SARTONIANA

Volume 5

1992

Sarton Chair of the History of Sciences
University of Ghent, Belgium

ISBN 90-70963-37-X
D/1994/2249/10

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GEORGE SARTON CHAIR

of the

HISTORY OF SCIENCES

1991-1992

Sarton Chair Lectures

INTRODUCTION

Michel Thiery, MD, PhD, FRCOG

As president of the Sarton Committee it is my privilege and pleasure to confirm today the laureate of the George Sarton Memorial Chair in the History of Sciences. The nominee for the academic year 1991-1992 is Professor Tore Frängsmyr of the University of Uppsala, Sweden. Tore Frängsmyr is Professor in the History of Science, the multidiscipline "created" by Sarton in the Twenties and consolidated both through his journal *Isis* and his magnum opus *Introduction to the History of Science*. Sarton himself defined the purpose of the history of science as "a means to explain the development of one essential phase of human civilization which has not yet received sufficient attention, the development of science, that is of systematized positive knowledge". According to him, the true meaning of social history lies in its attempt "to explain the progress of scientific thought, the gradual development of human consciousness, that deliberate tendency to understand and to increase our part in the cosmic evolution". Now, Ladies and Gentlemen, those of you familiar with the work of Frängsmyr, can testify how close his endeavors approach the goals set by Sarton. As loyal follower of the Master, our laureate published commentaries on his mentor's positivistic tradition in the history of science, a contribution for which he was granted an advisory editorship of *Isis* several years ago.

But also on account of its geographic tangent does the research work of Frängsmyr strike a sensible cord in the Belgian heart. Indeed, at least two among his favored subjects are related with scientists whose discoveries were made in the Low Countries.

The first of them is Niels Stensen, better known as Steno (1638-1686), the famous Danish physician and geologist, friendly with both Reinier de Graaf (1641-1673) and Jan Swammerdam (1637-1680), who was the first to describe the ductus parotideus seu stenonianus while residing at Amsterdam.

The second is Carl von Linné (1707-1778), who, as Frängsmyr, was professor at the University of Uppsala. Exiled to the Netherlands by his father-in-law to-be and having obtained his medical degree at Harderwijk in 1735, Linnaeus took time off to have the essentials of his botanical discoveries (*Systema naturae*, Leyden 1735 and *Genera plantarum*, Leyden 1737) published in the country where the aging Herman Boerhaave (1668-1738) attempted in vain to retain him.

As a physician I feel a particular affinity with our botanical genius because Linnaeus was also a splendid clinician and a medical pioneer. Initially appointed as professor of theoretical and practical medicine, Linné obtained already in 1742 the subjects coveted by him, i.a., materia medica, semeiotics, dietetics, hygiene, and ... sexuology, which he went on teaching for a lifetime. At the end of his medical carreer he drew up a system for the classification of diseases. His book published under the title *Genera morborum in auditorum usum* (Uppsala 1763), was a kind of "flora" by which physicians might be able to "determine" the diseases of which their patients were suffering. It goes without saying that, due to the poor insight in the etio-pathology of diseases in Linné's time, his "systema morborum" was to be a premature venture. But, as the laureate of today selected Linnaeus as the hero of his inaugural lecture, I shall not dwell any longer on this subject.

Dear Professor Frängsmyr. In 1986 several faculties of this University established a Chair devoted to the History of Science, which was named after their famous alumnus, the late Professor George Sarton, and it is, according to Robert K. Merton, the first Chair ever established in Sarton's honor. Each academic year a scholar known for his or her outstanding contribution to the history of science is named to hold the Sarton Memorial Chair and give several lectures in the field of his or her field of interest. This year, the members of the ad hoc Committee have elected you as laureate. Please, do accept our congratulations and our grateful thanks for having agreed to hold the present Sarton Memorial Chair.

LAUDATIO TORE FRÄNGSMYR

F. Lox

When an institute reaches its 25th anniversary, thoughts naturally turn toward writing its history, reviewing its realizations, and looking into its future. This very year the Museum for the History of Sciences and Technology of the University of Ghent, Belgium, celebrates its quarter of a century of existence. For us, history of sciences does not mean the description of a buried past but an active search for and a demonstration of the pathways along which the various sciences have evolved. By following the thread of Ariadne, mankind has attempted to understand the facts leading to realizations and discoveries and to create an environment for progress and for grasping the universe. Knowledge must be passed to the next generations. This is a basic concept of humanity. And in this context, Professor Frängsmyr, we situate your activities as a scientist and an educator. This Sarton Memorial Award may be considered a recognition of your intensive and successful endeavours in the field of the history of sciences. Within a relatively short period of time you have succeeded in publishing twenty-two books and chapters in books as well as a large number of scientific articles. All of us recognize that George Sarton is one the most referred authors in the province of the history of science and obviously you are following in his wake. We feel honored that you readily accepted to travel to the city where the "master" was born and to the University that promoted him, to accept the Chair which was created to perpetuate his memory. But today it is your contribution to the history of science we wish to honor in the first place.

Tore Frängsmyr was born in 1938 at Skelleftea, in the very north of Sweden, near the arctic circle. He started his study of philosophy at the University of Uppsala in 1958, at the age of twenty, and obtained his PhD in the History of Science at the Faculty of Humanities of that University in 1969. He initiated his scientific career as Assistant

Professor of the History of Science and was appointed Professor at the newly established University of Linköping in 1981 where he joined the program for the investigation of technology and social change. In 1982 he obtained a personal Chair as Research Professor in History of Science at the University of Uppsala. It was the start of a particularly productive period, stimulated by the recognition of his work by the University and in his country at large. Indeed, Professor Frängsmyr was elected Fellow of the Royal Swedish Academy of Sciences. In Stockholm he founded a new research institute called "The Centre for History of Science" of which he is the director. This center was gifted with archives and collections of manuscripts and instruments, among them the Nobel archives. Since 1988 Professor Frängsmyr also acts as editor of "Les Prix Nobel", the Yearbook of the Nobel Foundation.

The publications of Professor Frängsmyr concern a broad range of philosophical and physical subjects, among them the history of geology. His PhD thesis, for example, was devoted to the problem of "Geology and the doctrine of Creation during the 18th century". In that century — which was made famous by composers such as Bach, Haendel, Mozart and Beethoven — Sweden produced high ranking scientists, e.g. Carolus Linnaeus (1707-1778), Emmanuel Swedenborg and Anders Celsius. All of them had to compromise between the biblic and the new scientific explanations of the creation of the world. In the 18th century we recognise the remoulding of traditional psychological, economic and sociological views, enhanced by the increasingly pervasive impact of sciences and technology. In the 18th century you found inspiration to write about the German philosopher Christian von Wolff (1679-1754) who was influential in Sweden between 1720 and 1760. The initially rationalistic von Wolff who later on changed his attitude to an apologetic one for the orthodox Church was used as a weapon against the Enlightenment. These historical facts stimulated you to work on the Swedish Enlightenment as well and we hope to read you soon and to learn your views about the relations between philosophy, religion and science. Last but not least, Professor Frängsmyr studied and published about George Sarton and the beginnings of the "History of Science". It has been stated that Sweden became one of the pioneering countries of this then novel discipline. And indeed, already in 1932 one of your predecessors, Professor Johan Nordström, inspired by Sarton, established the Chair of "History of Science" at the University of Uppsala.

It seems to me that this discipline is acting as a virus : the more you become involved in it, the more you are broadening your activities. So also Professor Frängsmyr, who since 1989 has spread his activities worldwide. He became Secretary-General of the International Union of the History of Philosophy and Science and was appointed Director of the International Summerschool for the History of Sciences, the body which organizes lectures at the Universities of Bologna, Uppsala, and Berkely.

Dear Professor Frängsmyr, I also wish to mention that your flight to the roots of George Sarton detoured. As a matter of fact, the first steps towards todays's ceremony were made some years ago — I guess it was in March 1989 — when we met in Strasbourg, France, at the Round Table Conference implemented by the European Foundation of Scientific Research. All national delegates were involved to some extend with the history of sciences and I vividly remember that when each of us was invited to introduce himself, you referred to George Sarton to situate your work in the field of history of science. It was proud that Sarton, who was born in one of the smallest countries of all those represented at the meeting, was the only person mentioned by name and appeared to be recognized by all participants as the key figure of the discipline for the discussion of which we had been convened to Strasbourg. Now that you are in Ghent, personifying Sarton's ideals and ideas and at the same time closing the pathway through which Sarton's example has circled from this his native city to the United States, to your country and back to Ghent, we look forward at a still more intensive and close cooperation between our respective Institutes and Universities. We are convinced that your presentation will further stress the importance of the history of sciences for generations to come. This is the second reason why we are keen to have you amidst us today. And the final reason why I am happy that you came is my belief that your presence shall consolidate the link between the Ghent Museum of Science and Technology and your own Institution.

In 1988 Sarton-Medal holder Otto Gekeler presented Johann Beckmann, the famous 18th-century scientist of Göttingen, who coined the word "technology" and, above all, was the founder of the new discipline called "Commodity Science". But did you know that Professor Beckmann did travel on several occasions to your country to meet and exchange ideas with your Carolus Linnaeus ? Apparently the scientific world was already starting to shrink in the 18th century !

Ladies and Gentlemen, let me conclude my introductory remarks by quoting a sentence from the preface of Professor Frängsmyr's "Science in Sweden" which perfectly situates his involvement with the history of science : "When the Royal Swedish Academy of Sciences was founded in 1739, Linnaeus and his colleagues wished first and foremost to stimulate the growth of science in Sweden, but they knew that the best way to do this was to establish ties with science in other countries. Although external circumstances have changed, this remains the overall aim of the Academy's activities".

THE DISCOVERY OF THE ICE AGE : A SWEDISH PERSPECTIVE

Tore Frängsmyr

1

Of all scientific disciplines geology was the one that developed most slowly. In a way this was very natural. The geologists were a kind of historians, but in order to reveal the history of the earth they had to work with scientific methods. They had to do with historical source material that had disappeared, totally disappeared. New geological events had swept away the marks of those earlier processes they wanted to study.

Besides that the geologists had to fight ideological battles, or at least to face an opinion based on Genesis in the Bible. For many of them it was not, however, a personal conflict but rather a question of making a distinction between the Bible and science. As the Swedish chemist Torbern Bergman put it, the goal for the Bible was to give moral wisdom and not to be a text-book in science. This distinction was accepted by most serious scientists around 1800.

But even so, the most difficult problems for the geologists were of course connected with their specific geographical home areas. This statement is also true about Swedish conditions. For a long time Swedish geologists had been conscious of the special problems that pertained to the geological structure of Sweden. Among these problems were the questions of the changing shore lines of the Baltic, the transport of the erratic blocks, the scratches on bedrock surfaces of primary rock, and the long ridges of "diluvial" material (eskers).

Many of these problems were solved by the glacial theory that Louis Agassiz (1807-73) published in 1840, *Etudes sur les glaciers*. Agassiz was a young man, 29 years old, when he studied the glaciers in the Alps. He found that the effects on the rocks and on the ground from those active glaciers could also be found in other areas, far away from the Alps in Switzerland and even in other countries. And so he drew the conclusion that the whole Northern hemisphere had once a long time ago been covered by an enormous ice sheet, caused by the extension of a large amount of glaciers. As for Agassiz, it can be mentioned that after some years as a professor at Neuchâtel in Switzerland he moved to the United States, staying at Boston, Charleston and finally at Harvard, where he founded the Museum of Comparative Anatomy.

But still as a young scientist with brilliant ideas, Agassiz was met by deep scepticism. The opposition against his glacial theory was hard and arrogant, not a least in Britain. Some of his opponents found his theory too speculative. George Greenough described the glacial theory as a "climax of absurdity in geological opinions", Roderich Murchison said that nowadays everything is explained as effects of the ice; soon the day will come when even Hyde Park and Belgrave Square are regarded as formed by a glacier. William Conybear expressed the opinion that the glacial theory was "a glorious example of hasty unphilosophical entirely insufficient induction". But even those who did not make a joke of his arguments were sceptical. We must remember that geology was a very new discipline, it still had to face a lot of fundamental questions. For most leading geologists in England, France, and Germany, there were different schools about the origin of stone (called neptunism and volcanism), and there were also different meanings of the nature of geological processes (called catastrophism and uniformitarianism). When Agassiz published his theory, they had to think in a new way. In short, the opposition followed two lines. The first argument was that the glacial theory indicated that the earth had been very cold and slowly became warmer, but from a general scientific standpoint that time, the process had been just the opposite — from heat to cold. This had been clear through measurings of the temperature inside the earth. The other argument was a methodological one. Many saw the glacial theory as a catastrophist theory, like that of the Biblical Flood, and they were convinced of a more uniformitarian process. Others saw the theory as mere speculation, without any real connection with empirical work.

Agassiz' theory caused a great interest in Sweden. Agassiz himself and most European geologists had concentrated on the Alps and the British Isles, but the Scandinavian area was an essential part of the whole argumentation for a glacial period. And so Swedish geologists could to a great extent contribute to the development of this theory.

2.

But here, I think, it is necessary to say something about the Swedish background. Already in the 1690s a Swedish chemist and geologist, Urban Hiärne (1641-1724), discovered that the water level in the Baltic Sea had fallen, an observation earlier made by local fishermen. Because Hiärne and his contemporaries knew nothing about the glacial period, they did not think that the land had risen but that the water had decreased. This was the main problem in Swedish geology during the 18th century (and also most of the 19th), and almost all scientists and natural historians dealt with the question, such as Emanuel Swedenborg, Anders Celsius, Carl Linnaeus, Torbern Bergman, and Johan Gottschalk Wallerius. They all had different solutions to the question of what caused the phenomenon, but they agreed that it was a diminution of waters. In late 18th century it became more and more obvious that the reason probably was the upheaval of land in stead of diminution of waters, but these observations were not supported by any theoretical explanation.

Now, when we have the answer to this question, we know that it really was the key to understand the building and structure of the Swedish (and Scandinavian) landscape. The changing shore lines were caused by the upheaval of land; the earth's crust having been pressed down by the heavy ice sheet (about two kilometers thick) and now striving to regain the original level. (This process is still going on at the Swedish east coast). The erratic boulders had been transported by the ice, the scratches on bedrock surfaces of primary rock had been caused by stones frozen in and carried by the ice, and the "eskers" had been formed when the ice melted and left the stones and other material on the ground.

Still, this is *our* solution to the problem. During the time it was debated, it was not so easy. Charles Lyell, having first denied the phenomenon as such, was convinced after a visit to Sweden in 1834 that there actually had been an upheaval of land. But even so, he wanted with his drift theory to explain the transport of the erratic boulders by floating icebergs; after being deposited at the sea bottom, the boulders had been lifted up by the land elevation. Another theory was published by the Swede Nils Gabriel Sefström. After studying more than 400 localities with scratches, he found their directions in general to be from north to south. He came to this conclusion by distinguishing between what he called the "shock" side of the primary rocks, which had been polished, from the "lee" side, which showed sharp fractures with parts of the stone rubbed off. Sefström thought that a great inundation, a "petridelaunic" flood (i.g. of rolled stones), had swept over the country. Because the "eskers" had the same direction as the scratches, he thought them to be of the same origin.

Among those in Sweden who did not believe in Agassiz' theory was Jacob Berzelius, the famous chemist. In his general statement he followed the investigations made by the physicists Cordier, Fourier, Bischof, and others, showing that the inner parts of the earth were warmer than the crust, and consequently he accepted Elie de Beaumont's theory of a cooling earth. In the next step he defended Sefström's idea of a "petridelaunic" flood, believing that the erratic boulders must have been transported by water. He referred to Edward Hitchcock's studies of erratic findings in North America. Although Hitchcock accepted Agassiz's theory, he wanted --as a complement to the glacial theory-- also to count upon a flood (or another kind of violent water) with stones and pebbles. In the third step he had methodological objections. The glaciologists had done their field studies with specific prejudices, which were without empirical facts. A speculation or a hypothesis should be founded on empirical observations; otherwise there could be easy "slides to mistakes and errors". In this case he set up Sefström against Agassiz.

3.

But Berzelius belonged to an old generation. After him came a new generation with fresh appetite for new and exciting theories. His assistant for instance, Wilhelm Hisinger, was of another opinion. He pointed out that the erratic blocks around Berlin contained the same element as the Scandinavian mountains. Agassiz's arguments for an ice age he found most likely because of the findings of frozen elephants and buffaloes in Siberia. Sven Lovén, the zoologist, had still another attitude to the problem. As a young man he had, in 1836-37, gone on an expedition to Spitzbergen. Coming home he found that many of the fossil molluscs in Sweden were identical with the species found and still living in the Arctic area, and so he drew the conclusion that Scandinavia and Finland had been covered by a land ice of thickness of one thousand feet.

Löven's ideas were followed up by three geologists of a younger generation, Hampus von Post, Axel Erdmann, and Otto Torell.

Hampus von Post (1822-1922) was a very unusual man, making contributions to science as a geologist, entomologist, agricultural chemist, and botanist; after his university studies he worked as manager at Reijmyre glassworks for sixteen years (1852-1868), and after that as a teacher at Ultuna Agricultural High School. In three small papers, published in 1855-1856, he maintained that a special kind of stones were formed by the pressure of land ice, e.g. glaciers. He could not accept the glacial theory as a whole, but he was convinced that the ground soil in Sweden was created by the wearing of the ice.

Axel Erdmann (1814-1869) was influenced by von Post in his geological view. He was appointed the first chief of the Swedish Geological Survey in 1858. Ten years later he published the first handbook on the geological structure of Sweden, *Sveriges gvartära bildningar*. This is in many respects an important work, but the crucial point is that this was the first standard text-book based on the glacial theory. Erdmann really believed that Scandinavia had once been covered by an enormous ice-sheet, and he tried to see the results of this far-reaching process in the Swedish landscape. Some of his conclusions are not valid today, but they were of great interest in his time.

Another geologist, Carl Wilhelm Paijkull, wrote that the eskers must have originated during the glacial period, when Sweden was covered by an ice-sheet. He found evidence for his theory in a journey to Iceland, where he studied the glaciers, and so he could not accept the speculative theory of Sefström's petridelaunic flood. Instead of such violent revolutions he found that the slow work of land ice was more acceptable for explaining the geological structure of Sweden. Paijkull was not only a qualified geologist, he could also analyse in popular text-books these theories which had not yet been accepted among all geologists.

4.

Otto Torell (1828-1900) became a pupil of Sven Lovén, when he was only twenty years old. Lovén's discovery of an arctic fauna in Sweden became the starting-point of his geological activity. As a young student he made the historic finding of the arctic fossil mussel *Yoldia arctica* on the West coast of Sweden. If this arctic creature had lived in Sweden, the nature and the climate must have been arctic in former days. But the glacial theory was not generally accepted, and therefore he felt that he had to do his own field research in order to secure his scientific stand-point : "Since the most outstanding European geologists at that time (the middle of the 1850s), such as Lyell, Murchison, v. Buch, E. de Beaumont, Studer, Forchhammer etc., were totally against the glacial theory - although they could not agree on an alternative - and finally Berzelius in our own country acted resolutely against it, it was thus very natural that the theory was not totally accepted at such a late date among Scandinavian scientists. I felt myself the burden of all these authorities so heavily that it took me two and a half years of study and travelling to Arctic areas and the Alps, before the last doubt gave in."

In 1856 Torell went to Switzerland to study the glaciers, the year after he went to Iceland and after that to Norway and Spitzbergen, all of it with the goal of studying the effects of active glaciers and comparing them with geological observations in Sweden. In 1859 he published his doctoral thesis on the mollusc fauna of Spitzbergen, but his conclusions were of a much wider range, because he applied the glacial theory to the whole of Scandinavia. When the same animals now living in the Arctic area are found fossilized in other countries, and when the scratches and eskers in these countries are completely identical with these originated by gliding glaciers, "then it must be reasonable to regard as proved what has previously been a hypothesis, namely the earlier further extension of the Arctic region". Besides Lovén, Torell referred to Edward Forbes, who had studied the flora and fauna in England and found that the climate must have been much colder formerly, and to Murchison, who found at the river Dvina molluscs that obviously originated from the Arctic Ocean.

Torell was quite clear about the fact that Scandinavia had been covered by the ice-sheet, and that it was a land ice mass. According to Gordon L. Davies' book *The Earth in Decay* it was first in 1861 that Archibald Geikie recognized that the ice was a land ice, but the discussion was not finished before 1875 with the publication of James Croll's book *Climate and Time in Their Geological Relations*. Torell's conclusion was that the primary cause of the ice age had to be found in a change of the climate, a sinking of the temperature. But he was also aware of the glacial erosion. Following indications from von Post and Erdmann he concluded that the loose earth-layer of Sweden was created by the ice through grinding material from the primary rock to gravel, sand, and clay. This was not a self-evident matter in those days. Some geologists recognized the dominant role of the ice, but many did not. In 1859 Archibald Ramsay published a paper on the effects of glacial erosion. Davies writes : "There was nothing novel in the concept of erosion by glaciers, but Ramsay nevertheless found himself at the centre of a major controversy when in 1859 he was bold enough to suggest that the Pleistocene glaciers had played a major role in shaping the Earth's present landscapes."

After his disputation Torell went to Greenland to study the land ice there. All his travels had so far been paid for by himself, out of his own money. Now he realized that expeditions of this kind demanded much more resources, and so he started to plan a large expedition to the Polar sea. This resulted in the first "official" Swedish polarexpedition in 1861. For this purpose he saw many experts, e.g. Leopold McClintock and Roderick I. Murchison. Among his assistants was Adolf Erik Nordenskšld, later the famous conqueror of the North-East-passage, but the results of the expedition were not very sensational. They studied the glaciers and were confirmed in their belief of the effects of these phenomena.

Torell was not an industrious writer, but in 1864 he read a paper in the Academy of Sciences in Stockholm about the erratic blocks. Torell critized Lyell's drift theory. He could not agree with the idea of a large sea with floating icebergs, arguing instead that the ice was a land ice mass from the North. The ice had come from the Scandinavian mountains, the Gulf of Bothnia and the Baltic, and moved to the East and to the South. Torell preferred to talk of ice-streams instead of glaciers, and he distinguished between five different ice-streams. He was then also able to explain the diverging direction of the scratches, observed in Sweden. But first of all he saw the whole area as covered by the same ice sheet, coming from the North, not as Agassiz had suggested extensions of glaciers from different centra. In this argument he improved and completed Agassiz's theory.

During the coming years, 1865-68, Torell continued his studies in Germany, the Netherlands, and Switzerland. In 1867 he won a competition about how to explain the origin and the transport of erratic boulders in the North of Holland, but unfortunately this work was never published. This manuscript, in French and holding 383 pages, would have made Torell an international figure, but he was not interested enough to publish his work. Happily we have the main lines of the manuscript, because Torell's pupil Leonard Holmström gave a detailed report in his memorial sketch (biography).

Torell's view was that the origin of the blocks in Holland was to be found in Gotland and Esthonia and had moved through the Baltic down to the North of the Continent. But in many ways Torell was before his time. In 1875, on the 3rd of November, he held a lecture before the Geological Survey of Berlin, presenting his theory. The reaction was very negative. The listening German geologists became dismayed and regarded the idea of such an extensive ice-sheet as "ganz ungeheurlich"; Torell's view was regarded as "barer Unsinn". But the situation changed quite soon. In 1880 Torell was elected chairman at a congress arranged by the same society, and now everybody listened to him without any manifestations of dissatisfaction. This episode shows the difficulties encountered by the glacial theory. Having admitted some kind of glaciation the geologists usually had a different view as to the extent of the glaciers, but the next step was to recognize the whole process of glacial erosion and deposits.

5.

After 1868 Torell travelled abroad a great deal, for study and scientific congresses, but most of all his time was occupied leading the Swedish Geological Surveys, of which he was appointed chief in 1871. Although he did not publish much, especially not in foreign languages, he seems to have been respected as a geologist on an international level. In 1869 he was elected as a member of a committee - consisting besides himself of A. Ramsay and H. Bauermann - to analyze the effects of the glacial epoch. The aim of his studies and travels was above all to confirm his theories. It can be mentioned that he went to America and wrote a paper on glacial phenomena in North America, holding the position that Greenland had been the original place for a glaciation of a much wider range than that in Europe. His observations have been regarded as very important for the emergence of the glacial theory among American geologists; he has been mentioned as a pioneer besides James Geikie and Thomas Chamberlin.

Torell was also one of those geologists who realized the practical consequences of geological research. He suggested an endless number of projects, but it was not possible to carry out all of them. His was also the initiative of exploring different parts of Sweden in order to find natural resources. In this respect he was a kind of Enlightened scientist, always trying to find practical uses for his field of scientific research - for the good of his country.

Torell's work was in many ways brought to fruition by his pupil, the very famous geologist Gerard De Greer. He explained the phenomenon of land elevation by referring to and developing further the theory of Thomas F. Jamieson. He also demonstrated the origin of the eskers and introduced the method of geochronology by studying stratified loam. For understanding the geology of Sweden, the glacial theory was of definite importance. With the research contributions made by men like Lovén, Torell, and De Geer, a new foundation was given to geological research in Sweden, but also a contribution to the international development of science.

Note

This article is based on a study in Swedish, *Upptäckten av istiden : Studier i den moderna geologins framväxt* (With a Summary in English : The Discovery of the Ice Age), Lychnos-Bibliothek 29 (Almqvist & Wiksell International, Stockholm, 1976).

LINNAEUS AND HIS TIME

Tore Frängsmyr

The eighteenth century was a very successful period in the history of Swedish science, and Carolus Linnaeus was one of its outstanding figures. But how could a small country like Sweden become one of the leading nations in the new sciences and how could an impoverished student like Linnaeus make his way in the world of science so quickly ? I shall try to depict this man both as a scientist and as a representative of his time.

Linnaeus, ennobled as Carl von Linné, was born when Sweden was going through a difficult period. The king, Charles XII, was involved in wars throughout his reign and brought economic ruin to his country. After Charles' death in 1718, power was assumed by the Estates, and the monarch remained only formally the head of the realm. Everything possible now had to be done to repair the economy. The political party that held power from 1739-1765 embraced the mercantilist theories which were popular on the Continent. These implied that the government supported manufacturing industry in order to increase exports and reduce imports, while also protecting the interests of agriculture. In other words, the concern of the government was practical economy. Its ideology may be called a general utilitarianism, but here in the sense that the principle of economic utility was the overall goal. Firm central direction was to determine the detailed course of commercial development. Despite certain drawbacks, there was something new and exciting in this policy, a forward-looking optimism. The emphasis on industry and on the improvement of agriculture also made for a receptivity to modern science. There was an awareness that science could contribute to economic progress.

In the ecclesiastical field, however, policy was traditional and conservative. The orthodoxy of the seventeenth century still dominated and was even strengthened by new religious laws. A system of censorship monitored the books that were published, and in theological matters the church was the ultimate censoring authority. Theological faculties at the universities intervened on several occasions when doctoral dissertations touched upon religious questions.

Such, briefly, was the social background. Economics and religion came to exert a considerable influence on Linnaeus' career. This was the society into which he was born and he played his part in helping to develop it. When he was twenty-one, in 1728, he came to Uppsala University and but for a few years abroad he lived there until his death in 1778, fifty years later. He had always been interested in botany and very soon he was given the responsability of teaching the students. Both the professors were elderly and rather inactive, so Linnaeus came on the scene at the right moment. Not only was he very talented, he also had ample self-confidence and he had great plans for the reform of the study of natural history, especially botany. But at that time botany was a part of medicine, and to make a career in medicine Linnaeus needed a doctor's degree, which had to be obtained abroad, because it was not yet possible to take a degree of medicine in Sweden. (This situation was to change only a few years later).

So in 1735 Linnaeus went to The Netherlands, where after only one week he took his doctor's degree at the small University of Harderwijk with a dissertation on the subject of ague. He then went on to meet the great Boerhaave, who was very impressed by the young man. On Boerhaave's recommendation, he went to the well-known banker Georg Clifford at Hartecamp near Leiden, where he was placed in charge of Clifford's botanical garden. While working here, he published a large number of books, many of which had already been partially written in Sweden and were now completed and edited for publication.

The most important of these books was *Systema Naturae*, published in 1735 and containing a new classification system for botany. Classification had been a problem in botany since the Middle Ages. Botanists had tried to order the plants according to various characteristics, such as size or colour, but the imprecise nature of these characteristics caused complications. As knowledge of plants in newly explored parts of the world rapidly grew, it became more and more difficult to classify them. Linnaeus had been quick to realize that plants could be considered from their sexual aspect, and he devised a scheme based on this characteristic.

His classification system, also called the sexual system, took account of the number and arrangement of the stamens and pistils. *Systema Naturae* was a tremendous success and Linnaeus was always busy on new and enlarged editions, no less than sixteen editions being published during his lifetime. It should also be mentioned that the systematization of mineralogy and zoology was less successful. However, Linnaeus was the first to give man the name of "Homo sapiens" and place him among the animals, even though of course at the top of the ladder.

The new classification was what the botanical world needed. It gave botanists a system that was easy to understand and to use, and it gave them a common language. For a long time Linnaeus thought of it as a "natural system", a kind of blueprint of the Creator's work, but eventually he came to the conclusion that it was artificial. Nevertheless, and despite the criticism of some of his colleagues, the system worked, and Linnaeus, still a young man, became a world-famous authority. After three years abroad, however, he hastened home. The reason was important enough : his finacée had promised to wait three years for him. They married in 1739 and Linnaeus worked as a private doctor in Stockholm until he became Professor of Medicine at Uppsala University in 1741.

Linnaeus was not only a successful scientist, he was also a good organizer. In Stockholm he was one of the founders of the Royal Swedish Academy of Sciences (in 1739) and he became its first president. The Academy played an important role in the growth of science in the whole country and formed a body for international contact and collaboration. Back in Uppsala, Linnaeus soon became one of the leading professors. Uppsala was then - as it still is - a university town. Since its founding in 1477 the University had had its ups and downs. During Linnaeus' time it grew into an international university of good European standard. Linnaeus' was not the only famous name. Among his colleagues we find, for instance, Rosén von Rosenstein, the father of pediatrics, the mathematicians and astronomers Klingenstierna and Celsius (the inventor of a thermometer), and the chemists Wallerius and Bergman. In the expectation of economic benefit it seemed that all the fields of science could be developed, even in a small country of limited resources.

Linnaeus was an industrious worker. He restored and enlarged the botanical garden (the one now known as "the Linnaeus Garden" and maintained in the order that he first prescribed), he published books regularly, and he lectured to and made excursions with a steadily growing number of students. At the request of the Estates he made a series of journeys to differents parts of the country, the purpose being to inventory useful plants and other natural resources. He also organized a network of international journeys of exploration, sending out his "disciples" to nearly every corner of the world. In this connection he collaborated with the Swedish East India Company, and many of his students were employed as ship's chaplains although their real mission was to collect and describe plants and other specimens for the master in Uppsala. Linnaeus was convinced that it was possible "in principle" to find and list every plant in the world, and he could do this through his disciples; every new finding was of course put in its correct place in his System of Nature. He was happy when he received a tea plant from China but unfortunately it did not survive for very long in the Nordic climate. Nevertheless what he received was enough. Reports and collections came from Iceland and Australia, from China, America and South Africa. Through his disciples, in fact, he learned to know the whole world.

If Linnaeus' first great contribution to botany was the classification system, his second was "the binomial nomenclature" presented in his book *Species Plantarum* in 1753. The principle was that every plant should be identifiable by using just two names, as a person is identified by a first name and a family name. Earlier, plants had been known by the specific name followed by a long description of their characteristics. Now it was possible to define a plant by two words, the first giving the genus and the second the species : e.g. Linnaea borealis; Sinapis arvensis. The book contained a list of all the world's plants then known, a total of about 8000.

A third contribution was that Linnaeus succeeded better than anyone before in defining the species and he introduced a standard terminology for all parts of a plant that were essential for its definition. He had a sharp eye for distinctions and his descriptions were always clear and concise; with a few words he expressed what was important to know. Even modern botanists recognize his influence in this respect.

Linnaeus has been called "the Prince of Botany", and in Sweden he became a kind of national hero. He began to acquire this status during the Romantic era, when God and Nature were leading ideas among philosophers. Linnaeus was the right man for such a view of the world and a phrase was coined : "God created Nature and Linnaeus ordered it." But there is another aspect to the picture. Some critics have said that Linnaeus very soon became deskbound and abandoned his empiricism. He had a great gift for marshalling facts and impressions, for grouping them and making distinctions. But this led him to fanaticism. He wanted groups, catalogues and systems for everything. With this he moved farther and farther away from empirical science and became an abstract constructor. He showed no interest in experiments and modern science, the most important task continuing to be the classification and labelling of plants. He worked with Aristotelian logic and was scholastic in his method. In his personal relations, too, he had difficulties. He was very generous and helpful to his students as long as they listened to him and shared his views, but he was self-centered and did not like criticism. "Heretics", he called those who did not follow his scientific methods and pursue his scientific goals.

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Linnaeus, the man and scientist, cannot be described in a simply way. He was aware of and influenced by traditional cultural factors as well as new intellectual currents. He embraced the utilitarian spirit of the mercantilic politics with enthusiasm, but he refused to have the universities bound by a centralized political power. He was free from Christian dogmas and in that sense an enlightened philosopher, but in other respects he was deep down in mysticism and popular prejudices. One part of his soul Linnaeus had in the Enlightenment's belief in reason, another one in its opposite obscure superstition.

Linnaeus was a loyal supporter of the utilitarianism of his time. He was one of the leading scientists who founded the Academy of Sciences. As has already been mentioned its purpose was first of all practical and economical; the ambition was to serve people with scientific and technical innovations in order to give economic assistance to the nation. Linnaeus participated with enthusiasm in this work.

On the other hand, Linnaeus did not go too far. When in 1750 a commission for research and higher education suggested that the universities should be governed by the politicians in Stockholm, Linnaeus reacted. The idea was to organize the universities in detail and adapt them to the economic and commercial market. Linnaeus and his colleagues protested very sharply. They had never meant it this way. The economic use as a program should be for the nation in general, and for the future. Short-sighted adaptations to economic life were doomed to fail, since it was impossible to predict the future in details. According to Linnaeus economic use and academic freedom were not antitheses.

Still more complicated was Linnaeus's relation to the Church and the religion. He often expressed a deep religious feeling, but he critizised the dogmas and the powerful position of the Church. In the middle of the eighteenth century the Swedish Church tried to grasp the philosophical situation more than before. Linnaeus himself had his clashes with the theologians. He was critized when he expressed too free an opinion on the subject of the Creator and his intentions; only a practised theologian was allowed to pronounce on such weighty matters.

Linnaeus had his own way of expressing his feelings. In Nature he saw the eternal, omniscient, omnipotent God "from behind". He traced God's footsteps across the fields and observed, even in those which were scarcely discernible, "an infinite wisdom and might, an inscrutable perfection". The whole of nature bore the divine stamp and it had been given to Linnaeus to interpret the gospel. The study of nature became an act of devotion and a religious ritual. But he did not only read the Bible; he was also familiar with the classics. He often quoted authors such as Aristotle, Pliny, and Seneca, even when he was conveying his ideas about the way the world came into being and about its Creator : "If one will call him *destiny*, one is doing no wrong, for all things hang from his finger; if one will call him *providence*, one is also right, for all things happen at his sign and at his will."

That Linnaeus was in a way deeply religious is so apparent as hardly to need mentioning; he believed in God, in the Bible as the Word of God, and in himself as God's interpreter of Nature. It is also well-known that he was very conversant with the Old Testament and quoted from it frequently. But this does not mean he was an orthodox Christian. The aphorisms on Destiny and Nature bore the imprint of pagan philosophy and were scarcely looked upon kindly by orthodox theologians. Nor, for his part, was Linnaeus particularly well disposed towards them. Wherever one may think and write as he will, study flourishes, he declares as early as 1733 : "Where religion is free, the land flourishes. Where theology reigns, there is nothing but wretchedness."

Consequently, Linnaeus did not rely only upon the Bible but also upon human reason. And so he says about the process of Creation : "That the wondrous edifice of the earth was brought forth and shaped by the eternal Master, we are told not only by the Holy Scriptures, but also by common sense." Even more frequently he expresses his constant admiration for the all-wise order of nature, which does more than anything else to point to a higher power as the origin of all things. By the agency of the Creator the grass has appeared which feeds the cattle; fish, which do not have the warmth to be viviparous, have instead "by the providence of the Creator" been made capable of producing roe. Nature exists to show us the genius and greatness of our Lord; this is its main business. All is done to the glory of God, which is attested not only by moralists and theologians, but also by nature herself, and man has been put here to reflect this his Creator's wisdom. Here we meet the well-known physico-theological philosophy of the period; one should prove God's existence and greatness by studying nature and its complicated coherence. Linnaeus also sees clear signs of the hand of God in the chain of being which fills the whole of nature. One link differs so little from the next that if one could see the whole chain at once it would hardly be possible to distinguish them. All levels and forms of life, the manifoldness and the variety, were necessary expressions of the omnipotence of God.

Linnaeus often talked about what he called *oeconomia naturae*, the economy of nature, by which he meant a kind of balance or harmony in nature. There was always a war going on in nature, between individuals and groups. No plant or animal was allowed to grow too fast, because it would disturb this balance. There was a plan made by God to keep everything in nature in harmony, and this view has been said to be a sign of what we call ecological insight, and perhaps this is right because Linnaeus was also aware of what man could do to nature.

When it comes to antagonism between religion and the Enlightenment, geology can be said to be of special importance. This was very natural. The geologists tried to give a scientific explanation of the same period talken about in Genesis. Linnaeus did not recognize the biblical Flood as a geological event and he was not satisfied with biblical chronology. In his autobiography, he makes a remark which has often been quoted without ever really being elucidated : "Linnaeus would gladly have believed that the earth was older than the Chinese had claimed, had the Holy Scriptures suffered it." According to Chinese history, China would have been not merely inhabited but even a kingdom several hundred years old before Noah's Flood. As we know, Christian chronology was thinking in terms of a total period of 6000 years, 4000 having passed between the Creation and the birth of Jesus. This time table did not fit the Chinese chronology, and it did not fit Linnaeus' ideas either.

Time is a key concept in Linnaeus' outlook on the development of the earth's surface, as it was in his later doctrine of the formation of the species, when the species was characterized as the child of time, *temporis filia.* It was no coincidence that he was inclined to credit the earth with a longer history. When he contemplates nature, he is more than conscious of all the forces which affect and alter the face of the earth, provided there is enough time. He is fascinated and becomes lyrical when he examines the rock strata in the south of Sweden, and he thinks of the age which has been required for this work : "I feel dizzy when I stand upon this hill and look down upon the long period of time which has passed like waves in the Sound, leaving behind only these faintest traces of the former world, and which can now only whisper when all else has become still." This was new and radical thinking in eighteenth century geology. His insight into the importance of the time factor points ahead to the geological theories of the nineteenth century. And it becomes clear that he disregarded the Bible completely as a scientific textbook.

Here Linnaeus holds a position as an enlightened philosopher, guarding the scientific principles. It was not always in the way, neither for him, nor for his contemporaries. We have several examples of how new ways of thinking could run foul of older ones. One such question concerned the transformation of cereals. According to popular tradition, oats could be transformed into rye under certain conditions; various experiments appeared to confirm this state of affairs. The scientists said no; Linnaeus maintained that species had been constant since the Creation and that such a metamorphosis was unreasonable according to the laws of science. There were other problems. Another folk tradition stated that swallows wintered by sleeping on the bottoms of lakes. Here it happened that Linnaeus was on the opposite side, believing steadfastly that this was just what swallows did. But the versatile professor Johan Leche, of Turku, Finland, pointed out that it was physiologically impossible for a creature with lungs like the swallow to survive on the bottom of a lake.

Leche deemed such an idea to be "an absurd fable" and its dissemination to be an epidemic delusion, *error epidemicus*. It was the duty of science to expose popular prejudices and, by careful experiment and with the aid of physics and mathematics, to build up a view of the world founded on facts.

Other examples could be mentioned. In his anthropological writings Linnaeus also seemed very much to alter between standpoints of reason and folk tradition. As mentioned before he named man Homo sapiens and so he saw man as belonging to animals, as a part of nature. He also rejected all the rumours about the seven-headed hydra, which was said to be found somewhere in Holland; this was all nonsense, he declared. At the same time he had a lot of ideas about the hottentots, about troglodytes, tailed men and other curious forms of creatures between animals and man. They were all taken from folk tradition or from the literature, Linnaeus being not critically enough to have all these tales proved. Of course he had not himself seen these creatures.

In his old days he wrote down his reflections upon life, under the title of *Nemesis divina*. Here he gave expression to a very pessimistic view of life. He collected examples (meant for his son) from the Bible, classical literature and his own time, to show what would happen if you acted in a wrong way (and indirectly what would happen if you acted right). This was also a kind of harmony, of balance in the world, but in a moral and social meaning; the economy of nature also existed in the human society. Everywhere Linnaeus saw the traces of Nemesis divina, the punishing God, and he saw them mostly in his surroundings, among his colleagues and enemies. He mixed his personal feelings with folk tales, biblical stories and a general religious morality. In this case he could not make a clear distinction between scientific arguments and his own antipathies.

We have seen some examples of the complexity of Linnaeus. In the time of the Enlightenment, in the clash between old traditions and new ideas he was sometimes on one side, sometimes on the other. There were even open conflicts, between people and between groups, but the most serious conflicts probably took place within himself.

Further readings in English

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LARSON, James L., *Reason and Experience : The Representation of Natural Order in the Work of Carl von Linné* (University of California Press, 1971); *Svenska Linnésällskapets Arsskrift, 1978*, Commemorative Volume of the Bicentary Conference in 1978 : "Linnaeus : Progress and Prospects in Linnean Research" (1980).

Sarton Medal Lectures

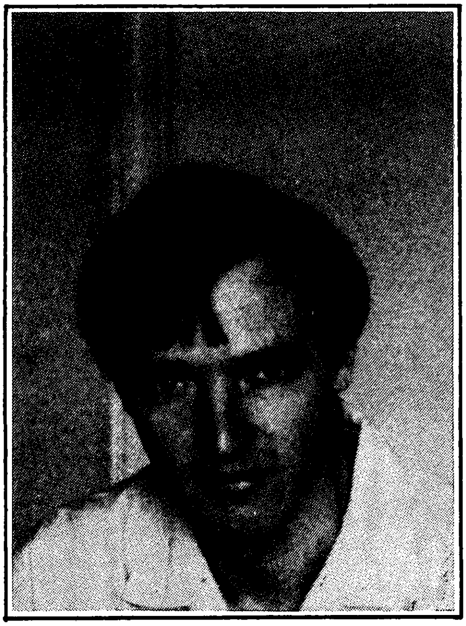
LAUDATIO FERNAND HALLYN

F. Vandamme

It is a pleasure for me to have the duty or rather the privilege to dwell on the very rich career of my friend and colleague Fernand Hallyn. He was unanimously chosen by the Faculty of Philosophy and Literature as well as by the board of the Sarton Committee for the Sarton Medal in view of his very rich work in the domain of history of science. For sure he is an authority also in literary criticism and semiotics. But rather early he managed to combine both. In fact this combination brings us very important and fructual new insight and perspectives on the early scientific writings and scientific constructions. He in fact has covered in such a study a large domain of scientific writings which have been paradigmatic for modern scientific endeavors and methodology.

His work proves again that interdisciplinary research is such a fructual basis for fresh and useful new insight and for better understanding of our past, present and future.

Fernand Hallyn was born in 1945 in Bruges and he studied in Bruges too. His Master degree and Ph.D. in Roman philology was obtained at the University of Ghent. His Ph.D. thesis concerned the metaphor in French baroque poetry. This perspective and outlook he has sophisticated into a very fine tool for brilliant and deep analysis of the power of expression and development of thought, ideas, targets and emotions. Without doubt a height in his young but already amazing large and influential list of publications is his book : "La structure poétique du monde : Copernicus, Kepler". We are confident that he will go to amaze us by further refining his methods and by obtaining even more fructual results.



TOUT PEUT-IL MARCHER ?

Fernand Hallyn

INTRODUCTION

L'importance de la "découvere" de la lunette par Galilée en 1610, rendue publique dans le *Sidereus Nuncius*, réside en premier lieu dans le retentissement que ses observations de la lune, des satellites de Jupiter et des nouvelles étoiles ont eu, et dans leurs effets sur la représentation du monde[1]. En optique, la signification initiale de la lunette galiléenne est plutôt de l'ordre de la provocation : si des questions théoriques sont suscitées, Galilée n'y apporte aucune réponse. Dans le premier tiers du XVIIe siècle, la théorie de la réfraction, essentielle pour l'explication scientifique du fonctionnement de la lunette, est développée d'abord par Kepler, qui n'en découvre cependant pas la loi générale, ensuite par Snellius et Descartes.

A première vue, les recherches qui entourent la découverte de la loi de la réfraction offrent une illustration parfaite du slogan lancé par Paul Feyerabend : "Tout peut marcher" (*"Anything goes"*)[2]; aucune démarche, aucune voie ne peut être exclue *a priori* de la science, sous peine d'inhiber son progrès. Il n'existe pas d'"obstacles" épistémologiques en soi. Une illusion peut conduire à la découverte d'une vérité, une vue juste peut en détourner. L'aveuglement peut être source de clairvoyance, et inversement. On ne saurait réduire la formation d'hypothèses ou la découverte de lois à des processus purement logiques. Tout ceci pourrait être illustré abondamment par les épisodes qui ont conduit à la découverte de la loi de la réfraction au début du XVIIe siècle. La thèse dite "anarchiste" de Feyerabend n'est pensable, toutefois, que du point de vue de l'observateur qui se place lui-même en dehors de la recherche telle qu'elle est menée pratiquement. Pour tel homme, muni de telles convictions liées au contexte où il évolue, seuls certains cheminements font sens et certains choix sont seuls possibles. L'univers discursif de la science est constitué de positions et d'oppositions, de débats, de conflits et de ruptures, où l'on soutient que, justement, tout ne peut pas marcher. L'action concrète, telle qu'une "poétique" de l'invention l'aborde[3], implique la négation de *"Anything goes"*, mais non la réduction à un logicisme; elle explore des zones restreintes et précises de liberté, les choix qui s'y opèrent ainsi que leurs motivations variées et multiples. C'est ce que je voudrais montrer ici sur les exemples concrets de Kepler et de Snellius à la recherche de la loi de la réfraction.

AVANT DESCARTES

Ptolémée

Dès l'Antiquité, la réfraction a fait l'objet, en particulier chez Ptolémée[4], d'une étude à la fois empirique et mathématique. Celle-ci s'appuyait sur une théorie, développée déjà par Euclide, selon laquelle la vue est l'effet de rayons[5] sortant de l'oeil.

Ptolémée admet une analogie entre réflexion et réfraction, considérant les deux comme des espèces d'un même genre de phénomènes. L'analogie est développée en deux sens : qualitatif et quantitatif.

Du point de vue qualitatif ou *physique*, il s'agit de deux cas d'altération du trajet des rayons, mais dans le premier ils "rebondissent" et dans l'autre ils sont "courbés" et traversent le nouveau milieu (fig. 1); en outre, la réfraction semble exiger une certaine "similarité" entre les deux milieux et elle est réversible, puisqu'elle ne se produit pas seulement lors du passage dans un milieu plus dense, mais aussi lors de la pénétration d'un milieu plus subtil".

Du point de vue quantitatif ou *géométrique*, l'image réfléchie ou réfractée apparaît toujours à l'intersection du prolongement du rayon incident (sortant de l'oeil) et de la perpendiculaire tracée depuis l'objet sur la surface qui altère le trajet (voir fig. 1). Reste, bien entendu, la question du rapport entre les angles concernés, qui est d'égalité dans le cas de la réflexion ($i = r$)[6]. Par inférence analogique, Ptolémée pose dans le cas de la réfraction, l'existence d'"une relation quantitative bien définie", mais ne parvient pas à la préciser[7]. Certes, à l'aide de dispositifs expérimentaux, il établit des tables de mesure pour le passage du rayon visuel de l'air dans l'eau, de l'air dans le verre, et de l'eau dans le verre. Il retouche légèrement les résultats et parvient ainsi à introduire une régularité dans la progression des angles de réfraction[8], mais n'en tire pas de véritable loi régissant le rapport avec les angles d'incidence. Il retient seulement que, lors du passage du rayon dans un milieu plus dense, si $i > i_1$, alors $i : r > i_1 > r_1$. Des études récentes ont fait apparaître trois raisons principales aux limites de cette approche grecque de la réfraction — raisons qui touchent aux possibles de "l'imaginaire scientifique"[9] des Grecs.

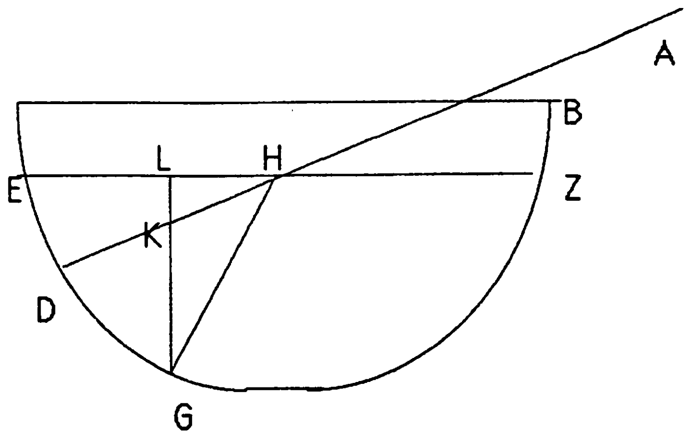


Fig. 1 - Observation de Ptolémée. L'oeil placé en A ne peut voir la pièce de monnaie en G aussi longtemps que la cuve opaque reste vide. Si la cuve est remplie d'eau jusqu'au niveau EHZ, la pièce semble flotter en K, à l'intersection de GKL, perpendiculaire de G sur EHZ, et de AHD, prolongement rectilinéaire du rayon incident ABH.

En premier lieu intervient une *limite langagière*. L'expérience ne peut donner de résultats que dans les langages dont on dispose pour la traiter, et pour Ptolémée, ce langage était la géométrie euclidienne.

Certes, les Grecs pouvaient rendre compte d'une variation continue par cette géométrie, mais "les techniques *arithmétiques* qui étaient principalement les leurs les amenaient plus à repérer des rapports de grandeur et à les disposer en séries discrètes — à dresser des tables — qu'à chercher à donner une expression unique à une variation continue"[10]. En d'autres mots, il manquait une élaboration de l'algèbre permettant de transformer la constance des différences secondaires entre les angles de réfraction en équation entre les angles d'incidence et de réfraction[11].

Notons ensuite que la *conception* même de l'optique limitait nécessairement la façon de poser le problème. D'une part, l'optique de l'Antiquité est une science de la sensation visuelle; elle s'occupe en priorité du regard et de ce qui lui arrive. Son but est d'expliquer l'effet de *signe* (fidèle ou déformant) des *événements* visuels. On pourrait dire que, parallèlement à une cosmologie et une astronomie géocentriques, s'élabore une optique anthropocentrique[12], qui n'est pas une science de la lumière ni des objets réfringents en soi, avec leurs propriétés physiques abstraites : "ce qui compte est bien *la variation de ce qu'on voit, et non la nature de l'objet transformant*". D'où la conséquence que les caractéristiques du dioptre ne sont pas traitées "indépendamment du regard de l'observateur"[13]. D'autre part, si la conception de l'optique détournait l'attention des objets transformants mêmes, elle ne devait pas non plus, dans la réfraction, diriger Ptolémée vers la considération des sinus. Axée sur les données *concrètes* de la perception, cette optique était naturellement orientée vers la recherche d'une régularité *directe* entre les angles d'incidence et de réfraction, et non pas vers la prise en compte des fonctions abstraites (les sinus) de ces angles, qui ne faisaient même pas partie de la façon de concevoir le problème[14]. L'analogie établie avec la réflexion, où une telle constance directe existe effectivement, ne pouvait évidemment que renforcer cette tendance.

Enfin, la satisfaction devant les résultats obtenus dépend toujours aussi d'une certaine conception générale du monde. Or, l'univers grec était nettement hiérarchisé; seules pouvaient prétendre à une mathématisation exacte la partie supérieure, céleste, du monde et les sens supérieurs, la vue et l'ouïe, de l'homme. Aussi, "dans la réfraction, le regard pouvait faire l'objet d'une étude quantitative; mais non le dioptre réfringent *en tant que tel*, car une lentille de verre, une sphère remplie d'eau ou même une pierre précieuse taillée seraient passées pour des objets trop grossiers pour se plier *par nature* à des relations constantes et harmonieuses"[15] (183). Au fond, *rien* ne pouvait donc vraiment marcher pour Ptolémée, à cause de l'imperfection de la nature sublunaire même.

Des "perspectivistes" à Kepler

L'Antiquité a connu plusieurs théories de la vision, mais la seule qui ait donné naissance à une étude détaillée de la réfraction est celle, adoptée par Ptolémée, qui pose l'émission de rayons visuels par l'oeil. Il faut attendre le début du XIe siècle et l'Arabe Alhazen pour voir paraître une alternative sérieuse, qui sera reprise, en Occident, par les "perspectivistes" (surtout les Anglais Roger Bacon et John Pecham et le Polonais Witelo)[16].

Pour Alhazen[17], la vision suppose une émission non pas depuis l'oeil, mais depuis les objets perçus lorsqu'il sont eux-mêmes frappés par la lumière[18]. Chaque point d'un objet illuminé "rayonne" en fait dans toutes les directions et chaque point de l'oeil reçoit donc un "rayon"[19] de chaque point qui se trouve dans son champ de vision. S'il ne résulte pas de cette situation une confusion totale et si nous arrivons à voir des objets distinctement, c'est que tous les rayons émis n'ont pas la même force. Appliquant au rayonnement une métaphore mécanique, Alhazen pose que les rayons perpendiculaires à un point donné sont les plus forts, tout comme un projectile touche son objectif avec le plus de force lorsqu'il l'atteint perpendiculairement[20]. Chaque point de l'oeil ne retient, dès lors, que le rayon dont l'incidence est perpendiculaire à son plan; les autres sont réfractés et affaiblis, de sorte qu'ils n'arrivent pas à stimuler la "puissance visuelle" (*virtus visiva*).

A partir de l'école "perspectiviste", la réfraction est donc installée, mais de manière toute négative, au coeur même de la théorie de la vision : elle y est omniprésente, mais constamment éliminée. D'autre part, la métaphore mécanique est teintée d'animisme : si les rayons réfractés sont éliminés pour leur faiblesse, cette sélection optique répond à une *intentionnalité* du cristallin, qui *choisit*, parmi toutes les impressions qu'il reçoit, de manière à obtenir une image cohérente[21]. L'oeil n'est pas encore une *machine* optique[22].

Dans l'analyse de la réfraction en elle-même, une approche mécanique, axée sur les causes physiques, domine. Au rayon qui vient la frapper, une surface réfringente oppose une certaine résistance; elle ne parvient pas à supprimer sa puissance de pénétration, mais modifie tout de même son trajet. A chaque rayon incident sont attribuées deux composantes, l'une perpendiculaire et l'autre parallèle à la surface du milieu réfringent (fig. 2). Lors du "choc" produit par la rencontre d'un tel milieu, ces deux composantes sont affectées différemment. Dans le cas du passage dans un milieu plus dense, la composante perpendiculaire (la plus forte des deux) subit moins les effets du choc, et le mouvement composé a tendance à se rapprocher de la "normale" (la perpendiculaire touchant la surface réfringente au point d'incidence). Ce rapprochement est d'autant plus considérable que l'incidence même est plus oblique.

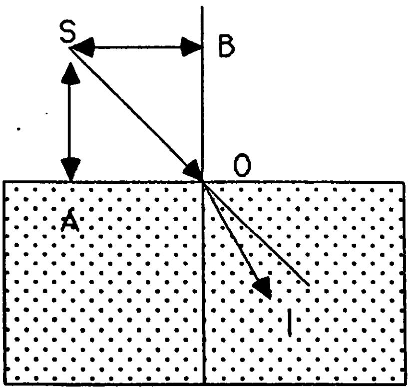


Figure 2. - Le rayon SO, qui tombe sur une surface réfringente, est décomposé en une composante perpendiculaire à la surface (SA) et une composante parallèle (SB). Si le nouveau milieu est plus dense, la force de la composante perpendiculaire sera moins freinée que celle de la composante parallèle (plus faible) et le rayon se rapprochera donc de la normale.

Dans le cas du passage dans un milieu moins dense, au contraire, la composante parallèle ou faible profite le plus de la diminution de la résistance; son influence devient plus importante, et le rayon s'éloigne de la normale.

Pas plus que Ptolémée, les "perspectivistes ne passent à une théorie des lentilles. Leur optique reste en premier lieu une théorie de la vision naturelle, même si Roger Bacon, par exemple, analyse dans le détail les réfractions produites par des surfaces planes, convexes et concaves. Ils ne dissocient pas l'étude des dioptres de celle du regard.

Les "perspectivistes" s'expriment, par ailleurs, dans un langage aristotélicien. Witelo définit la réfraction par "la résistance d'une qualité passive [la densité] opposée à une qualité active [la lumière]". La lumière est la "forme en acte", le milieu réfringent la "matière" plus ou moins apte à la recevoir. Une certaine densité du milieu "empêche [la lumière d'atteindre] le but vers lequel le mouvement était dirigé". Lorsque la lumière passe dans un milieu moins dense, il y a "victoire de la forme en acte sur une matière mieux adaptée à la recevoir"; dès lors, "la forme se diffuse en s'écartant de son trajet initial..."[23] On le voit : il est question de résistance, et donc de violence; de victoire et donc de guerre; de but et donc d'intention. La distinction de la forme et de la matière colore la métaphore mécanique d'animisme. Cette tendance apparaît le plus nettement chez Bacon : la lumière "désire" le chemin le plus facile, elle le "cherche", le "choisit", etc.[24] Les adaptations du trajet d'un rayon en fonction du milieu sont présentées comme des comportements obéissant à des *intentions*.

Nous retrouverons ce langage chez Snellius. Mais, comme nous le verrons également, Kepler ne pourra pas l'admettre. Ce sont pourtant les "perspectivistes", et avant tout Witelo, qu'il choisit comme point de référence pour l'exposé de son optique à lui. Cela se comprend : les traités d'Alhazen et de Witelo avaient été publiés en 1572 à Bâle par les soins de Friedrich Risner, un élève de Pierre de La Ramée, et il s'agissait bien là, à l'époque, de la théorie optique la plus cohérente[25].

Kepler étudie longuement la réfraction dans la quatrième section de ses *Paralipomènes à Vitellion* de 1604, généralement considérés comme le premier traité d'optique moderne. En 1604, Kepler ne songe évidemment pas encore à la lunette. S'il traite de la réfraction, c'est qu'elle constituait, dès avant Galilée, un important problème astronomique. Mais au lieu de représenter une arme dans l'exploration du monde, elle apparaissait comme une cause d'illusions. Dès l'Antiquité, Ptolémée avait abordé, dans son *Optique*, la question de la réfraction atmosphérique, qui était censée se produire lors du passage des rayons de l'air sublunaire dans l'éther supralunaire, provoquant des changements dans les dimensions apparentes des corps célestes. De même, Cléomède avait traité des phénomènes tels que des "éclipses paradoxales" de la lune, ayant lieu lorsqu'elle se trouve au-dessus de l'horizon, mais non dans la zone d'ombre de la terre. Dans les *Paralipomènes* (dont le sous-titre est : *Partie optique de l'astronomie*), Kepler introduit, au chapitre IV, son examen de la réfraction par le rappel d'une discussion sur la réfraction atmosphérique entre Tycho Brahé et Rothmann.

Dans les *Paralipomènes*, trois approches de la réfraction se succèdent, mais se révèlent toutes infructueuses. Kepler découvre une constante valable pour les petits angles uniquement : si l'incidence n'est pas supérieure à 30°, les angles d'incidence et de réfraction sont proportionnels entre eux[26]. Il revient à la réfraction dans sa *Dioptrique* de 1611, après la lecture du *Message céleste* de Galilée, mais toujours sans avoir trouvé la loi générale[27]. Les trois tentatives des *Paralipomènes*, qui correspondent respectivement à l'*induction*, à l'*analogie* et à la *déduction*, méritent, malgré leur insuccès, qu'on s'y attarde : elles illustrent quelles étaient, pour Kepler, les voies possibles.

Kepler : l'induction "impure"

Dans la première partie de son examen de la réfraction, Kepler déclare partir de l'expérience. Il passe en revue une série de tentatives pour établir la mesure de la réfraction par voie empirique. L'expérience montre qu'elle tient nécessairement à la combinaison de deux facteurs : la densité du milieu réfringent[28] et la grandeur de l'angle d'incidence. Kepler essaie en vain d'établir entre ces deux facteurs un rapport qui corresponde parfaitement aux tables des réfractions que, à la suite de Ptolémée, Alhazen et Vitellion ont proposées pour diverses réfractions terrestres (air-eau, etc.), ainsi qu'à celle établie par Tycho Brahé pour la réfraction atmosphérique. La démarche se veut nettement *inductive*, puisqu'elle vise à découvrir une loi générale à partir de la prise en considération d'une série de facteurs présents dans un ensemble de cas particuliers. A un moment donné, Kepler est tout près de découvrir cette loi générale de la réfraction, qui pose un rapport constant entre les sinus des angles d'incidence et de réfraction pour des milieux bien définis, mais il abandonne la piste. Il cherche, en effet, à établir un rapport entre *sinus* :

> Et je n'ai pas négligé non plus de me demander si, une fois la réfraction horizontale établie à partir de la densité du milieu, les autres (réfractions) correspondaient aux sinus des distances au sommet. Mais le calcul ne l'a pas confirmé, et d'ailleurs il était tout à fait inutile de se poser la question. En effet, les réfractions (dans ce cas) croîtraient selon la même forme dans tous les milieux, ce qui répugne à l'expérience[29].

Comme c'est le cas pour ses prédécesseurs, ce que Kepler appelle ici *angle de réfraction*, n'est pas ce que nous appelons ainsi. Il ne s'agit pas de l'angle formé par le rayon réfracté avec la normale, mais — dans la tradition de "perspectivistes" et de l'Antiquité — de la déviation subie par le rayon pénétrant dans un nouveau milieu (fig. 3).

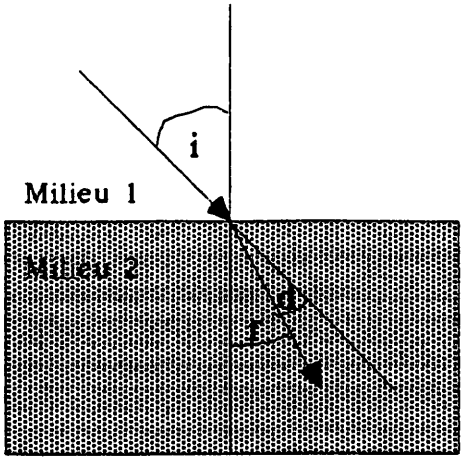


Figure 3. - Outre l'angle d'incidence *i*, Kepler, n'envisage pas l'actuel angle de réfraction *r*, mais l'angle de déviation *d*.

On peut se demander pourquoi Kepler a si vite abandonné l'hypothèse des sinus, sans faire entrer en ligne de compte les sinus d'autres angles que cet angle de déviation. Le fait est d'autant plus remarquable, ainsi que le constate Gérard Simon[30], que Kepler rappelle le principe du retour inverse de la lumière, déjà connu de Ptolémée : le trajet suivi par un rayon reste le même si l'on inverse le sens de la propagation de la lumière. De par cette loi, l'angle de réfraction (au sens actuel) dans une direction donnée devient l'angle d'incidence dans le sens inverse. Il n'aurait donc pas été étrange d'en tenir compte et d'arriver ainsi à la loi générale.

Pour expliquer l'aveuglement de Kepler, on a invoqué notamment l'influence du vocabulaire de l'optique : *refringere* invite en effet, étymologiquement, à penser une brisure, une déviation subies[31]. Mais on sait que, loin d'être le prisonnier aveugle d'une langue reçue, Kepler se montre toujours attentif aux problèmes et aux pièges terminologiques et qu'il n'hésite pas à forger des néologismes (*foyer, convergence, divergence...*) pour mieux signifier sa manière de penser les phénomènes. Les *Paralipomènes* commencent d'ailleurs par une mise au point terminologique où la justesse de *frangere* est soulignée[32]. Il faut donc chercher une autre réponse.

Chez Alhazen et Witelo, la "normale", ou la perpendiculaire élevée sur le point d'incidence, jouait un rôle important de par la métaphore mécanique qu'ils appliquaient à la lumière et qui invitait, comme nous l'avons vu, à penser le trajet du rayon réfracté en fonction de son rapport perpendiculaire au plan de séparation des deux milieux, c'est-à-dire en fonction du rapport exemplifié par la normale. Or, c'est là une théorie que Kepler ne peut accepter, car sous la métaphore mécanique du projectile il décèle un animisme qu'il ne peut que rejeter :

> La lumière, disent-ils [Alhazen et Vitellion], cherche une compensation pour le préjudice reçu lorsqu'elle est frappée obliquement. Car plus la rencontre du <milieu> plus dense l'a affaiblie, et plus elle se concentre en se rapprochant de la perpendiculaire pour heurter le fond du milieu plus dense par un coup plus droit. Car ces coups-là sont les plus forts. Sur quoi ils ajoutent je ne sais quelle subtilité : que le mouvement de la lumière tombant obliquement sur la surface du dense est composé d'un mouvement perpendiculaire et d'un mouvement parallèle a cette surface, et que ce mouvement ainsi composé n'est pas aboli, mais seulement gêné par la rencontre avec le diaphane plus dense. Que par suite le mouvement entier, tel qu'il est composé, se fortifie de nouveau, c'est-à-dire qu'il subsiste dans le mouvement désormais altéré par la surface dense, comme un vestige de la composition primitive, de telle sorte qu'il n'est ni tout à fait perpendiculaire ni tout à fait parallèle. Que cependant il s'incline davantage vers la perpendiculaire que vers la parallèle parce que le mouvement perpendiculaire est plus fort. Ils n'expliquent pas la chose beaucoup mieux que ne le fait Macrobe, qui prétend au livre VII des *Saturnales* que la vision hésite et revient sur elle-même après le choc. Comme si l'espèce de la lumière était douée d'une intelligence susceptible d'évaluer tant la densité du milieu que le préjudice qu'elle encourt, et ceci de son propre jugement sans force extérieure; et comme si elle se brisait elle-même dans une action, non pas dans une passion[33].

Cette critique combine une pensée encore aristotélicienne du mouvement et un refus de l'animisme. Comme le montre la fin du passage cité, la dynamique de Kepler reste fondamentalement aristotélicienne. Le mouvement est pensé en termes d'action (ou de mouvement naturel) et de passion (ou de mouvement violent, c'est-à-dire subi). La diffusion naturelle de la lumière est rectiligne; sa brisure (par réfraction) ne saurait être qu'une passion, un changement violent, dont la cause est extérieure à la lumière même. Or, recourir, dans ces conditions, à l'angle formé par le rayon réfracté et la normale, revient, selon Kepler, à penser en termes d'action de la lumière réfractée, comme si on la jugeait animée et capable d'orienter elle-même son mouvement en fonction d'une ligne perpendiculaire dont l'existence est purement idéelle.

L'induction qui fait appel à la normale est donc *impure* aux yeux de Kepler : au lieu de s'en tenir strictement aux donnés physiques de l'expérience, elle projette sur eux une métaphore animiste, leur attribuant une intentionnalité et un finalisme. Mais l'induction telle qu'il entend la pratiquer lui-même est, au fond, également impure, puisqu'elle est informée par des conceptions théoriques spécifiques, et surtout par une théorie du mouvement qui reste tout de même aristotélicienne (alors que Descartes fera appel au mouvement inertial) et qui limite *a priori* le nombre de variables admissibles dans la solution du problème.

Les effets, heureux ou malheureux, des présuppositions théoriques qui commandent, consciemment ou non, la sélection des éléments et des relations éventuellement pertinents dans l'induction, peuvent varier d'un cas à l'autre, même chez un seul auteur. Ainsi le refus opposé par Kepler à l'intervention de la normale dans la loi de la réfraction rappelle son refus de considérer que les planètes pourraient se mouvoir en cercle autour d'un centre auquel ne correspond aucun corps. Tout comme il reproche à Alhazen et Vitellion d'expliquer le trajet de la lumière en fonction d'une ligne à laquelle ne correspond aucune réalité matérielle, il écrit, dans son *Astronomie nouvelle*, au sujet de la trajectoire des planètes : "Je ne nie pas que l'on puisse penser un centre, et un cercle autour de ce centre. Je dis seulement que si ce centre n'existe que dans la pensée et n'est jamais marqué par un signe extérieur, il n'est pas possible qu'un corps réel exécute autour de lui un mouvement parfaitement circulaire."[34] La normale est en optique ce que le centre des orbites circulaires est en astronomie : une considération de type finaliste, incapable de jouer le rôle d'une cause efficiente dans l'ordre des relations physiques en tant que telles. Mais alors que, en astronomie, cette option s'intègre dans le cheminement qui conduit Kepler vers la découverte des orbites elliptiques, elle l'amène, en optique, à ne pas prendre en considération les relations qui auraient permis de découvrir la véritable loi de la réfraction.

Kepler : l'impasse de l'analogie

A la fin de sa première approche de la réfraction, Kepler déclare que la méthode inductive qu'il a poursuivie jusque là était, au fond, une démarche aveugle : "Ainsi jusqu'à présent nous avons suivi une méthode de recherche presque aveugle et nous avons compté sur la chance."[35] Il passe alors à une nouvelle tentative où il recourt à une analogie entre des phénomènes qui concernent tous deux la lumière : il essayera d'établir des relations d'équivalence dans la "mesure" des phénomènes de réflexion et de réfraction. Il avoue ne pas connaître les "causes" spécifiques des éventuelles correspondances; sa démarche reste donc tâtonnante, mais il espère que les "mesures" le mettront sur la piste des "causes" : "Je désirais en effet obtenir une mesure des réfractions; peu m'importait qu'elle fût aveugle pourvu qu'elle fût : car j'avais l'espoir tenace qu'une fois connue la mesure légitime, la cause se dévoilerait elle aussi."[36] Il s'agit bien, on le verra, de l'*analogie* telle que Kant l'oppose, en tant qu'instrument heuristique, à l'induction[37] : il ne s'agit plus d'inférer une généralisation à partir d'un ensemble de cas de même espèce, comme lors de l'approche inductive précédente (qui ne portait que sur la réfraction), mais de *spécifier* des propriétés de la réfraction grâce à l'explicitation systématique de ses similarités mathématiques avec la réflexion. L'analogie entre réflexion et réfraction apparaissait déjà, on l'a vu, chez Ptolémée. Pour Kepler comme pour Ptolémée, les similarités entre réflexion et réfraction sont à la fois d'ordre qualitatif et d'ordre quantitatif. Mais les conditions dans lesquelles se déploie l'analogie keplérienne ne sont évidemment plus celles de l'Antiquité. La tentative des *Paralipo-*

mènes est inspirée par le succès que le recours aux analogies — "mes maîtres les plus fidèles, instruits de tous les arcanes de la nature"[38], dit Kepler — avait eu dans le chapitre précédent des *Paralipomènes*, consacré à la seule réflexion. Kepler y avait développé l'analogie entre la position de l'image dans un miroir et celle dans l'eau, tout en critiquant, à ce sujet, les insuffisances de ses prédécesseurs "perspectivistes" et en leur reprochant, sur ce point également, une présupposition fonaliste et animiste.

Du point de vue qualitatif, Kepler compare la réfraction lors du passage à un milieu moins dense avec la réflexion dans un miroir convexe (l'image étant réduite dans les deux cas), tandis que le cas du passage à un milieu plus dense est rapproché de la réflexion dans un miroir concave (l'image étant dans les deux cas agrandie). L'établissement de rapports quantitatifs est nettement plus difficile et conduit Kepler à une longue dissertation sur les sections coniques en vue de découvrir des similarités entre les effets de milieux plus ou moins denses et ceux des miroirs elliptiques, paraboliques et hyperboliques. Il se lance dans des complications toujours plus subtiles, mais abandonne finalement cette voie parce que, quelles que soient les correspondances dans les "mesures", elles ne lui découvrent nullement ce qu'il avait espéré trouver, c'est-à-dire la "cause" des réfractions : "... je me suis efforcé de rassembler dans un seul et unique ensemble les mesures des différentes réfractions, et pourtant je dois reconnaitre que la cause ne réside pas dans cette mesure."[39] Ayant l'impression que l'analogie l'enferme dans un formalisme sans issue, il décide, pour sa troisième tentative, de partir de l'examen des causes : "Occupons-nous donc maintenant aussi des causes de cette mesure et que Dieu nous soit favorable!"[40]

Lorsque, en se souvenant visiblement de Kepler, Descartes abordera la réfraction dans la VIIIe des *Règles pour la direction de l'esprit*, il tirera une leçon méthodologique de l'abandon de la deuxième approche des *Paralipomènes*. Il établira explicitement comme règle qu' il est vain d'espérer passer de l'étude mathématique de la réfraction à son explication physique : celle-ci doit précéder celle-là[41]. Or, la nécessité de partir des "causes" avant de pouvoir aborder les "mesures", c'est précisément ce que reconnaît Kepler lorsqu'il abandonne sa deuxième tentative pour entamer la troisième : "Si l'explication générale que nous avons donnée ci-dessus dans les propositions optiques est correcte, l'explication particulière doit nécessairement en dériver tout aussi correctement."[42] Voilà que s'affirme donc la foi en un cheminement déductif.

Kepler : le filtre déductif

La pensée de Kepler est dominée par un double système de causalité. D'une part le monde a été créé sur le seul modèle parfait possible : Dieu, dont il est la figure anagogique. Rappelant à ce propos, dans les toutes premières pages des *Paralipomènes*, ses convictions cosmologiques les plus fermes, Kepler souligne que "la figure la plus éminente de toutes, la Surface Sphérique", a servi de modèle à la Création, parce qu'elle est elle-même l'image de la Trinité divine : le centre de la sphère du monde (le soleil) représente le Père, la surface des étoiles fixes correspond au Fils, et l'espace intermédiaire à l'Esprit qui relie les deux autres, — "et quoique le Centre, la Surface et l'Intervalle soient assurément trois, ils ne font pourtant qu'un, de sorte que l'on ne peut, même en pensée, en séparer un des autres sans que le tout soit détruit"[43]. Or, la sphère a aussi été "l'archétype" de la lumière, grâce à laquelle les parties du monde communiquent entre elles :

> Le Soleil est donc ce corps dans lequel réside la faculté de se communiquer soi-même à toutes les choses, faculté que nous nommons lumière, et de ce fait il a droit à une place au milieu de la totalité du monde, il a droit au centre, afin de se diffuser également et perpétuellement tout à l'entour. Tout ce qui participe de la lumière imite le soleil[44].

A la base de l'optique keplérienne, il y a donc une analogie qui détermine *a priori* la forme du monde aussi bien que la diffusion de la lumière. Le monde est fait sur un modèle divin, il obéit à la nécessité d'une analogie *métaphysique*. Mais si le monde est bien, du point de vue des causes finales, le plan signifiant d'une sémiosis verticale, reliant physique et métaphysique, la nécessité d'une explication spécifique des causes efficientes régissant les rapports entre les objets dans leur réalité purement physique n'en est pas pour autant éliminée. Au contraire, aucune analogie métaphysique ne peut être reconnue comme valable aussi longtemps que n'a pas été établie sa compatibilité avec "les modes d'apparition et les causes de la connexion de chaque élément" dans un ordre purement physique[45]. Indépendamment de toute figuration du divin, il importe de montrer, comme il est dit dans les *Paralipomènes*, que "toutes ces affections touchent la vue par une nécessité matérielle, dans laquelle il n'y a de place pour aucune considération de fin ou de beauté"[46]. Une explication par causalité exclusivement physique, d'où tout finalisme et toute intentionnalité propre des phénomènes est éliminée, doit garantir que les spéculations de l'homme sur le sens métaphysique du monde ne sont pas gratuites, mais ancrées dans la réalité physique en tant que telle. Tout en continuant à affirmer, et avec force, la *sémiosis* verticale du monde, Kepler pose donc également, et avec une conviction égale, l'indépendance de l'explication physique. Celle-ci ne peut pas faire intervenir des déductions de l'analogie métaphysique se rapportant à la fin de la Création. Mais une telle étanchéité est-elle possible ?

Il faut bien constater qu'en astronomie, par exemple, signification métaphysique et causalité physique demeurent étroitement liées. Si Kepler *arrive à penser* sa thèse *physique* d'orbites elliptiques dont un foyer est occupé par le soleil, c'est qu'il voit en celui-ci le symbole *métaphysique* du Père, principe générateur et source d'énergie vitale, et que ces implications métaphysiques l'invitent à se le représenter comme l'origine d'une relation dynamique qui génère le mouvement dans le monde. Même si le fonctionnement du système solaire peut être exposé en termes purement physiques de relations contextuelles entre des corps matériels, la possibilité de le penser découle, en somme, de l'analogie présupposée entre le monde et la Trinité divine qui fait occuper au soleil la place du Père[47].

Or, si un refus est opposé à l'analogie animiste en optique, la théorie de la lumière n'en est pas moins, elle aussi, travaillée par l'analogie avec la Trinité qui est à la base de l'optique keplérienne. Dans ses *Paralipomènes*, Kepler recherche aux faits des explications purement physiques, certes, mais celles-ci sont pensées *dans le champ de possibilités* délimité par l'analogie initiale.

Kepler admet que l'angle de réfraction dépend de deux facteurs[48]. La réfraction augmente, certes, avec l'obliquité de l'incidence, mais ne lui est pas simplement proportionnelle. C'est pourquoi Kepler fait intervenir un autre élément, les lignes BM de la fig. 4. Il constate d'abord qu'elles augmentent comme les sécantes des incidences[49]. Mais comme le recours à celles-ci conduit à l'absurde dans le cas de la pénétration de la lumière dans un milieu moins dense[50], il préfère retenir dans tous les cas les sécantes de l'angle formé dans le milieu plus dense (fig. 4)[51], se justifiant par le principe du retour inverse de la lumière. Pourquoi ce recours aux sécantes ? C'est qu'à une même différence de degrés ou d'obliquité correspondent des différences croissantes des sécantes : le recours aux lignes BM empêche donc la réfraction d'être simplement proportionnelle à l'angle d'incidence. D'autre part, ces mêmes lignes BM correspondent au plan de séparation des deux milieux traversés par la lumière et apparaissent, par là, parfaitement aptes à mesurer les effets de la *passion* subie par la lumière lors de son contact physique avec le milieu réfringent, la *violence* exercée par celui-ci — ce qui n'est pas le cas des perpendiculaires (comme la normale) auxquelles d'autres ont recours et dont le rôle ne s'expliquerait aux yeux de Kepler, nous l'avons vu, que par une impensable *réaction intentionnelle* de la lumière même. Le recours aux lignes BM est donc conforme à l'exigence d'une causalité purement physique ou d'une "nécessité matérielle" (la "résistance du milieu"[52]) telle que Kepler la conçoit et l'exige. Mais en même temps on voit aussi qu'il ne pense pas en termes de rayons isolés : la ligne BM délimite une zone de la diffusion *en nappes sphériques* de la lumière. Une telle diffusion avait certes déjà été admise par d'autres, mais ceux-ci n'en reviennent pas moins, dans l'analyse concrète, à l'"abstraction" que constituent les lignes ou les rayons réfractés. C'est le propre de Kepler de développer de manière cohérente tout son raisonnement *physique* en restant rigoureusement fidèle à la présupposition, nécessaire de son point de vue *métaphysique*, d'une dispersion sphérique de la lumière[53]. La conviction métaphysique constitue bien l'horizon inaliénable de la déduction physique des causes de la réfraction.

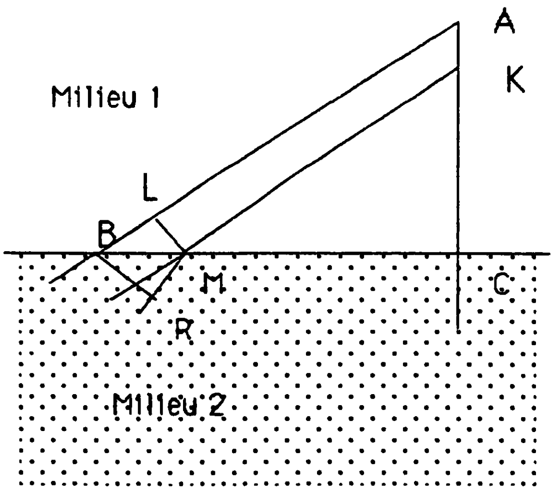


Figure 4. - Soient AB et KM "issus du soleil et quasi parallèles", leur écart correspondant à LM. Lors de la rencontre de la surface réfringente BC, Kepler prend en considération, outre la densité spécifique, deux grandeurs corrélatives : l'angle BAC et la longueur de la ligne BM. (*Paralipomènes*, IV, 6, prop. 1, 2 et 6, trad. cit., pp. 242-243.)

Si la voie déductive ne conduit pas Kepler à la loi de la réfraction, on ne saurait pourtant voir dans l'analogie métaphysique de la lumière uniquement un obstacle. Ailleurs dans les *Paralipomènes*, elle a aussi, comme le souligne Gérard Simon ""sa fécondité, puisque dans son analyse de la formation des images sous l'effet de miroirs ou de lentilles, elle l'amène naturellement à chercher ce qu'il advient de faisceaux de rayons et non de rayons isolés, et le conduit aux concepts de convergence et de divergence"[54]. Le principe de l'analogie métaphysique qui travaille la déduction agit comme un *filtre* : il limite le nombre de réponses possibles aux questions, il focalise et aveugle à la fois, conduit tantôt à l'erreur ou l'impasse, tantôt à la vérité. Il est aussi à la base de la cohérence de l'oeuvre de Kepler, dans ses réussites et ses échecs qui s'en trouvent indissociablement liés.

Snellius : succès de l'animisme

L'existence d'un manuscrit aujourd'hui perdu dans lequel Willebrord Snell énonce la loi de la réfraction (en termes de cotangentes, et non de sinus) est attestée par plusieurs témoignages. Golius (fig. 5) en informe Constantin Huygens dans une lettre du 1er novembre 1632 en précisant que Snellius s'était basé tant sur les observations de Vitellion que sur les siennes propres[55]. Isaac Vossius nous apprend que la loi apparaît dans un traité d'optique divisé en trois livres, mais inachevé[56].

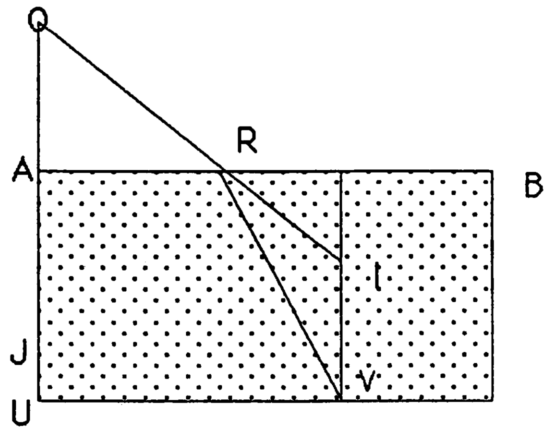


Figure 5 - La loi de Snellius (d'après Golius) : une source lumineuse *V*, au fond de l'eau est réfractée en *R* et semble, à l'oeil en *O*, venir de *I*; le rapport de RV à RI est constant pour un milieu donné (pour l'eau : 4/3, pour le verre 3/2). Snellius admet le raccourcissement du rayon vertical (vu de *O*, *U* semble situé en *J*), ce qui est nié par Huygens dans le passage cité ci-dessus.

Christian Huygens évoque, dans sa correspondance, un "livre manuscrit de Snellius (...) qui était écrit exprès touchant la nature de la réfraction et qui finissait par cette règle dont il remerciait Dieu, quoiqu'au lieu de considérer les sinus, il prenait, ce qui revient à la même chose, les côtés d'un triangle, et qu'il se trompait en voulant que le rayon qui tombe perpendiculairement sur la surface de l'eau se raccourcit et que cela fait paraître le fond d'un vaisseau plus élevé qu'il n'est."[57] Dans sa *Dioptrique*, le même Huygens déclare que Snellius avait découvert la loi "grâce à beaucoup de labeur et beaucoup d'expériences"[58]. Depuis, c'est devenu un lieu commun de dire que la découverte s'est faite grâce à l'expérience seule, à l'expérience pure. Est-ce bien vrai ?

Si nous n'avons plus l'ouvrage même de Snellius, nous connaissons au moins une sorte d'abrégé, énumérant les titres des propositions des deux premiers livres et du début du troisième[59]. Ce texte nous renseigne peu, cependant, sur la manière dont la loi a été découverte. Certes, il témoigne d'expériences très précises, mais qui ont pu servir avant tout de vérification[60]. Mais le document le plus important concernant les présuppositions de la recherche est constitué par des annotations laissées par Snellius dans son exemplaire personnel de la traduction que Friedrich Risner publia en 1572 d'Alhazen et de Vitellion et dont on ne s'est guère préoccupé jusqu'à présent à ce propos[61]. Il s'agit en quelque sorte des "paralipomènes" de Snellius. Ces notes nous apprennent, entre autres, que jusqu'en 1622, Snellius avait fait de nombreuses expériences sur la réflexion, mais non, semble-t-il, sur la réfraction. Elles nous renseignent également sur certaines idées générales de leur auteur, qui avait lu Kepler et concevait la science dans la tradition humaniste : s'il faisait des expériences, le savoir se constituait également, pour lui, par référence constante aux classiques; il cite les témoignages d'Hésiode et d'Ovide aussi bien que le traité de Philopon.

Snellius pensait toujours le mouvement en tant qu'*action* (naturelle) ou *passion* (subie). Il écrit en effet :

> Il est naturellement vrai, comme l'affirment Ptolémée, Alhazen, Vitellion et les autres spécialistes d'optique, que la Nature agit en tout selon les lignes les plus courtes. Mais il ne s'ensuit pas pour autant immédiatement que la réflexion se fait toujours selon des angles d'incidence et de réflexion égaux parce que dans ce

> cas-là les lignes sont les plus courtes. Car que nous répondront-ils au sujet des rayons réfractés dont il est manifeste qu'ils ne sont pas les plus courts, mais suivent quelqu'autre loi en rayonnant ? Lorsqu'ils assument cela, ils l'assument probablement de la nature dans son action libre, non gênée[62].

Si la théorie du mouvement fait ainsi intervenir l'opposition déjà rencontrée chez Kepler, on ne trouve pas chez Snellius la même critique de l'animisme. Au contraire, celui-ci apparaît nettement au sujet de la réfraction :

> Il est propre au visible de rayonner également dans tous les sens et aussi, parce que le rayonnement est fini, de s'exténuer si bien que, lorsqu'il rencontre un milieu plus dense dont la surface résiste au mouvement qui s'exténue, par quoi ses rayons sont rassemblés, il concentre ses forces pour pouvoir pénétrer. Et tout choc oblique est d'autant plus faible qu'il est plus éloigné de la perpendiculaire, plus il est oblique, et c'est pourquoi, pour avoir plus de force, il se concentre plus près de la perpendiculaire[63].

Le même animisme dans l'explication des faits physiques domine l'annotation suivante, qui porte toujours sur la réfraction :

> Dans un milieu plus dense, toutes choses, même celles qui sont considérées sous l'angle perpendiculaire, semblent plus proches, parce que la densité est en quelque sorte ennemie de la lumière. Car une eau trop concentrée, comme celle qui est trop profonde, engendre une certaine opacité, telle que le regard ne peut la traverser entièrement. C'est pourquoi la lumière, comme une éponge qui sent qu'on la détache, se contracte...[64]

Il apparaît clairement dans ces passages que Snellius est un héritier de l'animisme que Kepler récuse. L'interprétation qu'il donne à ses expériences, les réponses possibles qu'il en attend, peuvent donc parfaitement faire intervenir des considérations où la lumière agit en fonction d'un but à atteindre — finalisme que Kepler rejette, comme nous l'avons vu. Il n'est donc pas surprenant que Snellius ait envisagé ces rapports à la normale dont Kepler se détournait comme d'une impossibilité. Un certain "retard" théorique se révèle donc heureux, au moins dans ce cas-ci. Mais en même temps on comprend aussi que Descartes, dans l'hypothèse où il aurait eu connaissance du manuscrit, a pu juger que la loi de Snellius relevait d'un faux savoir et qu'elle était à *re-* trouver à partir d'autres principes. Les présomptions du droit à l'appropriation d'une découverte ne sont pas nécessairement basées sur une priorité chronologique. Par rapport à Snellius, à qui on l'oppose toujours, Descartes offrait une justification théorique de la loi qui était entièrement différente et qui devait lui apparaître de loin supérieure. S'il n'est pas vrai que tout peut marcher aux yeux d'un chacun, il y avait là sans doute, à ses yeux, un argument suffisant pour garder le silence sur le Hollandais et s'approprier *de droit* la découverte en tant que *loi*, c'est-à-dire susceptible d'être imposée à tous dans la représentation scientifique de la nature.

Notes

1. Sur cette question, je me permets de renvoyer à l'introduction de ma traduction française : Galilée, *Le Messager des étoiles*, Paris, Seuil, 1992.

2. P. Feyerabend, *Against Method*, Londres, Verso, 1978.

3. Sur cette notion de "poétique" scientifique, voir mon ouvrage : *La structure poétique du monde : Copernic, Kepler*, Paris, Seuil, 1987.

4. L'*Optique* de Ptolémée n'est connue que par une traduction latine d'une version arabe perdue. Il manque le livre I et la fin du livre V. Voir l'édition d'A. Lejeune, Louvain, Publ. de l'Université, 1956.

5. Précisons que pour Ptolémée, les rayons sont une commodité de mathématicien, mais n'ont pas d'existence physique. Cf. D.C.

Lindberg, *Theories of Vision from Al-Kindi to Kepler*, Chicago, Univ. of Chicago Press, 1978, pp. 15-16.

6. I désigne l'angle d'incidence et *r*, selon les cas, l'angle de réflexion ou de réfraction. Puisque Ptolémée considère que les rayons sortent de l'oeil, l'angle d'incidence correspond, chez lui, à l'actuel angle de réfraction. Jusqu'au début du XVIIe siècle, Kepler inclus, on entend par "angle de réfraction" l'actuel angle de déviation (voir *infra*, fig. 3).

7. Comme la réflexion pouvait être considérée comme un cas particulier de la réfraction, où la différence de densité entre les milieux était telle qu'elle empêchait toute pénétration et obligeait le rayon de rebondir, on peut supposer que Ptolémée espérait obtenir une relation du type : $i : r : i_1 : r_1$.

8. Lorsque, dans le cas du passage de l'air dans l'eau, les angles d'incidence sont successivement de 10°, 20°, 30°..., les angles de réfraction sont de 8°, 15°1/2, 22°1/2, etc. La différence des différences entre deux angles de réfraction successifs (15 1/2 - 8 = 7 1/2; 22 1/2 - 15 1/2 = 7; ...) sont d'un 1/2°. Cette régularité est présente dans les tables de Ptolémée, mais il ne la formule pas expressément. Il en était pourtant conscient, car il a manifestement retouché le résultat d'au moins un cas (l'incidence à 80°) : cf. G. Govi, *L'ottica di Claudio Tolomeo*, Turin, R. Accademia delle Scienze, 1885, pp. xxiv-xxvii.

9. La formule est de G. Simon, *Le regard, l'être et l'apparence dans l'optique de l'Antiquité*, Paris, Seuil, 1988, p. 180.

10. *Ibid.*, p. 176.

11. Voir la démonstration de A. M. Smith, "Ptolemy's Search for a Law of Refraction : a Case-Study in the Classical Methodology of 'Saving the Appearances' and its Limitations", *Archive for History of Exact Science*, XXVI (1982), spéc. pp. 235-236.

12. Cf. E. Cantore, "Genetical Understanding of Science : Some Considerations About Optics", *Archives internationales d'histoire des sciences*, XIX (1966), pp. 333-363. En fait, la solidarité de l'astronomie et de l'optique de l'Antiquité va beaucoup plus loin. Si l'astronomie cherchait à "sauver les apparences" (à ramener la diversité des mouvements célestes à une régularité géométrique sous-jacente), l'optique cherchait en quelque sorte, de même, à "sauver les apparences des apparences", c'est-à-dire à ramener la vision à une semblable régularité euclidienne : voir A.M. Smith, "Saving the Appearances of the Appearances : the Foundations of Classical Geometrical Optics", *Archive for History of Exact Science*, XXIV (1981), pp. 73-99.

13. Simon, *o.c.*, p. 177

14. Cf. A.M. Smith. "Ptolemy's Search..." (*o.c.*), p. 237.

15. G. Simon, *o.c.*, p. 183. Cf. également A. De Pace, "Elementi aristotelici nella *Ottica* di Tolemeo", *Rivista critica di storia della filosofia*, XXXVI (1981), pp. 123-138.

16. Pour une vue d'ensemble, cf. D. C. Lindberg, *Theories of Vision from al-Kindi to Kepler*, Chicago, Univ. of Chicago Press, 1976, chap. 4-6. Sur Robert Grosseteste, qui ne semble pas avoir connu Alhazen, voir B.S. Eastwood, "Grosseteste's 'Quantitative' Law of Refraction : a Chapter in the History of Non-Experimental science", *Journal of the History of Ideas*, 1967, pp. 403-414.

17. Au moyen âge, son traité d'optique était appelé *Perspectiva* ou *De Aspectibus*. Trad. franc. d'un ouvrage plus bref par R. Rashed : "Le discours de la lumière", *Revue d'histoire des sciences*, XXI (1968).

18. Deux arguments pour cette théorie de la *réception* par l'oeil : après avoir regardé le soleil, on continue à le voir, même les yeux

fermés; et la douleur dans l'oeil que provoque la vue d'un objet brillant tel que le soleil.

19. Le but n'est pas de résumer ici la théorie complète de la vision chez les perspectivistes. D'où quelques simplifications : ainsi, le rayon n'est en fait, pour Alhazen et les autres, qu'une fiction mathématique.

20. Witelo attribue cette prépondérance de la direction perpendiculaire à l'influence céleste (*Perspectiva*, II, théorème 47).

21. Cf. A.M. Smith, "Getting the Big Picture in Perspectivist Optics", *Isis*, LXXII (1981), pp. 568-589, surtout pp. 581-582.

22. Il faut nuancer ce qu'affirme M. Authier à ce propos ("La réfraction et l"oubli' cartésien", dans M. Serres (éd.), *Eléments d'histoire des sciences*, Paris, Bordas, 1988, p. 257).

23. Witelo, *Perspectiva*, II, théorème 47.

24. R. Bacon, *De multiplicatione specierum*, II, 3. John Pecham critique ce vocabulaire : *Perspectiva communis*, I, proposition 15.

25. Il y avait eu des éditions indépendantes de Witelo en 1535 et 1551 déjà, à Nuremberg.

26. *Paraplipomènes à Vitellion*, IV, trad. C. Chevaley, Paris, Vrin 1980. Voir également la *Dioptrique* de Kepler, axiomes 6-8, au sujet du cristal et du verre (*Gesammelte Werke*, t. IV, p. 357) : si l'angle d'incidence ne dépasse pas 30°, l'angle de réfraction est dans le rapport : r = 2/3 i. Dans un ouvrage antérieur, mais publié seulement en 1611 (*Photismi*), Maurolico avait posé pour tous les angles le rapport : r = 5/8 i.

27. Dans une lettre du 2 mars 1629, Kepler reconnaît ne pas avoir trouvé "les véritables causes qui déterminent la quantité des

réfractions" (*Gesammelte Werke*, t. XVIII, p. 388). Dans sa *Dioptrique*, il réclame néanmoins une sorte de priorité théorique pour l'invention de la lunette, en se référant à un schéma de ses *Paralipomènes* (trad. cit., p. 364) où il illustre, en les *juxtaposant*, mais ne les *combinant* pas, la divergence et la convergence produites par des verres concaves et convexes.

28. Plus tard, après avoir eu connaissance des expériences de Harriot, Kepler sera amené à distinguer la densité mécanique et la densité optique, mais, dans la *Dioptrique*, il ne reprendra pas ses recherches à partir de cette distinction. Descartes, de son côté, sera amené à insister sur la distinction entre la densité et la dureté.

29. *Paralipomènes*, trad. cit., p. 210.

30. G. Simon, *Structures de pensée et objets de savoir chez Kepler*, Lille, Univ. de Lille III, Atelier de Reproduction des Thèses, 1979, t. I, p. 485. Kepler affirme cette réversibilité dans les *Paralipomènes* (IV, 6, prop. 6. trad. cit., p. 246) et, avec plus d'insistance, dans sa *Dioptrique* (axiome III).

31. C. Chevaley dans *Paralipomènes*, trad. cit., p. 69.

32. *Paralipomènes*, trad. cit., pp. 105-106.

33. *Paralipomènes*, trad. cit., pp. 210-211.

34. *Gesammelte Werke*, t. III, p. 258.

35. *Paralipomènes*, trad. cit., p. 217.

36. *Ibid.*, p. 218.

37. Voir *Logik*, §§ 81 et 84, ainsi que l'analyse qu'en fait E. Melandri, *La linea e il circolo*, Bologne, Il Mulino, 1968, pp. 586 sqq., définissant l'analogie comme une "induction intensionnelle", alors

que l'induction au sens courant devient une "induction extensionnelle".

38. *Ibid.*, p. 224.

39. *Ibid.*, p. 241.

40. *Ibid.*

41. *Règles utiles et claires pour la direction de l'esprit et la recherche de la vérité*, trad. cit., p. 27. L'annotation de cette traduction, faite par J.-L. Marion et P. Costabel, mentionne certaines allusions aux *Paralipomènes*, mais non pas cette relation-ci.

42. *Paralipomènes*, trad. cit., p. 241.

43. *Ibid.*, p. 107.

44. *Ibid.*, p. 108. La diffusion sphérique de la lumière est déjà affirmée par les "perspectivistes". Mais Kepler la replace dans son univers (physique *et* métaphysique) à lui et la soumet à ses propres exigences épistémologiques.

45. *Gesammelte Werke*, t. XVI, p. 158.

46. *Paralipomènes*, trad. cit., p. 177.

47. Voir mon ouvrage *La structure poétique...*, chap. 9.

48. Voir les propositions I, II, III et VI de la 6ème partie du chapitre IV des *Paralipomènes*. Traditionnellement, on admet une relation additive entre ces deux facteurs, ce qui conduit à une contradiction avec des formules ultérieures. C. Chevaley (trad. cit., p. 467) propose d'y voir une relation multiplicative, ce qui lève la contradiction et est plus conforme au sens habituel des termes latins employés par Kepler. La conception d'un angle de réfrac-

tion ayant deux composantes sera critiquée par Descartes dans ses *Règles... pour la direction de l'esprit...*: voir le commentaire de P. Costabel dans la trad. cit., p. 197.

49. *Loc. cit.*, prop. II (trad. cit., pp. 242-243). Voir fig. 4. Puisque sec $\alpha = 1 : \cos \alpha$, on constate que dans le triangle BLM (où cos BML = LM : BM) séc BML = BM : LM; et aussi : BM = LM.séc BML. D'autre part, BML = BAC (puisque l'angle LBM est commun aux triangles rectangles ABC et BLM) et donc BM = LM.séc BAC. Comme AB et KM sont parallèles, LM est constant et les lignes BM augmentent donc comme séc BAC.

50. *Loc. cit.*, prop. VI (pp. 244-245). Dans ce cas, le rayon réfracté s'éloigne de la perpendiculaire. Si l'incidence est rasante (tendant vers 90°, et donc cos i tendant vers 0), la sécante de l'incidence (1 : 0) tendrait vers l'infini. Or, l'angle de déviation, loin de devenir infini, ne dépasse jamais 90°.

51. *Ibid.* Dans la fig. 4, il s'agit de l'angle RBM. Au rapport BM : LM (voir note 3), Kepler substitue donc BM : BR ou la sécante de l'angle RBM (dont le cos est : BR : BM).

52. *Paralipomènes*, trad. cit., p. 241.

53. Cf. G. Simon, *Structures de pensée...* (o.c.), pp. 507-508.

54. G. Simon, *o.c.*, p. 486.

55. *De Briefwisseling van Const. Huygens*, éd. Worp, t. I (1911), p. 263.

56. *De lucis natura et proprietate*, Amsterdam, 1662, pp. 36-37.

57. C. Huygens, *Oeuvres complètes*, t. X, p. 405.

58. *Ibid.*, t. XIII, p. 7.

59. Voir C. De Waard, "Le manuscrit perdu de Snellius sur la réfraction", *Janus*, XXXIV (1935). pp. 51-73.

60. Ainsi l'établissement des valeurs maximales de l'angle de réfraction pour le verre et pour l'eau (I, 33; De Waard, *o.c.*, p. 66).

61. Voir J.-A. Vollgraff, *Risneri Opticam cum annotationibus Willebrordri Snellii*, Gand, Plantin, 1918.

62. *Ibid.*, p. 27b.

63. *Ibid.*, p. 29b.

64. *Ibid.*

LAUDATIO Prof. Dr. H. DE RIDDER-SYMOENS

K. De Clerck

Après le professeur Robert Feenstra, que nous avions l'honneur de vous présenter l'année passée, c'est maintenant le tour au professeur Hilde de Ridder-Symoens, elle aussi spécialiste dans le domaine de l'histoire des Universités.

Hilde Symoens est née à Sint-Jans-Molenbeek le 19 avril 1943. Elle a passé une grande partie de sa jeunesse dans l'ancien Congo Belge. Elle fit ses humanités à l'Athénée Royal de Léopoldville. Après l'Indépendance du Congo (1960), elle retourna en Belgique et en octobre 1960 se fit inscrire à la Faculté de Philosophie et Lettres, Section Histoire, de l'Université de l'Etat à Gand. Après avoir obtenu, au bout de quatre ans, son diplôme de licenciée en histoire, elle fut, presque sur-le-champ, nommée Assistante à l'Université de l'Etat à Gand. De 1965 à 1969, elle assuma la fonction d'Aspirant du "Nationaal Fonds voor Wetenschappelijk Onderzoek/Fonds National de la Recherche Scientifique". La thèse de doctorat qui fut le fruit de ces recherches portait sur "Les Brabançons à la Faculté de Droit d'Orléans, 1444-1555" (*Brabanders aan de Rechtsfaculteit van Orléans, 1444-1555*). En tant que Chargé de recherches (depuis 1969), puis Chercheur qualifié (depuis 1971) au "Nationaal Fonds voor Wetenschappelijk Onderzoek", elle s'est ensuite livrée à des recherches approfondies sur la mobilité géographique des étudiants dans le Bas Moyen Age. Il en est résulté un nombre fort élevé de publications scientifiques ainsi qu'un prestige sur le plan international qui est toujours allé croissant. C'est ainsi qu'elle fut nommée e.a. Secrétaire général de la "Commission Internationale pour l'Histoire des Universités", Présidente du "Groupe de Contact [belge] pour l'Histoire des Universités", Vice-présidente du "Groupe de Contact [belge] pour l'Histoire du Moyen Age" et Secrétaire des "Belgisch-Nederlandse Historische Congressen" (Congrès historiques belgo-néerlandais). Elle fait également partie du conseil de rédaction de plusieurs

Revues. En outre, depuis 1986 elle enseigne l'histoire du Moyen Age à l'Université Libre d'Amsterdam.

Elle a successivement organisé, en 1987 (Gent), 1988 (Siène), 1990 (Madrid) et 1992 (Gent), des congrès internationaux qui ont remporté un vif succès. Lors de ces occasions ainsi qu'en d'autres circonstances, le Prof. H. de Ridder Symoens a démontré sa grande compétence en matière de sujets aussi divers que la fonction sociale des Universités européennes, l'histoire de l'éducation et de l'enseignement, la vie intellectuelle et culturelle pendant la Renaissance, et la profession-nalisation de la société.

Le Comité Sarton se félicite de pouvoir honorer, en la personne de Madame H. de Ridder-Symoens, une savante belge au renom international.

LA PLACE DES ARTS LIBÉRAUX DANS LE CURRICULUM SCOLAIRE ET UNIVERSITAIRE AUX XVe ET XVIe SIÈCLES

Hilde De Ridder-Symoens

Notre connaissance de l'organisation de l'enseignement et des activités scolaires est fondée sur deux piliers : traités théoriques et témoignages personnels. Ces derniers sont particulièrement intéressants parce qu'ils reflètent la situation réelle de l'enseignement et montrent la relation entre les différents niveaux scolaires, relation qui est déterminée par les arts libéraux. Jusqu'au XVIIe siècle, les *septem artes liberales*[1] constituent la pierre angulaire de l'éducation générale des jeunes garçons. Pour différentes raisons, qui sont liées à l'infrastructure scolaire et au caractère de l'enseignement supérieur, dès le XIIIe siècle, les universités ont eu le quasi monopole de la transmission du savoir non-manuel. Depuis la Renaissance, les universités perdent lentement ce monopole en faveur d'autres institutions d'enseignement supérieur (académies, écoles spéciales, etc.). Une bonne connaissance des arts libéraux était considérée comme la base nécessaire et idéale pour pouvoir suivre les cours dans les facultés supérieures de théologie, de droit et de médecine. Mais les arts libéraux possédaient une finalité en soi. Ils procuraient la formation intellectuelle et la culture générale (introduction non-spécialisée dans les branches principales du savoir) de "l'intellectuel" au moyen âge et aux temps modernes. Près de trois quarts des étudiants quittaient l'université après quelques années d'études dans la seule faculté des arts, avec ou sans diplôme final (*maîtrise ès arts*).

Le bagage intellectuel exigé au moyen âge par la faculté des arts à l'entrée était limité à une connaissance du latin suffisante pour pouvoir suivre les cours universitaires. Les futurs artiens (étudiants ès arts) pouvaient apprendre la grammaire latine dans les écoles capitulaires, les écoles latines urbaines ou par des cours privés. Dans le courant du XIVe siècle et plus encore au XVe siècle, plusieurs facultés d'arts commencèrent à organiser cet enseignement préparatoire, d'un côté pour subvenir aux déficiences manifestes des *novicii*, d'un autre côté pour attirer des futurs étudiants à un âge très jeune (8 à 9 ans). Ce système était, à la fin du moyen âge, le plus élaboré au sein de l'Université de Paris. Les écoliers y sont accueillis dans des pensionnats privés ou dans les collèges universitaires. Dans ces derniers, des communautés indépendantes d'écoliers-pensionnaires sont mises sur pied dès la fin du XIVe siècle. On les nomme collèges d'exercice. Une fois que le niveau de connaissances requis était atteint, les écoliers entamaient leurs études dans la faculté des arts et devenaient ainsi de vrais étudiants universitaires. Vers 1500, les collèges d'exercice introduisirent une répartition des élèves en groupes de niveau homogène, ordonnés graduellement selon le niveau de connaissances. Cette organisation "classicale" a été introduite à Paris par des anciens élèves des écoles de la Dévotion Moderne, en particulier au Collège de Montaigu par Jean Standonck (1490-1504), *alumnus* des Frères à Gouda et à Louvain. Erasme, Vives et Calvin y passèrent quelque temps. En effet, les Frères de la Vie Commune avaient, dans les anciens Pays-Bas, mis sur pied tout un réseau de pensionnats et d'écoles latines où les élèves recevaient une initiation progressive à la grammaire latine et à d'autres contenus de savoir artien. En outre, les anciens Pays-Bas n'étaient pas dépourvus d'écoles urbaines, qui étaient souvent de très bonne qualité.

Il s'ensuit que, vers 1500, les futurs artiens étaient souvent tellement bien préparés qu'ils pouvaient aisément suivre le cursus universitaire ès arts et même sauter certains cours. C'est ce que nous apprend notamment le journal de Maarten Snouckaert.

En 1567, Maarten Snouckaert, seigneur de Zomergem (près de Gand), a écrit un livre de mémoires (*memorieboek*) qui donne, outre quelques renseignements économiques, une description brève mais détaillée de sa jeunesse et de son éducation. C'est le fait que cette éducation différait peu de celle des autres garçons de son milieu bourgeois qui rend ce petit livre si intéressant. Il y a en effet peu de témoignages de ce genre pour le moyen âge et le XVIe siècle.[2]

Après sa naissance à Gand en 1514, Maarten Snouckaert fut confié à une nourrice pendant 15 mois. Après son retour au foyer familial, sa mère l'initia graduellement à la religion catholique. La famille Snouckaert déménagea à Bruges lorsqu'il avait trois ans. A cinq ans, on l'envoya chez un maître particulier pour y apprendre à lire, et à sept ans, il fut introduit par un autre maître à la lecture et aux premières notions de latin. Un an plus tard, en 1522, on le plaça chez un chanoine du chapitre de Sint-Donatien à Bruges, qui lui donna des cours chez lui à la maison. Maarten y apprit les rudiments de la grammaire et de la langue latines. Les livres scolaires employés étaient humanistes (une version retravaillée de la Doctrinale d'Alexandre de Villedieu par Herman van der Beek et les colloques d'Erasme et de Peter Schade Mossellanus). Afin d'apprendre le français, Maarten et son frère, plus jeune de deux ans, furent ensuite envoyés à Lille. Ils y logèrent chez un prêtre et suivirent des cours à l'école capitulaire de 1525 à 1528. Le père Snouckaert était fâché que les garçons apprenaient peu de français à Lille. Selon Maarten, c'était inévitable, puisque les élèves étaient obligés de parler le latin uniquement. A Lille, la pédagogie humaniste et les auteurs humanistes avaient également fait leur entrée dans l'école du chapitre. Après Pâques 1528, âgés respectivement 14 et 12 ans, Maarten et Willem furent envoyés à l'école renommée des Frères de la Vie Commune à Gand. En plus des cours de religion, de grammaire et de stylistique latine et grecque, on y enseignait également des matières qui appartenaient au paquet traditionnel des arts libéraux (littérature et logique).

En 1529, le père Snouckaert jugea que ses deux fils avaient acquis assez de connaissances pour commencer les études universitaires. Maarten avait alors 15 ans, Willem 13. Comme il était d'usage, le choix tomba sur l'Université de Louvain. Ils s'inscrivirent à la faculté des arts, dans la pédagogie de la Lis. Ils logèrent cependant chez des amis, et non pas dans la pédagogie de la faculté.

Comme les garçons avaient déjà appris la logique de Petrus Hispanus chez les Frères gantois, ou *Fraters*, comme ils étaient nommés, ils furent dispensés de la première année de logique. Ils purent approfondir immédiatement la physique ou philosophie naturelle, l'éthique et la métaphysique (un an et demi d'études). Le père jugea inutile de leur faire passer les examens finaux pour obtenir la maîtrise ès arts puisque, selon lui, on trouvait "plus de maîtres que d'hommes sages dans la faculté des arts". En janvier 1531, les frères Snouckaert s'inscrivirent au Collège des Trois Langues, fondé en 1517 par Jérôme Busleiden en marge de la faculté des arts de Louvain. Ils y suivirent notamment les cours de l'humaniste Goclenius sur les oeuvres de Cicéron et du poète Lucane. En même temps, ils entamèrent les études de droit dans la faculté et en cours privé.

Malgré le fait que leur père était décédé entre-temps, les deux Snouckaert partirent en 1534 pour la France, en pérégrination académique. Se perfectionner dans la langue française était un des buts de ce voyage. Leurs tuteurs optèrent pour l'université de Poitiers, où ils n'étaient d'ailleurs pas les seuls Flamands. A côté de leurs études de droit, ils suivirent les sermons en français afin d'apprendre la langue. En raison de la menace de guerre, les frères quittèrent Poitiers après quelques mois, avec un certain nombre de concitoyens. Willem se rendit à Paris pour y étudier le grec et les mathématiques; Maarten poursuivit ses études de droit à Toulouse et Cahors. Après toute sorte de péripéties, Maarten obtint en 1539 à Toulouse son baccalauréat en droit d'abord, et tôt après sa licence. Pendant son voyage de retour, il rencontra son frère Willem à Paris. Celui-ci termina ses études par un doctorat en droit.

Ce rapport détaillé sur l'éducation scolaire des deux Flamands de la bonne bourgeoisie contient bien des éléments mentionnés plus haut. Il montre comment, à la fin du moyen âge, sous l'influence des pédagogues et professeurs humanistes, les écoles "secondaires", sans oublier les maîtres privés, ont repris une part des tâches des facultés d'arts et, par là, se sont lentement développées en un réseau scolaire homogène qui préparait à l'enseignement supérieur. Dans les pays catholiques, le système a été perfectionné par les jésuites. Dans les pays protestants, les écoles latines — *gymnasia* — se fondent sur les mêmes concepts humanistes; ils diffèrent à peine de leurs contreparties catholiques. Cette division en niveaux scolaires (primaire, secondaire, supérieur) avait des conséquences importantes pour l'enseignement dans les facultés d'arts.

Comme le niveau de connaissances des nouveaux inscrits était de plus en plus élevé, les professeurs ès arts pouvaient se consacrer à d'autres matières dans le domaine de la philologie et des lettres, de la philosophie et des sciences naturelles. Ces changements dans le contenu de l'enseignement universitaire ne se sont opérés que lentement, d'abord en Italie, puis dans les autres pays. Aux Pays-Bas certainement, les écoles latines du niveau secondaire ont plus vite emprunté la didactique et le contenu humanistes que les facultés d'arts au Nord des Alpes, Louvain inclus. De ce fait, il était possible que des jeunes Néerlandais[3] eussent été familiarisés avec les *studia humanioria* (les humanités) dans les écoles latines et que, par la suite, ils eussent été confrontés avec une approche scolastique (dialectique) dans les facultés d'arts à Louvain, Cologne ou Paris.

Ces changements se reflètent également dans les titres. Dans le courant du XVIe siècle, les facultés d'arts changent souvent leur noms en faculté de philosophie.

Les collèges en exercice continuaient à fonctionner. Ils gardent un certain lien avec l'université, mais ils doivent être considérés comme des "écoles secondaires". A Louvain, une école préparant à l'Université, le Collège de la Sainte-Trinité, fut installé officiellement en 16??. Bien que les élèves fussent inscrits dans la matricule universitaire, ils ne pouvaient pas être considérés comme des étudiants universitaires. A Douai, l'université appartenant aux anciens Pays-Bas jusqu'en 16??, les jésuites dirigèrent un pareil collège dans les bâtiments de l'abbaye bénédictine d'Anchin. Ils y préparèrent les écoliers à l'université et à la prêtrise.

La question qui se pose sur la place des arts libéraux dans l'enseignement, secondaire et/ou universitaire, reste très actuelle. Elle est dans le coeur des débats sur l'éducation aux Etats-Unis.[4] Les contenus de savoir que les Américains appellent *liberal arts* ne sont presque pas enseignés dans la *Senior High School* (élèves de 16 à 18 ans), mais dans les deux premières années du *college* ou *university* au niveau d'*undergraduate*. Après cette "éducation générale" qui aboutit au baccalauréat (BA, *bachelor in arts*), les étudiants continuent leurs études dans les *professional schools* (droit, médecine, génie civil, etc.) ou les *graduate schools* (lettres, histoire, sciences naturelles, etc.). Cependant, une majorité des étudiants quittent le collège ou l'université après avoir obtenu le baccalauréat.[5] Une des raisons pour lesquelles les arts libéraux (*liberal arts*) aux Etats-Unis sont enseignés au niveau universitaire et non pas au niveau secondaire comme en Europe aujourd'hui, est une conséquence du fait que l'enseignement supérieur y fut organisé avant l'enseignement secondaire, et que ce dernier ne s'est jamais développé jusqu'au niveau européen des deux dernières classes des collèges, lycées, athénées, gymnases, etc.[6] Ceci, en fait, correspond jusqu'à un certain degré à la situation en Europe avant les changements qui — notamment, mais pas uniquement sous l'influence de l'humanisme — se sont opérés tout à la fin du XVe et surtout au XVIe siècle.

Notes

1. Les sept arts libéraux, ainsi définis au haut moyen âge, regroupent toutes les disciplines qu'un homme cultivé est supposé maîtriser. Les trois premiers arts (le *trivium*) initient au langage (grammaire, rhétorique, logique ou dialectique); les quatres autres (le *quadrivium*) à la science des nombres (arithmétique, géométrie, astronomie, musique). En réalité, le *trivium* était de loin plus important dans le cursus scolaire en général.

2. Le résumé du journal qui suit est tiré de l'article suivant : P. Vandermeersch, Een uitzonderlijk egodocument : Maarten Snouckaert (1514-1569?) over zijn jeugd, zijn opvoeding, zijn studies, *Liber Amicorum Achiel De Vos* (Evergem 1989) 219-228.

3. Néerlandais considéré ici comme ressortissant des anciens Pays-Bas ou Dix-Sept Provinces (à peu près le Benelux actuel).

4. Un séjour de trois mois (printemps 1993) au 'Center for Studies in Higher Education' de l'Université de Berkeley, Californie, m'a donné l'occasion de connaître et de comprendre, ce qui n'est-pas forcément la même chose, un peu le système universitaire

américain. J'y ai été frappée par les discussions menées sur la place que la formation générale, c.-à.-d. les arts libéraux, doivent recevoir dans l'enseignement (supérieur ou secondaire). Voir à ce sujet notamment : S. Rothblatt, The Limb of Osiris : liberal education in the English-speaking world, in S. Rothblatt & B. Wittrock (eds.), *The European and American University since 1800 : historical and sociological essays* (Cambridge U.P. 1993).

5. D'après des chiffres récents, trois quarts parmi eux (C.A. Ottinger, *Higher Education Today : Facts in Brief*, American Council on Education. Division of Policy Analysis and Research, May 1990, p. 55).

6. M. Trow, Comparative perspectives on British and American higher education, in S. Rothblatt & B. Wittrock (eds.), *The European and American University since 1800 : historical and sociological essays* (Cambridge U.P. 1993), 288-289.

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LAUDATIO ROGER BLONDEAU

Michel Thiery

Ladies and Gentlemen,

It is my pleasure to introduce to you Mr. Blondeau or, more appropriately, to comply with a tradition, by reading the laudatio of a man whom most of you certainly know through one or several of the exciting books he has written, be it on the bibliobiography of famous Flemish scientists or on the rise of the iconoclastic "movement" in the "Westhoek" (western corner of Flanders) in the early 16th century.

The person who hides behind the bulk of printed pages is less well known, however. Unassuming, not to say shy, Blondeau is the bee (or should we put it the bumble-bee ?), laboring quietly but with the tenacity characteristic of the true West-Fleming to produce those marvels. In his beautiful house with sign-board "Ter Wijngaerde" ("In the Vineyard", no doubt chosen because he is a true "burgundian", in the Haringestraat (Haringstraat after the animal species which long ago was the richess of that part of Flanders and which during the last World War has saved the population from starvation) of Roesbrugge (between Veurne and Poperinge and close to the French border), is a haven of peace and tranquility. Here Blondeau thrones — flanked by his spouse and his sister-in-law — in the middle of a room cluttered with books and archivalia.

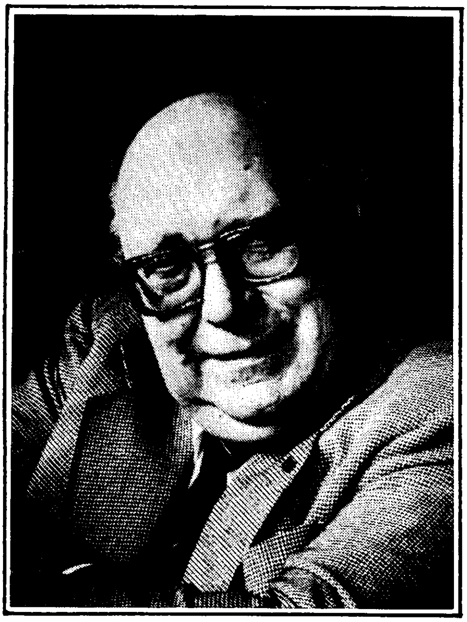
Roger-A. Blondeau was born in the aftermath of the Great War — on april 18, 1919 — in Beveren-aan-de-Ijzer, another historical place. Having completed his classical secondary studies with flying colors he entered the State University of Gent as a student in mathematics and physics. The outbreak of the Second World War put an end to his dream to become an astronomer. The doctorandus, who had to make a living for himself, was compelled to quit the Alma Mater and became a civil servant. But his mathematical vein and complementary studies (mostly at night) of fiscal science were put at his advantage : he rose from the ranks and ended his professional career as honorary inspector at the Ministry of Finance. Nor did the lengthy detour prevent him from taking up the study of topics paralleling his original study direction. Indeed, in his spare time Blondeau started to explore the historical background of physics and mathematics and, above all, of astronomy. He became interested in the complex and perplexing life and deeds of Fernand Verbiest (born in Pittem in 1623), the Flemish Jesuit who for two decades dwelled in China holding the leading function of "calendarian" to K'ang-si and directing the emperor's observatory. In 1970 this fascinating story was completed and made the subject of Blondeau's first opus : *Mandarijn en astronoom, Fernand Verbiest s.j. aan het hof van de Chinese keizer* (Mandarin and astronomer. Verbiest at the court of the Chinese emperor). Blondeau's flirtation was bound to grow into a lifelong affair, and he will come back to it in two more books : *Fernand Verbiest s.j.. Als Oost en West elkaar ontmoeten* (Verbiest. When East and West meet) in 1983 and five years later *Fernand Verbiest s.j. als wetenschapsmens* (Verbiest as scientist). The latter is an in-depth analysis of Verbiest's endeavors and of the impact the missionary-astronomer and his fellow brothers had on the evolution of the natural sciences in 17th-century China.

The Verbiest episode was not an endpoint. From 1970 on Blondeau started his biobibliographical investigation of a long list of Flemish scientists, from Jacob van Maerlant to the foundation of the Academies. Many of these studies were published as articles. Later on they were reshaped to form a coherent whole : *Wetenschap in de taal der Vlamingen* (Science in the tongue of the Flemings). This book was issued this very week and I wish you to circulate a copy among this audience. There is more in the making and if my information is correct Blondeau is currently drafting a book on "our" Mercator, the geographer and cartographer who is to be officially commemorated (he died in 1594) in 1994. There seem to be two more books in the pipeline : one about Simon Stevin, the other about Jan Palfyn.

Circumstances did prevent Blondeau from pursuing his academic studies but the harvest of his "sideline" activities has been most impressive. Fortunately, his work was fully recognized. Blondeau was elected a member of the "Belgian Committee for the History of Sciences" and of the "Permanent Commission for the History of Sciences" of the Royal Academy. He was granted the prestigeous Professor Gillis price for the History of Sciences in 1974.

Gent did not wish to lag behind and a year ago Blondeau was proposed by the Faculty of Applied Sciences as a candidate for the G. Sarton Memorial Medal. The members of the *ad hoc* committee applauded and confirmed the proposal. And that was it.

My dear Roger : we have known each other for many years, be it mainly telephonically and by correspondence. Whenever I needed some data or an elusive document you obliged me by delving in your fantastic archives. Today it is my privilege to convey to you the gratitude of our Alma Mater, your Faculty and the Sarton Committee. Accept my most cordial congratulations. To Mrs. Blondeau we extend our deep respects. Ad multos annos !



THE LONG AND DIFFICULT TRANSITION FROM PTOLEMY TO COPERNICUS

R.A. Blondeau

Many hypotheses concerning the Creation and world view were already in existence during the oldest civilisations.

Due to a lack of knowledge and insight, these hypotheses became so entangled with magic elements that they are now spoken of in terms of myths and legends. The Ancient Greeks rationalised the presentation of this world view and turned it into an acceptable form.

Sixty years ago, Marnix Gijsen paid an exceptional tribute to the Greeks in his booklet *Oduseus Achterna*. He said, amongst other things : 'It is the duty of every civilised man to be grateful to Greece and its culture. The Greeks taught us to think and reason. They gave us the archetype of every form of art; from the ornamental scribble on pottery to the complete image, the rousing drinking song to the faultlessly constructed drama, the heroic epic to the novel'.

But, he neglected to mention that the Ancient Greeks had also considered every possible variation of every modern scientific theory. From the shape of the Earth, the creation of the Universe, the structure of matter to the evolution in biology.

Several Greek philosophers of physical science attempted to bring the rotation of the celestial bodies into a more balanced system. Clever as some of them might have been, they were all overshadowed by Aristotle!

Aristotle worked following his own philosophical thinking, both in matter and natural position, and in movement. Referring back to Plato and Eudoxus of Knidos, and based more on reasoning than observation, he saw things quite simply. He was convinced that the Universe was made out of perfect geometric shapes : the circle and the sphere. The Universe was, according to him, a collection of concentric spheres which fitted into one another, with the Earth as the stationary centre. The fixed stars were attached to the outside sphere, but the Sun, the Moon and the five known planets had their own spheres. He adjusted their rotational axes and rotation speeds so that they imitated the movements of the celestial bodies as closely as possible.

The movements of the planets were particularly difficult to realize. Although they were all part of the general turning direction, from time to time they slowed down, stopped for a instant, turned back a little and then continued their orbit. In order to follow all these variations as reliably as possible, new rotational spheres had to be added, until there were no less than fifty five.

How the mechanism actually worked is not clear. The spheres were kept in movement by an incorporeal substance and behind the sphere of the fixed stars there was a prime mover. The design had come into being four centuries B.C. and was not completely free of magic and religious elements. But taking everything into consideration, up to that moment in time it was actually the most scientific explanation concerning the shape of the Universe. This system, supported by Aristotle's authority lasted for six centuries.

After more accurate observations of the celestial bodies it appeared that something was wrong with Aristotle's world view. Because the space was limited, adjustments were no longer possible. This was the reason why, in the second century A.D. the Greek scholar, Claudius Ptolemy, eliminated the concentric spheres and created a new design. It was made up of free-moving, circling celestial bodies. Aristotle's mechanical model had to give way to a mathematical theory.

Not much is known about Claudius Ptolemy. He worked in Alexandria — founded by Alexander the Great in the fourth century B.C.

— as an astronomer, a geographer and a mathematician. A sort of university with famous libraries was founded and Alexandria became the scientific centre of the Ancient World.

Ptolemy wrote several studies, amongst which at least three have survived many centuries :

1° The work in which he explains his world system, *Syntaxis Mathematica*, better known as the *Amalgest*;
2° A handbook of geography, *Geographia*, from which the first printed Latin translation was published in Italy in 1475;
3° *Tetrabiblos*, the basic book of astrology. This claims that the stars have an influence on the character, behaviour and events in a person's life. This latter work is still used in some circles.

When developing his world view, Ptolemy kept to the main basic principles of Aristotle which were :

- The Earth is the stationary centre of the Universe;
- All movements are circulative.

However, Ptolemy's elaboration was not only more refined, it was more in accordance with the phenomena. It was more logical, deeply thought out, and far more complicated — particularly concerning the orbits of the planets.

The Sun and the Moon orbited the Earth in a simple circle, while the planets were placed on smaller circles, the epicycles. The centre of the epicycle also orbited the Earth. This way Ptolemy knew more or less how to deal with the strange regressive movements of the planets.

This system of epicycles, in which the centre ran through a bigger circle was called a deferent but was not Ptolemy's invention. It had been thought up much earlier by the mathematician Appolonius of Perga, who laid the foundation for the study of conic sections.

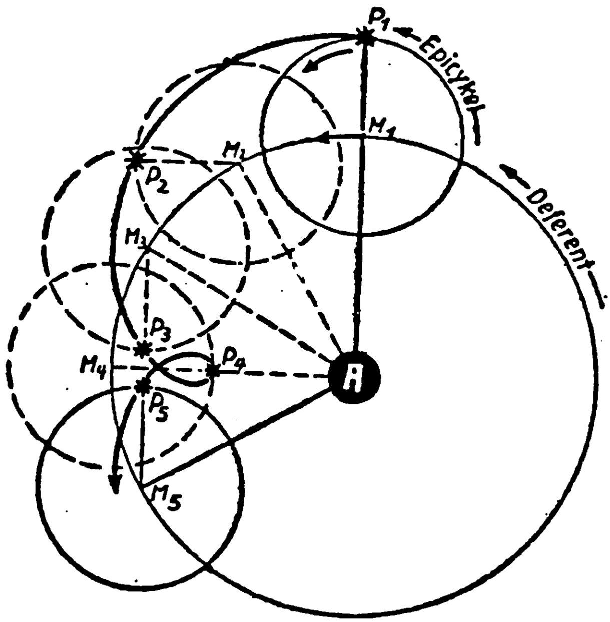


Fig. 1 : The system of Ptolemy. Planet P moves on the epicycle; Centre (M) of the epicycle moves on the deferent around the Earth.

These epicycles alone were not enough to be able to follow all the movements and changes in size and luminosity of the celestial bodies. Therefore the centre of some circles had to be placed 'offcentre' from the Earth, they in turn described a circle which also had its own eccentricity. The reader will be spared any more circles and complicated movements, but it must be pointed out that Ptolemy needed 42 circles, twisting and turning within each other, in order to follow the Sun, the Moon and the five planets.

When Ptolemy published his world view, Greece had become a Roman province only a couple of years earlier. The Romans took over many ideas from the Greeks, but they did not produce any scientists who worked on Ptolemy's system. After the fall of the Roman Empire, during the fifth century, the barbarians spread like a destructive flood over

Europe. With the advent of the Merovingians and the Carolingians a slow progress towards our own civilisation started but nothing was known about the written and scientific studies of the Ancient Greeks.

The Arabic civilisation, which had adopted many ideas from the Greeks, accepted Ptolemy without question. When, in the 9th century, Mohammedan knowledge and learning was dying in the East, in the West it was enjoying a revival. The movement started in Spain, in the cities of Cordoba and Toledo.

There was a great interest for Arabic learning growing in Europe and soon a stream of classical books was being translated from Arabic into Latin.

Ptolemy's work had already been translated into Arabic by Gerardus of Cremona in 1175, so it was available for the Western world. It is not known to what extent this translation was distributed — in those days printing had not yet been invented — but the Western World must have known about Ptolemy's system because so many doubts had been voiced by sceptics from the 12th to the 15th century. Some of them had more admiration for Aristotle's concentric spheres than for Ptolemy's complex circles.

This point of view can be illustrated by a typical quote from King Alphonse X of Castile. Whilst attending an astronomical congress, halfway during the 13th century, it is said that he was rather condescending about Ptolemy's system : 'If God had asked me for advice during the Creation, it would have been a lot simpler!'

Even though the great Dominican theologian and philosopher Thomas of Aquino had introduced Aristotle's philosophy into the learning of the church, and had declared himself radically in favour of the geocentric system — or more specifically the antropocentric system — and even though he had been canonised in 1323, it was actually two priests who, in their writings in the 14th and 15th century, first mentioned a moving Earth in order to explain the celestial phenomena.

Nicolas of Oresme was one of these priests. In the 14th century he lectured in Paris and later became Bishop of Lisieux, and in the 15th century more importantly there was Nicolas Cusanus.

Cusanus, who became a Cardinal, was born in Kuss — which explains his name — situated on the left bank of the river Mosel, between Trier and Cochem, opposite Bernkastel. Cusanus was a versatile man and in his thinking he could be considered as a typical transitional figure (stepping stone) between the Middle Ages and the New Age, between the mystic and physics. He not only wrote a series of philosophical and theological works, but also dealt with subjects in physics : movement, weights and measures. He compiled a map of Central Europe and collected astronomical instruments, including a celestial globe, which is said to be the oldest in Germany.

Together with many manuscripts and incunabula these instruments can be seen in a wonderfully restored library, in the St Nikolaus hospital, a home for the aged that he founded in 1458. This institution could house 33 old people (from the age of 50 onwards in those days). Everyone in the home was entitled to a daily ration of half a litre of Mosel wine. The wine came from the family vineyard of the Cardinal. This must have been a very efficient and highly appreciated geriatric treatment, long before geriatry was acknowledged as a medical discipline.

Even though Cusanus strongly distanced himself in his writings from the certainty which surrounded the existing cosmological ideas, but only in vague terms, his opinion was that the Earth was a star and it moved just as the other stars did, but he gave very few details. In one of his pamphlets, probably in 1444, he did state that the Earth turned on its axis, but there was no mention of a yearly rotation around the sun.

Cusanus is presented as the predecessor to Copernicus. This is rather an exaggeration. In fact Copernicus did not have a predecessor. He was unique. Where others suggested in a few sentences that the Earth moved, he gave the system a geometric shape — accurately calculated in all its consequences. His work comprised more than 200 pages of text and 146 explanatory geometric drawings.

Nicolas Copernicus was born in 1473 in the town of Thorn. Thorn was in fact a West Prussian city, but seven years before his birth, West Prussia had become a Polish province. His father, grandfather and great grandfather were actually born in Krakow, so it could be said that he was Polish.

He lost his father when he was ten and was brought up by his mother's brother, a priest who later became a Bishop. He studied mathematics and astronomy at the University of Krakow and then went to Italy where he enrolled at the University of Bologna. He continued to study the same subjects for four more years and also became proficient in Greek grammar. He then went to Ferrara to obtain a degree in ecclesiastical law and afterwards to Padua to study medicine.

At the age of thirty four he returned to his homeland where, for six years, he acted as secretary and personal physician to his uncle who, in the meantime, had become Bishop of Ermland in Heilsburg. Whilst working for his uncle, he devoted himself to astronomical observations and studied many classical works on astronomy.

Thus Copernicus was a scholar in astronomy, medicine and the law. Until the late 17th century there was a strong affinity between astronomy and astrology and medicine. It was believed that the stars and constellations had an influence on limbs and organs and so the position of the celestial bodies was consulted in diagnosis and prognosis of the sick. It must not be forgotten that in 1430 *astrology* was taught as a science at the University of Louvain, by Professor Jan van Wesel, Doctor of Medicine and great grandfather of Andreas Vesalius. One hundred and fifty years later Pope Sixtus V issued a papal edict in which he condemned astrology, but permitted astrological predictions as long as they only concerned the weather, agriculture and health!

During the years that Copernicus was secretary to the Bishop of Heilsberg, his heliocentric world system began to take definite shape. In the dedication to Pope Pius III in his master-work (which would be published more than thirty years later) he said he knew, through studying the writings of the old philosophers, that they had varying opinions over the question of whether the Earth was in movement or not. His professor of astronomy in Bologna, Dominico Maria di Novara, who also dared to think that the Earth might move, obviously had a great influence on his work.

Before 1514 (the precise year is unknown), he wrote a short dissertation, in which he developed the most important points in his heliocentric hypothesis. This dissertation was not printed, but copies of it were sent to friends and acquaintances and then handed down throughout the years.

There was no response to the first dissertation. It not only propounded the unlikely suggestion that the Earth orbited the Sun, a fact that was unsubstantiated but it also contained serious astronomical mistakes.

Copernicus spent many more years adjusting his system with the help of his latest findings and observations. After his uncle's death in 1512 he moved to Frauenburg where he became a canon at the cathedral.

In 1539, Georg Rheticus, a young German scholar, visited Copernicus in order to become familiar with his theories. Rheticus was a protestant and a professor at the University of Wittenberg where Luther had started the Reformation twenty years before.

Two months later Rheticus wrote a compact summary of Copernicus' work — the *Narratio Prima* — and had it printed in Danzig in 1540. This short essay had a certain popularity and was reprinted in Basle the following year.

In the meantime Copernicus had finished his master-work on the rotation of the celestial bodies. He kept this a deep secret because he was afraid of the reactions it might cause.

Earlier, in 1539, Luther, who had heard about Copernicus' work remarked in one of his after-dinner speeches : 'This fool will reverse the whole art of astronomy, turn it upside down; but in the Holy Scriptures it is written that it was the Sun and not the Earth that Joshua commanded to stand still.' Above all there was Copernicus' own uncertainty. This uncertainty must have been present despite what was later claimed and this is understandable when one realizes that he was the first to be diametrically opposed to the doctrines of Aristotle, Ptolemy and the holy Thomas of Aquino. Doctrines which had dominated for centuries.

He had no hard evidence for his system, but based it on the fact that the movements in the Universe were easier to explain if one assumed that the Earth and the Planets orbited the Sun and that the Earth rotated around its axis every twenty-four hours. For the rest he only had aesthetic thoughts, which he noted as follows (page 9 of his book) : 'Seated in the middle is the Sun. Who would wish to move this lamp to another place in this beautiful temple from which she spreads her glowing light? The Sun, as if seated on a royal throne, rules over a family of celestial bodies who orbit around her. We find through this positioning a remarkable balance in the world and a harmonious link, which cannot be achieved in any other way.'

As a result of the success of Rheticus' compact summary, Copernicus decided to publish his master-work after all. The task of supervision was again entrusted to Rheticus who, in 1542, had been appointed professor in Leipzig. He, in turn, passed on the supervision of the publishing and distribution to his friend Andreas Osiander, a well-known Lutheran theologian. It was printed in Neurenberg and appeared in 1543. Copernicus would have received a copy just before he died on May 24th of that year.

The work consisted of six "books", in the sense that each book was a chapter. It began with a preface entitled : *To the reader concerning the hypotheses in this work*, in which is written : 'These hypotheses are not necessarily true or plausible, it is sufficient that they make the calculations possible and that they produce results which are in accordance with the observations.'

At first this preface appeared to have been written by Copernicus himself. It was not signed separately and clearly formed an integral part of the work. But the ideas presented in the book were seriously weakened by this preface, because of its content which was full of hypotheses and implausibilities.

In the first book Copernicus deals with the sphere, the shape of which characterizes all the celestial bodies and within which, he believed a rotating axis was intrinsic. He goes on to clearly describe the heliocentric system, which he invented :

- He places the Sun in the centre and allows the planets and the Earth to describe a perfect circular orbit around it. Only the Moon orbits the Earth. The Sun and the far distant field of the fixed stars are stationary.
- He demonstrates that the movement of the Earth when it orbits the Sun causes the moving planets to be seen from different angles from which the apparent strange regressive movements originate. In his system he did not need Ptolemy's epicycles to explain these movements, therefore it became much simpler.

Those who think they know everything about Copernicus' work after having only read the first book will become extremely enthusiastic about the simplification of the heliocentric model. Many readers of Copernicus have not actually got further than the first book because the following books are considered as an explanation for mathematicians, written in stiff, difficult language, containing many geometrical arguments.

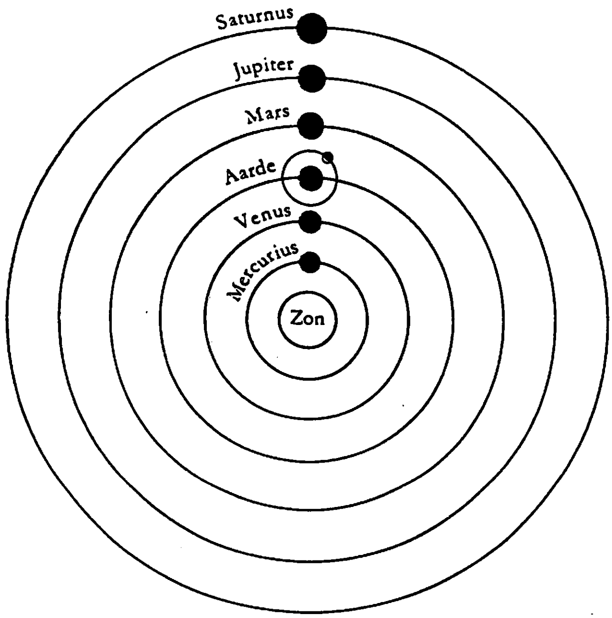


Fig. 2 : This diagram is taken from Copernicus' first book. It represents his system in a simplified way.

In these books, where all the observed movements are described in detail, an extremely complicated construction is presented. In order to follow all the movements and deviations in the firmament as correctly as possible, Copernicus, amazingly enough had to use again the epicycles and eccentricities.

Whereas Ptolemy needed 42 circles, Copernicus finally ended up with 38, so the simple, attractive image of the first book was effectively destroyed.

In some history books the impression is given that — with the appearance of Copernicus' work in 1543 — a turning point in scientific thinking was reached and that this happened overnight. This was far from the truth! The transition from Ptolemy to Copernicus, more specifically from the geocentric to the heliocentric thinking, was a slow and difficult process, even after 1543!

De Revolutionibus Orbium Caelestium. This is the title of Copernicus' work and about a 1000 copies would have been printed. But the circulation took place so slowly that, for a long time, Ptolemy's system was hardly affected.

On February 23rd 1547, about four years after the appearance of Copernicus' work, the Flemish cartographer, Gerard Mercator, wrote a letter to one of his patrons, the Bishop of Arras. This letter dealt with the magnetism of the Earth and the magnetic pole. In his explanation it can be seen that he adheres to the theory that the Earth is the centre of the Universe.

Mercator made geographical maps, astronomical apparatus, terrestrial and celestial globes, and was in touch with geographers and astronomers. He lived and worked in Louvain in the shadows of the university and must certainly have known about Copernicus' system yet he did not even mention it. Just like all the other scientists, he ignored it.

Indeed Copernicus' hypotheses were not accepted as reality because they conflicted with day to day experiences. The same arguments as those which Aristotle had put forward eighteen centuries before — in which he had refused to accept a rotational Earth — were still in use.

The rotation of the Earth around its axis every 24 hours was not compatible with the stability of buildings. A bird flying independently from the rotating Earth but in the same rotational direction, would not progress but fall behind. A stone dropped from the top of a tower would not land at its foot but, because of the rotation of the Earth, end up somewhere westwards of the tower.

The movement of the Earth around the Sun also caused confusion. If two stars were observed on a particular day, they could be seen at a certain angle. But six months later, after the moving Earth had completed half an orbit and was approaching these stars, they could be seen at a greater angle or parallax, creating the impression that they had diverged. Take an avenue of trees as an example : upon entering it, in the distance the trees look as if they were planted touching each other, but upon penetrating deeper they diverge.

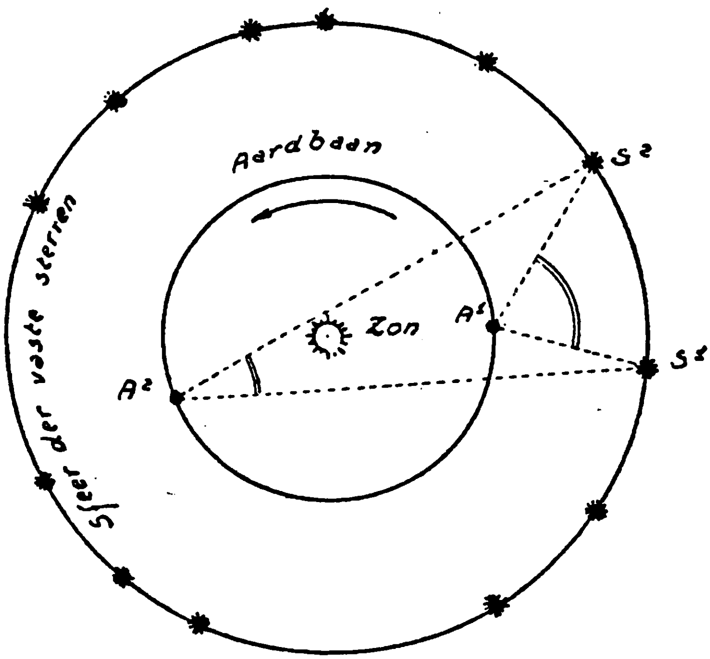


Fig. 3 : Stars S1 and S2 are seen from the Earth at a certain angle. This angle is bigger when the Earth is closer to the stars.

Because none of these consequences were determined and were contrary to the theory — the theory was completely ignored. Indeed, Copernicus pointed out that no parallax with the fixed stars could be established, because the sphere of these stars was too far away. It was not believed that there could be such an unbridgeable and enormous gap between the last planetary orbit — in those days Saturn's orbit — and the sphere of the fixed stars.

Nevertheless Copernicus' explanations were considered as an ingenious piece of work and his numerical values were the result of patient observations and exact measurements. These numerical values soon became the base of new astronomical tables, but the final conclusion, the renewed vision that the Sun instead of the Earth, was the centre of the Universe was not pursued.

Copernicus' book was not successful. Even though the original version never sold out, 23 years later in 1566 there was a reprint in Basle. Somebody obtained a copy of Copernicus' work and on his own initiative had it reprinted, including all the original mistakes and a whole series of new ones. In those days there were no authors or publishing rights.

It is interesting to compare the situation concerning the publication of other astronomical books in those days. The basic handbook of astronomy, *De Sphaera*, was written by the Englishman Sacrobosco about 1230 and was based on Ptolemy. There were no less than fifty nine printed editions until the end of the 17th century. Clavius' commentary on this work was published in 1570 and was reprinted nineteen times during the following 50 years. Between 1472 and 1600 Ptolemy's *Amalgest* and its corresponding planetary theory by Peurbacht was reprinted about forty times in Germany alone. In the first seventy five years Copernicus' work was reprinted only once!

Not only did the scientists view Copernicus' work unfavourably, but religious circles refused point blank to accept it.

Even before the appearance of his work in 1542, Copernicus' system was condemned by the University of Wittenberg. Others would follow this example : in 1553 the University of Zurich, in 1573 the University of Rostock, in 1576 the Sorbonne and in 1582 the University of Tübingen.

The Catholic Church — which did not interpret the Bible as literally as the Protestants — was, in the beginning, quite sympathetic towards Copernicus. It was only after the Trente Council — the counter Reformation council — that they began to observe every variation very closely. The Order of Jesuits was very active in this matter. In 1581 the German Jesuit and astronomer Christophorus Clavius questioned not only the physical absurdity of the system, but also the fact that it conflicted with several passages in the Holy Scriptures.

Then another figure appeared on the scene. The Danish astronomer Tycho Brahe who was born a few years after the death of Copernicus.

Brahe was the most superior astronomer who had existed up to that time, because for more than thirty years he had systematically observed the Heavens. He was very ingenious in the construction of precise measuring instruments and was Copernicus' opponent both on scientific and religious grounds.

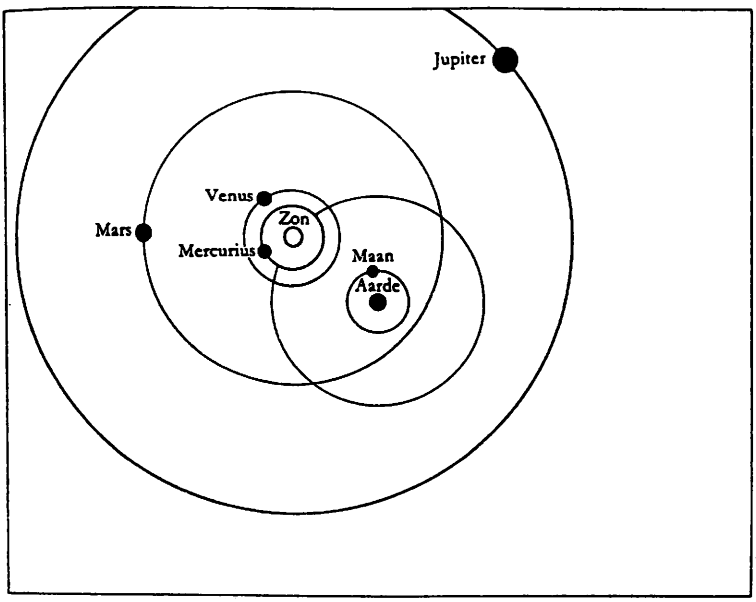


Fig. 4 : The system of Tycho Brahe. The Moon and the Sun orbit the Earth, the planets orbit the Sun.

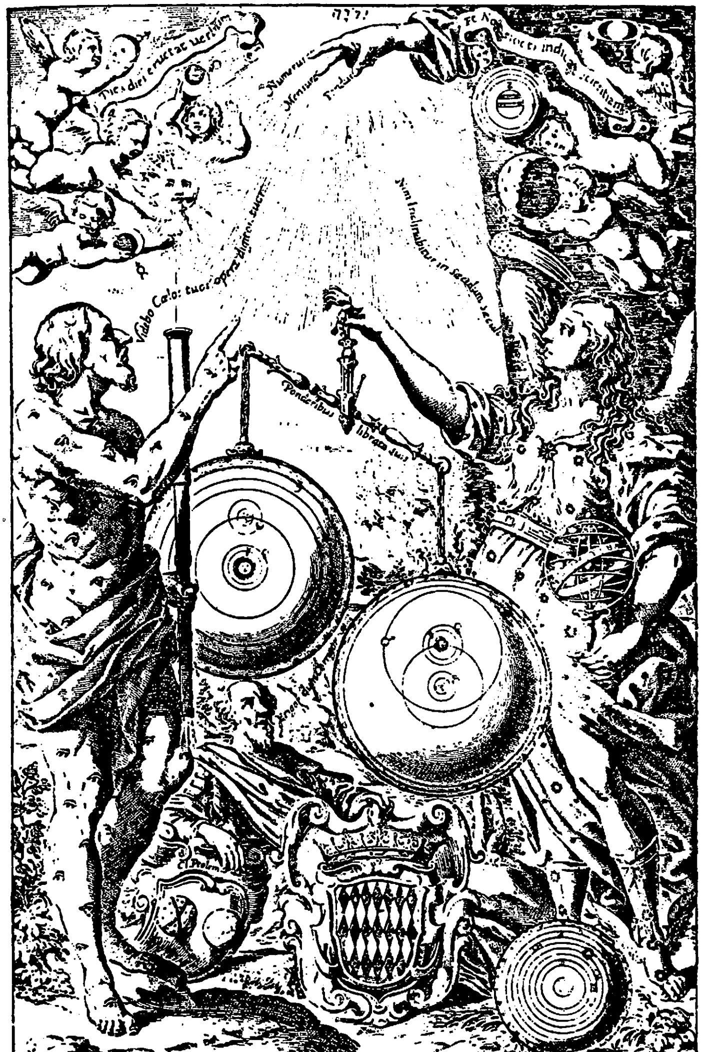


Fig. 5 : Engraving from *Amalgestum Novum* by J.B. Riccioli (1651). Urania, the muse of astrology, balances the system of Copernicus against that of Tycho Brahe. Copernicus' is found to be too light. At the bottom lies the rejected system of Ptolemy.

His opinion was that the cumbersome heavy Earth could not possibly be a fast-moving celestial body, but the Sun and the planets were more likely to be because they appeared almost as weightless lights. In addition, there was no change in parallax in the stars so this led to his conclusion that the Earth certainly was not orbiting the Sun. This was why, forty years after the publication of Copernicus' system he decided to develop his own world view.

This world view was a sort of compromise solution. It went halfway towards accepting Copernicus' system — that all the planets, except the Earth, orbited the sun. At the same time it also related to Ptolemy, in that the Sun together with the planets, orbited the Earth whilst the Earth stayed a non-moving centre.

Nowadays Brahe's system is only mentioned in passing, as if it had never had any meaning and is only considered as a curiosity, used to tide over temporarily the mental leap from Ptolemy to Copernicus.

In a wide historical context this is indeed so, but in the 17th century Brahe's world system was dominant.

Many facts are witness to the uncertainty of the views in those days. For instance Dr. Nicolaas Mulerius, of Bruges, was the first Professor of medicine and mathematics at the University of Groningen, which was founded in 1614. He was responsible for the third printing of Copernicus' work *De Revolutionibus Orbium Caelestium.* Mulerius prefaced this edition with a complete commentary which, strangely enough, reduced the heliocentric system to shreds.

Meanwhile there were some who still adhered stubbornly to Ptolemy. In 1605 Justus Lipsius, the great classic philologist, asserted that the abdication forced upon King Alphonso X of Castile by his son, and who had roundly criticised Ptolemy's system, was to be considered as a punishment from God because he had been so insulting about Ptolemy.

Had Copernicus any followers? One could be forgiven for asking. Actually there was one. Gemma Frisius who assembled globes and astronomical instruments in Louvain. He not only received a copy of *Narratio prima*, he also studied Copernicus' master-work and had quoted from it in a personal treatise in 1545. In February 1555, three months before he died, he expressed his enthusiastic acceptance of Copernicus' views in a letter to one of his students.

As well as Gemma Frisius there were some free-thinking minds who dared to break with the scientific and religious authoritative tradition. Giordano Bruno was one of them and because of his differing opinions he was burned at the stake. Later came Galileo, who was also very difficult to keep under control.

In 1609 Galileo was the first to point a modest, home-made telescope at the firmament and find out that there were four satellites orbiting Jupiter. This proved that the Earth was not the centre of all circular movements and thus lent weight to Copernicus' teachings.

In 1613, Galileo published a work in which he came out openly in favour of the accuracy of Copernicus' system. He went to Rome to plead the case, but was not able to prevent a commission of theologians from condemning the theory of a double Earth movement as being in conflict with religious belief. In 1615 Copernicus' book was confined to the Index of forbidden books and the following year Galileo was forbidden to teach the system. Thus, with these decisive actions the Catholic Church showed that Copernicus was officially banned.

The protestants also found it hard to accept. Simon Stevin, who had left Flanders in 1581 and went to Northern Holland, appeared to be a supporter of Copernicus, as was mentioned in his *Wisconstige Gedachtenissen* which was published in 1608. He even corrected Copernicus regarding the Earth's movements. In Holland, where there was much more tolerance, he did not have much to fear. Nevertheless Professor Struik, the eminent historian of mathematics, did suggest that the highly appreciated Stevin should not be appointed as a professor at the School of Engineering in Leiden because of his so-called Copernican heresy.

Filip van Lansbergen from Ghent, who had studied mathematics in England, became a reformed clergyman in Antwerp and after the fall of the city in 1585 emigrated to the North. There he was expelled from office as a clergyman in 1613. The reason for this dismissal was that he supported the principle of "the static Sun and the moving of the Earth". In 1629 he wrote a piece in which he defended Copernicus' system, but was reprimanded by professor Libert Fromondus of Louvain.

Copernicus' greatest defender should have been Joannes Kepler. Kepler was an astronomer and the successor to Tycho Brahe at the court of Rudolph II in Prague. Not only was he a great admirer of Copernicus, he even corrected his system by proving through his three laws of planetary motion, that the planets moved in elliptical paths around the Sun and followed well-determined speeds. These laws, published in 1609 and 1619, were slow to be understood because they were actually hidden under an excess of calculations and mythical considerations. These were oriented towards building up a sort of harmonious world order, supported by a complicated geometrical foundation. When Galileo died in 1642, he had no knowledge of Kepler's laws, although both scientists regularly wrote to each other!

Giovanni Domenico Cassini, professor of astronomy in Bologna, was invited to go to Paris on the invitation of Minister Colbert in 1669 to establish a national observatory. He was a man who had made notable astronomical discoveries, but when he died in 1712, he knew nothing about Kepler's laws, he had ignored Copernicus completely and throughout his life stayed an adherent of Ptolemy.

Michael Floris from Langeren was a cosmographer and mathematician to the Spanish king in Flanders and he had a plan to found an observatory in the castle of Gaasbeek about 1640. Nothing came of this, but he did construct a remarkable planetarium which was unsuccessful and very unpopular because, being based upon Copernicus' system, it was considered an unreal fantasy.

In 1644 and 1653 the Flemish priest Godfried Wendelen from Herk-de-Stad, defended Copernicus in his writings, even though he knew that in 1616 Galileo had been forbidden to support the system and was forced to renounce it. But Wendelen's was only a weak voice and he was looked upon strangely by the rest of the fraternity.

In 1651 the Jesuit Giovanni Batista Riccioli, a professor of philosophy, theology and astronomy in Bologna, published a work in two parts, *Amalgestum Novum* — a voluminous summary of the astronomical knowledge of those days — in which he produced 77 pieces of evidence against Copernicus' system and only 49 in favour.

In 1689, Jan Luyts, a professor at the University of Utrecht, published another work on astronomy, in which he completely rejected Copernicus' system. And two years later, in 1691, Martijn van Velden, who was a professor at Leuven and appeared to be a supporter of the heliocentric system, was ordered by the Principle of the university — after a long and exhaustive lawsuit — either to change his thesis or scrap it altogether.

It was about 40 years later — in the 18th century — that the first positive proof that the Earth moved appeared. In 1726 James Bradley, who was a professor at Oxford, determined the aberration of starlight, which is the inclined incidence of light rays in a telescope. This can only be explained if it is accepted that the Earth is moving very fast.

This was the beginning of the turning-point. Slowly Copernicus began to be accepted, two centuries after the appearance of *De Revolutionibus Orbium Caelestium.* In 1791 the Italian astronomer Giovanni Batista Guglielmini dropped a metal ball from the 78-meter Asinelli tower in Bologna. The deflection of the ball as it fell proved convincingly the Earth's rotation.

Even so, on May 5th 1829 in Thorn, when a monument was erected in honour of Copernicus opposite the house where he was born, the clergy of the town refused to take part in the festivities, because his book was still on the forbidden Index. But it did not stay there much longer as it was removed in 1835.

To cap it all, three years later the German astronomer Friedriech Wilhelm Bessel from Koningsbergen ascertained a difference in parallax with a star from the constellation of the Swan. In 1851 the French physician Léon Foucault proved the rotation of the Earth once again in the Pantheon in Paris with his renowned pendulum experiment.

In 1854, in Warsaw a triumphant new edition of *De Revolutionibus Orbium Caelestium* was published as a tribute to the great Polish scientist. In this edition it was mentioned for the first time that the preface, in which Copernicus' system is portrayed as a hypothesis, was not written by Copernicus himself but by Andreas Osiander, who had been responsible for the initial printing.

This was based upon rather dubious proof that might have come into Kepler's hands. Earlier there had never been any mention of it, because Copernicus was considered as a questionable case. But now he was considered as the great visionary who had advanced the Earth's path three centuries before. It could not be shown that he had ever been unsure or doubted himself. A great man is great in any field. And that is how it was going to be. That is how history is written.

During the 19th century the paths of the planets were calculated precisely by using high mathematics. Today the results have been confirmed brilliantly by space travel.

The question could be asked whether this final result is not enough and if it makes sense to try and unravel the thinking and doubting which was a part of the construction of our world view.

The answer to that in the words of the philosopher is : 'If we really want to understand something, we should go back to its origin'.

But in order to end on a more poetic note, here are the words of the Dutch historian Johan Huizinga :

It is good for he who progresses
through the wind of centuries,
to stop for a little while
and look back to the place
which he left in the morning!

THE LIFE AND CAREER OF GEORGE SARTON : THE FATHER OF THE HISTORY OF SCIENCE*

*Eugene Garfield***

The year 1984 marked the centennial of the birth of George Alfred Leon Sarton, the father of the history of science. Sarton was the author of numerous major works in the field, including the three-volume, 4, 236-page opus "Introduction to the History of Science", which many still consider one of the field's most definitive and ambitious works. Sarton also founded the field's primary journal, *Isis*, which he edited for forty years. But in spite of the importance Sarton placed on the history of science, he considered the discipline a means, not an end. Sarton's ultimate goal was an integrated philosophy of science that bridged the gap between the sciences and the humanities — an ideal he called "the new humanism." The forces and ideas that molded this idealistic scholar were a unique confluence of his Old World bourgeois upbringing and the experiences under German occupation during World War I that forced him to seek refuge in the United States.

The year 1984 marked the centennial of the birth of George Sarton, a pioneer in establishing the history of science as a discipline in

* This article originally appeared in the *Journal of the History of Behavioral Sciences* (*21*:107-17, 1985) and is re-published with kind permission of the author.

** I wish to thank Robert K. Merton for suggesting the idea of writing this essay, as well as Stephen A. Bonaduce and Cecelia Fiscus for their help with its preparation and its bibliographic research.

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its own right. Founder and editor for forty years of *Isis*, the field's primary journal, Sarton also wrote what many consider to be one of the most definitive works of this infant field, the mammoth *Introduction to the History of Science*.[1] The three-volume, 4, 236-page work consists of five tomes, in which Sarton reviews and catalogs the scientific and cultural contributions of every civilization from antiquity through the fourteenth century.

Among the other major works by Sarton, author of fifteen books and over three hundred articles, are *A History of Science*[2], a two-volume reworking of his lectures covering the acquisition of knowledge from ancient science and the Golden Age of Greece through the Hellenistic period; *A Guide to the History of Science*,[3] a bibliography; *Appreciation of Ancient and Medieval Science during the Renaissance*;[4] and *The History of Science and the New Humanism*.[5] His work has been published in a wide variety of periodicals, ranging from the *Proceedings of the American Philosophical Society*, the *Yale Review*, and *Science* to *Natural History*, the *Nation*, and, of course, *Isis*.

An indication of the impact of Sarton's works is given by data accumulated from 1955 to 1984 in the Institute for Scientific Information's (ISI's) *Science Citation Index (SCI)*, *Social Sciences Citation Index (SSCI)*, and from the *Arts & Humanities Citation Index (A & HCI)*. Most of Sarton's works were published well before the earliest date for which *SCI* data are available. Thus, these works may have passed through their peak citation years before they were indexed in *SCI*. Even so, his three-volume *Introduction to the History of Science*, for example, has been cited over one hundred and fifty times.

For Sarton, science was "the totality of positive knowledge."[6] According to a 1953 article by William H. Hay, Sarton's devotion to compiling the history of science was born of his conviction that such study is the key to the history of humanity, yielding unique insights concerning the complexity of human nature.[7] The purpose of Sarton's *Introduction to the History of Science*, as he puts it, is to

> explain briefly, yet as completely as possible, the development of one essential phase of human civilization ... the development of science ... No history of civilization can be tolerably complete which does not give considerable space to the explanation of scientific progress.[8]

In fact, for Sarton,

> the history of science is the only history which can illustrate the progress of mankind. In fact, "progress" has no definite and unquestionable meaning in other fields than the field of science.[9]

Despite the importance that Sarton placed on the history of science, however, the discipline was a means, not an end; his ultimate goal was an integrated philosophy of science that bridged the gap between the sciences and the humanities — an ideal he called "the new humanism."[10] In the division between scientist and humanist, Sarton saw a "chasm ... cutting our culture asunder and threatening to destroy it."[11] Waging his war on two fronts, Sarton admonished humanists who trivialize science as a mere technical occupation to respect it as one of the most impressive activites of which humanity is capable,[12] and at the same time he implored scientists to immerse themsleves in the scholarly traditions of the humanities. Sarton perceived the history of science as the synthesis of science and the humanities that would help to make "scientists who are not mere scientist, but also men and citizens."[13]

In an essay on the coverage of history and sociology of science journals in *Current Contents (CC)* that appeared in *CC* some years ago,[14] I noted that early in my career as an information scientist I almost became a historian of science myself. When I was a young, upstart member of the Johns Hopkins University Welch Medical Library Indexing Project in Baltimore, Maryland, I had plenty of exposure to the field. For instance, my boss, Sanford V. Larkey, a physician by training, was fascinated by Elizabethan medicine.[15] My friend and mentor Chauncey Leake, one of the project's advisors, was one of those rare individuals who combine an interest in the history of science with active research. His work includes articles on Galileo and Egyptian medical papyri.[16] And during my stay on the project, I often attended Oswei Temkin's and Richard Shryock's lectures on the history of medicine.

Even without the nodding acquaintance with the history of science that I developed at Johns Hopkins, however, the name of George Sarton was familiar to me, since my original interest in citation indexing involved its application to the humanities literature. The first paper I presented on the subject of citation indexing, given in Philadelphia in 1955, was on citation indexes to the Bible.[17] It was fascinating to trace the history of a point Sarton made in his book *A History of Science : Ancient Science through the Golden Age of Greece*[18] concerning a passage in the Book of Joshua, which alludes to the translation of *harbot zurim*. In a note appending the section on prehistoric medicine, Sarton contends that the phrase has been mistranslated as "sharp knives"; the correct meaning, he claims, is "flint knives."[19] Thus, Joshua 5:2 in the Authorized (King James) Version of the Bible reads, "At that time the Lord said unto Joshua, make thee sharp knives and circumcise again the children of Israel the second time." According to Sarton, however, the passage should read, "And at that time the Lord said unto Joshua, make thee stone knives of the hardest flint, and having again a fixed abode, circumcise the children of Israel."

Who was this giant, George Sarton ? What was this polymathic scholar really like? His daughter, the well-known poet and novelist May Sarton, described him as

> an exceedingly charming man; this charm made itself felt at once, on first meeting, in his beaming smile, the smile of a delighted and sometimes mischievous child, that flashed out below the great domed forehead and sensitive eyes behind thick glasses.
> He was stout, with beautiful hands and small feet, a stocky man who walked down Brattle Street in Cambridge, Massachusetts, at exactly the same time every morning, with the propulsive energy of a small steam engine, a French beret on his head, a briefcase in one hand, in a coat a little too long for him because he could

not be bothered to have his clothes altered and insisted on buying them off the rack to save time.[20]

George Alfred Leon Sarton was born in Ghent, East Flanders, Belgium, on 31 August 1884. His father, Alfred Sarton, was the director and chief engineer of the Belgian State Railroads. His mother, Léonie van Halme, died when George was less than a year old.

The Victorian household in which Sarton grew up was dominated by the personality of Alfred Sarton. In her book *I Knew a Phoenix*,[21] May Sarton recalls her impressions of her grandparents. Of her grandfather she wrote, "[he] was a confirmed bachelor, who had for a brief interlude happened to be married."[22] For May, the phrase "In my father's house," with which George used to begin so many anecdotes about his life in Belgium before World War I, always brought a sharp image of Alfred Sarton — "ultra-sensitive, sardonic, with bright deep-set eyes"[23] — into focus.

But of Sarton's mother, little is known — even to those in her immediate family. Léonie Sarton died of a hemorrhage a year after George's birth "because she was too modest to call for help, while her husband, swinging his cane, ready to go out, waited for her in vain."[24] She played Chopin and loved candied violets and *fleur d'oranger*. Innocent and extravagant, she shocked her husband's conservative family by buying her gloves by the dozen. Yet she is a lonely figure as well. "All around her hangs the parfume of sadness, the silence her husband never broke to tell little George something of that vanished young mother who so soon became younger than her son."[25]

Loneliness also haunted the childhood memories of Sarton himself. An isolated, only child, he was both pampered and neglected by the household servants, who, with the best of intentions, took his medicine for him when he was ill — especially if the concoction had a disagreeable taste.

In spite of Sarton's starvation for tenderness as a child, however, he was not without an active imagination and a "streak of Flemish humor,"[26] in the words of May Sarton :

> When ... still eating in a high chair, George was allowed to be present at dinner, but if he so much as babbled a single word, his father, without raising his head from his newspaper, reached forward to touch the bell (a round brass bell on a stand, tapped with one finger) and when the maid appeared, said simply, *"Enlevez-le"* (Remove it). When George was alone at a meal, formally served him in the dining room in his high chair, and he did not like something he was given to eat, he repeated the lordly gesture and the lordly phrase and was delighted to see that, like "Open Sesame" in reverse, he could thus have the unhappy cabbage, or whatever it was, removed from his sight.[27]

George Sarton first studied at the Athénée — the equivalent of our primary and secondary schools — of his native town, and then at the one in Chimay for four years. In 1902 he entered the University of Ghent to study at the Faculté de philosophie et de lettres. One of his teachers there was the well-known classical scholar Joseph Bidez, whose influence Sarton remembered with gratitude. Sarton found, however, that the traditional presentation of the humanities did not parallel his interests. So he abandoned the study of philosophy, and in 1904, after a year of private reading and reflection, he reentered the University in the Faculté des sciences, in which he began work in the natural sciences. As he later wrote in his journal, "I hope thus to become more than a writer of fine phrases, and bring my effective aid to the progress of the sciences."[28]

Sarton's studies included chemistry, crystallography, and mathematics. He received the degree of *docteur ès sciences* from the University of Ghent in 1911 for a thesis in celestial mechanics entitled "Les Principes de la mécanique de Newton." For his work in chemistry, he was awarded a gold medal offered by the four Belgian universities — Ghent, Louvain, Brussels, and Liège.

Almost immediately after obtaining his doctorate, on 22 June 1911, Sarton married Eleanor Mabel Elwes, the daughter of a Welsh civil and mining engineer, and the young couple established themselves in an old country house in Wondelgem, near Ghent. Sarton, whose small private income was too modest to sustain a family, purchased the house with the proceeds of the auction of his deceased father's wine cellar. The sale itself was widely regarded as scandalous but it was perhaps typical of the iconoclastic Sarton. In the following year, 1912, Sarton's only surviving child, Eleanore Marie (later shortened to May), was born, and the journal *Isis*, Sarton's *"Revue consacrée à l'histoire de la science,"* was founded.

Sarton liked to refer to his wife, Mabel, as "the mother of those strange twins, May and *Isis*,"[29] and the history of science owes a debt to Mrs. Sarton for the survival of its first journal. When the journal was in its early days, she wrapped and mailed each issue, and in her last years, she watched her husband so that he did not overtax himself.[30] An artist and a distinguished designer of furniture, Mabel Sarton helped George meet the expenses incurred by *Isis* by supplementing his income with her own. She was inspiration, companion, and helpmeet to her husband, and when she died in 1950, he felt that a part of himself had been extinguished.

Isis, a review devoted to the history and philosophy of science, was to be, as Sarton defined it, "at once the philosophical journal of the scientists and the scientific journal of the philosophers, the historical journal of the scientists and the scientific journal of the historians, the sociological journal of the scientists and the scientific journal of the sociologists."[31] The title of the new journal was meant to evoke "the period of human civilization which is perhaps the most impressive of all — its beginning."[32]

Like other scholarly journals, *Isis* would publish original research articles, notes, queries, personal items, and book reviews. But a unique feature of the journal was its critical bibliography. During the forty years he served as the editor of *Isis*, Sarton himself regularly compiled this index of the major publications dealing with the history of science throughout the world. Its purpose was to make scholars aware of resources and the growing literature of the field, and to provide a forum for the correction of errors.[33]

By September 1912, Sarton had recruited a distinguished editorial board for the journal that included Henri Poincaré, Svante August Arrhenius, Émile Durkheim, Jacques Loeb, Friedrich Wilhelm Ostwald, and David Eugene Smith. The wide range of fields represented by the work of these scholars reflected Sarton's conviction that the history of science was by nature an encyclopedic discipline, his orientation toward universal history, and his philosophical belief in the brotherhood of man.[34]

With a discipline to be forged, esoteric theories and rigorous consistency were less important to Sarton than establishing professional techniques, methodologies, and an intellectual orientation of comparison, summation, and synthesis. Thus, Sarton was a combination of propagandist and proselytizer, and *Isis* was the intended organ of the new discipline. It was through *Isis*, according to Arnold Thackray and Robert K. Merton, that he hoped to "systematically and holistically" combine "methodological, sociological, and philosophical perspectives with purely historical inquiry," enabling such inquiry to gain its full significance.[35] The first issue of *Isis* appeared in March 1913. In 1924, when the History of Science Society was founded, *Isis* became its official publication, but the Society did not assume full financial responsibility for the journal until 1940. The annual deficit it ran for twenty-eight years was met by Sarton, who had no private or independent income.[36]

As subscriptions to *Isis* trickled in from all over the world, Sarton was hard at work taking voluminous notes for his monumental *Introduction to the History of Science*. At the start, Sarton had intended to bring his *History* up to the present, but the task on the scale he had planned proved beyond even his extraordinary efforts. In fact, according to I. Bernard Cohen,[37] a member of the board that assumed the duties of editing *Isis* when Sarton stepped down from the position, Sarton would explain to students that, had he known as much about the history of science when he began his *Introduction* as he did when he finished the two-volume, 2155-page work on the fourteenth century,[38] he would never have gotten even that far.

The spring and summer of 1914 was an idyllic time for the Sartons. As May Sarton recounted, "We were beautifully happy and independent, all three."[39] But on 28 June, the Archduke Ferdinand was assassinated in Sarajevo. According to May, all through July of that year, as her father worked quietly in his study, and her mother wondered why the plum tree would not bear fruit, diplomats hurried back and forth across Europe. The war that most people referred to as "a scare" lurked around the corner.

Despite the seemingly far-off nature of the threat, however, the Belgian newspapers were filled with rumors, and preparations of a sort were made. Of that time, May wrote :

> The Civil Guard, to which my father at one time belonged, drilled now and then on the village green, and took uniforms out of mothballs. But no one really believed in that impossible war as a reality. In any case ... Belgium itself was neutral. Nothing could happen here.
>
> [But] on August second, the Germans demanded free passage, were refused, and on August third the Wehrmacht marched in in their spiked helmets ... My father, though no longer a member of the Civil Guard, got out his heavy Civil Guard coat, took down the old musket, and reported for patrol duty. He was set to guard the railway intersection. There, alone, a lantern in one hand, his gun in the other, he paced up and down all night hoping that the German army would not come hurtling down the track. Fortunately, it did not.[40]

Twenty-six German officers and infantrymen were billeted at the Sarton's house in Wondelgem, and Sarton was responsible for their safety; if any of the enlisted men failed to make curfew, Sarton would have been taken into his garden and shot. Indeed, it was to prevent just such an occurrence that he buried his Civil Guard coat, since members of the Guard were treated as spies. Little by little, as the war continued and Sarton realized — after a brief, frustrating stint in the Red Cross at Brussels — that he could be of more use continuing his work, the Sartons came to the decision that they should leave the country. They could take very little with them, so the precious notes for Sarton's *Introduction to the History of Science* were stored in a metal trunk that joined his buried Civil Guard coat in the garden. A distant cousin managed to dig up the notes, and returned them to Sarton after the war.

The Sartons first went to England, where George got a job as a censor in the War Office. Although the flood of reflugees from Belgium was welcomed, the War Office did not pay enough to support a family of three, and employment opportunities in the history of science were nowhere to be found. The Italian historian of science Aldo Mieli offered Sarton the hospitality of his home at Chianciano, near Sienna.[41] Instead, Sarton left his wife and child in England while he went to the United States in search of a position that would support his family and his dream of completing his *History of Science*. In September 1915, Mabel and May Sarton completed the hazardous passage across the Atlantic and joined George at the New York home of Leo Baekeland, the eccentric Belgian inventor of Bakelite, the first successful plastic.[42]

By good fortune, Sarton had reached the United States at a time when the history of science was becoming a recognized activity. Although it was far from being an established discipline and was almost unthought of as a profession, it was beginning to reach maturity.[43] Nevertheless, Sarton endured an uncertain time in which he must have wondered whether he would have to abandon his dream of a life exclusively devoted to the history of science. Despite this, he turned down a good job as a librarian at Rice University, Houston, Texas, because the University could not meet the one condition about which he was adamant : that his imployer take over the publication and financial support of *Isis*, which had been out of print since shortly after the invasion of Belgium.[44]

The summer of 1916 found Sarton delivering a course of lectures at the University of Illinois, Urbana, and through *Isis* board member David Eugene Smith, among others, he soon received other appointments.[45] In the same year, for example, he gave a series of six lectures on science during the time of Leonardo da Vinci at the Lowell Institute in Boston, Massachusetts, and later was a lecturer at George Washington University in Washington, D.C.

Among those who helped Sarton arrange this frenetic but sustained round of temporary appointments was L.J. Henderson, a biochemist and a junior but influential member of the Harvard University faculty.[46] Henderson had been teaching a course on the history of science regularly since 1911, and supported Sarton's goals for the discipline. He managed to obtain an appointment for Sarton as a "lecturer in philosophy" at Harvard that extended until 1918, when the United States' involvement in World War I caused financial problems for the University.

In response to Sarton's renewed appeals for work, Robert S. Woodward, second president and successful organizer of the Carnegie Institution, Washington, D.C., provided the crucial financial support Sarton needed.[47] Woodward had a personal interest in the history of science, and Sarton had been in touch with him even before the exile from Belgium. Although Woodward had initially been unsympathetic to Sarton's dream of establishing the history of science in its own right, he had slowly softened his position. With the help of Carnegie Institution trustee Andrew Dickson White, Woodward created the post of research associate in the history of science for Sarton. Characteristically, almost as soon as he heard that he had secured a permanent position with a regular salary, Sarton made plans to revive *Isis*, which had been dormant during the years of the war.[48]

Thus began Sarton's nearly lifelong association with the Carnegie Institution, but although he was officially employed full-time in

Washington, D.C., he remained in Cambridge to study at Harvard's then-new Widener Library.[49] When the war ended and he recovered his notes — which were greatly augmented by the mass of new data he had accumulated in the United States — he found himself secure in one of the world's great libraries, with the salary guaranteed by the Carnegie Institution and with no specific responsibilities or duties other than those he set for himself. He was free at last to pursue the mission that he had never forgotten.

Sarton's sense of mission found its first, and perhaps best, expression in his mostcited work, the *Introduction to the History of Science*.[50] The development of this work is a microcosm of the evolution of Sarton's concept of the unity of scientific and cultural endeavors. Sponsored by the Carnegie Institution, it was not conceived as a work of historical narrative, but rather as a bibliography that would serve as the basic source material for such a history.[51] It would deal with all science, covering the enterprise from its earliest beginnings up through the twentieth century. Sarton at first imagined that it would be a relatively short work.

Gradually, there emerged the concept of a colossal work that would consist of three series of books. The first would survey cross sections of civilization by half-centuries; the second would deal with different types of civilization; and the third would discuss, in detail, the histories of various "special" sciences. The entire work would comprise some twenty-six volumes, but Sarton lived to complete only the first three volumes of the first series.[52]

The first of these volumes, *From Homer to Omar Khayyám*,[53] was published in 1927 and contained 840 pages. It represented nine years of active work and covered the period from Homer through the eleventh century. The second volume took another four years to complete. Published in 1931 in two large parts consisting of a total of 1,252 pages, it was titled *From Rabbi Ben Ezra to Roger Bacon*[54] and covered the twelfth and thirteenth centuries. The third volume, also printed in two parts,[55] did not appear until 1947; when it did, it was apparent that the project could not continue, for it covered only the fourteenth century and comprised 1,018 pages. Sarton estimated that a similar work dealing with the fifteenth century would have taken him ten to fifteen years to complete.[56]

During the course of his labors on the *Introduction*, Sarton found himself hampered by his lack of knowledge of Arabic.[57] Spending the academic year 1931-1932 in the Near East, he taught himself to read classical and modern Arabic. Sarton also knew some Hebrew, Chinese, and Portuguese and was familiar with Latin and Greek. He was fluent in French, English, German, Italian, Dutch, Flemish, Swedish, Danish, Turkish, and Spanish. In 1936 he found the time to begin a companion journal to *Isis*, and he named it *Osiris*. The purpose of the journal was to publish articles that were too long for *Isis* but not quite comprehensive enough to become books. Sarton edited ten volumes of this new journal.

In 1940 J.B. Conant, President of Harvard, elevated Sarton from his position of lecturer, which was an annual appointment, to tenured professor of the history of science.[55] However, Sarton continued to draw the major portion of his salary from the Carnegie Institution, which also provided him with a research and traval budget, money for the purchase of books and periodicals, and full-time secretarial assistance. When Sarton had published what would prove to be the last volume of his great *Introduction*, he resolved to put to paper the lectures that he had given for so many years at Harvard. He planned to complete the project in nine volumes, but again the task eluded him. He published two : *A History of Science : Ancient Science through the Golden Age of Greece*,[59] cited over sixty times from 1955 through 1984, according to the SCI, SSCI, and A&HCI, and *A History of Science : Hellenistic Science and Culture in the Last Three Centuries B.C.*,[60] cited over forty-five times from 1955 through 1984. Ironically, although Sarton had been wont to say that his real interests lay in the modern period, when he died, the bulk of his published work covered antiquity and the Middle Ages.[61]

Among the honors bestowed on Sarton were the Prix Binoux of the Académie des sciences, Paris, in 1915 and again in 1935, and the

Charles Homer Haskins Medal of the Medieval Academy of America in 1949. He was made a Knight of the Order of Leopold in his native Belgium in 1940, and was granted numerous honorary degrees from such institutions as Brown University, Harvard University, and Goethe University, Frankfurt am Main, FRG. The scholarly honor societies to which he belonged include the American Academy of Arts and Sciences, the American Philosophical Society, the Royal Society of Edinburgh, the Royal Flemish Academy of Belgium, and the Arabic Academy of Damascus. A founding member of the International Academy of the History of Science, he also served as President of the International Union of the History of Science, and Honorary President of the History of Science Society. He also claimed honorary membership in the history of science societies of Belgium, England, Holland, Germany, Israel, Italy, and Sweden.[62]

The honor that gave Sarton the most pleasure was the award of the George Sarton Medal, which he was the first to receive. On the occasion of Sarton's retirement as editor of *Isis* in 1952, a committee under the chairmanship of Frederick G. Kilgour, then of the Yale Medical Library, secured funds from Charles Pfizer and Company, a pharmaceutical and chemical manufacturing firm in New York, for a medal to be struck in Sarton's honor.[63] The obverse of the medal features a profile of Sarton, while on the reverse is a figure of the goddess Isis, copied from a drawing made by Sarton's late wife for her husband's bookplate. The medal bears the inscription, "To further the history of science."

The Council of the History of Science Society, which makes the award, felt that there was no person to whom the medal might be more appropriately awarded than to George Sarton himself.[64] In making the award, Dorothy Stimson, President of the Society, said,

> It is most fitting that the George Sarton Medal ... should go first to Dr. Sarton himself ... Dr. Sarton has established, to a greater extent than anyone else, our present foundations of knowledge and understanding of the history of science. This he has achieved through more than 40 years as a pioneering, dynamic scholar and

> editor. He is truly the dean of the historians of science in this country.[65]

Subsequent winners of the medal include Charles and Dorothea Waley Singer (1956), Lynn Thorndike (1957), John F. Fulton (1958), Oswei Temkin (1960), Joseph Needham (1968), Henry Guerlac (1973), and Thomas S. Kuhn (1982).

In 1960 the History of Science Society, under the auspices of the American Association for the Advancement of Science (AAAS), also established the George Sarton Memorial Lecture. The first lecturer was René Dubos; subsequent lecturers have included Ernst Mayr (1971), Thomas Kuhn (1972), I. Bernard Cohen (1978), Henry Guerlac (1982), Derek de Solla Price (1983), and Arnold Thackray (1984). The speaker for 1985 is Daniel Kevles.

Yet despite the honors and accolades Sarton accumulated by the end of his career, his influence during his lifetime was relatively limited. According to Thackray and Merton, Harvard's administration considered Sarton a "marginal, if illustrious, man. In 1940, he had still to produce his first successful Ph.D. candidate, his undergraduate courses remained small, and he almost completely avoided all committee service and routine academic administration."[66]

Although Sarton's influence on the history of science may not be immediately obvious, it is nonetheless real. His emphasis on critical bibliography, his instigation of sweeping surveys of the vistas of science, the journal he founded, and, above all, his classic *Introduction to the History of Science* all served to create the elements required by a struggling new field, as opposed to methods to be emulated or finished products for display.[67] His presence at Harvard was instrumental in the creation of what later became one of the leading centers of the history of science in the world. And at least part of the reason for Sarton's lack of influence was that, during the greater part of his career, there were no departments of the history of science, and therefore no jobs. However, though the outward face of the history of science today may show little trace of Sarton's influence, the bony foundation across which that skin is drawn was assembled through his efforts.

George Sarton died at 7:30 A.M. on 22 March 1956 of congestive heart failure. He appeared to have been in excellent health, and was eagerly anticipating a visit to Montreal,[68] where he was to give a lecture at McGill University entitled "The History of Science and the New Humanism." A few minutes after departing from his home in Boston for the airport, however, he felt ill and asked the taxicab driver to turn back. He died only a few minutes after he reached his house, while sitting in his favorite armchair. A simple funeral service, which, in accordance with Sarton's wishes, was identical to that held for his wife some six years earlier, took place two days after he died, in the Harvard Memorial Church.

Sarton has been called a great teacher, a superb organizer of facts, and an unrivaled integrator of knowledge.[69] "His erudition was such that even his informal comments were based on exact knowledge and frequently opened new leads for the author of a paper under discussion,"[70] Stimson wrote a year after Sarton died. "The encyclopedic range of his writings led the way to fresh and fertile fields for other scholars."[71] In an obituary for Sarton in *Archives internationales d'histoire des sciences*, F.S. Bodenheimer wrote, "He was a good man ... a courageous man ... a wise and reasonable man ... a great scientist ... [and] a great humanist."[72]

During the presentation of the first George Sarton Medal, Sarton had this to say about himself and his career :

> Scholars of a later age reviewing my life will sometimes wonder whether I was crazy; I was not crazy, but seemed to be, because I was overwhelmingly dominated by two passions, a passion for science and another equally ardent one for the humanities ... [I]t is impossible to live reasonably without science, or beautifully without arts and letters. He who studies the history of science and teaches it should always remain in touch with the living science of his own time ... [T]he past cannot be separated from the present without grievous loss. The present without its past is

insipid and meaningless; the past without the present is obscure. The life of science, like the life of art, is eternal, and we must view it from the point of view of eternity.[73]

George Sarton was a remarkably gifted and versatile scholar who had exceptional organizational ability and a seemingly endless capacity for work. He also had a broad streak of idealism, conceiving a lofty view of humanity and its essential reason for existing, and rejoicing in that heritage, which he took every opportunity to proclaim. He has come to epitomize the history of science to scholars throughout the world, and the imposing number of books, articles, and lectures he produced in the more than forty-five years he devoted to his field stand as a monument as much to his determination and faith as to his scholarship.

Notes

1. George Sarton, *Introduction to the History of Science. From Homer to Omar Khayyam* (Baltimore : Williams & Wilkins, 1927), vol. 1; Sarton, *Introduction to the History of Science. From Rabbi Ben Ezra to Roger Bacon* (Baltimore : Williams & Wilkins, 1931), vol. 2, two parts; Sarton, *Introduction to the History of Science; Science and Learning in the Fourteenth Century* (Baltimore : Williams & Wilkins, 1947), vol. 3, two parts.

2. George Sarton, *A History of Science. Ancient Science through the Golden Age of Greece* (Cambridge : Harvard University Press, 1952); Sarton, *A History of Science. Hellenistic Science and Culture in the Last Three Centuries B.C.* (Cambridge : Harvard University Press, 1959).

3. George Sarton, *A Guide to the History of Science* (Waltham, Mass. : Chronica Botanica, 1952).

4. George Sarton, *Appreciation of Ancient and Medieval Science during the Renaissance (1450-1600)* (Philadelphia : University of Pennsylvania Press, 1955).

5. George Sarton, *The History of Science and the New Humanism* (Bloomington : Indiana University Press, 1962).

6. Ibid., p. 118.

7. William H. Hay, "George Sarton : Historian of Science and Humanist," *American Scientist* 41 (1953) : 282-286.

8. Sarton, *Introduction to the History of Science,* vol. 1, p. 3.

9. George Sarton, *The Study of the History of Science* (New York : Dover Publications, 1957), p. 5.

10. F.S. Bodenheimer, "George Sarton," *Archives internationales d'histoire des sciences* 9 (1956) : 295.

11. Hay, "George Sarton," p. 282.

12. E.J. Dijksterhuis, "Obituaries : In Memoriam George Sarton," *Centaurus* 4 (1956) : 369-381.

13. Bodenheimer, "George Sarton," p. 296.

14. Eugene Garfield, "Additional History and Sociology of Science Coverage in *Current Contents,*" in *Essays of an Information Scientist* (Philadelphia : ISI Press, 1980), vol. 3, pp. 723-726.

15. Sanford V. Larkey, "Childbirth in the Days of Queen Elizabeth," *American Journal of Obstetrics and Gynecology* 27 (1934) : 303-308; Larkey, "Public Health in Tudor England," *American Journal of Public Health* 24 (1934) : 912-913 and ibid., pp. 1099-1102.

16. Chauncey D. Leake, "Interrelations of Sciences with Humanities and Ethics. Reflections on Quadricentennial of Galileo's Birth," *Texas Journal of Science* 17 (1965) : 5; Leake, *The Old Egyptian Medical Papyri* (Lawrence : University of Kansas Press, 1952).

17. Eugene Garfield, "Citation Indexes — New Dimensions in Documentation" (Paper presented at the Annual Meeting of the American Documentation Institute, Philadelphia, 2-4 November 1955).

18. Sarton, *A History of Science : Ancient.*

19. Ibid., p. 8.

20. May Sarton, "An Informal Portrait of George Sarton," *Texas Quarterly* 5 (1962) : 101-102.

21. May Sarton, *I Knew a Phoenix* (New York : Rinehart, 1959).

22. Ibid., p. 12.

23. Ibid., p. 13.

24. Ibid.

25. Ibid., p. 14.

26. Ibid.

27. Ibid., pp. 14-15.

28. Ibid., p. 64.

29. Ibid., p. 69.

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