement.”⁷²

⁷⁰ p. 29.

⁷¹ *Ibid.*

⁷² Karl Jaspers, *Wahrheit und Wissenschaft*, Discours pour le cinquième centenaire de l’Université de Bâle, Basel, Helbing und Lichtenhahn 1960, p. 12.

Ces propos de l'un des maîtres de la pensée existentielle caractérisent la situation de notre époque où la science a perdu beaucoup de ses prétentions de naguère, et ceci aux yeux des savants eux-mêmes. "La tentation des scientifiques d'aujourd'hui, observe un contemporain, n'est plus le scientisme; ce n'est plus cette espèce de dogmatisme simpliste du XIX^e siècle, dans lequel les gens pensaient que la science finirait par résoudre toutes les énigmes de la destinée humaine. Aujourd'hui, les savants, au contraire, sont devenus plus sceptiques, c'est-à-dire qu'ils sont de plus en plus conscients qu'après tout ils ne donnent jamais que des hypothèses qui, à un moment donné, représentent la meilleure interprétation de l'ensemble des faits connus, mais qui sont toujours provisoires et doivent être ainsi perpétuellement révisées. Par suite ils auront tendance à considérer qu'en réalité il ne peut jamais y avoir de vérités. ... et par conséquent à mettre en question la possibilité même d'une vérité absolue."⁷³

La mentalité scientifique de notre époque serait donc caractérisée par la transformation du dogmatisme de naguère en une sorte de relativité généralisée. Le temps de la suffisance est bien passé. L'activité scientifique conserve sans doute aujourd'hui un droit de priorité budgétaire, pour des motifs techniques, économiques et militaires. Mais les savants ont perdu leur bonne conscience, et les meilleurs d'entre eux s'interrogent. Ils savent que l'humanité ne deviendra pas sage et heureuse, par le seul bienfait de la science et en dépit d'elle-même. Autrement dit, la science, qui s'était crue maîtresse d'humanité, fait retour à l'humanité. Elle reprend sa place dans la mutualité des significations humaines qui composent le devenir de la civilisation.

Cette situation nouvelle rompt avec la tradition deux fois séculaire, en Occident, de l'Aufklärung et du positivisme. L'histoire des sciences, dont la découverte et la mise en honneur, de Fontenelle à George Sarton, était liée à cette tradition, ne disparaît pas pour autant. Mais elle va changer de signification et presque de nature, dans la mesure où elle cesse de s'inscrire dans la perspective d'une apologétique de la raison militante et triomphante. L'arrière-plan de philosophie de l'histoire et d'idéologie ayant disparu, l'histoire des sciences doit être désormais considérée comme un aspect du devenir de la culture, solidaire de tous les autres aspects. Elle ne peut plus prétendre faire cavalier seul, ni entraîner par son seul dynamisme tout le cours de la civilisation. Le positivisme n'était encore

⁷³ Jean Daniélou, *la Crise du sens de la Vérité*, dans: *l'Enseignement de la Philosophie*, Recherches et Débats du C. C. I. F., Fayard 1961, p. 149.

qu'une métaphysique. Une fois levée cette hypothèque, l'histoire des sciences peut enfin poser ses propres questions, et tenter d'y répondre, d'une manière vraiment positive.⁷⁴

I believe that Sarton himself would have been well satisfied with this more modest program of activity for the history of science and the tendency foreseeing a less pretentious role for science as a means of approaching human aims and ideals as roughly delineated by Gusdorf. He always felt keen for the possible abuses of the power gained as a result of becoming equipped with knowledge capable of being put into practical use. Science should therefore be checked and controlled in its utilizations for human needs. Scientific and technological power and skill should always be coordinated and supplemented with virtue, and standards of right and moral excellence.

At the very beginning of his *The History of Science and the New Humanism*, a book quoted in special by Gusdorf, as we have just seen, and quite representative of the enthusiasm he exhibited for the cause of science, Sarton speaks in the following words: "... in our days an educated man can no longer behave as if the gigantic efforts of scientists did not concern him—as if they belonged so—to—say to another world; he must recognize the scientific spirit as being at least on the same level as the religious spirit, the artistic spirit, the spirit of justice, one of the four glories of humanity."⁷⁵

The following passage from the same book may also be considered quite relevant to the same question:

"Before considering the very complex case of mankind as a whole, suppose we had to tell the history of a single man. How would we set about it? The main point of the story, I take it, would be to explain the development of his genius, the gradual accomplishment of his special mission. If he became a great mathematician, we would try to show how and when his mathematical bent revealed itself, how a growing boy devoted more and more attention to mathematics, how other interests were by degrees sacrificed to this dominating one. A boy who toys with mathematical ideas, what fun; but little by little they engross the whole of his mind until finally we have the awful feeling that there is no choice or freedom left. No more playing with mathematics, but rather mathematics playing with a human mind and using it to the limit. That is how genius looks when we

⁷⁴ Georges Gusdorf, *Les Sciences Humaines et la Pensée Occidentale I, De l'Histoire des Sciences à l'Histoire de la Pensée*, Payot, Paris 1966, pp. 118-125.

⁷⁵ See, *op. cit.*, p. 10.

come nearer to it. Nothing very comfortable or pleasant, but rather a fearsome mystery. Our story should be focused upon that very mystery. Its value will depend upon our ability to evoke the genius — everything else however much there may be of it being subordinated to this — to evoke its growth, its struggles, its fulfillment, its influence; it will depend also upon our success in making other people realize the mystery involved. It is clear that all else is relatively indifferent, in as much as we are interested in this man because of his mathematical genius. To be sure our curiosity is not restricted to the mathematical side of him — if we are sufficiently interested in his genius our curiosity is properly insatiable— but that side is the essential, every other, auxiliary. A biography which would be focused, let us say, on the account of his diseases, or of his loves or hatreds, might be entertaining, it might obtain the favor of superficial readers, but it would be false.

The case of mankind is not essentially different from that of a single man, though it is infinitely more complex. To begin with, the main direction is not so easy to discover, for there are many. Which is the purpose of mankind? Is such a question too ambitious? Is it at all possible to answer it? I believe it is. Without venturing into metaphysics, we may safely assume that the main purpose of any creature is indicated by its specific function. What can man do which other animals cannot? His purely physiological functions he shares with many of them; it cannot be that he lives only to live and reproduce his kind. Indeed if we look back we see that the men who came before us have not simply perpetuated their own flesh, but produced a quantity of things, material and immaterial, which constitute the best part of our inheritance. The totality of these things we call civilization. They include such material objects as buildings, statues, paintings, furniture, instruments and tools of every description, and such immaterial things as artistic and scientific methods, ideals, hopes, fears and prejudices. They represent the creative activity of man, his net creations above and beyond those which had no aim but to make his net creations above and beyond those which had no aim but to make his life possible, or to lighten it, make it more agreeable, and insure its prosperity and continuation. Is it not as daylight that if we want to write the history of man it is this creative activity, specific to him, which must provide us with our Leitmotiv? Everything which pertains to that activity must be in the foreground of our picture; everything else, however interesting, in the background.

To put it briefly we might say that, as far as we can discern, the main purpose of man is to create such intangible values as beauty, justice,

truth. I trust that the reader will not require any definition of these terms; that he can distinguish order from chaos, beauty from ugliness, justice from injustice, truth from untruth. It is not necessary that he be able to distinguish them in each and every case; there will always be enough ambiguous cases to rejoice the heart of casuists, but we shall not allow the latter to sidetrack us. It will suffice to recognize that there have been in all times at least some men who were obsessed by the idea of creating beautiful things, of improving social conditions, of discovering and publishing the truth. The fact that they were not free from illusions, that their experiments were not always successful, that even the best of them made mistakes, does not affect the general statement. Considered as a body these men were those who fulfilled the distinctive mission of mankind, and to them we owe most of the privileges and of the pleasures of our lives, the nobility of our minds, the grace of our hearts."⁷⁶

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 predominant but not necessarily exclusive and one not stereotyped but preferably leaving room for variations and adaptations to special conditions and needs.

Such lack of rigidity 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.

⁷⁶ George Sarton, *The History of Science and the New Humanism*, 1931, pp. 21-24.

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.

In concluding I find it convenient to refer to Professor Sami Hamarneh and quote a few passages from his paper on Sarton, "Sarton and the Arabic-Islamic Legacy". He says;

"The Two ventures that meant so much to Sarton and were a great source of satisfaction to him in their realization and execution were the publication and enthusiastic reception of *Isis* and the *Introduction*. To them he devoted the best part of his life's energies, and because of them he is best remembered. From the beginning, Sarton planned that the two publications would "go forward hand-in-hand." It was intended that *Isis* contain certain articles dealing with the general historical aspects of science and culture, the findings of research, news items, queries and answers, book reviews and systematic critical bibliographies. The latter added new spirit, dimension and organization to this entirely new academic discipline which he worked so hard to establish, and of which he became to outstanding pioneer. So it was, that before his passing from the scene, the subject of the history of science had become firmly established as a permanent feature of the academic landscape, not only in the New World but in many countries of the Old as well."⁷⁷

"Another dream of Sarton's was fulfilled in January 1924 when the 'History of Science Society' in the U.S. was incorporated. Two years later, *Isis* became its official organ. Although from its incorporation the Society supported *Isis*, the fact remains that for the best part of fourty years, Sarton continued to pay a good portion of its operational and publication costs out of his own pocket, In 1952, after his retirement from Harvard, he relinquished this responsibility, and the editorship of *Isis* passed to other hands. But it never again reflected the same spirit it had once enjoyed under Sarton's fatherly devotion.

"It should be explained here that the completion of the exhaustive five-volume *Introduction* constituted only the first part of Sarton's larger and more ambitious project of a history of science to the end of the nineteenth century. But the data and preparations needed for continuaiton were so tremendous that he had to stop at the fifteenth century - they could

⁷⁷ Sami Hamarneh, "Sarton and the Arabic-Islamic Legacy", *Journal for the History Arabic Science*, vol. 2, 1978, p. 302.

not have been completed in one person's lifetime at the same level of scholarship and perfection. The project as envisaged would have been impossible as the sole effort of one person. Admittedly, it would have required a team or even generations of scholars with varied talents and academic qualifications. Sarton himself wrote: "It is already clear that I shall not be able to carry my investigation down to the twentieth century." It is hard to explain the scope of his scholarly research. Consideration of their apparatus as of January 1931, for example, will be illuminating. He had consulted some 3100 books; 4000 booklets, monographs and reprints, and about 41000 bibliography cards. By 1947 'the arsenal' had grown into 3400 books, 13500 pamphlets, and 80 000 cards and other documents. Add to these the availability of the Harvard libraries. As it was, Sarton accomplished an enormous intellectual feat with disciplined erudition — a task to which he devoted the best years of his life. His hard 'labor of love,' vigorously promoted and increased interest in areas that had been disastrously neglected. And for the periods he covered, this was the first survey of human civilization to be published as completely and accurately as humanly possible."⁷⁸

Again, Sami Hamarneh writes: "Volume one of the *Introduction* (1927) took nine years of preparation and covered a two millenia period, 'a kind of wager, the very idea of it,' Sarton wrote, 'causes me to shudder.' By September 1930, Sarton had completed the final draft for the second volume (in two parts). Publication was completed by July 1931, after thirteen years of preparations while volume three (also in two parts) took twenty seven for completion. In them he used both analytical and synthetic investigation. His intention was to enable scholars to know as exactly as possible the state of knowledge at the time for each topic. The work contained the first tolerably complete account of medieval science and technology, integrating eastern and western cumulative knowledge into a single synthesis.

"By the end of 1947, 103 numbers of *Isis* (in 35 volumes) had already appeared plus 67 critical bibliographies, and seven volumes of *Osiris*. With irony Sarton explained, 'If I were to attempt volume four this would take ten to fifteen years (or more). This would be tempting Providence.' Indeed he died in less than nine years from the time of his writing that statement. He therefore preferred to devote 'the rest of his life to shorter (and smaller) undertakings.' He thought of smaller books carrying his investiga-

⁷⁸ *Ibid.*, p. 304.

tions of the late medieval period into the Renaissance and the early modern periods. But even here, and at his advanced age he reiterated, 'I was determined to examine everything with my own eyes,' to secure accuracy and veracity."⁷⁹

It would have been a great blessing for the historians of science and the students of intellectual history, had Sarton been able to bring his *Introduction* down to the end of the fifteenth century. For the sixteenth century has been conceived as an integral part of modern times and as a period of dissolution of continuity with the Middle Ages. As a consequence of this tradition both the teacher and the researcher will find reference works without much difficulty for these later centuries, while for the fifteenth century the situation is quite different. For that century it is not easy either to gain all-round pictures for that era as a whole or for major aspects of it, or, again, to place its specific problems into sufficiently enlightening backgrounds. A special volume, or a pair of tomes as for the fourteenth century, on the fifteenth century as a part of the *Introduction* would have therefore brought this work of Sarton to a much better stopping point, as a reliable guide for students of intellectual history and science historians.

Sarton's tremendous coverage and his extraordinarily wide range of interest transcended of course both the medieval era and the World of Islam to both of which his *Introduction* shed much light. For both of these needed a comprehensive synthesis even if of an encyclopedic and eclectic nature. But it may be said that Sarton's *Introduction* may be characterized as more complete as far as its treatment of the World of Islam is concerned. For it served to bring the Islamic world more clearly into the spotlight as a major phase and stage of the world's intellectual history. And it also helped interested scholars by providing them with a detailed general and reliable guide for the period in which, at least relatively speaking, it deserved such a presentation most urgently. Moreover, there is the all-important question of precursors upon whose works Sarton could build up both as far as medieval Islam and the European late Middle Ages are concerned. But these are big questions which can be taken up in an article as the present one only in a superficial manner.

Professor Sami Hamarneh has the following to say concerning this aspect of Sarton's greatness of achievement with respect to his treatment of medieval Islam. He says:

"For almost a century before Sarton completed his five-volume *Introduction* several Orientalists and Arabists had been producing monumen-

⁷⁹ *Ibid.*, p. 305.

tal works on the Islamic-Arabic legacy. To name a few, we mention Wüstenfeld, Choulant, Ahlwardt, Mueller, Houstma, Fluegel, Suter, Brockelmann, Pertsch, and Meyerhof. But Sarton's contribution regarding the place and relevance of this civilization, its history of science and technology and its universal impact remains unique. He became a worthy successor to these pioneers and scholars. He was the first and most dynamic among them to give a prominent place to Arabic-Islamic science and technology as he did in *Isis*, the *Introduction*, and other publications for over four decades of prolific life. These contributions go beyond mere transmission of an ancient and classical legacy leading to new significant observations, conclusions and ideas."⁸⁰

⁸⁰ *Ibid.*, p. 309.

