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FRANCIS GANO BENEDICT'S *REPORTS OF VISITS TO FOREIGN LABORATORIES* AND THE CARNEGIE NUTRITION LABORATORY

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Summary: *Between 1907 and 1932/33 Francis Gano Benedict, director of the Carnegie Nutrition Laboratory, made seven extended tours of European metabolism laboratories. Benedict compiled extensive reports of these tours, which contain detailed descriptions and hundreds of photographs of the apparatus, laboratories and people that Benedict encountered. The tours took place during significant decades for physiology, covering the rise of American physiology, the effect of the First World War on European laboratories and the emergence of an international community in metabolism studies. This essay provides an introduction to Benedict's Reports of Visits to Foreign Laboratories and their central themes, situating them within the history of American physiology and the Carnegie Nutrition Laboratory. It concludes with an assessment of these volumes as a source for the history of early twentieth-century nutrition physiology.*

Key words: *Francis Gano Benedict, physiology, laboratory history, Carnegie Nutrition Laboratory, metabolism research.*

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Introduction[1]

In 1907 Francis Gano Benedict, physiologist and biochemist, was appointed director of the newly established Carnegie Nutrition Laboratory (CNL) in Boston. In this year he undertook the first of what was to become a series of seven extended tours of European institutions involved in metabolism research. From 1907 to 1933 Benedict travelled to Europe every three years, excepting the war and immediate post-war period, visiting established, new and lesser known laboratories at universities and vocational schools, medical clinics, agricultural experiment stations and government, industrial and private research centres in Great Britain, most Continental and several Eastern European countries, Scandinavia and Russia.

Benedict compiled extensive reports of these visits for the Carnegie Institution of Washington (CIW).[2] They are replete with hundreds photographs of laboratories, equipment and apparatus, physiologists and their assistants, technicians and families, lecture handbills, cuttings from foreign newspaper interviews and articles describing his tour, hand-drawn diagrammes and sample protocols. The volumes provide a unique and detailed synchronic and diachronic history of metabolism research laboratories, their personnel, research programmes and apparatus over a twenty-five year period from Benedict's particular, changing and subjective perspective as a beginning, established and, eventually, leading American laboratory director.

The purpose of this essay is to provide a preliminary exploration of these volumes and their main themes. It will summarize the focal point of each European tour, beginning with Benedict's survey of the material conditions in European laboratories on his first tour, his attempts to network with European scientists on his second, and his emergence as an established metabolism researcher by his third visit. The post-war visits are described in a single section, since they are dominated by Benedict's observations of the effect of the First World War on European science and scientists.

Although Benedict played a crucial role in establishing the study of metabolism in the United States, he and the CNL have received little attention in the history of science or medicine.[3]

1. I thank the Max Planck Institute for the History of Science for its generous support of this project, and the colleagues at the MPI, Gordon McOuat and the anonymous referees for their criticism and suggestions.

2. The *Reports* are held in the Francis Gano Benedict papers, 1870s-1957. GA 7. Harvard Medical Library, Francis A. Countway Library of Medicine, Boston, Mass., Boxes 6 and 7. They have been digitised by the Max Planck Institute for the History of Science as part of The Virtual Laboratory: http://vlp.mpiwg-berlin.mpg.de. A map of the tours can be found in E. Neswald, (2010), "An American Physiologist Abroad: Francis Gano Benedict's European Tours", The Virtual Laboratory 2010. http://vlp.mpiwg-berlin.mpg.de/references?id=art77.

3. Benedict is only briefly mentioned in the seminal works of Kenneth Carpenter and Harvey Levenstein. K. Carpenter, *Protein and Energy. A Study of Changing Ideas in Nutrition*, Cambridge: Cambridge University Press 1994; H. Levenstein, *Revolution at the Table. The Transformation of the American Diet*, Berkeley: University of California Press 2003. The only extensive consideration of his research to date is in M. Hamin, *Tables Turned, Palates Curbed: Elements of Energy, Economy, and Equilibrium in American Nutrition Science, 1880-1930*, Diss. University of Pennsylvania 1999, esp. chp 7.

Research into the history of American nutrition has focussed largely on the activities of Benedict's predecessor, Wilbur Olin Atwater, and Atwater's work with the United States Department of Agriculture, while studies on the activities of the CIW concentrate on its projects in astronomy, geophysics and genetics. While a history of the CNL or a detailed description of Benedict's contributions to metabolism research must be reserved for a later date, by presenting Benedict's *Reports*, this essay hopes to draw attention to a neglected figure in the history of nutrition and to contribute to an appreciation of the role and difficulties of trans-Atlantic interactions both in metabolism research and, more generally, in the history of physiology.

American Nutrition Research and the Carnegie Nutrition Laboratory

Benedict's tours, laboratory and strategies developed within a specific constellation of American physiology, government and social interest in research on human and animal nutrition and debates on the lack of basic research in the United States. In the nineteenth and early twentieth century, American physiology was highly dependent on European physiology. American universities had no established research tradition, and students interested in laboratory techniques and research methods travelled to European centres for their degrees and for advanced training (Frank, 1987). This generation also viewed travel as crucial later in their careers as a means of keeping in touch with developments in the discipline, since research science was not yet widely established at American universities. Most basic research was conducted under the auspices of the US government and concentrated on such politically and policy-relevant areas as geological surveys and astronomy (Rheingold, 1972). Research on agriculture, including human nutrition, was undertaken under the direction of the United States Department of Agriculture. In the early 1880s, Wilbur Olin Atwater, an agricultural and biochemist, travelled to Leipzig, Berlin and Munich, where he learned the techniques of calorimetry and respiration gas analysis in the laboratory of Carl Voit, Germany's leading nutrition scientist. Appointed director of the Storrs Agricultural Experiment Station, professor at Wesleyan University and for a few years Director of the Office of Experiment Stations for the USDA, Atwater conducted chemical analyses of American foodstuffs, organised extensive surveys of food consumption and developed with his Wesleyan colleague, the physicist Edward Bennet Rosa, a respiration calorimeter for the study of human nutrition and metabolism. The respiration calorimeter of Atwater and Rosa was the first apparatus that could prove the conservation of energy in human metabolism, and, that humans were, in essence, energy transformation machines in which energy input equalled energy output.[4]

4. Energy conservation in the metabolism of dogs had been proven by Max Rubner in the 1880s. See M. Rubner, "Die Quelle der tierischen Wärme", *Zeitschrift für Biologie* 20 (1893): 73–142.

In 1895 Francis Gano Benedict, a German-trained American chemist, became Atwater's assistant at the Storrs and Wesleyan laboratories.[5] When Atwater and Rosa acrimoniously parted ways, Atwater turned to his technically highly skilled assistant to refine the calorimeter, conduct complex experiments and develop additional apparatus. Through the 1890s and into the first years of the twentieth century, Atwater and Benedict conducted metabolism experiments at the Wesleyan lab, while Atwater directed a nationally dispersed crew of researchers who studied animal nutrition, the digestibility of foodstuffs and human nutrition practices through extensive dietary surveys of groups ranging from the Harvard rowing crew to Chinese migrant workers in California, the rural poor of Virginia and Maine lumbermen.

In 1902 the trustees of the Storrs Experiment Station withdrew their financial support for this research programme. By then, Atwater could turn to another organisation, however – the privately-endowed Carnegie Institution of Washington. Founded in 1902 by steel magnate Andrew Carnegie amidst debates about perceived deficits in American scientific research productivity and education, the CIW was endowed with 10 million dollars and followed the mission of supporting basic research in the sciences.[6] The trustees of the institution decided to achieve this goal through the establishment of private research laboratories.

In its first years, the CIW provided Atwater with grants to continue his metabolism experiments at Wesleyan, while the trustees considered and then approved the establishment of a Carnegie laboratory for research on human nutrition. As the country's leading nutrition scientist, Atwater was the natural choice for director, but in 1904 he was severely disabled by a stroke, and he died a few years later. After some discussion, the trustees nominated Benedict in his place. The official appointment took place in 1907, after a year of planning and negotiations.

After courting both New York and Boston as potential sites for the new institution through 1906, Benedict eventually decided on Boston and purchased property neighbouring Harvard Medical School. He cited several reasons for this choice, including climate and the proximity to medical schools and hospitals.[7] If Carnegie and the trustees of the CIW had originally intended a complete separation of their laboratories from the universities, close and friendly relationships had advantages. Benedict negotiated with Harvard the use of its

5. The most extensive biographical information on Benedict can be found in the obituary written by E. DuBois and O. Riddle, "Francis Gano Benedict (1870-1957). A Biographical Memoir", *Biographical Memoirs*. National Academy of Sciences 1958, 65-99.

6. See for example: [C.W. Eliot], "The New Education. Its Organization", *Atlantic Monthly* 23/136 (1869), 203-220; C. Snyder, "America's Inferior Position in the Scientific World", *North American Review* 174 (Jan./June 1902), 59-72; S. Newcomb, "Conditions which Discourage Scientific Work in America", *North American Review* 174 (Jan./June 1902), 145-158. See also J. Trefil and M.H. Hindle, *Good Seeing. A Century of Science at the Carnegie Institution of Washington 1902-2002*, Washington DC: Joseph Henry Press 2002; H.S. Miller, *Dollars for Research. Science and its Patrons in Nineteenth Century America*, Seattle and London: University of Washington Press 1970.

7. FGB Papers, Box 2, Folder 26: BNL, Locations Studies and Reports (1903-1906), Benedict to Robert Woodward, 23 April 1906.

utilities and academic infrastructure, (heating, electrical systems, generators, libraries). From the medical school, he hoped for a steady supply of varied experimental and clinical subjects, qualified personal to assist and medical students to work in the labs or as volunteers for experiments. It was already clear by the early planning stage that, Benedict intended to continue developing the methods he had learned and perfected in Atwater's laboratory and to adopt many elements of Atwater's laboratory and project organisation, but that the CNL was to have a different focus than the research programme that Atwater had directed under the auspices of the USDA. While Atwater had concentrated on nutrition, public health and household economics, Benedict was primarily interested in metabolism and in establishing predictive norms for clinical use and for the identification of pathological conditions.

During Benedict's thirty years as its director, the CNL conducted an extensive programme of plotting normal human basal metabolism from birth to death in both sexes. This project eventually provided metabolic norms that were widely accepted in the first half of the twentieth century and, in some cases, with modifications, are still used as guidelines in contemporary metabolism predictions (Frankenfield et al, 1998). The CNL collaborated with numerous American and foreign visiting and associated researchers and other Carnegie laboratories in studies of metabolism during different physical activities and in diabetes, the affects of reduced diet, fasting and alcohol on metabolism and the metabolism of several species of animals. In his final decade at the CNL, Benedict directed a project to assess racial differences in metabolism, which, although fraught with the ubiquitous early twentieth-century racist assumptions, was ideologically undogmatic in the interpretation of its inconclusive results. Technical expertise and precision measurement were Benedict's *forte*, and the CNL produced numerous papers on the methods and techniques of respiration calorimetry and the measurement of variables.[8] It is perhaps in the measurement techniques and standards of precision, as well as the comprehensiveness of his metabolism plotting project that Benedict's most significant contributions to the scientific history of nutrition and metabolism research lie. Scientific researcher was only one of Benedict's roles, however, and in his *Reports*, he assumes a variety of persona, presenting himself as laboratory director and representative of the CIW, as scientific diplomat and liason officer, ethnographic observer, international research coordinator and technical expert.

On the Road in the Service of the Carnegie Nutrition Laboratory: *The Reports of Visits to Foreign Laboratories*

Professional, information-gathering foreign travel was included in the original plans for the new laboratory. In a memorandum to Robert S. Woodward, trustee of the CIW, composed in 1906, Benedict detailed the predicted budget needs of his new laboratory. In-

8. See the bibliography in DuBois and Riddle, "Biographical Memoir".

cluded was his recommendation that the CIW provide the not unsubstantial sum of $750 for him to visit "all the laboratories in American and in Europe where research work on nutrition is being carried out."[9] The CIW approved $1000,[10] and it continued to provide funding for European and American travels for Benedict and members of his staff until at least 1933.

Reports of these visits were compiled by Benedict and the travelling member of staff, but, with the exception of Benedict's seven tours and the report composed by one of his assistants, Walter Miles, in 1920, these reports have not yet been located, and their content and format are unknown.[11] The aims and character of Benedict's visits changed significantly over the quarter century of the *Reports*, reflecting changes in the status and interests of the CNL and in the capacities and interests of European nutritional physiology. In following, I will give a brief chronological overview of the *Reports*, before summarizing some of their recurrent themes. Benedict prefaced each *Report* with an introduction, explaining the main purposes and interests of the trip. Although these descriptions were most likely at least in part instrumental and written with the financial backers in mind, they were composed after the tour, as Benedict compiled his notes into a coherent report, and they define a thematic core of each visit.

Benedict's first three tours were undertaken in the early years of the CNL – in 1907, after funding had been secured, a site found and some apparatus ordered, but while the building was still under construction; in 1910, after research had begun; and in 1913, by which time the laboratory had established its research programme, its researchers had gathered considerable technical expertise and it had begun developing an international reputation. The reports of these three trips reflect specific concerns of each phase.

1907: Constructing the ideal laboratory

Benedict's first European tour took him from Bonn through Switzerland and Southern Germany to Budapest, St. Petersburg and Finland, from Scandinavia to Scotland and England and back across the channel to France. It concluded in Berlin, the central location for metabolism research in Europe and the site of several laboratories involved in studying animal and human metabolism. On this tour he visited a total of eighteen European cities and forty-five laboratories at university physiological institutes, hospitals and private sanatoria, agri-

9. FGB Papers, Box 2, Folder 26: BNL, Locations Studies and Reports (1903-1906): Letter and report from Benedict to Woodward, April 23 1906: "Suggestions Regarding Location, Equipment, Force, and Estimate of Cost of a Laboratory for Research in Human Nutrition. Memorandum to accompany estimates of cost", 4.

10. FGB Papers, Box 3, Folder 86: R. S. Woodward Correspondence, Woodward to Benedict, 26 Feb 1907.

11. The reports are typed on carbon paper and were dictated to his secretary. While only one copy of Benedict's *Reports* has been located, copies of Miles' *Report of Visits to Foreign Laboratories (1920)* have been found at the Yale University Medical Historical Library and the Archives of the History of American Psychology.

cultural, veterinary and medical schools. The explicit purpose of the tour was to gather information about the various possibilities of laboratory construction. Benedict justified the trip in his *Report*:

> In making this tour, the first thought was to secure all possible suggestions regarding the interior equipment of laboratories especially fitted for investigations in metabolism, calorimetry, and physiological chemistry. The second important commission was to enable the Director to become acquainted and, so far as feasible, to become familiar with all existing forms of apparatus for studying gaseous exchange, animal calorimetry, and general methods of research into human and animal nutrition (Benedict, 1907, 1-2).

Such an information-gathering visit was viewed as a necessary prerequisite to setting up the Boston laboratory.[12]

Benedict's description of the tour offers significant insights into the material culture of early twentieth-century laboratories. When negotiations ended in 1907, he was in the enviable position of being able to build a well-funded laboratory from scratch, including designing the building and designating the use and allocation of space, determining the best placement of gas, water and lighting fixtures, acquiring all laboratory furnishings and all laboratory apparatus and equipment. His attention was thus directed not only, as is to be expected, at the more elaborate equipment, such as calorimeters, respiration and gas analysis apparatus and variations in minor apparatus such as pumps and thermometers. Nor was his interest purely educational, although he did observe and describe in great detail the various experimental techniques and procedures of the laboratories he visited. He also especially noted the seemingly trivial details of these labs, weighing various options with the aim to combine them into an ideal laboratory set-up.

If descriptions of instruments and apparatus are commonplace, Benedict's *Report* reveals the small and at first glance insignificant details that have great effects on laboratory practice, on the convenience and ease of conducting experiments and keeping track of procedures. On the basis of his observations of existing laboratories, Benedict discussed and compared the advantages and disadvantages of various materials for covering table surfaces and floors, looked at the spatial distribution of workspaces, assessed means for making table heights adjustable, considered the most convenient locations for water faucets and stopcocks, studied drawer, cupboard and shelving arrangements and materials, power, heating and water supply systems and methods of decreasing the effects of vibrations. His considerations of these seemingly trivial details emphasise the extent to which they could affect and either enable or inhibit the flow of experimentation. They

12. Benedict to Woodward, April 23 1906: "Suggestions".

provide a basic material structure, within which experimentation takes place, but which become so self-evident for the researchers and seem so unimportant to most visitors that they disappear from laboratory reports and descriptions. Benedict's *Report* of 1907 shifts these questions to the foreground. Significantly, he did not seek to imitate the set-up of an existing lab, but to view as many alternatives as possible, in order to combine them into an *ideal* lab.

In light of Benedict's attention to detail in visiting other laboratories, it is perhaps ironic that he provided so little information about the choices he made for his own. Nonetheless, some information can be gleaned from his annual reports to the CIW and from professional correspondence. Through his prior work at Wesleyan and visits to US laboratories, he was aware of metabolism laboratory needs, and most architectural decisions about the CNL building were made prior to his first tour. Benedict incorporated some features that he had seen in European laboratories, such as flexibility in internal furnishings, with workspaces and shelving installed as needed, and the use of enameled lava as a tabletop material, which he imported at considerable expense from a quarry in France.[13] In addition, he examined a great number of major and minor apparatus, both on this tour and on the ones following. Although the CNL constructed its own calorimeters and respiration apparatus, Benedict purchased much specialised minor equipment and auxiliary apparatus that he had seen on his tours such as nose clips, pellet-making machines, manometers, gas analysis apparatus and precision measurement instruments from European instrument makers, laboratory workshops and equipment companies.

1910: In pursuit of tacit knowledge

By Benedict's second tour in 1910, construction was finished and active research work had begun at the CNL. It had amassed a respectable collection of apparatus for metabolism research – as Benedict claimed, one of the most complete collections in the world (Benedict, 1908: 161). Although he continued to appraise new and unknown apparatus and modifications, on this tour the communicative necessities of physiological research are the main focus. The CNL was the only research laboratory devoted solely to the study of human metabolism, and Benedict viewed the establishment of this laboratory as the internationally leading centre in this field as a part of the mandate bestowed upon him by the CIW.

The 1910 *Report* thematises in particular the importance of direct personal interaction for the coordination of research programmes across laboratory boundaries, the formation of scientific communities and the exchange of experiential knowledge. Metabolism experiments were time-consuming and difficult to execute. Benedict described the need to co-

13. For a description of the laboratory building, see: F.G. Benedict, "Nutrition Laboratory", in *Carnegie Institution of Washington Year Book* 7 (1908), 158-162; for enameled lava acquisition see NLF, Box 1, File 8. Benedict to John Woodward, 22 June 1908; NLF, Box 1, File 13, Benedict to Woodward, 18 November 1908.

ordinate experiments and research in order to avoid unnecessary duplication, to facilitate the verification of results, avoid the repetition of procedural errors and to organize cumulative and mutually supportive experimentation across laboratories. Informal interaction functioned, in addition, Benedict claimed, to cultivate the "personal element" of friendship and trust, instead of contentious rivalries, thus encouraging the free exchange of ideas, criticism and tentative results.[14] International exchanges of researchers served to raise the prestige of the hosting laboratories and standardize research methods beyond national boundaries.

The exchange of researchers was not only important for community-building and standardization. Benedict was well aware of the crucial role played by tacit knowledge in experimentation. He explained, "while a scientific investigator may write a description of his apparatus in the most beautiful language, he will, without fail, inadvertently overlook certain important minor details, which, though they may not affect the principle or the apparatus, nevertheless play a very important role in the successful conduct of experiments with it" (Benedict, *Report 1910*, 1). It was thus vital to personally inspect the equipment and observe it in use. Benedict participated in and observed experiments, received detailed explanations of how apparatus functioned and compared different versions of the same apparatus. The photographs in this large *Report* show not only apparatus taken from various angles, but also document valve constructions, ventilator connections, motors and pumps, the spatial distribution of tubes and containers for gas collection and analysis – possible variations in the experimental system. Combined with written descriptions, supplemented by reports of discussions with physiologists about their techniques and by observation of experiments in process, they provided Benedict with a translation into his own experience of experiments and apparatus that he read about in published research papers.

1913: Among equals

While Benedict justified the first tours with the new status of the nutrition laboratory and the need to gather information on apparatus and techniques, by 1913 the CNL was well-established and these justifications were no longer applicable. Instead, he emphasised the reciprocity of information exchange, with the CNL now able to contribute on equal grounds:

> The main object of the European trip, therefore, is to keep in touch with the different workers in the lines of research in which we are interested and to seek new ideas and methods for use in our own investigations. A second point of almost equal value is to disseminate information regarding our own researches […]. Such intercourse is of mutual benefit and renders possible a *entente cordiale* between ourselves and the laboratories visited (Benedict, *Report 1913*, 6-7).

14. FGB Papers, Box 7, F. G. Benedict, *Report of Visits to Foreign Laboratories 1910*, 2.

Instruments, apparatus, personnel and techniques were part of this exchange, and Benedict used these trips to recruit visiting researchers and assistants. The CNL's first official foreign visitor was the Austrian researcher, W. Falta, who collaborated there with the visiting American scholar, E. Joslin, in studies of diabetes (Benedict, 1909: 183-84). As the laboratory became more widely known, it was visited by researchers from nearly all European countries – both junior researchers and established scientists, such as Edward Cathcart and Max Rubner – who came to view the lab, collaborate with Benedict and use or train on its specialist equipment. While learning about new apparatus and techniques remained a theme of the trip, Benedict also aimed to acquaint foreign scholars with CNL apparatus and innovations. He writes of this tour, "It has likewise been possible to introduce into European laboratories apparatus which has been devised in this laboratory by supplying sketches, blue prints, and occasionally sending a model for reproduction" (Benedict, *Report 1913*, 8). Interested in establishing a standard apparatus in metabolism research, Benedict promoted the Universal Respiration Apparatus developed by his lab, translating essays on its construction and principles into French and German.[15]

Benedict's new confidence in his expertise and that of his lab emerge clearly in this *Report*. He assumed an advisory role in specific research questions and in the use and set-up of apparatus and discussed with European scientists new research questions that the CNL was or should be pursuing, such as metabolism at high altitudes and in diabetes and the metabolism of alcohol. He also sought to advertise the work done in Boston and the importance of his and his lab's contributions to the field. CNL monograph publications were distributed freely to numerous European laboratories, CNL publications were translated or abstracted for foreign journals and handbooks, and on his travels Benedict observed library practices with an eye to improving the dissemination of his laboratory's results.[16] The 1913 tour aimed to situation the CNL within the international research landscape.

The post-war visits (1923, 1926, 1929, 1932/33)

Benedict reached the height of his international influence in the years immediately following the First World War. His direct political involvement during the war had been limited to an advisory position to the US government's Food Administration, which supplied food aid

15. F.G. Benedict, (1912), "Ein Universalrespirationsapparat", *Deutsches Archiv für Klinische Medizin* 107, 156-200; F.G. Benedict, (1918), "Effets physiologiques d'une réduction prolongée du régime alimentaire expérimentée sur vingt-cinq sujets", *Bulletin de Société Scientifique d'Hygiène Alimentaire* 6 : 422-30. From 1928 onwards Benedict contributed numerous articles on the apparatus and methodology of his laboratory to E. Abderhalden, [ed.], *Handbuch der biologischen Arbeitsmethoden,* Berlin/Vienna: Urban & Schwarzenberg 1920-1939.

16. Since its establishment, the CNL had staff translators for its own publications and to translate the linguistically less accessible Russian metabolism research into English. Benedict mentions CNL translation work in numerous annual reports in the *Carnegie Institution of Washington Year book*. For a discussion of libraries see FGB Papers, Box 7, F. G. Benedict, *Report of Visits to Foreign Laboratories 1913*, 35, 210, 324-326.

to allied nations, but as a committed internationalist, he increasingly viewed his role – and that of science – in political terms. Although on his first tours, Benedict had visited the laboratories privately, in the 1920s he began attending meetings of the International Congress of Physiologists, endeavouring to facilitate the re-establishment of international scientific relations. The CIW, along with the Rockefeller Foundation, had advanced to a significant international research funding institution that now sought to extend its benevolence to the European scientific community. As a representative of that organisation in his field, Benedict wielded considerable influence, advocating for particular labs and researchers with their national governments and organising research exchanges and donations of equipment and periodicals.

Benedict's previous visits had put him in a good position to act as an intermediary in post-war European science. As an American, he had been in a privileged position prior to the war, since he was simultaneously an insider in the research field and an outsider in regard to inner-European national and scientific rivalries. He was thus able to move freely between laboratories, discussing research questions by proxy with scientists who would not speak to one another, and collecting the physiological gossip and opinions of the day. Due to his cultivation of the "personal element", on his return to Europe, Benedict was able to reinsert himself into a still conflicted European scientific scene without difficulty.

Although he originally planned to tour European laboratories every three years, the First World War led to a longer interruption in his visits. Benedict kept in touch with his European colleagues as far as possible and spoke out strongly in defense of Germany until the evidence for German atrocities could no longer be doubted.[17] European travel was interrupted during the war and remained harrowing in the aftermath, especially in Germany, where political upheaval, rampant inflation and a continuing dispute with France made border crossings difficult and the issuing and acknowledgement of visas often arbitrary. In 1920 Benedict sent Miles on a scouting mission to Europe to evaluate whether a resumption of the tri-annual tours seemed advisable. In 1923 he returned himself. The travel conditions were not good, as his report of an unexpectedly short sojourn Munich illustrates:

> Although Professor Müller did everything he could to secure my entrance into Munich under as comfortable conditions as possible, we were subjected to a great deal of annoyance and difficulties before being allowed to cross the Bavarian frontier from Vienna, and had it not been for a letter from Professor Kossel, inviting me to lecture at Heidelberg, I probably would not have been allowed to enter. As it was I had to forego my expected stay in Munich and had to be out in six hours, going on to Heidelberg (Benedict, *Report 1923*, 73).

17. See his correspondence with Max Rubner, Archive of the Max Planck Society, Abt. III, Reps. 8, 41/4/5-7.

Aware through Miles' *Report* of 1920 and through his correspondence with European colleagues of the effect of the war on European physiological research, Benedict emphasised that the importance of this visit and the following ones lay in the resumption of contact with foreign researchers, both with the Boston laboratory and amongst one another.

Despite the difficulty of travel, Benedict visited thirty-two cities on this trip. Conspicuously absent was Russia, where the political situation had changed radically due to the Revolution. Although American laboratories had seen war-time cuts to their budgets,[18] the decade between 1913 and 1923 had been a devastating one for Europeans. The state of many European laboratories was depressing. Some had been commandeered as hospitals during the war, others stripped of useful metals, and most had pre-war equipment that by the 1920s was out-dated and often in need of repair. Especially in Germany in 1923, inflation was out of control. A German professor told Benedict "of an incident with regard to a certain foundation of which he is a member of the committee. The postage of a letter sending the income cost more than the entire income from the fund" (Benedict, *Report 1923*, 74). With food shortages and a highly instable currency, funding for nutrition research was not the most pressing problem confronting the population, or even the physiologists. Benedict's German colleagues had suffered personally during the war and its aftermath, many grown thin through the wartime food shortages, shabbily dressed and poorly nourished, while others had lost family members in battle or through diseases aided by malnutrition.

Benedict and his colleague Graham Lusk of Cornell University organized donations of money and instruments to the hardest-hit German labs, and the CIW donated instruments to laboratories in other countries as well.[19] By Benedict's next visit of 1926, there were some signs of recovery, especially in Germany, but this was by no means universal. Benedict described the situation in the Physical-Chemical Institute of the University of Budapest:

> His [Professor Hári's] laboratory is without doubt the dirtiest and shabbiest I have ever seen. I do not understand how it is possible for any one to do any work under such conditions [...]. All the instruments were dirty. In the calorimetry connections there were half a dozen pieces of glass tube pieced out with rubber tube instead of one length. Hári said there is *always* a leak in the system; it is never completely tight, but they simply correct for the nitrogen leaking in (Benedict, *Report 1926*, 94).

On his return to Europe Benedict was impressed, and not positively, by the shift from metabolism to research on vitamins: "Perhaps the most important information that I got [in Aberdeen] was in the discussion with Orr of the vitamine research, which was sweeping through Europe. I found Orr to be an intense 'anti-vitamine' man, so it was rather good to

18. Archive of the Max Planck Society, Abt. III, Reps. 8, 41/8/2, Benedict to Max Rubner, 1 Nov 1922.

19. FGB Papers, Box 6, F. G. Benedict, *Report of Visits to Foreign Laboratories 1923*, 73, 10.

get a little leavening into the large mass of vitamine dough that had been rising for several months" (Benedict, *Report 1923*, 214). In the second half of the decade though, perhaps as a sign that better times had come, a new topic of metabolism research had begun to emerge. Since the late nineteenth century, physiologists had studied the effects of inanition, fasting and starvation on metabolism.[20] By 1926 they were pointing to the need for a study of the metabolism of the obese.[21]

Not surprisingly, the political situation dominated the post-war *Reports*. With tensions still running high between Germany and France, England and Belgium, Benedict began increasingly to describe his role as that of a go-between and scientific diplomat, re-establishing the broken ties between physiologists of different countries. His self-proclaimed role as an emissary of knowledge was now complemented by that of a messenger of international understanding, and, as the representative of a financially powerful American funding organization, Benedict did his best to wield his influence in political circles in support of his physiologist colleagues. By his final visit in 1932 he was referring to his "diplomatic missions," which involved not only advocating for specific laboratories and researchers with their governments, but also functioning, in his own words, as a "liaison officer" facilitating communication and clarifying misunderstandings between laboratories.[22] The painful actuality of the European political situation can be especially felt in the *Report* of the August 1932-February 1933 tour, when Benedict closed his descriptions of several German institutions with the information that in the *weeks* since his visit, the hosting researcher had "come under the ban" or left the country "on account of the Jewish situation": "One can not understand the thing at all" (Benedict, *Report 1932/33*, 200, 202, 205).

What stands out in these post-war years is a shift of research centres. Germany, and especially Berlin, had been the foremost site of physiological and metabolism research into the early twentieth century. Benedict's enthusiasm had also been fired by the research laboratory of Franz Tangl in Budapest and the St. Petersburg apparatus and programme. In the 1920s Benedict no longer visited Russia. The great Berlin metabolism tradition had also reached an end. Nathan Zuntz, who had done significant research on respiration and developed several apparatus for the study respiration under varying conditions, had passed away. The elderly Rubner was involved in other research and, Benedict confided, "I rather felt that he had 'shot his bolt' many years ago and had been

20. C. Lehmann, et al., "Untersuchungen an zwei hungernden Menschen", *Archiv für pathologische Anatomie und Physiologie und klinischen Medicin* 13. Folge – 131 Supp. (1893), 1-228; F.G. Benedict, *The Influence of Inanition on Metabolism*, Washington DC: Carnegie Institute of Washington 1907; F.G. Benedict, *A Study of Prolonged Fasting*, Washington DC: Carnegie Institute of Washington 1915.

21. For example, Professor Erich Graf of the University of Würzburg Medical Clinic, in FGB Papers, Box 6, F.G. Benedict, *Report of Visits to Foreign Laboratories 1926*, 167, 197, 382.

22. FGB Papers, Box 6, F. G. Benedict, *Report of Visits to Foreign Laboratories 1932/33*, 19.

more or less living upon that […]" (Benedict, *Report 1926*, 88). Benedict was impressed by the new facilities of the *Kaiser-Wilhelm-Institut für Arbeitsphysiologie*, which had completed its move from Berlin to Dortmund by his visit in 1929, describing with enthusiasm the care put into laboratory construction and underlining his admiration with numerous photographs of the facilities. At his return visit three years later, this state-of-the-art laboratory was largely closed or on stand-by, due to lack of sufficient funds to buy heating coal (Benedict, Report 1932/33, 184). Emerging as new centres of physiological research were the Scandinavian countries, especially Sweden and Denmark, and, to a lesser degree, Holland.

What also stands out is the altered status of the CNL. If Benedict had visited the European labs in 1907 and 1910 seeking to benefit from their established expertise and as an equal partner in 1913, by the 1920s the tables had turned, and American leadership in metabolism studies was apparent. Benedict had completed his birth-to-death metabolism project and, in collaboration with the statistician Arthur Harris of the Carnegie Laboratory for Experimental Evolution (Genetics), the results had been put on a firm biometric statistical foundation (Harris and Benedict, 1919). His lab had acquired an impressive degree of technical and experimental expertise, invited guests for training and exported apparatus and technical knowledge across Europe and the United States. The war had interrupted European metabolism research, while the CNL had continued its work, and Benedict now held lectures in numerous cities on his tour, informing the European institutions of the latest results of the CNL research and of innovations that had passed them by during the war and the following turbulent years. He was well aware of the grounds for this shift in research capacities and argued against American arrogance. The new American scientific leadership position was an accident of history, he claimed, not a sign of innate superiority: "I believe a spirit of humility should surround all our laboratory life. It is only as the result of a great cataclysm in which, by the grace of God, America was not too heavily involved, that we are financially and economically in a better situation than most of the European countries" (Benedict, *Report 1926*, 387). Nonetheless, he concluded in 1929,

> Many of the research workers are so handicapped by lack of material equipment and small means that one can expect very little in most places in the coming decade […]. Unfortunately it would appear that further immediate scientific surveys of the European situation are not needed […]. In any event, one of the deciding factors in taking up another tour to Europe would be not what the Nutrition Laboratory would receive but what the Nutrition Laboratory could give (Benedict, *Report 1929*, 306-7).

The final tour of 1932/33 only confirmed this assessment (Benedict, *Report 1932/33*, 355).

Thematic threads

Two recurrent themes of these volumes deserve particular attention. First, Benedict was, at least in the assessment of a contemporary, not a particularly gifted physiological theorist, but his technical skill and interest in technical questions were "of the highest order."[23] This interest and ability is reflected in the *Reports*, which contain extensive descriptions of instruments, apparatus and experimental set-ups, of successful constructions and blind alleys, but only briefly mention physiological theories. As such, they provide much detailed information for a history of the material culture of early twentieth-century physiology laboratories, the development of human and animal calorimeters and respiration apparatus and metabolism experimentation, but they also contain insights into instrumental practices that go beyond this special field. Second, Benedict was both an intensely social scientist, as his extended networking activities and "diplomatic missions" show, and he was the director of a large research laboratory with national and international connections and ambitions and affiliated with a financially powerful institution. Laboratory cultures, human interactions, gossip and communication play a prominent role in his *Reports*.

Instruments

A respiration calorimeter is conceptually a very simple piece of apparatus, consisting of an isolation chamber and a means for measuring energy input and output. Putting this concept into practice successfully, however, requires a high degree of skill, since isolation is difficult to maintain, when input and output are necessary (the experimental subject needs to breath), and output takes on a great number of forms, including solid and fluid waste, exhaled gases and water in breath and perspiration, heat and motion. Some of these can be measured directly, others indirectly, and many require a number of stages of chemical analysis. Physiologists wrote extensive descriptions of calorimeters and respiration apparatus, their calibration and stabilization, methods of measurement and of gas analysis, the effect of environmental variables and the means to correct for them, and the conduction of experiments with them under various conditions.[24] Use of the calorimeter and respiration apparatus, procedures for analyzing gaseous exchange and the demands of precision measure-

23. Graham Lusk to Max Rubner, 7 July 1931, Archive of the Max Planck Society, Abt. III, Rep. 8, Nr. 59/9/12-13.

24. See, for example, M. Rubner, "Calorimetrische Methodik. " In: Medicinische Facultät zu Marburg [ed.], *Zu der fünfzigjährigen Doctor-Jubelfeier des Herrn Carl Ludwig*, Marburg 1890, 33-68; W.O. Atwater and E.B. Rosa, *Description of a New Respiration Calorimeter and Experiments on the Conservation of Energy in the Human Body*, Washington D.C.: Government Printing Office 1899; W.O. Atwater and F.G. Benedict, *A Respiration Calorimeter with Appliances for the Direct Determination of Oxygen*, Washington DC.: Carnegie Institution of Washington 1905; F.G. Benedict and Th.M. Carpenter, *Respiration Calorimeter for Studying the Respiratory Exchange and Energy Transformations of Man*, Washington DC.: Carnegie Institution of Washington 1910.

ment required skill and training on the part of the experimenter and his assistants, and the apparatus and experiments often required that the experimental subjects also undergo special training or be tested for their experimental "aptitude", thus raising the question of whether the results obtained using trained or particularly adaptable subjects could even be viewed as representative.

The difficulty of constructing and experimenting with the apparatus of metabolism research is shown clearly in Benedict's *Reports*. His returning visits to the same laboratory chronicle in occasionally comical tones the stages of apparatus construction and stagnation. Many calorimeters took years to build and years to calibrate, if successful calibration was achieved at all, and many researchers developed their own apparatus, leading to great variety in calorimeter and respiration apparatus construction. Only few of these proved reliable and gave good experimental results, even when the constructing researcher had been able to observe and use calorimeters in other laboratories, that is, to acquire experiential knowledge. Few researchers, it seems, had the necessary skills and insights to build a functional respiration calorimeter or apparatus and use it productively.

In addition, apparatus that functioned well for a particular investigator could not always be translated into another laboratory setting or be used satisfactorily by another researcher. Sometimes it demanded an unusual degree of skill. Thus, while Benedict was full of admiration for the gas analysis apparatus demonstrated to him by August Krogh of Copenhagen, he "regretted that probably very few people can use it and secure such results as Krogh has secured with it," in part because of Krogh's rare ability to estimate to 1/20 of a subdivision (or to 0.0001 per cent oxygen) on his measurement scale (Benedict, *Report 1910*, 216-220). Occasionally the complexity or idiosyncrasy of the apparatus was tied too strongly to the experience of the researcher who developed it. Benedict observed with frustrated amusement the calorimeter-building activities at the Parisian *Société Scientifique d'Hygiene Alimentaire* over the course of a decade and a half, concluding in 1929, "I cannot image a more imposing display of machinery without any practical use. As I have so often remarked, if [Professor J.] Lefèvre should die tomorrow, no living man would or could go on with his equipment" (Benedict, *Report 1929*, 20).

Using a standard apparatus, equipment and materials did not always provide a solution. Small variations in the research question required modifications of an established apparatus and these were not always undertaken, as when Benedict criticized the experiments of another laboratory:

> The application of the Universal [Benedict] apparatus to the experiments with the dog showed again the apparent impossibility of people using a well worked out piece of apparatus and applying it to their own problems and at the same time giving too little attention to the various basic points necessary to have successful usage of such an apparatus (Benedict, *Report 1932/33*, 254).

There could be no one-size fits all apparatus, and even the modification of existing apparatus required a high degree of experimental and technical skill.

Further complicating the matter, not all instrument makers or suppliers were reliable. Many researchers did not have the skills to build and modify their own apparatus or make their own chemicals and instead relied on outside sources. Often they could not get their experiments to work and could not figure out why. Searching for the reason for the poor reputation of his own Universal Respiration Apparatus in France, for example, Benedict found that the instruments attributed to his design that were sold by a Parisian instrument-maker and the chemicals attributed to his name that the main French provider supplied were substandard.[25] The experiments could not have succeeded, no matter how closely the researchers had followed his procedures. The need for standard instruments and methods or for a means to make experiments on different apparatus and from different laboratories commensurable is a persistent theme in the *Reports*.

Laboratory Cultures

The study of laboratory administration was an explicit goal of Benedict's 1913 tour, and various aspects of laboratory culture and inter-laboratory or inter-researcher disputes are frequently mentioned throughout the *Reports*. Benedict could afford to be frank – his reports were confidential – and they bristle with physiological gossip. The atmosphere in the laboratory and the spatial arrangement of the workplaces, he found, had a considerable effect on research productivity. Laboratories could be set up so that projects took place in separate rooms, or so that all researchers worked together in one large room. The presence or absence of strong direction could make the difference between a successful and a stagnating research programme. In Oscar Hagemann's laboratory in Bonn, for example, he found,

> One striking reason for the lack of coordination is the fact that no one is in charge of an experiment. Each individual is given a certain part to do and does his or her part utterly independent of anyone else, and no one has entire charge or supervises. Even when the experiment is made at night, Professor Hagemann goes home and a girl with an assistant is left there with no trained man in charge" (Benedict, *Report 1910*, 23).

In some laboratories, researchers communicated freely with one another, while in others "there seemed to be a tendency for each man to work by himself and not tell any one else what he was doing" (Benedict, *Report 1913*, 206). In regard to Tangl's laboratory in Budapest, in contrast, Benedict commented,

25. FGB Papers, Box 6, F. G. Benedict, *Report of Visits to Foreign Laboratories 1929*, 31, 36, 48.

> In very few laboratories have I noted such complete harmony and such a charming atmosphere as in the laboratory at Budapest [...]. The men all worked together, there were apparently no bitter rivalries, no secrets, and none of the little unpleasantness that one sees so frequently in foreign laboratories [...]. Altogether there is quite a charming atmosphere about the laboratory and one can easily understand how so much work can be done (Benedict, *Report 1913*, 164).

The sense of community and loyalty that Benedict found in this laboratory may have affected its ability to survive the death of its director, regulate the question of succession, withstand the collapse of its budget and continue productive research under drastic material conditions.

Although the war and economic crises were clearly significant factors in the decline of many European laboratories, another can be found in the question of generational change and succession. Benedict's *Reports* reveal how vulnerable laboratory research programmes could be to changes in leadership personnel. Laboratory traditions, even those that had generated the most productive research programmes such as that of the Munich laboratory under Voit or that of Zuntz in Berlin, might continue only as long as that strong director remained active. In absence of an "heir apparent", the research programme might stagnate or undergo radical change under a new director, while the remaining, older personnel were left to stumble along on their own. In his 1907 *Report*, Benedict described the atmosphere in Munich as paralyzed, since Voit was quite elderly and his successor unclear (Benedict, *Report 1907*, 117). By 1910, the famous Pettenkofer-Voit respiration apparatus – the foundational apparatus of nutrition physiology – had been dismantled and removed, and by 1913 Benedict concluded that, with the exception of one of Voit's old assistants, "From the standpoint of technique and from experimental work one need look for very little in the line of metabolism in the Munich laboratory at the present time [...]" (Benedict, *Report 1913*, 178). The laboratory that had founded a new research specialty, trained a generation of European and North America scholars and generated an internationally renowned research programme was no longer active in the field. Zuntz' laboratory suffered a similar fate in the 1920s.

Another thread that emerges from these reports is the increasing presence of women in European physiological laboratories. Benedict was an early supporter of women in science, as Toby Appel has shown, (1994: 46-47) and he wrote numerous collaborative papers with female researchers, including his scientifically-trained wife. Although European laboratories were slower to integrate women, the *Reports* contain numerous references to their presence as technicians, students and assistants. In 1910 Benedict found that Hagemann, plagued by a scarcity of capable male assistants and weary of hung-over German students, had begun hiring young middle-class women for calculations, experimental observations, technical assistance and basic chemical analysis (Benedict, *Report 1910*, 22-23). In the

1920s Zuntz' laboratory employed a qualified female experimenter, and Benedict was suitably impressed by Marie Krogh in Copenhagen, who collaborated with her husband, August and was a capable independent researcher in her own right. Physiology was often a family affair. In 1929 Benedict commented on one laboratory he visited, "Here again we found, as one finds so frequently now in European laboratories, a very intelligent, well-trained wife working assisting her husband, not infrequently in independent research and frequently without compensation" (Benedict, *Report 1929*, 37). Whether or not he compensated his own wife, who assisted at the CNL, is currently unknown.

Conclusion

Benedict's *Reports* offer insights for a number of different histories and studies. While acknowledging their partial and subjective nature, one can trace, for example, the shifting constellations of laboratory dominance, the emergence of new research centres, the collapse of established ones and the conditions that contributed to the continuation or decline of research programmes. The decades they encompass were crucial ones in the history of physiology, covering the transition of research leadership in physiology from France and Germany to the United States, with European research divided among multiple national centres. While the rising status and independence of American physiology began in the early twentieth century, Benedict's *Reports* document unambiguously the conclusions drawn by Gerald Geisen from quantitative data, that the emergence of the United States as a leader in the field was a direct result of the effect of the war on European, and especially German physiology (Geison, 1987).

The *Reports* also provide a thick description of the material culture of early twentieth-century laboratories and of laboratory culture. They describe in great detail construction materials, apparatus, techniques and experimental set-ups, track the increasing presence and changing status of women in European laboratories, compare laboratory organisation, note the role of national politics and academic rivalries, as well as differences between publicly and privately expressed opinions, and describe the difficulties of establishing and maintaining international scientific communities. In particular, the copious visual material, photographs of laboratories and apparatus in different constellations and detail provide a visual record of laboratory set-ups and instrumentation in the early twentieth century.

In addition, the *Reports* span an important generational and theoretical transition in nutrition physiology. In 1907, nutrition physiology in Europe and North America was dominated by researchers who had trained with the first generation of quantitatively-oriented metabolism and physiology researchers, and a significant number of them had spent time in the laboratories of Voit in Munich or Carl Ludwig in Leipzig. Rubner, who first fully experimentally articulated the thermodynamic approach to metabolism, was a product of this school, as was Atwater, with whom Benedict trained, and the heavy emphasis on calorimetry, respiratory exchange, and precision measurement was its defining characteristic. By the 1920s, however, many of these second-generation nutrition physiologists had left active research or found themselves increasingly marginalized by younger scholars and the newer research on vitamins. Neither Benedict nor the CNL were able to make this transition. The laboratory continued its focus on energy transformations and metabolism measurement after Benedict's retirement in 1937, but in the mid 1940s the CIW decided this was no longer a productive field for its support. The CNL was dissolved in 1946 and its inventory sold, donated or dispersed among other CIW laboratories.[26]

26. See the correspondence between Thorne M. Carpenter and Walter Gilbert, Archive of the Carnegie Institution of Washington, Nutrition Laboratory Files, Box 3, File 1.

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SOBRE LA DIFUSIÓN DEL TELÉFONO DE BELL EN SUS COMIENZOS (1876-1877)

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Resumen: *El trabajo sigue los pasos dados por Bell para perfeccionar y dar a conocer su teléfono, y muestra un conjunto de informaciones que sobre ello fueron apareciendo en la prensa y publicaciones especializadas de Norteamérica, Inglaterra, Francia y España, desde las primeras presentaciones posteriores al registro de su patente en febrero de 1876, hasta la llegada material de su invento a Europa en los últimos meses de 1877.*

Palabras clave: *Historia del teléfono, Alexander Graham Bell, difusión de los inventos en el siglo XIX.*

On the diffusion of Bell's telephone at its beginnings (1876-1877).

Summary: *This paper follows Bell's steps to improve and publicize his telephone and presents a recollection of informations that appeared along this process in the daily press and specialized journals of North America, England, France and Spain, from the early lectures that followed the filing of his patent in February 1876 to the material arrival of his invention in Europe late in 1877.*

Key words: *Telephone history, Alexander Graham Bell, diffusion of inventions in the 19th century.*

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Introducción

Deseando valorar la credibilidad de la noticia, proporcionada por una sola fuente, según la cual el notable *electricista* español Enrique Bonnet Ballester ensayó en Cádiz teléfonos de Alexander Graham Bell en 1876, investigamos si antes de la llegada material de aparatos a Europa en el otoño del año siguiente, había circulado información escrita suficiente como para permitir que alguna persona receptiva a tan atrevida novedad experimentara con las ideas del inventor escocés. Pronto vimos que la respuesta era afirmativa y de ello hemos dejado constancia sucinta en una comunicación reciente sobre la obra telefónica de Bonnet (Sánchez Miñana y Sánchez Ruiz, 2010). No obstante, la importancia del avance técnico de Bell y la peculiaridad de ser, además, uno de los primeros netamente originados en América y exportados a Europa, nos han movido a documentar esa difusión inicial cuanto nos ha sido posible, dado que, por otra parte, no hemos encontrado ningún trabajo en la misma línea. Han guiado nuestros pasos los relatos de la actividad de Bell en estos años de 1876 y 1877 que hacen algunos de sus biógrafos, y muy especialmente el de Bruce (1990: capítulos 17 al 21), al que remitimos al lector interesado.

El presente estudio revisa prensa generalista y publicaciones especializadas de Inglaterra, Francia y España, además de Norteamérica. En cuanto a las patentes, en principio la mejor fuente de información escrita sobre cualquier invento, podían jugar un papel en su difusión cuando eran anunciadas con prontitud por las oficinas estatales correspondientes, y más si los datos proporcionados eran reproducidos por alguna revista técnica. En el caso del teléfono de Bell y Europa, solo su patente inglesa se pidió lo bastante pronto, el 9 de diciembre de 1876, como para haber contribuido a darlo a conocer.[1] Pero a pesar de anunciarse la solicitud en *The Commissioners of Patents' Journal*, probablemente también en la revista *The Engineer*, y ponerse a la venta la memoria una vez impresa,[2] no parece que ésta fuera utilizada por ninguna publicación.

1. Bell registró su invención en esta fecha, mediante un agente (William Morgan-Brown), con el número 4.765 del año y el título «Improvements in Electric Telephony (Transmitting or Causing Sounds for Telegraphing Messages) and Telephonic Apparatus», y entregó la especificación definitiva por el mismo conducto el 9 de junio de 1877 (Archivo de la Intellectual Property Office). Por entonces debió de solicitar sus primeras patentes en el continente, en Francia, Bélgica, Alemania y Austria. La del primer país fue la nº 119.626, registrada el 25 de julio de 1877, por «Perfectionnements dans la téléphonie électrique ou la transmission des sons comme dépêches télégraphiques, ainsi que dans les appareils téléphoniques», y otorgada el 26 de octubre siguiente (Archivo del Institut National de la Propriété Industrielle).
2. El anuncio se ha localizado también en *The London Gazette*, 5-I-1877, 79, y 13-II-1877, 704. Una copia de la primera memoria depositada («provisional specification») figura en WEAVER, W. D. (ed.) (1909), *Catalogue of the Wheeler Gift of Books, Pamphlets and Periodicals in the Library of the American Institute of Electrical Engineers*, vol. II, Nueva York, American Institute of Electrical Engineers, 297.

Primeras noticias

Bell hizo las primeras demostraciones públicas de su todavía muy imperfecto teléfono, después de obtener la patente básica del mismo el 7 de marzo de 1876.[3] Tuvieron lugar el 10 de mayo en el *Boston Athenæum*, para la *American Academy of Arts and Sciences*, y el 25 en el *Massachusetts Institute of Technology* (MIT) para su *Society of Arts*. No parece que la primera trascendiera a la prensa local, si bien la conferencia de Bell quedó recogida en las actas de la Academia, cuya fecha de publicación se desconoce.[4] De la segunda demostración se hizo eco el *Boston Evening Transcript* del 31 de Mayo, que insertó una crónica redactada para el periódico por el secretario de la Sociedad.

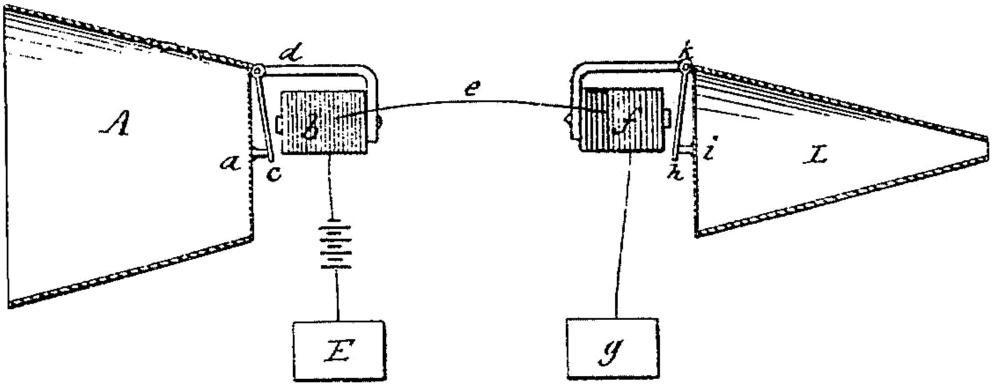

Figura 1. Transmisor y receptor en circuito en la primera patente telefónica de Bell en Estados Unidos, nº 174.465, solicitada el 14 de febrero de 1876 (U. S. Patent Office).

Llama la atención que las primeras noticias del teléfono localizadas en Europa no procedan de este documento sino de una nota aparecida en *The Daily Expositor*, de Brantford (Ontario, Canadá), con información proporcionada seguramente por la familia de Bell que, con el padre al frente, Alexander Melville Bell, continuaba viviendo en esa ciudad después de la marcha de su hijo a Boston.[5] La copió una de las revistas técnicas entonces más importan-

3. La famosa patente de los Estados Unidos nº 174.465, solicitada el 14 de febrero de 1876.

4. BELL, A. G. (1877), «Researches in telephony», *Proceedings of the American Academy of Arts and Sciences*, new series, vol. IV; whole series, vol XII; from May, 1876 to May, 1877. Selected from the records, Boston, Press of John Wilson and Son, 1877, 1-10. Es posible que la publicación de las actas se fuera haciendo por fascículos, sin esperar a reunirlas en un tomo.

5. De hecho, el inventor había mostrado algunos de sus aparatos en la oficina de telégrafos de la ciudad en setiembre del año anterior, durante unas vacaciones, y el *Expositor* había informado de ello, recogiéndolo después el *Toronto Globe*, a lo que parece sin más consecuencias.

tes en materia de electricidad, *The Telegraphic Journal*, de Londres, en su número del 1 de junio de 1876.[6]

La nota del *Telegraphic Journal* / *Expositor* está ciertamente bien documentada, pues comienza diciendo que Bell acaba de conseguir en Washington la patente de «un nuevo método de telegrafía, por medio de una corriente ondulatoria en lugar de intermitente», y subraya las palabras «ondulatoria» e «intermitente», con lo que está señalando la clave del invento, su radical diferencia con los varios teléfonos llamados *musicales*, existentes o en desarrollo.[7] La descripción de los aparatos es menos afortunada, aunque no incorrecta:

> ... de un electroimán en un extremo de un único alambre se oyen salir los sonidos de la voz humana —tonos y palabras hablados o cantados delante de una membrana conectada con un electroimán en el otro extremo del alambre.

Mucho más precisos fueron los redactores del artículo «Experiments in telephony» de otra importante revista técnica inglesa, ésta de carácter general, *The English Mechanic and World of Science and Art*, en la primera página del número del 11 de agosto, basándose, según ellos, en un informe que tenían en su poder de la demostración del MIT:

> Dos electroimanes de un solo polo, de resistencia de 10 ohmios cada uno, se pusieron en circuito con una batería de cinco elementos de carbón, siendo la resistencia total de unos 25 ohmios. Enfrente de cada electroimán se colocó un a modo de parche de tambor de tripa de batihoja[8] de unas 2¾ pulgadas de diámetro, con un trozo circular de cuerda de reloj pegado en el centro de la membrana de cada parche. Uno de estos teléfonos se colocó en la sala del experimento y el otro en el sótano de una casa vecina.[9]

6. En «Notes», 160. La revista cita al *Expositor* pero no indica su fecha.

7. En el transmisor de los teléfonos musicales un diapasón abría y cerraba sucesivamente al hacerlo vibrar el circuito de una pila. La corriente intermitente así generada hacía vibrar a otro diapasón idéntico en el receptor, mediante un electroimán. Utilizando en ambos extremos sendos conjuntos de diapasones ajustados, por ejemplo, a las notas de una octava, se podían enviar melodías sencillas. Bell, como otros antes y a la vez que él, trabajó en estos sistemas, con vistas a la telegrafía *múltiplex*, es decir la transmisión simultánea de mensajes, y fue precisamente experimentando con ellos como llegó a inventar su teléfono, en el que los sonidos daban lugar a corrientes no intermitentes sino de variación continua, reproduciendo todos sus matices.

8. Batidor de oro o plata («goldbeater's skin» en el original).

9. Esta descripción, que continúa en el original, coincide literalmente con la publicada por el *Boston Evening Transcript* del 31 de mayo. Los redactores de *The English Mechanic* pudieron tener ésta a la vista o quizá ya dispusieran del acta de la sesión de la Society of Arts que, escrita por la misma mano, no tendría por qué ser diferente.

El relato de William Thomson

En Junio Bell había llevado su invento a la Exposición Universal de Filadelfia, *The Centennial Exhibition*, conmemorativa del primer centenario de la independencia de los Estados Unidos. Allí estuvo sólo unos pocos días, prácticamente lo justo para que el 25 pudieran verlo y probarlo oficialmente los miembros del jurado. Transmisor y receptor ya no eran idénticos, pues mientras que el primero era del tipo ensayado en el MIT, el segundo llevaba un electroimán especial que actuaba no sobre la pequeña pieza de acero pegada a la membrana sino sobre una lámina enteramente metálica a él sujeta. Los jueces quedaron muy impresionados, y especialmente uno de ellos, Sir William Thomson, futuro Lord Kelvin, para quien al día siguiente se hizo en privado una nueva demostración.

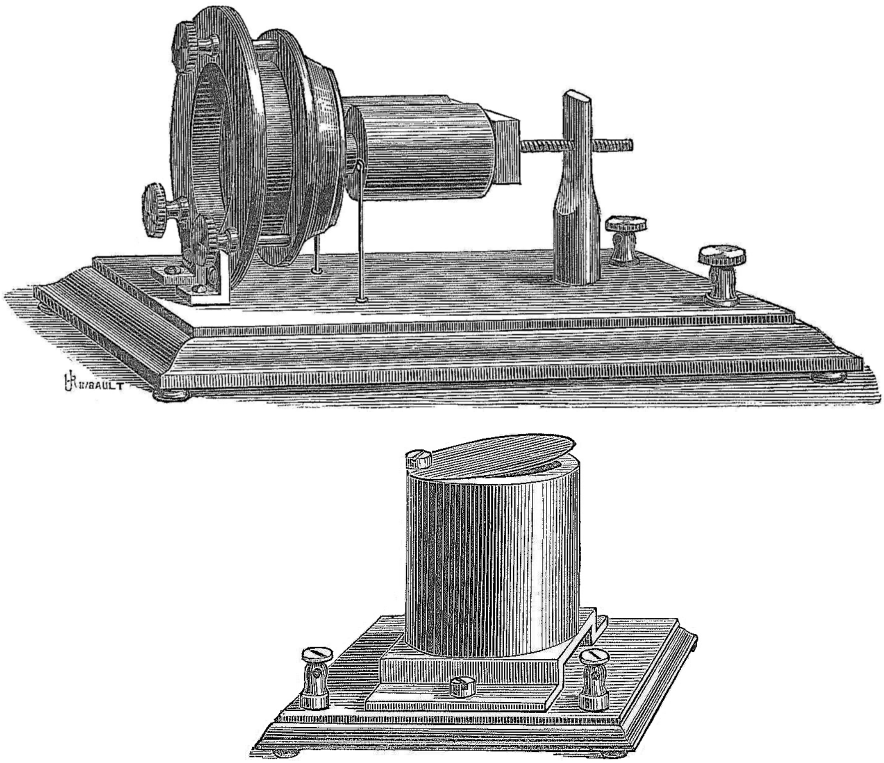

Figura 2 y 3. Transmisor (arriba) y receptor del tipo de teléfono presentado por Bell al jurado de la Exposición Universal de Filadelfia. El ejemplar dibujado es seguramente el que regaló a Thomson y éste mostró en la British Association («Bell's articulating telephone», *Engineering*, 22-XII-1876).

Antes de embarcarse rumbo a Inglaterra, Thomson pasó por Boston y Bell le regaló un juego de teléfonos como los que había visto en Filadelfia. Con ellos se presentó en Glasgow el 7 de setiembre, ante la sección de «Physical science» que presidía, dentro del congreso anual de la *British Association for the Advancement of Science*, y en su discurso inaugural comenzó por repasar las novedades científicas que más le habían llamado la atención en su viaje a Estados Unidos. Refiriéndose en particular a la Exposición, éstas fueron sus entusiastas palabras sobre el invento de Bell, según la crónica telegráfica que el *Times* de Londres publicó al día siguiente.[10]

> En el departamento canadiense oí «To be or not to be —there's the rub», a través de un alambre de telégrafo eléctrico; pero dejando a un lado los monosílabos, la articulación eléctrica se elevó a mayores alturas, y me dio pasajes tomados al azar de periódicos de Nueva York: —«Ha llegado el S. S. Cox» (yo no pude tomar el S. S. Cox), «la ciudad de Nueva York», «el senador Morton», «el Senado ha resuelto imprimir mil copias extra», «los americanos en Londres han resuelto celebrar el próximo 4 de julio». Todo esto lo escucharon mis propios oídos, hablado con inconfundible claridad por la armadura, en forma de delgado disco circular, de otro pequeño electroimán como éste que llevo en la mano. Las palabras las gritaba con voz clara y alta mi colega juez el profesor Watson en el otro extremo de la línea, acercando la boca a una membrana tirante, tal como la que ustedes ven aquí, provista de una piececita de hierro dulce, a la que así se le imprimían, en la vecindad de un electroimán en circuito con la línea, movimientos proporcionales a los sonoros del aire. Ésta, la mayor con mucho de todas las maravillas del telégrafo eléctrico, es debida a un joven compatriota nuestro, Mr. Graham Bell, de Edimburgo y Montreal y Boston, que ahora está naturalizándose ciudadano de los Estados Unidos. ¿Quién puede sino admirar el atrevimiento de invención que concibió un modo tan nimio de llevar a la práctica el concepto matemático de que si la electricidad ha de transportar todas las delicadezas de calidad que distinguen al habla articulada, la intensidad de su corriente debe variar de forma continua y, tan aproximadamente como se pueda, en proporción directa a la velocidad de las partículas del aire implicadas en la constitución del sonido?

Todo parece indicar que lo dicho por Thomson, avalado por su prestigio,[11] constituyó de hecho, no obstante los precedentes mencionados, el verdadero anuncio del teléfono en Europa. Inmediatamente la revista inglesa *Nature* publicó íntegras las comunicaciones de

10. El texto completo de la crónica, que recoge todas las intervenciones del día en el congreso, ocupa más de siete columnas enteras del inmenso periódico, y debe de ser el tomado taquigráficamente por la propia organización, puesto que coincide con el publicado posteriormente en las actas («Report of the Forty-sixth Meeting of the British Association for the Advancement of Science; held at Glasgow in September 1876. London: John Murray, Albemarle Street. 1877»).

11. Thomson no pudo hacer funcionar ante su auditorio los aparatos que trajo de Estados Unidos, al parecer por haberse doblado el diafragma del receptor, que pudo quedar tal como lo representa el dibujo de *Engineering* de 22 de diciembre siguiente, citado más adelante.

Glasgow,[12] y al poco le siguió la francesa *La Nature* con un breve resumen de lo tratado, en el que destacaba la noticia del invento, aunque omitiendo los pequeños detalles descriptivos dados por Thomson.[13] *The Telegraphic Journal* se ocupó también del congreso, en lo relativo a la electricidad, aportando nuevos datos sobre los aparatos de Bell. Nuestra traducción intenta conservar la poco afortunada y algo confusa redacción del original.[14]

> [El transmisor] consiste en una membrana estirada verticalmente en un marco circular de latón y que lleva en su centro un trozo delgado oblongo de hierro dulce, que vibra con la membrana frente a los polos de un electroimán de herradura, fijado en posición horizontal y con su alambre en circuito con el instrumento receptor o teléfono, que no es nada más que un electroimán vertical metido en una caja de hierro (similar a los que fueron objeto de un artículo de Mr. C. V. Walker, leído a la Society of Telegraph Engineers el pasado diciembre[15]), y provisto de una armadura de chapa de hierro del espesor del papel de dibujo,[16] atornillada en su borde al extremo superior de la caja pero no tocándola en ningún otro punto.
>
> Las vibraciones de la membrana debidas a la voz hacen que el inductor de hierro dulce vibre frente al electroimán, induciendo así un conjunto de corrientes inducidas en el alambre que lo rodea y por lo tanto en el del instrumento receptor en la estación distante; y una vibración correspondiente se establece en la placa delgada de hierro o armadura, suficiente para producir vibraciones sonoras audibles, por las que las palabras articuladas pueden distinguirse con claridad. La peculiaridad esencial de este instrumento y la que hace que difiera de intentos previos de obtener el mismo resultado consiste en el hecho de que las vibraciones comunicadas no solo son síncronas con las de la membrana transmisora, sino a la vez similares en calidad, debido a las variaciones en la amplitud y por tanto a la influencia inductiva del inductor vibrante.

En el mismo artículo la revista aseguraba a sus lectores que muy pronto les ofrecería «una descripción ilustrada de los dos instrumentos», pero no lo hizo hasta el 1 de agosto de 1877,[17] cuando ya, como se verá, Bell había desarrollado otros mejores. Fue otra publicación técnica inglesa, *Engineering*, la que incluyó por primera vez sendos dibujos de ellos en

12. Vol, XIV, 14-IX-1876. Las palabras de Thomson relativas al teléfono en p. 427.
13. Año IV, semestre 2º, nº 174 (30-IX-1876), 282-284.
14. 1-X-1876, 257, en «A summary of electrical science at the British Association».
15. La cita parece equivocada. Walker presentó su comunicación sobre este tipo de electroimanes en la sesión ordinaria de la Sociedad del 12 de abril de 1876 (véase *Journal of the Society of Telegraph Engineers*, tomo V (1876), 153-180). La confusión de la revista puede venir de que su supuesto inventor, John Faulkner, los había mostrado a la Sociedad en una reunión que llamaban «conversazione», celebrada el 21 de diciembre anterior.
16. «Cartridge paper» en el original.
17. «The telephone», 175-177.

su número de 22 de diciembre de 1876,[18] acompañados de un relato del funcionamiento de los aparatos muy similar al del *Telegraphic Journal*, aunque más sucinto y claro, y de la cita textual de las palabras de Thomson en Glasgow.

A diferencia del *Times* de Londres, no parece que los diarios de París recogieran el anuncio de Thomson. De hecho, de los cuatro consultados, *Le Figaro*, *La Presse*, *Le Temps* y *Journal des Débats Politiques et Littéraires*, solamente los dos últimos se ocuparon del congreso. *Le Temps* publicó cinco pequeñas crónicas, pero de lo dicho por Thomson sólo se quedó con que había «contado los detalles de su viaje por América y combatido la opinión de los geólogos que asimilan la tierra a una bomba llena de un fluido en ignición».[19] Aun fue más breve el *Journal des Débats* que en un único *suelto*, el 14 de setiembre, escribió simplemente que Thomson había relatado el viaje que acababa de hacer a los Estados Unidos. De todos modos, unas semanas después, el 5 de octubre, el afamado redactor científico de este periódico, Henri de Parville, comenzaba su folletón con la frase «desde hace algunos días se hace cierto ruido en torno a "una verdadera maravilla telegráfica", por emplear la expresión utilizada», para continuar refiriéndose al aparato «experimentado en América por los profesores Thompson [sic] y Watson», es decir el de Bell probado por los jueces de la Exposición, y afirmar sin reparos que era el mismo inventado años atrás por Johann Phillip Reis,[20] uno de sus precedentes de corriente intermitente.

Alimenta la sospecha de que la novedad del teléfono pasó desapercibida a la gran prensa parisina el hecho de que uno de sus colegas de Madrid, siempre atentos a ella, obtuviera la noticia de una fuente francesa bien distinta. Fue *El Imparcial* del 5 de octubre:

> En el Journal Officiel de la République Française hemos leído la sorprendente noticia de un invento que casi podemos llamar maravilloso:
>
> «Sir William Thomson, presidente de la sección de ciencias físicas de la Royal Britannic [sic] Association, explicaba hace poco en Glasgow ante un distinguido auditorio, las maravillas científicas que había presenciado en su reciente viaje a América. Entre éstas, los progresos realizados en telegrafía son tan pasmosos que parecen milagros.
>
> Con solo un alambre, merced a la ingeniosa combinación debida a Elisa [sic, por Elisha] Gray, pueden enviarse simultáneamente cuatro telégramas [sic]; el telégrafo automático de Edison trasmite [sic] 1,015 palabras en 57 segundos; pero la maravilla de las maravillas es sin duda alguna el telégrafo que habla, que trasmite a la extremidad opuesta del alambre clara y distintamente todo lo que se dice en el punto donde se opera.
>
> Yo he oído, dice el sabio físico, con mis propios oídos, y de la manera más inteligible, lo que un pequeño disco circular reproducía, palabra por palabra, que en el otro

18. «Bell's articulating telephone», 518-519.

19. Primera crónica del 10-IX-1876. Las otras aparecieron los días 12, 15, 16 y 17.

20. Parville le llama «Reuss».

> extremo del alambre articulaba mi colega el profesor Watson. Este había aplicado su boca cerca de una membrana muy tirante, que sostenía una ligera pieza de hierro dulce, colocada de tal suerte que pudiese comunicar a un aparato eléctrico magnético vibraciones proporcionales a la conmoción sonora del aire. Estas se trasmiten por el alambre al disco del extremo opuesto, que repite fielmente, según yo he experimentado, todas las palabras».

Esta traducción del *Imparcial* es la primera noticia del teléfono de Bell que se ha encontrado en las publicaciones españolas, tanto generales como especializadas,[21] y la más correcta, no obstante imprecisiones como la referencia genérica a «un aparato eléctrico magnético» en lugar de a un electroimán. El propio periódico parece que intentó completarla, recurriendo a un corresponsal en París, pero el resultado, aparecido el 2 de noviembre siguiente, no fue precisamente bueno. Aparte su referencia a Bell como «joven aldeano inglés», valga como muestra, la descripción del transmisor:

> ... es una caja cubierta en la parte superior por una membrana elástica, tal como una piel: en una palabra, un tambor cuadrado. Sobre una de las superficies laterales un agujero da paso a un tubo exterior con su boquilla, es decir, un portavoz. Interiormente, bajo la membrana y tocando con ella, se adelanta una ligerísima lámina de metal, puesta en comunicación con un hilo telegráfico. Se habla por el torna voz y la membrana retiembla; cada vibración de ésta, fuerte o débil, larga o rápida, se comunica a la laminita de metal y el hilo telegráfico la trasmite al punto de destino. Tan sencillo es el aparato trasmitidor.[22]

Seguramente estos artículos del *Imparcial* serían copiados por otros periódicos. Así ocurrió con el segundo, aparecido en el *Diario de Cádiz* del 4 de noviembre.

En cuanto a la prensa técnica española de 1876, solo hemos localizado una muy pobre noticia del «telégrafo parlante» en la *Gaceta de los Caminos de Hierro* del 29 de octubre, copiada por la también madrileña *Gaceta Industrial*.[23]

Un año de perfeccionamientos, experiencias y noticias

Bell, tras su fugaz presencia en la Exposición de Filadelfia, continuó trabajando para perfeccionar el teléfono y dar a conocer sus posibilidades. *The Boston Globe* del 19 de julio, bajo

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21. La *Revista Europea*, Madrid, 6-VIII-1876, 188-189, publicó una pequeña descripción del teléfono *musical* de Johann Philipp Reis, resumiendo sin figuras la firmada por Charles Bontemps en *La Nature*, 17-VII-1876, 108-110.

22. En la sección «Carta de París», firmada «S».

23. *Gaceta de los Caminos de Hierro*, año XXI, nº 44, en «Crónica general», 709, y *La Gaceta Industrial*, penúltimo número de 1876, sin fecha (seguramente 10-XII), en «Noticias diversas», 351.

el titular «Telegraphing a tune», se refirió encomiásticamente a algunos de sus experimentos recientes. Thomson, que había ido a visitarle, le acompañó en los de la noche del 13 sobre líneas telegráficas, y se llevó como regalo los aparatos que después exhibiría en su conferencia de Glasgow.[24]

De vacaciones en Brantford desde el 24 de julio, Bell hizo allí demostraciones públicas el 3 y 4 de agosto.[25] La noticia del *Expositor* fue recogida por *The Globe* de Toronto y de aquí pasó al *Scientific American* del 9 de setiembre,[26] que con ella difundía por primera vez el invento entre sus muchos lectores, interesados en la tecnología profesionalmente o por simple afición. La revista compuso su artículo anteponiendo a la noticia del *Globe* información aparentemente tomada del *English Mechanic* del 11 de agosto, sin citarlo y sin tampoco mencionar el contexto en que se había producido, es decir la conferencia de Bell en el MIT, y es importante señalar que esta información incluía la descripción técnica transcrita más arriba.

A finales de agosto, Bell ya estaba de regreso, y el *Boston Globe* informó el 20 de octubre[27] de su primera conversación «de larga distancia», mantenida el día 9 anterior con su ayudante, el mecánico Thomas A. Watson, a través del hilo telegráfico de unas dos millas de longitud que comunicaba una fábrica en East Cambridge con su oficina en Boston. Las frases enviadas y recibidas, que cada uno había anotado cuidadosamente para cotejarlas después, las publicó el 19 otro diario más madrugador, el *Boston Advertiser*, en una crónica que reprodujo el *Scientific American Supplement* del 25 de noviembre.[28] A esta prueba se refirió también en su número de enero de 1877 la revista mensual, también de los Estados Unidos, *The Manufacturer and Builder*.

Un conjunto de mejoras en los teléfonos, y especialmente la sustitución de los electroimanes por cierto tipo de imanes permanentes, permitió semanas después abordar la comunicación a distancias mucho mayores. Utilizando los alambres del telégrafo del ferrocarril, Bell conversó desde Boston el 26 de noviembre con Watson, que se encontraba en Salem, a 16 millas, un logro que fue recogido por cuatro periódicos de Boston, entre ellos el *Globe* del 27.[29] El 3 de diciembre Watson se desplazó hasta North Conway, New Hampshire (143 millas), con resultados menos satisfactorios por el mal estado de la línea. La experiencia con

24. Véase la carta de Bell a sus padres de 17 de julio de 1876 en «The Alexander Graham Bell's family papers at the Library of Congress», colección de documentos en Internet de esta biblioteca de los EE. UU.

25. Estas son las fechas que da BRUCE, 201-202, que no coinciden con las del *Scientific American*. Las primeras parecen las correctas, a juzgar por las cartas de Bell a su prometida, Mabel Hubbard, que en la colección mencionada figuran como del 4 y 6 de agosto de 1876, advirtiendo que la primera debió de ser del 5 (Bell escribió «Saturday, August 4th» y el sábado era 5) y la segunda fue del 4, pero está identificada incorrectamente.

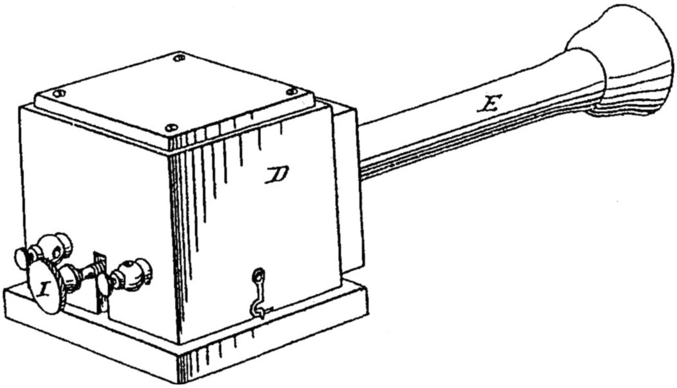
26. «The human voice transmitted by telegraph», 163-164.

27. «The new art of telephony».

28. «Telephony. Audible speech by telegraph», 765. Una reproducción fotográfica de parte del artículo del *Advertiser* puede verse en CASSON, H. N. (1922), *The History of the Telephone*, 10th edition, Chicago, A. C. McClurg & Co, entre pp. 48 y 49.

29. «Professor A. Graham Bell's great invention – Talking over the telegraph wires – Successful experiments yesterday».

Salem se repitió el 21 de enero de 1877 e informó de ella el *Globe* del día siguiente.[30] Fue tan bien que el superintendente del telégrafo escribió a Bell pidiéndole la representación del teléfono para tres estados de Nueva Inglaterra, asegurándole que se podría vender muy bien y que ya tenía muchos pedidos.[31]. Los nuevos aparatos, montados en caja de madera y con boquilla saliente, en un formato que con más o menos variaciones iba a ser el utilizado en los próximos meses, quedaron patentados el 30 de enero de 1877.[32]



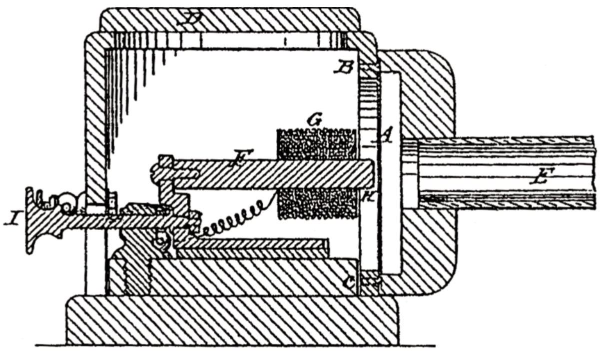


Figura 4 y 5. Aspecto exterior y corte vertical del transmisor/receptor del teléfono de caja en la patente de Bell en Estados Unidos nº 186.787, solicitada el 15 de enero de 1877 (U. S. Patent Office). El dibujo del corte lo publicó también *Scientific American*, 31-III-1877, en «The telephone».

Otro ensayo con éxito, entre la oficina de una empresa en Boston y la casa de uno de sus directivos en Malden, Massachusetts, el 31 de enero, motivó que en el *Transcript* de la primera ciudad apareciese al día siguiente una columna que, además de la noticia, incluía una

30. «Speaking by telegraph». Reprodujo la noticia el *Hartford Courant* (de Hartford, Connecticut) del 25.

31. Carta de Bell a su prometida del 21 de enero de 1877, colección citada.

32. Patente de los Estados Unidos nº 186.787, solicitada el 15 de enero de 1877.

reseña biográfica de Bell, con referencia a sus antecedentes en la enseñanza de los sordomudos, y, sobre todo, una excelente descripción de los teléfonos perfeccionados.[33] Esta columna la reprodujo el *New York Times* del 3 de febrero,[34] y parcialmente el *Boston Globe* del 15[35] y el *Scientific American* del 24.[36] Este último periódico, cuyo *Supplement* acababa de copiar en su número del 10 el artículo antes citado de *Engineering* sobre los teléfonos regalados a Thomson[37], puso así al día a sus lectores:

> El teléfono, en su forma actual, consiste en un imán permanente compuesto,[38] a cuyos polos se unen bobinas de telégrafo ordinarias de alambre aislado. Frente a los polos, rodeados por estas bobinas de alambre, se coloca un diafragma de hierro. Completa la disposición básicamente una boquilla para converger el sonido sobre este diafragma. Como es bien sabido, el movimiento de acero o hierro frente a los polos de un imán crea una corriente de electricidad en las bobinas que rodean los polos del imán, y la duración de esta corriente de electricidad coincide con la duración del movimiento del acero o hierro movido o vibrado en la proximidad del imán. Cuando la voz humana hace vibrar el diafragma, se inducen ondulaciones eléctricas en las bobinas que están en torno al imán, precisamente análogas a las ondulaciones del aire producidas por la voz. Estas bobinas se conectan con el alambre de la línea, que puede ser de cualquier longitud, siempre que el aislamiento sea bueno. Las ondulaciones que se inducen en estas bobinas viajan por el alambre de la línea, y, pasando por las bobinas de un instrumento de construcción precisamente similar en la estación distante, el diafragma de este instrumento las convierte de nuevo en ondulaciones del aire.

La Nature de París, del 17 de marzo, también se hizo eco del *Transcript*, incluyendo, en particular, una traducción del texto que antecede. La revista terminaba asegurando que daría próximamente el dibujo y la descripción del «telégrafo parlante», pero lo que hizo, el 7 de abril, fue publicar una colaboración basada en el artículo, ya superado, de *Engineering*.[39]

33. «The telephone», con los titulares: «Fresh triumphs for professor Graham Bell's invention. – A wire conversation yesterday between Boston and Malden.– 'The last rose of Summer' by telegraph.– Full description of the new wonder».

34. «The telephone», con los titulares: «History of the instrument and its inventor – A conversation by wire on Wednesday between Boston and a neighboring town – The 'Last Rose of Summer' sung by telegraph».

35. «The telephone – Something about the construction and working of Professor Bell's wonderful invention». La parte copiada es precisamente la relativa a la descripción del teléfono.

36. «The speaking telegraph», 120.

37. Nº 58, 912. Llama la atención que la revista tuviera que recurrir a otra europea para hacerse con ilustraciones de los teléfonos de la Exposición de Filadelfia, lo que seguramente indica que no habían aparecido en ninguna publicación de los Estados Unidos.

38. «Compound» en el original. Se refiere a que el imán no era de una sola pieza sino que estaba formado por varias láminas superpuestas y apretadas.

39. *La Nature*, año V, semestre 1º, nº 198, 251, y nº 201, 289-291. Ambos artículos llevan por título «Le télégraphe parlant». El segundo está firmado por Ch. Bontemps.

Los sonidos recibidos por el nuevo teléfono podían escucharse a cierta distancia del mismo, lo que permitió a Bell plantearse demostraciones ante audiencias mayores. La primera tuvo lugar el 12 de febrero en el *Lyceum Hall* de Salem, donde se habían congregado unos centenares de asistentes, en comunicación con Watson, acompañado en el laboratorio de Boston por algunas personas. El acto tuvo mucha difusión, especialmente porque, una vez terminado y aprovechando el enlace, un periodista del *Globe* transmitió su crónica a otro colega que se encontraba con Watson. El reportaje salió al día siguiente con los titulares: «Enviado por teléfono — Primer despacho de periódico enviado por una voz humana a través de los alambres», y de él se hicieron eco numerosos periódicos fuera y dentro de Estados Unidos, entre los ingleses el *Times* de Londres y el *London Athenæum*,[40] y entre los franceses, con mucho retraso, el parisiense *Journal des Débats*, que lo mencionó en su folletón de Parville del 18 de mayo, dedicado al teléfono. El *Scientific American* dedicó al acontecimiento su artículo de portada del 31 de marzo,[41] ilustrándolo con dibujos de las escenas en los dos extremos de la transmisión, y dando nuevas explicaciones técnicas del teléfono, acompañadas de figuras tomadas de la patente de enero. Sobre este último trabajo se basó una colaboración aparecida en *La Nature* del 21 de abril.[42] *The Telegraphic Journal* del 15 de marzo se limitó a dar cuenta de las pruebas en Salem (hubo una segunda el 23 de febrero), sin entrar en detalles sobre los aparatos empleados.[43]

Las exhibiciones continuaron durante el resto de aquel invierno y la primavera de 1877. Fueron más de doce, todas en poblaciones de Nueva Inglaterra, salvo tres en la ciudad de Nueva York con Watson situado en diversos lugares próximos. Mientras tanto las mejoras en el teléfono no cesaron, y cuando Bell, recién casado con Mabel Hubbard, partió para Inglaterra el 4 de agosto, parece que llevaba en su equipaje ejemplares de caja perfeccionados y otros nuevos de mano. Pronto todos se darían a conocer en Europa.[44]

Las publicaciones españolas se ocuparon algo más del teléfono en esta etapa que en la inicial de 1876. El 20 de marzo *El Imparcial* mencionó las pruebas entre Boston y Salem,[45] y el 1 de abril, la *Revista de Telégrafos*, primera y entonces única dedicada a las aplicaciones de la electricidad, se decidió por fin a tratar del nuevo invento, publicando un artículo tomado de

40. El *Times* del 28 de febrero tituló la noticia «Stentor distanced». La del *Athenæum* fue reproducida por el *Boston Globe* del 23 de marzo sin citar la fecha. Según Casson, ob. cit., 245, habría aparecido el 3 de marzo.

41. Continuado en la p. 200.

42. Año V, semestre 1º, nº 203, 328-330, «Le télégraphe parlant. Téléphone de M. A. Graham Bell». Firmado por Gaston Tissandier e ilustrado con una versión libre de los dibujos del *Scientific American* de las escenas en ambos extremos de la línea.

43. pp. 65-66, en la sección «Notes».

44. El de mano fue conocido como «butterstamp telephone», por su forma parecida a la de una especie de sello en seco utilizado para marcar o decorar las porciones de mantequilla al moldearlas. Este aparato también recordaba al pomo de una puerta («door knob»), como escribió el *New York Times* del 1-VIII-1877 en un artículo tomado del *Boston Advertiser*, anunciando la novedad, bajo el título «The telephone and its uses».

45. De nuevo en una carta del corresponsal en París que firmaba «S», fechada el 13 de marzo.

un colega italiano,[46] y después, en los números de 1 de junio y 1 de agosto[47] tradujo los antes citados de *La Nature* de 7 y 21 de abril, respectivamente. El 25 de marzo la *Revista Europea*, citando al *Transcript*, había escrito sobre los ensayos entre Boston y Malden,[48] el 10 de abril *La Gaceta Industrial* incluyó un *suelto* sobre la demostración de Salem,[49] y el 7 de mayo el *Diario Oficial de Avisos de Madrid* hizo una referencia al teléfono como posible complemento de otro invento («electroscopio») que supuestamente podía transmitir imágenes.[50] El *Memorial de Ingenieros y Revista Científico-Militar* de 15 de junio confeccionó un artículo con materiales aparentemente de diversas procedencias y citando al *Scientific American*, con dibujos del aparato de caja utilizado.[51] Algo parecido había hecho, dentro de la prensa generalista, *La Ilustración Española y Americana* del 30 de abril, que optó por acompañar su artículo, lógicamente poco técnico, con una vista del auditorio de Salem durante una de las demostraciones.[52] Como *La Nature* del 7 de abril, pero aún más tarde, el 25 de junio, *Anales de la Construcción y de la Industria* publicó un artículo de Miguel Ángel Rebolledo, basado en el de *Engineering* de 22 de diciembre anterior y con sus mismas ilustraciones.[53]

Mención aparte merecen sendas crónicas de Nueva York de Antón Mojujo y T. Bermúdez Reina en *La Época* y *Los Lunes de El Imparcial* de 22 y 30 de abril.[54] Ambas se refieren

46. Pp. 249-251. Lo tradujo de *L'Elettricista*, revista mensual dirigida por el telegrafista Lamberto Cappanera, cuyo primer número había salido en Florencia el 2 de enero de 1877. El artículo en cuestión debió de aparecer en el siguiente (incluye una carta fechada en Londres el 22 de enero, que la redacción dice haber recibido al entrar en prensa el número), y resume la información de varias publicaciones estadounidenses, cubriendo desde las experiencias de Bell en Brandtford en agosto hasta las de North Conway en diciembre de 1876, pero su descripción técnica del teléfono es bastante confusa. La carta es mucho más clara al detallar la estructura de los aparatos, si bien se trata de los traídos por Thomson. Poco más que la *Revista de Telégrafos* madrugó el *Journal Télégraphique*, publicado en Berna por la Oficina Internacional de las Administraciones Telegráficas, que había escrito por primera vez sobre el teléfono de Bell en su número de 25-II-1877, 509-510, y aparentemente sin entender su radical novedad respecto de los aparatos *musicales*.

47. «El telégrafo parlante», 279-282, y «Nuevas noticias sobre el teléfono de M. A. Graham Bell», 309-311. El primero incluye las figuras del original. No así el segundo, que carece de ilustraciones.

48. «Los nuevos inventos. El telégrafo parlante», firmado por A. León, 381-382.

49. En «Noticias diversas», 111. Copiado por *Gaceta de los Caminos de Hierro*, 20-V-1877, 8, en «Inventos y adelantos notables».

50. Dentro de la sección «Nuevos inventos y aplicaciones industriales», firmada por León Sala.

51. «Telefonía eléctrica», 93-95.

52. La revista parece indicar en el texto (p. 275) que la ilustración representa un auditorio de North Conway, a 143 millas de Boston, en el que tienen lugar experimentos con esta ciudad, mientras que el pie del propio dibujo (p. 277) es «Boston (E. U.).– Experimentos realizados con el "teléfono" del Dr. Bell, para trasmitir la voz humana a largas distancias». El grabado salió en el *Frank Leslie's Illustrated Newspaper* de 31-III-1877, y muestra el auditorio de Salem, quizá durante la demostración del 23 de febrero.

53. «El teléfono o telégrafo parlante», 184-188, y lámina XVI.

54. La primera lleva fecha del 1 de abril y se contiene en una larga carta sobre asuntos diversos. La segunda, del 10, se titula «El teléfono – Última maravilla de la electricidad». Su autor debió de ser Teodoro Bermúdez Reina (1841-1899), militar (artillero) y periodista sevillano.

tanto al teléfono *parlante* de Bell como al *musical* de Elisha Gray, que por entonces se presentaba en la ciudad,[55] y no resultan especialmente esclarecedoras. Mojujo no vacila en escribir que el aparato de Bell «puede ponerse en comunicación con un interruptor y un aparato de Morse, y obtener un despacho hablado en una estación, escrito en signos Morse en la otra», y no obstante haber citado los ensayos desde Boston con North Conway y Salem, cree que el invento no presenta por el momento «ninguna aplicación práctica, sino en muy corta escala». Bermúdez, que pasa por alto dichos ensayos, dice del teléfono de Bell que «se halla todavía en embrión, y hasta ahora no han podido oírse conversaciones a más distancia de unos cuantos metros», y, como si transmitir música o voz fueran cuestiones distintas, augura un futuro prometedor a ambos inventos, si bien asegura que el teléfono parlante, en cuya perfección, dice, hay que tener fe, «será el más grande adelanto del siglo XIX».

No hay que ser, sin embargo, demasiado severos con estos cronistas: en fecha tan avanzada como el 20 de julio, la revista inglesa *The Engineer*, sacó un largo artículo ilustrado en la misma línea,[56] donde describía los teléfonos musicales, y especialmente el de Gray; añadía, muy bien informada, los parlantes en que Edison ya trabajaba por encargo de la Western Union para competir con los de Bell, y dejaba para el final el aparato de éste... de la Exposición de Filadelfia.

Bell y su teléfono en Europa

Un año después del anuncio de Thomson en Glasgow, los participantes en una nueva edición del congreso de la *British Association for the Advancement of Science*, en Plymouth, pudieron escuchar tres conferencias sobre el teléfono, acompañadas de demostraciones de los nuevos aparatos de Bell. Dio las dos primeras William Henry Preece, ingeniero del telégrafo del *Post Office*, el 17 y 18 de agosto de 1877, estando dirigida la segunda especialmente a obreros. La tercera, el 21, corrió a cargo del propio inventor,[57] que había llegado a Plymouth el día anterior, procedente de Glasgow, donde había sometido sus teléfonos a experimentos en la Universidad con los ayudantes de Thomson, y también los había mostrado a algunos notables locales.

55. Mojujo menciona los ensayos hechos el día anterior a su carta y la demostración que iba a tener lugar al día siguiente entre Filadelfia y Nueva York (Ver el *New York Times* del 3-IV-1877 con noticias de ambos extremos de la transmisión).

56. Pp. 37-38.

57. El desarrollo del congreso puede seguirse en *The Times* de 16, 17, 18, 20, 21, 22 y 23-VIII-1877, que publicó amplios resúmenes de las sesiones, y, más condensado, en *Nature* del 23 (lo relativo a Preece y Bell en p. 342). Las actas, «Report of the Forty-seventh Meeting of the British Association for the Advancement of Science; held at Plymouth in August 1877. London: John Murray, Albemarle Street. 1878», recogieron la primera conferencia de Preece (pp. 374-377), y el título de la de Bell («On recent experiments in telephony», p. 201), pero no el texto.

La conferencia de Preece del 17, figuras incluidas, se publicó en *Engineering* del 24 de agosto, *The Telegraphic Journal* del 1 de setiembre (sin figuras y con algunas variantes), y *Nature* del 6 (con las figuras y además con un grabado del teléfono de mano utilizado). También apareció, traducida, en *La Nature* del 29, con las ilustraciones de las revistas inglesas.[58] La conferencia de Bell acompañó a la de Preece en el *Telegraphic Journal* del 1 de setiembre.[59]

El 31 de agosto *Engineering* describió, probablemente por primera vez, el aparato de mano utilizado en Plymouth, con referencia a un corte longitudinal del mismo, dibujado aproximadamente a escala.[60] Vista y corte aparecieron el 1 de octubre en el *Telegraphic Journal*,[61] y el 6 en la portada del *Scientific American*.[62]

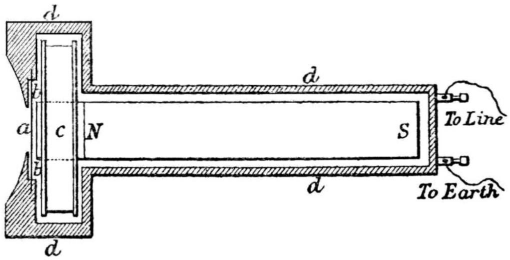


Figura 6. Corte del transmisor/receptor del teléfono de mano («Telephonic telegraphy», *Engineering*, 31-VIII-1877).

Tras las primeras exhibiciones en el Reino Unido o quizá antes, algunos teléfonos de Bell comenzaron a circular por Europa, procedentes de su equipaje o enviados directamente por su suegro, Gardiner Green Hubbard, que desde el 9 de julio de 1877 presidía en Boston la primera compañía telefónica y buscaba explotar las patentes de su yerno fuera de los Estados Unidos.[63] Así, por una carta suya a Bell, fechada el 13 de noviembre,[64] se sabe que ya había vendido dos aparatos con destino a Escandinavia y enviado una «caja de teléfonos» a un intermediario en París «para la escandinava y otras agencias». El propio Bell dio otros

58. «The telephone», en *Engineering*, 152-153, con la nota al pie: «Paper read before the British Association at Plymouth»; *The Telegraphic Journal*, 199-200, bajo el titular «Electrical Science at the British Association», y *Nature*, 403-404. «Le téléphone», *La Nature*, nº 226, 274-276. Muy tardíamente la conferencia apareció también en *La Revue Scientifique*, 10-XI-1877, «Le téléphone», 444-446, con la vista del teléfono de mano.

59. «The discovery of the telephone», 200-201.

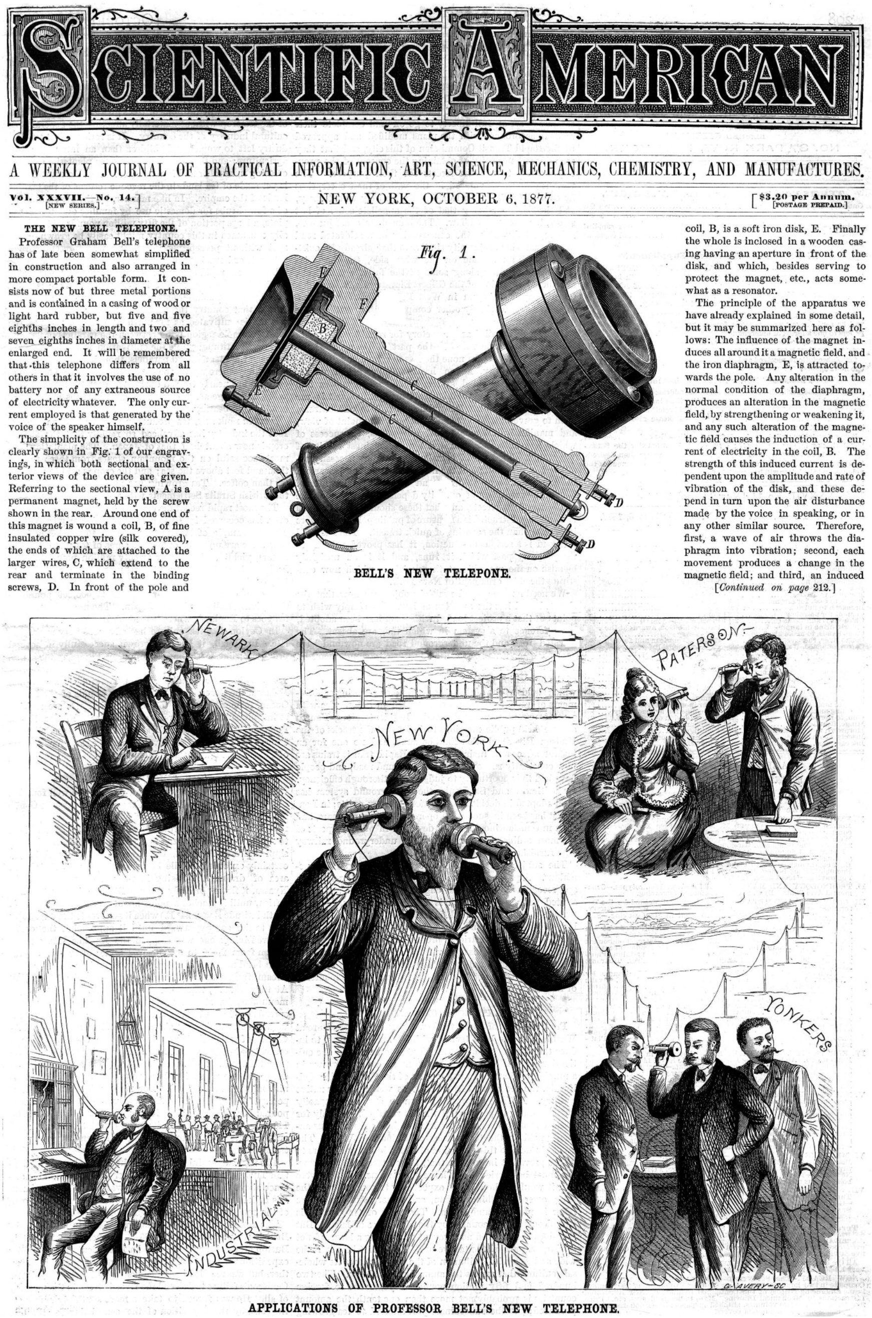
60. «Telephonic telegraphy», 170.

61. «The telephone», 233.

62. Continuado en p. 212, «The new Bell telephone».

63. La Bell Telephone Company nació como una «asociación voluntaria», sin capitalización declarada, de Bell, su suegro, Watson y Thomas Sanders. Este último había aportado hasta entonces la mayor parte de la financiación.

64. Colección citada.



SCIENTIFIC AMERICAN

A WEEKLY JOURNAL OF PRACTICAL INFORMATION, ART, SCIENCE, MECHANICS, CHEMISTRY, AND MANUFACTURES.

Vol. XXXVII.—No. 14. [NEW SERIES.] NEW YORK, OCTOBER 6, 1877. $3.20 per Annum. [POSTAGE PREPAID.]

THE NEW BELL TELEPHONE.

Professor Graham Bell's telephone has of late been somewhat simplified in construction and also arranged in more compact portable form. It consists now of but three metal portions and is contained in a casing of wood or light hard rubber, but five and five eighths inches in length and two and seven eighths inches in diameter at the enlarged end. It will be remembered that this telephone differs from all others in that it involves the use of no battery nor of any extraneous source of electricity whatever. The only current employed is that generated by the voice of the speaker himself.

The simplicity of the construction is clearly shown in Fig. 1 of our engravings, in which both sectional and exterior views of the device are given. Referring to the sectional view, A is a permanent magnet, held by the screw shown in the rear. Around one end of this magnet is wound a coil, B, of fine insulated copper wire (silk covered), the ends of which are attached to the larger wires, C, which extend to the rear and terminate in the binding screws, D. In front of the pole and coil, B, is a soft iron disk, E. Finally the whole is inclosed in a wooden casing having an aperture in front of the disk, and which, besides serving to protect the magnet, etc., acts somewhat as a resonator.

The principle of the apparatus we have already explained in some detail, but it may be summarized here as follows: The influence of the magnet induces all around it a magnetic field, and the iron diaphragm, E, is attracted towards the pole. Any alteration in the normal condition of the diaphragm, produces an alteration in the magnetic field, by strengthening or weakening it, and any such alteration of the magnetic field causes the induction of a current of electricity in the coil, B. The strength of this induced current is dependent upon the amplitude and rate of vibration of the disk, and these depend in turn upon the air disturbance made by the voice in speaking, or in any other similar source. Therefore, first, a wave of air throws the diaphragm into vibration; second, each movement produces a change in the magnetic field; and third, an induced

[*Continued on page* 212.]

BELL'S NEW TELEPONE.

APPLICATIONS OF PROFESSOR BELL'S NEW TELEPHONE.

Figura 7. Portada del *Scientific American*, 6-X-1877, con, entre otros, dibujos del teléfono de mano (corte longitudinal y conjunto).

dos en octubre a Alfred Niaudet, de la maison Bréguet, afamados constructores parisienses de instrumentos científicos y material telegráfico, en una entrevista que tuvieron en Londres.[65]

Louis-François-Clément Bréguet, patriarca de la casa, presentó los teléfonos a sus colegas de la *Académie des Sciences* el 29 de octubre.[66] Su sobrino, Alfred Niaudet, lo hizo a la *Société Française de Physique* el 2 de noviembre,[67] y a la *Société des Ingénieurs Civils* el 7 de diciembre.[68] Antes, el 28 de noviembre, su hijo Antoine había organizado una demostración en los talleres para un pequeño grupo de periodistas.[69] El diario de Madrid *La Iberia* del 11 de noviembre publicó un interesante artículo informando sobre el teléfono, su inventor y la presentación de Bréguet a la Academia,[70] y en él se basó quizá su colega *La Época* para dar el mismo día una noticia escueta de esta última.

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65. El testimonio del propio Niaudet en la presentación el 7 de diciembre a la Société des Ingénieurs Civils que se cita más adelante, fue el siguiente: «Una circunstancia fortuita me condujo a Londres el mes pasado. Se me puso en contacto con el inventor. Le propuse dar a conocer su invento en Francia. Él me confió los dos primeros teléfonos que hayan tocado el continente europeo y que aquí están». Bell escribió a su suegro el 28 de octubre: «Edison intenta ser el primero en Francia, etc., de modo que he enviado teléfonos a París para que los exhiban». Y el 1 de noviembre: «Adjunto cartas recibidas de Monsieur Alfred Niaudet, de la firma de Bréguet Hermanos [sic], los mejores electricistas de Francia. He dado a Mr. Niaudet teléfonos para que los exhiba en Francia». Parece claro que Niaudet se confundió al referirse en diciembre a «le mois dernier». De hecho, se vio con Bell en noviembre, el 21, pero fue en París (carta de Bell a su mujer de esta fecha, como las anteriores, en la colección citada).

66. *Comptes rendues des séances de l'Académie des Sciences*, tomo 85, París, 1877, 776-777. Se puede observar que la ilustración del teléfono (corte longitudinal) que lleva este texto es igual a la publicada en el *Scientific American* del 6 de octubre anterior y a la incluida en la solicitud de privilegio de introducción de Tomás J. Dalmau (ver más adelante), en ambos casos acompañadas de una vista del aparato entero. Sin embargo en todos estos dibujos se aprecian algunas pequeñas diferencias con los publicados por *Engineering* del 31 de agosto, *Nature* del 6 de setiembre y *The Telegraphic Journal* del 1 de octubre, publicaciones todas que refieren a la conferencia de Preece en Glasgow: el mango aquí es cilíndrico y no ligeramente cónico, la cabeza del tornillo de ajuste entre los terminales de conexión no lleva la pequeña anilla para facilitar –se supone– su giro, etc. Parece que el teléfono utilizado por Preece tuvo que ser uno de los que viajaron en el equipaje de Bell. ¿Serían los de Bréguet y Dalmau de un modelo posterior, no obstante haber sido dado el de Bréguet a su sobrino Niaudet por el propio Bell? Un resumen de la intervención de Bréguet en la Academia puede verse en *La Revue Scientifique*, 10-XI-1877, 454.

67. *Le Temps*, 4-XI-1877. *La Nature*, año V, semestre 2º, nº 232 (10-XI-1877), 383-384, «Première expérience du téléphone à Paris». El tono elogioso de la revista contrasta con el del periódico, que rezuma decepción: «... los éxitos obtenidos [con el teléfono], aunque muy curiosos desde el punto de vista teórico, han sido singularmente exagerados al transmitirse a distancia».

68. *Mémoires et compte rendu des travaux de la Société des Ingénieurs Civils*, vol. 30, París, 1877, 839-849. Tanto este texto como los de *La Nature* y *Comptes rendus* van ilustrados con un corte longitudinal del teléfono de mano.

69. *Le Figaro*, 29-XI-1877.

70. Es una crónica titulada «El telégrafo parlante», sin fecha, pero aparentemente escrita al día siguiente de la presentación de Bréguet a la Academia, es decir, el 30 de octubre. Su desconocido autor comienza refiriéndose al escepticismo con que se acogieron en Francia las primeras noticias sobre el invento que llegaban de América, superado después al recibirse las del *Scientific American*, y después toma de *La Nature* («según nuestras noticias —dice— el primer periódico científico francés que se ha ocupado seriamente del teléfono de Mr. Bell»), la descripción del aparato aparecida el 17 de marzo anterior. A continuación informa de que Bréguet con anterioridad a su intervención en la Academia había hecho un ensayo sobre una distancia de 30 km,

Los teléfonos llegaron a Alemania de manera bien distinta. El 24 de octubre, un jefe del *Post Office* británico que visitaba al máximo responsable de la institución homóloga del Imperio, Heinrich von Stephan, le mostró, como curiosidad, un par de aparatos que le había dado Bell. Stephan, de antemano interesado en el invento, no perdió el tiempo y dispuso la realización de ensayos, que tuvieron lugar por primera vez entre dos dependencias de su organización en Berlín el 5 de noviembre.[71]

En España, según una fuente contemporánea:[72]

> ... ocho días después de que M. Bréguet dio a conocer dicho aparato [el teléfono] en Francia presentando un modelo a la Academia de Ciencias de París, los Sres. Dalmau e hijo lo dieron a conocer en España, entregando a la Escuela Industrial de Barcelona un modelo que acababan de recibir de los Estados Unidos. Después de algunos días salieron de sus propios talleres los teléfonos que luego se han ensayado a variadas distancias.

Tomás José Dalmau García, el *hijo* de esta razón social de óptica y construcción de instrumentos científicos de Barcelona, solicitó el 19 de noviembre privilegio de introducción en España del teléfono de Bell. Los dibujos que ilustran la descripción del aparato en la memoria presentada, son los ya entonces muy difundidos del modelo de mano, que debió de ser el recibido de Estados Unidos.[73]

Hay que señalar que en Cuba ya funcionaba el 31 de octubre de 1877 una línea telefónica entre el cuartelillo de los Bomberos del Comercio de La Habana y el domicilio de su vicepresidente, conexión que fue demostrada públicamente al día siguiente. Los aparatos, al parecer del modelo de caja y en número de cuatro, los procuró e instaló uno de los voluntarios del cuerpo, llamado Enrique Hamel, que ya había dispuesto con anterioridad una red telegráfica de alarma de incendios (Sánchez Miñana, 2011).

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en presencia de algunos colegas académicos y miembros del Bureau des Longitudes, entre los que cita a «Jaye» y el «comandante Perier», muy probablemente el astrónomo Hervé Faye y el militar François Perrier. Después de asegurar que Bréguet dijo en la Academia: «Desde que tengo este pequeño aparato en la mano ya no duermo», el artículo termina con la noticia biográfica de Bell aparecida en *La Nature* del 21 de abril. Tanto la confidencia de Bréguet como la noticia del ensayo previo no han podido localizarse en publicaciones francesas de la época. En el texto de su presentación aparecido en *Comptes Rendus*, Bréguet solo menciona que había probado con éxito la comunicación intercalando una resistencia equivalente a 1.000 km de alambre telegráfico, manifestación recogida también en la crónica de *La Iberia*.

71. HUURDEMAN, A. A. (2003), *The Worldwide History of Telecommunications*, Wiley, 169, y *La Lumière Électrique*, vol. 2 (1880), nº 2, 36, «La téléphonie en Allemagne».

72. *Crónica Científica*, tomo I (1878), nº 1, 5-8, «Los teléfonos».

73. Archivo histórico de la Oficina Española de Patentes y Marcas (OEPM), expediente PR 5753. El privilegio se concedió a Dalmau, el 20 de febrero de 1878, para «un procedimiento que ha introducido de los Estados Unidos para reproducir y transmitir los sonidos y voces articulados, con su verdadero tono, intensidad y calidad, según invento del profesor Graham Bell de Boston».

Algunos comentarios finales a modo de conclusiones

Un estudio como el abordado en este trabajo necesitaría completarse con una revisión de lo sucedido en los países de lengua e influencia germánica, que a los autores no les ha sido posible llevar a cabo. Sin embargo, no parece muy aventurado suponer que la descripción técnica del primitivo teléfono de Bell se difundió en Europa antes que en los Estados Unidos, primero con el artículo de *The English Mechanic* de 11 de agosto de 1876, seguido en setiembre por la conferencia de Thomson en la *British Association*, donde mostró los aparatos traídos de América, y por último con la publicación de dibujos de ellos en *Engineering* de 22 de diciembre. Avala esta hipótesis el hecho de que todavía el 10 febrero del año siguiente el *Scientific American Supplement* copiara este artículo. Por entonces la situación se invirtió, y llama la atención que la primera noticia detallada de los teléfonos perfeccionados por Bell y Watson apareciera en un diario, el *Transcript* de Boston del 1 de febrero, y no en las revistas especializadas. Desde este momento y hasta la llegada material de los aparatos a partir de agosto, la información en los países europeos estudiados se hizo más abundante, pero a la vez, curiosamente, algo más confusa. Mientras algunas revistas describían los nuevos modelos, otras no pasaban de lo contado por *Engineering* en diciembre anterior. Incluso alguna, como *La Nature*, hizo sucesivamente ambas cosas en sus números de 17 de marzo y 7 de abril. Se podría aventurar también que el interés por los primeros teléfonos (representados por los que funcionaron para el jurado de la *Centennial Exhibition*) apenas se manifestó fuera de Inglaterra, y que fueron finalmente las noticias de demostraciones públicas en Estados Unidos con aparatos perfeccionados, las que atrajeron la atención de periódicos y revistas en todas partes.

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THE EVOLUTION OF P2P NETWORKS FOR FILE EXCHANGE: THE INTERACTION BETWEEN SOCIAL CONTROVERSY AND TECHNICAL CHANGE

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Summary: *Since the irruption of Napster in 1999, Peer-to-Peer computer networks for file exchange have been at the heart of a heated debate that has eventually evolved into a wide social controversy across the world, involving legal, economical, and even political issues. This essay analyzes the effects of this controversy on the technical innovations that have shaped the evolution of those systems. It argues that the usual image of a single two-sided conflict does not account for most of the technical changes involved. P2P entrepreneurs and creators show a wide range of motivations and business strategies —if any — and users are not a monolithic group with a common set of goals and values. As a result, the actual historical evolution of those networks does not follow a simple linear path but a more complex and multidirectional development.*

Key words: *P2P networks, file exchange, social shaping of technology, copyright controversy*

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Introduction[1]

Ever since the irruption of Napster in 1999, Peer-to-Peer (P2P) computer networks for file exchange[2] have been at the heart of a heated debate that has eventually evolved into a wide social controversy in many countries. At stake was —and still is— the model that our societies choose for creation, ownership, modification and distribution of creative material subject to intellectual property rights, such as music, videos or software in general. In the middle of this battle, which came to be known by the media as "copyright wars", even well established civil rights such as privacy of information and communication are being put into question in the name of the fight against piracy. Copyright holders plead for the right —or even the obligation for Internet Service Providers (ISPs)— of analyzing Internet traffic in order to spot illegal downloads, and ISPs —notably Comcast in the US— have tried to slow down the P2P traffic to avoid saturation of their networks and paying excessive transit fees, resulting in another public controversy known as "net neutrality".

Important as these issues are, the underlying debate is also a technological one. P2P networks are typically created on top of the Internet, which was originally a non-centralized and evenly distributed computer network. However, with the advent of the web, the privatization and the increased commercialization of Internet services[3], the net has evolved towards an asymmetric structure where a few servers provide content and a multitude of clients retrieve it. P2P networks constitute an attempt to re-empower the individual computers and hence their users, changing the Internet structure, the traffic patterns, and the balance of power.

Both ISPs and copyright owners have tried to hinder the advancement of these networks with some degree of success. Nevertheless, the P2P community has managed to modify or deploy these technologies in new and changing conditions in order to avoid the legal prosecution of its users. Along the way, P2P users have also proved not to be a monolithic group with clear and shared community oriented goals. Thus, the appearance of downloaders[4],

1. We would like to thank our colleagues Raquel Xalabarder and Joan Arnedo, and two anonymous reviewers, for their useful comments, critiques and suggestions.

2. The term P2P is used in the field of computer networks to denote a certain architecture and way of interconnecting computers regardless of the applications that run on them. In the present study we will focus exclusively on the networks conceived for the purpose of exchanging files. As in Schoder *et al.* (2005), we consider P2P networks as entities comprising three layers: communication protocols and techniques, client applications running in computers, and the communities of users themselves.

3. For a detailed historical account of the origins of the Internet see Abbate (1999). For a specific discussion on the privatization process see also Abbate (2010). The commercialization of the Internet may also be framed within the broader issue of the commercialization of scientific research —a hot topic considering present neoliberal approaches to science policy— for an introduction see Mirowski and Sent (2008).

4. Although in P2P jargon users who do not share are often called *leechers*, we have opted for the term downloaders because we believe it captures the interpretation that this group makes of P2P networks, i.e., networks for "downloading files" rather than for "sharing files".

who did not share files of their own, and entrepreneurs that tried to control these networks for their own profit, forced additional changes in the technology that were not an intrinsic necessity. All together, the picture is one of a public controversy among different social groups and actors, and also of an evolving technology, where both the social and the technological aspects seem to be intertwined with one another.

The main goal of this work is to analyze the controversy around P2P networks for file exchange by explaining why this technology evolved in different forms of distinctive network architectures and use cases as a result of the very dynamics of the controversy. This is indeed a clear case of technology —including technical details— being shaped by a social construction process where different social groups compete to embed their values and visions in the very artifact design (Pinch and Bijker, 1987). Although P2P networks have been studied from other points of view (mainly from the legal one) that take the technological artifact for granted, we will focus here in their technical shaping and evolution. To our best knowledge, such an approach combining both technological and sociological elements has never been done within this controversy, with the exception of limited studies that referred to only one instance of these networks (Spitz and Hunter, 2003).

The predecessor of P2P: IRC networks

Shawn Fanning is well-known for developing Napster, the first P2P network for file exchange, launched in early 1999. When asked in an interview what his motivation was, he replied: "It was rooted out of frustration not only with MP3.com, Lycos, and Scour.net, but also to create a music community. There really was nothing like it at the time" (Varanini, 2000). The word "frustration" is recurrent, and comes in other accounts of the same story (Greenfeld *et al.*, 2000). It points to the fact that there were a growing number of users looking for places from where they could download for free music files in mp3 format after the music industry, personified in the Recording Industry Association of America (RIAA), had started suing music downloading sites forcing them to remove copyright infringing mp3 files.

A second element worth noting in Fanning's answer is the word "community". Ante (2000b) describes Fanning as an assiduous IRC (Internet Relay Chat) user during years, and therefore he must have been well acquainted with online communities and the file exchange that was already taking place in those networks. It can be argued that IRC networks gave him the inspiration for creating Napster.

At the end of 1998 Fanning notices a problem —people cannot get the mp3s they want— and a technology —IRC— which can bring together people from anywhere in the world and enable them to exchange files, albeit in a rudimentary way. His contribution was to understand this environment, and to propose a better technological solution that he thought would solve this social need. As we will see, his solution would create new problems for some of the actors involved, and this would spark a controversy that would ultimately determine how the technology evolved.

IRC was a popular protocol for text communication over the Internet created in 1998 by Jarkko Oikarinen, from the University of Oulu, Finland, who liked tinkering with communication software in his free time[5]. The IRC application he created consisted of two parts: a server —called IRCd or IRC daemon— which was a piece of software he set to run in his Department server, and a client, which needed to run in each of the computers of the users that wanted to chat.

Oikarinen, as many other Internet pioneers, could be considered as a *hacker* in the sense defined by Eric Raymond and later used by Pekka Himanen in order to describe his Hacker Ethic (Himanen 2001). In line with this hacker spirit, and as soon as he noticed that his application became popular among his colleagues, Oikarinen provided his IRC to other computer enthusiasts in Finnish universities, who helped him improve it and contributed to making it popular in Finland and beyond. Thus, from its very beginning IRC was an open application, and it became a standard *de facto* when other hackers started to develop new client applications to connect to IRCD using other operating systems than Unix.

A distinctive feature of IRC when compared with other text messaging protocols is its "relay" characteristic, a direct inheritance of BBSs and USENET, two of Oikarinen's sources of inspiration. Relaying allowed for the interconnection of servers running in different machines and for the creation of networks of thousands or even millions of users that could chat with one another in real time. Furthermore, a very interesting protocol strongly related to IRC, and arguably another key inspiration for Shawn Fanning, was the Direct Client Connection (DCC), which allowed direct connection between clients -or peers- without going through a server.

From a functional point of view, two users wanting to establish a DCC connection with one another first need to connect to two servers within the same IRC network, then contact each other in a public chat room or channel, and finally open a DCC connection using their IRC client applications. Once this connection is open, they are free to chat directly as well as to exchange files.

DCC was first implemented by the Australian Roy Trollo as part of IRCII[6] (Rollo), a UNIX client he maintained for some time during the first half of the 1990s, and later integrated in other IRC clients. Some time later, based on DCC, the Windows client mIRC introduced a new functionality called */fserve* that provided a relatively simple way to set up a file server, and together with it came many scripts for creating file bots that automated the file sharing process. Still, file sharing under IRC required some advanced computing skills, so it could not become the kind of mainstream phenomenon that Napster became. Nevertheless, it prepared the road for Internet users that grew used to sharing files through exchange networks that socially could be considered P2P, even if the underlying computer networks were not.

5. http://www.irc.org/history_docs/jarkko.html (last checked 3/10/10)

6. A description of the DCC protocol can be found at http://www.irchelp.org/irchelp/rfc/dccspec.html (last checked 3/10/10).

Napster

Shawn Fanning created Napster while he was a first year university student. He was inspired when he saw his fellow students struggle with IRC and websites to download music while looking for mp3 files, and he came up with the idea of linking computers directly without going through a centralized file server. It was the year 1999, and the dot-com bubble was very much growing, so in a typical reaction in those times he quit studying and started programming compulsively in order to be the first to hit the market with his application (Greenfeld *et al*., 2000).

Napster shared many of the characteristics of IRC. To start with, the application came in two parts: client software that the users had to download, and a server[7] which was controlled by Fanning's company, the Napster Corporation. Then, in order to use the service, users had to register a nickname which became their persistent Napster identity, valid for the IRC-like chat rooms available through the program and also visible when downloading and uploading files.

Upon logging in, the Napster client uploaded to the server the list of mp3 files that the user was willing to share. This allowed the Napster server to keep an up-to-date list of all the files being offered for sharing. When users wanted to download a file, they had to submit a query to the server, and received in return a list of the 100 best matching files, sorted according to an estimation of the closest distance for downloading. In the end it was the user who decided from where to download the file and opened a DCC-like direct connection with the peer sharing the file.

The original program, written entirely by Fanning and released in June 1999 was an immediate hit: it reached 10 million users in its first 9 months of activity, and 80 million in the 9 months that followed (Lessig, 2004). However, the program was also highly controversial, and its legality was very quickly put into question by the music industry. The fact that Napster did not store the mp3s and that it was the users who performed the actual file sharing made Fanning and his partners believe that they were not guilty of any copyright infringement. Nonetheless, the industry felt directly attacked, and in December 1999 the RIAA filed a complaint against Napster Corporation, opening a legal battle that lasted more than a year and a half, when Napster was finally forced to shut down their service. Although the legal details of this case fall beyond the scope of this paper, it is interesting to note that the Napster trial exposed some of the conflicting meanings about what P2P networks were. Whereas Napster Corporation tried to present themselves as an ISP and therefore operating under the shelter of the Digital Millennium Copyright Act (DMCA), the court ruled in favor of the music industry, considering it to be a listing service that offered a search engine, directory, index, and links to music files. Furthermore, even if Napster's lawyers managed to demonstrate that

7. In practice Napster, had at least 160 servers interconnected with each other (Saroiu *et al*., 2002) in order to support the heavy traffic generated in its peak moments. Here we consider them as only one centralized server, although this is not strictly true.

there were a number of legal uses of the P2P network, the fact that millions of users engaged in unauthorized exchange of copyrighted material prevailed (Spitz and Hunter, 2003) and reinforced the discourse of the music industry that equated P2P with piracy.

Gnutella

Whereas some hackers tried to copy and reverse engineer Napster[8], others like Justin Frankel decided to take the P2P idea further and improve the system. Frankel had become famous in 1996 at the age of 18 for creating the popular mp3 player application Winamp. Leaving university soon after, he associated with Tom Pepper to create a company called Nullsoft and to develop Shoutcast, an application for setting up Internet radio stations inexpensively. The unique combination of Winamp and Shoutcast caught the attention of AOL, one of the biggest ISPs of the time, who bought Nullsoft in 1999 and kept the founding team on board.

When Frankel came across Napster, as an expert in online music, he was fascinated by it, but he also understood the problem of keeping a centralized database while faced with a combative and piracy-concerned music industry (Kushner, 2004). His response to Napster was a P2P network called Gnutella, which he started together with Pepper as a pet project inside Nullsoft. Besides opening the scheme to non-mp3 files too, the most significant innovation was that Gnutella completely got rid of the centralized server, making it impossible to shut down. In this new network there were to be only nodes that would talk to one another and that would collaboratively find the files requested. It was thus a real P2P network where only one type of software was needed, at the same time client and server. This new architecture matched both his hackeristic views and his knowledge about the music distribution industry:

> "Napster was a company built on people doing things that are illegal. That's wrong (...) I decided to take the wind out of Napster's sails (...) I would not be getting any money from it. I'd be giving power to people, and what can be wrong with that?" (Kushner, 2004).

In March 2000 Frankel and Pepper posted an early version of the program on the web page of Nullsoft, but they were required almost immediately by their management to remove it. At that time AOL was in merger talks with Time Warner, a music and media corporation that had sued Napster for Copyright infringement and that did not appreciate this new piece of software. Thus, the original Gnutella was only allowed to live for a few hours in Nullsoft's servers, although enough time to spread the idea and raise the interest of other hackers that would continue with the project within the open source community and re-

8. By May 2000, 7 months after the launch of Napster, there were in the SourceForge website up to 19 open source projects to develop various Napster clients and also a Napster like server http://web.archive.org/web/20000511171541/http://opennap.sourceforge.net/ (last checked 3/10/10).

lease a protocol specification known as Gnutella 0.4, which became the official reference for those who started to develop Gnutella applications.

The specification stated that the nodes of the Gnutella network were to be called *servents* (SERVers + cliENTS), and that there would be no hierarchy among them. A servent would join the network by connecting to one or more existing servents. Then, when a servent wanted to look for specific content in the network, it would send a query to all the servents it was connected to and who, in their turn, would also send it to all servents they were connected to, etc. If a servent recognized that it had the content requested available, it would send a message back with its contact details, and this message would be routed backwards in the opposite way it had been transmitted in first place. File exchange between a servent that had launched the request and the one that had replied positively would take place out of the Gnutella network using a direct one-to-one http connection.

The choice to route the query messages back and forth through the network was a way to send the queries in an anonymous way, without sender identification, and thus protected from eavesdropping and potential legal action. Nevertheless, this technological choice was rather controversial. As soon as Gnutella started to be deployed, users realized that it was much slower than Napster. As Ripeanu and Foster (2001) noticed in their measurements of Gnutella network, the combination of queries and overhead traffic mushroomed as soon as the number of hosts grew, slowing down the overall performance and user experience, and even blocking those peers who were accessing the network using a dial-up modem and did not have enough bandwidth available.

Delio (2000) describes the atmosphere of those days, where several developers disagreed on the implementation choices for Gnutella. Despite the interest from the hacker community for a completely decentralized network, the technical problems and the entrepreneurs looking for the next big commercial success drove the development towards a more hierarchical structure. For instance, a now defunct company named Clip2 built an application which was a Gnutella "super peer" that hid traffic from some servents. Clip2 ceased operations in mid-2001, but the idea of two types of nodes persisted and was taken by version 0.6 of the protocol that was introduced in 2002 (Klingberg & Manfredi, 2002). Thus, starting from this version, Gnutella distinguished between two kinds of peers: *leaf nodes* and *ultra peers*. Leaf nodes were just connected to one or more ultra peers, whereas ultra peers had connections to both leaf nodes and other ultra peers, and were responsible for routing queries and keeping the network alive. The decision for a peer to operate in one or the other mode was taken dynamically, and therefore the same software client was used for both types of nodes.

In either version of the protocol the network lacked central control, as its creators had carefully engineered in order to avoid legal action from the music industry and other copyright holders. Their approach was successful against the first wave of lawsuits like the one that ended up closing Napster. However, it would not stand up against the industry's change of tactics, which moved from targeting P2P companies to the actual users. In 2002 the RIAA started asking the ISPs for the names of the subscribers behind file exchanges, and even filed a suit against the American ISP Verizon that had refused to collaborate alleging a breach of confidentiality. In a parallel action, several lobbies of the industry jointly addressed more than 2,300 higher education institutions in the United States urging them to collaborate in stopping file sharing across their networks and eventually, the first law suits against individuals were filed in September 2003.

KazAa and the FastTrack network

Despite the prominence of American companies and individuals, P2P was not only an American phenomenon. Niklas Zennström and Janus Friis, two young Europeans and former employees from the Swedish low cost telecommunication services provider Tele2, were responsible for writing an important chapter in P2P history.

In 1999, while living in Amsterdam and looking for ideas to start up their own company, they heard about Napster. Roth (2004), in an article after an interview with them, recalls that Zennström had been frustrated about having to buy network capacity for his ISP in order to cope with the traffic imbalance caused by too many Europeans downloading and streaming content from servers in the United States. Thus, the idea of P2P and users sharing content among themselves caught on in their minds, and they decided to create a network where users could exchange any kind of content.

Zennström and Friis invested their own money and hired a team of programmers in Estonia with whom they had had relations through Tele2. In barely 4 months the team developed a protocol which they called FastTrack that had three major advantages over Napster. Firstly, it supported exchanging all kind of files and not only mp3s; secondly it did not require a big farm of computers that grew exponentially with the number of users; and thirdly, it could resume file downloads that were interrupted by locating another peer that had the same file.

Although the details of the protocol are a company secret, some parts of it have been described (Ding *et al*., 2005). FastTrack can be classified as somewhere between Napster with its centralized search and the completely decentralized Gnutella. The protocol is based on two types of nodes: *standard nodes* and *supernodes*, which were the equivalent of ultra peers in Gnutella version 0.6. In the same fashion as Gnutella, the network is difficult to shut down, as the peers work independently from a central server.

Zennström and Friis licensed the protocol to two companies that generated their own clients and P2P networks, Grokster and iMesh. Additionally, the FastTrack creators also launched their own network, called KaZaA, with its own client program. Each of these networks worked according to the same protocol, but were incompatible with one another. As for their business model, all three used client software with targeted advertising, which proved controversial among users and triggered some reverse engineering hacker projects to deactivate this feature.

FastTrack networks became the most popular P2P networks during 2003 and 2004. In a longitudinal measurement study, Karagiannis et al. (2003) observed the dominance of FastTrack over all the other P2P protocols, and Liang et al. (2004) reported that on any given day in 2004 there were 3 million users in the KaZaA network -the most popular of the three- and refer to sources that measured FastTrack traffic to be 76% of all P2P traffic in the Internet.

FastTrack's success was another blow for the media industry that initiated a new set of legal actions against the different parties involved in these networks. Even if the lack of a central server made FastTrack in theory less vulnerable than Napster, the three companies Grokster, iMesh and KaZaA were sued by the media industry in the United States. Of the three, Grokster decided to fight and lost, being forced to close by the Supreme Court in 2005. iMesh reached a settlement, which allowed it to continue operating by distributing files protected with Digital Rights Management (DRM) technologies[9], and KaZaA decided to ignore the legal actions for a certain time, on the basis that they were not operating under United States' jurisdiction. Thus, the industry also sued the KaZaA companies in their home countries, but the European side of the case proved more difficult for them, as the legal texts were different than in the United States, and neither the Dutch nor the Estonian courts found any of the accused parties guilty. However, by the time the sentences were ready, Zennström and Friis had already moved on to their new venture, the P2P telephony operator Skype, after selling KaZaA to an obscure Australian company incorporated in Vanuatu, Sharman Networks, that would eventually sign a settlement with the industry.

Structured Overlay Networks

The networks described so far, even when functionally very different, share a common approach to solving the problem of sharing files in a large network. They all start from the point of view of the downloader, and proceed as follows: first they look for the content they want to download, and then they contact the peer that hosts the file and request it. The underlying assumption is that files are available in an unstructured way in the network, and that they first need to be found before they can be downloaded.

Nevertheless, this approach is not the only one to solving the downloading problem. Soon after P2P gained interest from the public, different groups at universities and other research institutions started to make proposals for new network architectures. A distinctive set of solutions correspond to *structured networks* like Pastry (Rowstron and Druschel, 2001), Tapestry (Zhao *et al.*, 2001), CAN (Ratnasamy *et al.*, 2001) or Chord (Stoica *et al.*,

9. DRMs are a set of technologies that impose limitations on the use of digital content, for example by restricting reproduction to one specific device or during a limited period of time. Their use is controversial, and is highly criticized by organizations like the Free Software Foundation or the Electronic Frontier Foundation. Content distribution companies like Apple Corporation have now abandoned these techniques for music files, although they keep using them for other types of files.

2001). All these implementations share in common the fact that files are stored in a predictable way in the network, and that every node that contains files is identified with a certain key that relates directly to the file it contains. The correspondence between files and locations is kept through a Distributed Hash Table (DHT). As follows from this term, these algorithms are distributed, and thus do not have a central server that can be switched off. They are also based in hash functions, which can generate unique signatures per files.

Due to their complexity and limitations in handling queries and the dynamic behavior of users that join and leave the network (Taylor & Harrison, 2009), these networks were never very popular as a stand alone. Nevertheless, their techniques were later incorporated by other P2P networks, reaching some success as part of bigger systems. The most successful of these was Kademlia, a network proposed by two researchers at New York University, Maymounkov and Mazières (2002), which was later to be included in variants of eMule and BitTorrent protocols.

Mojo Nation

Mojo Nation is a failed P2P network that we are including here both as an example of the multidirectional development of the technology and also because of the influence it had on the future of P2P networks. It was the creation of Jim McCoy, a veteran of the Internet that left his job at Yahoo in May 2000 to start a company that he called Autonomous Zone Industries —the name being a reference to a novel by the anarchist author Hakim Bey— and develop his own P2P network. With a powerful libertarian inspiration (Cave 2000), McCoy defined his software as "a cross between Napster and eBay" (McCullagh, 2000), although it also compares to Freenet[10], which seems to have started as a simultaneous development with no connection between the two projects. He used his own money to finance his venture, but failed to raise new rounds of capital, and eventually went out of business in the year 2002 in what seemed a combination of lack of funding, not enough users, and bigger than anticipated technical issues.

As a network, Mojo Nation represented an important milestone in the development of P2P technologies, with distinctive features arising directly from McCoy's ideology[11]. One of the most important differences with other P2P networks was that files were not directly shared from the publishing peer's computer, but rather split, distributed and replicated through other computers of the network. In this way, when a P2P user wanted to retrieve one file from the network, the file would not just come from only one place but from several, and it would need to be reassembled before it could be used. This technique was known as *swarming*, and was conceived in order to make content available even after the origina-

10. We will discuss Freenet later on.

11. A technical description of the protocol was available at the Mojo Nation site: http://web.archive.org/web/20020127125928/www.mojonation.net/docs/technical_overview.shtml (last checked 3/10/10).

tors turned their machine off, providing the possibility of round-the-clock trading and market liquidity. Nevertheless, swarming also had a positive side effect with a dramatic improvement in download speed, allowing the network to get over the upload throughput limitations. In a traditional point to point connection between two residential users, the uploader was very likely to have more severe limitations in the uploading speed, either because of the asymmetry of ADSL technology, or simply because of ISP-imposed limitations. By using swarming, the downloader could combine several uploaders at their maximum speed until the combination reached the maximum download throughput.

As regards the conflict with music and media industries, McCoy also foresaw several mechanisms which he thought would keep him safe from being sued. Firstly, the search function was outsourced to users that were willing to run file trackers and get credit for that. So, at least in theory, Autonomous Zone Industries could not be liable for providing a service like Napster. Secondly, in the event that a content creator reported a violation of copyright, McCoy's company could mark the blocks in the network related to that file as bad blocks, and effectively stop the sharing of that file. And thirdly, he also foresaw a "tipping" mechanism by which users could make a donation to the content creators. As part of his vision, he expected that the music labels would publish using his network and be financed with the donations from downloaders (Cave, 2000).

With all its complexity, Mojo Nation attracted quite a lot of interest of the media in the year 2000, but the network never really took off. Nevertheless, some of its ideas were taken up by Bram Cohen, an employee of Autonomous Zone Industries that left the company in 2001 to start his personal project, the BitTorrent P2P client.

BitTorrent

BitTorrent constitutes another important landmark in P2P evolution, both due to its widespread success and to the acceptance (albeit partial) of the music and movie industries. Its protocol was created by Bram Cohen, a hacker who in the same tradition as Fanning and Frankel also dropped out of college during the 1990's , although he only reached notoriety at the age of 26 when he created BitTorrent (Berfield, 2008).

Cohen left Mojo Nation not with the idea of starting a company, but rather to develop a project that would give him personal satisfaction and that would suit his hackeristic values. As he expressed in an interview in 2005 after working for several start-ups that went bankrupt, he just wanted to write something for himself in his own way and give it away for free: "You get so tired of having your work die. I just wanted to make something that people would actually use" (quoted in Thompson, 2005).

BitTorrent initially became popular among Linux fans, who used the program to share and download Linux distributions, and it progressively gained acceptance among other types of users. CacheLogic, a consultancy firm that provided services for ISPs, estimated based on real Internet traffic measurements that between January and June 2004 BitTorrent's share of P2P traffic rose from 26% to 53% worldwide, whereas Fastrack descended from 46% to 19% in the same period[12]. In a second study a few months later they reported that BitTorrent was responsible for 30% of all Internet traffic at the end of 2004[13].

From its beginnings, the project was completely open source, non-commercial, coordinated by Cohen himself, and allowed for alternative software clients written by other parties. However, by the end of 2004, Cohen decided to start a company using venture capital to exploit the protocol success. Faithful to his hackeristic approach, Cohen then separated the source code availability from his own commercial enterprise, which meant moving the BitTorrent project from his personal web page to two new ones: www.bittorrent.org for the protocol development and evolution, and www.bittorrent.com, for BitTorrent Inc.

Bittorrent.org defines BitTorrent as a "free speech tool" that enables content to be published at low cost using cooperative distribution, and uses the motto "give and ye shall receive!"[14]. From this declaration we can extract two key ideas that are embedded in the protocol. First, BitTorrent was designed to share and to publish, rather than to download, and whatever didn't fit in this use was stripped out of the basic form of the protocol. This even goes to the extreme of not providing any centralized or distributed content search mechanism that could appeal to downloaders. Peers need to contact a tracker —the original publishing peer— for a specific file and they will be instructed on where to find other peers that are downloading the same file. The location of the tracker and of the content it hosts is handled outside the protocol, typically through a simple search with an Internet browser or sometimes through more sophisticated methods like DHTs established by the P2P application.

The second key idea is the motto "Give and ye shall receive". Being designed as a tool for publishers, it follows that those who need to pay a price in terms of computing power and bandwidth are the receiving peers. So, the protocol forces the downloaders to share among themselves and makes free riding virtually impossible.

The general behavior of the protocol is described in a paper by Cohen (2003). The most significant architectural element is the splitting of files into smaller blocks, an idea he borrowed from Mojo Nation's swarming distribution. Thus, as a tracker begins publishing a file, it sends different blocks to the peers that approach it and then, since each peer receives a different block, peers can start sharing the pieces they already have without the need to contact the tracker. Combining this feature with the forced upload results in faster file distribution as more users try to download the same file. This particular feature makes the protocol particularly well suited for distributing popular files.

12. http://web.archive.org/web/20061104200853/www.cachelogic.com/home/pages/studies/2004_09.php (last checked 3/10/10).

13. http://web.archive.org/web/20061018024808/www.cachelogic.com/home/pages/studies/2005_06.php (last checked 3/10/10).

14. http://www.bittorrent.org/introduction.html (last checked 3/10/10).

BitTorrent seems to provide an elegant technological solution to the debate for all the different parties. Coming from the world of hackers, it suits those who want to share. "Give and ye shall receive" is part of the hacker culture, and therefore this group feels at ease with this implementation. But BitTorrent also suits users more focused on downloading than in sharing. The improved download speeds are an advantage that arguably moved users away from FastTrack and other networks to BitTorrent. Finally, the separation of the search function from the file sharing itself is an architectural change that suits both the content providers and the P2P entrepreneurs. For the content providers, the technology now becomes neutral. Thus, they will not sue the technology developers this time, but the sharers that make unauthorized content available. Regarding the P2P entrepreneurs, they were liberated from the tedious task of developing a closed-garden proprietary software. By using commoditized standard software, a new generation of entrepreneurs focused on launching websites that offer search services like The Pirate Bay or Mininova, financed by advertising. These sites became the new target of the media industry, that tried to have the sites closed or banned using their legal arm.

eDonkey, eMule and the eDonkey2000 network

eDonkey was the original creation of Jed McCaleb, a UC Berkeley dropout who combined working as a programmer in the Bay area with his own software projects, often released as shareware. When he discovered Napster, he was so taken by the idea of summing up individual computers to make a "massive hard drive" (Healey, 2005) that he quit his job and decided to start writing his own competing application.

McCaleb's network was based on clients and servers, like the original Napster, although he proposed replacing Napster's central server with independent servers that could specialize in specific types of content, and would thus allow for faster and more targeted searches. However, what he did not foresee was that many operators would link their servers to create one large server network that was to be known as eDonkey2000 (ed2k), and that transformed the architecture of the network into something similar to Gnutella 0.6 or FastTrack. Another improvement he devised was a simple swarming mechanism, which he thought would be a way to accelerate the download of big (heavy) files -hence the name Donkey.

In 2001 McCaleb incorporated a company called Metamachine, and a few months later was joined by Sam Yagan, a Harvard graduate and young entrepreneur. McCaleb continued in charge of the technical developments while Yagan took care of the business side.

As eDonkey started to gain in popularity, its story was similar to that of KaZaA and the other FastTrack companies. As a way of getting some revenue they offered two P2P client applications, one free with advertising, and one premium that was advertising-free but had to be purchased. However, this did not please a number of users, and a team led by a hacker named Hendrik Breitkreuz decided to create an open source and improved client for Windows, which they called eMule.

In 2005, ed2k was reported by Sandvine[15] as the most popular P2P network in some European countries, including 72% of all file sharing in Germany and 80% in France, while BitTorrent was already more popular in the United States and the United Kingdom. However, Metamachine eventually got into trouble with the copyright industry, and despite attempts to reach an agreement for distributing copyrighted material, they were forced out of business in 2006 and stopped maintaining the eDonkey client.

When Metamachine ceased operations, eMule was already a mature and popular project able to work in the e2dk network. Furthermore, it had been extended with support for a DHT based network, which means that although some content was still residing in dedicated servers, many files resided in a decentralized structured network. eMule continues to be used to this date, and its web page reports that the program has been downloaded more than 417 million times[16]. The protocol is slower than BitTorrent, though it is still preferred by many users who find a wider selection of older material for downloading that is not easily found in many Torrent sites[17].

Content distribution P2P platforms

Although the music, movie and gaming industries have traditionally shown strong opposition to P2P networks, they too have experimented with this technology at different stages. As part of the judiciary or extra-judiciary agreements with some networks like iMesh or KaZaA, they asked the latter to start distributing DRM-protected files, but this didn't succeed. In a development of this strategy, the main Hollywood studios cut a deal in 2006 with BitTorrent Inc in order to create the Torrent Entertainment Network, a content distribution network that combined free files with for-rent and for-sale premium ones, all distributed using the BitTorrent protocol. This network was not a commercial success, and ended up closing in December 2008.

Other experiments were carried out by broadcasters like the UK based British Broadcasting Corporation (BBC) and British Sky Broadcasting (BSkyB) using proprietary P2P technology from a company called Kontiki in combination with DRM. This technology creates managed networks that only allow authorized content to be injected by the broadcaster and move control away from the users. BBC's iPlayer, running on this technology, was maintained between 2005 and the end of 2008, when BBC replaced it with http-based video streaming software after complaints about excessive bandwidth consumption both from users and ISPs[18]. In 2010 Sky also replaced its Sky Player application with a browser based service.

15. http://www.sandvine.com/news/pr_detail.asp?ID=88 (last checked, 3/10/10). Sandvine is a company that provides services for managing Internet traffic to ISPs, and therefore has access to real life data of its customers.

16. http://www.emule-project.net/home/perl/general.cgi?l=1&rm=download (last checked 3/10/10).

17. http://sharereactor.com/faq/ (last checked 3/10/10).

18. See http://crave.cnet.co.uk/software/0,39029471,49291924,00.htm (last checked 3/10/10) and http://news.bbc.co.uk/2/hi/technology/7336940.stm (last checked 3/10/10).

Darknets and anonymous P2P networks

Darknets and *anonymous* P2P networks represent another attempt by file sharers to go underground in the face of growing opposition to P2P usage. We can define a darknet as a private computer network where all the users know and trust each other and can be used to exchange files and messages confidentially inside the group. Biddle *et al*. (2002) connect these networks with the tradition of copying music tapes and computer programs with family and friends, and argue that darknet-based P2P networks will play an important role in the future as an easy way to exchange copyrighted material with no risk of being prosecuted.

These networks are typically designed for groups of up to 50 users, all of them sharing the same public encryption key and being able to access all the shareable content of all the other users. They do not need a central server, and due to their size they are good at locating popular content of common interest for its users, but not rare files outside the small world they represent.

Perhaps the most popular darknet to date is WASTE, an open source project created in 2003 by the P2P hacker Justin Frankel, also creator of the original Gnutella. As in the case of Gnutella, he chose to publish his program in the webpage of Nullsoft, once again infuriating its parent company AOL Time Warner, which requested the program to be removed immediately. The program as well as some variants of it continues to live in the open source community Sourceforge.

Whereas darknets are useful in small world scenarios, they are not scalable and therefore lack some of the advantages of the more conventional P2P networks. As an alternative, there is a growing number of networks that are being developed with the ambition of becoming a large, anonymous P2P network. A number of these derive from an anonymous protocol called Onion Routing that was originally developed by a group of researchers in the US Navy (Goldschlag *et al*., 1999). Examples of them are Tor and I2P.

An alternative network is Freenet, a project that grew out of a paper written by the Irishman Ian Clarke while he was a final year student at the University of Edinburgh (Clarke, 1999). Freenet is a decentralized network that allows sharing, not only of files, but also of web pages, e-mail and notice board messages in a completely anonymous way. After graduating in 1999, Clarke continued the development as an open source project, inviting other hackers to collaborate.

Freenet presents striking technical similarities with Mojo Nation, although it is generally slow and the contents shared can get lost and are not guaranteed to be found again. Freenet was conceived as a non-commercial tool to promote freedom of speech and defense of democracy by ensuring that "the government cannot control its population's ability to share information, to communicate"[19]. The project is still under development, but the network is fully usable and its creators have made efforts to make it available to the peoples of

19. http://freenetproject.org/philosophy.html (last checked 3/10/10).

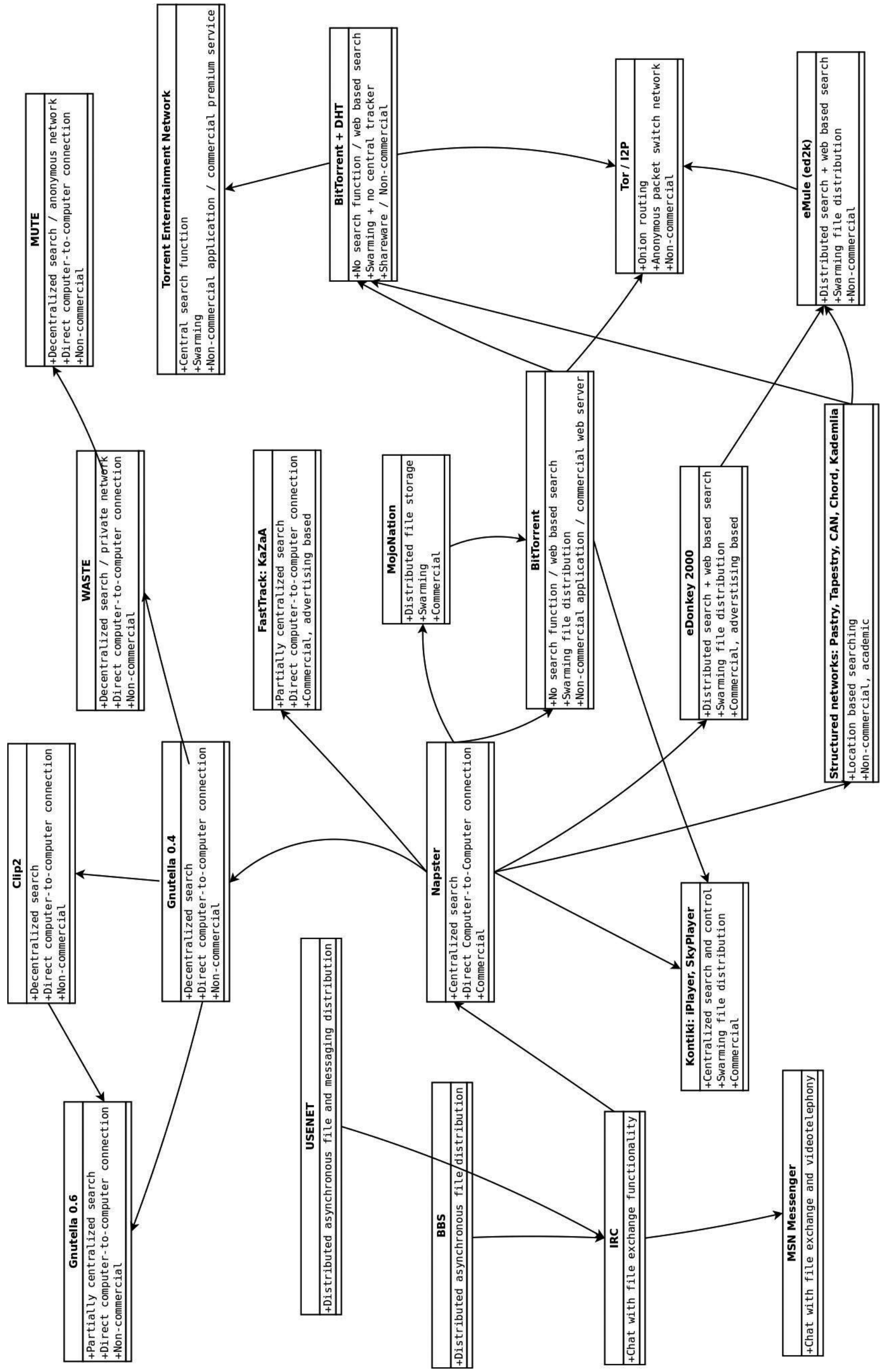

Figure 1. Evolution of P2P networks

China, Iran, and other countries notorious for government Internet monitoring practices. As per the copyright conflict, Clarke is very assertive: "You cannot guarantee freedom of speech and enforce copyright law," and therefore, Freenet "must prevent enforcement of copyright"[20].

Another network that reached some notoriety was MUTE, an open source project developed in 2004 by the independent programmer Jason Roher. Roher conceived his network as a form of social activism against the tactics of the RIAA, "encouraging people to break an unjust law as a form of social protest" (Wen, 2004).

Roher's P2P client program created a decentralized anonymous network based on the Gnutella 0.4 protocol. Besides encrypting all the file communications to avoid eavesdropping, his main contribution was masking the IP addresses of the participant computers and avoiding direct computer-to-computer connections for download. His rationale was that RIAA lawsuits started with the IP address of a computer, which was enough to discover the name and address of the sharer.

Conclusion

The historical evolution of P2P we have outlined does not follow a simple linear path towards more efficient or horizontal ways of sharing files through the Internet: it resembles more the so-called multidirectional model proposed by constructivist accounts of technology (Pinch & Bijker, 1987). Many forces and actors with different and sometimes conflicting interests have had a role in shaping P2P networks. In particular, the controversy that has developed around music and film downloading and its very evolution from the so-called copyright wars to a broader societal debate on cultural production and access to it has been one of the key features in triggering most of the technical innovations we have analyzed.

Designers of most P2P networks share a very similar profile: young, visionary and technologically savvy people who understood the potential uses of P2P networks and in a very entrepreneurial way tried to create new forms of business with them. Most of them showed an ambiguous ideological stand fluctuating between utilitarian and romantic individualism; a trend that has also shaped the evolving, and sometimes messy, ethos of the Internet (Streeter, 2011, 113). But as entrepreneurs, they also had different approaches to creation of value and business models. In general, the early experiences suffered from the dot com bubble illness that Porter (2001) describes as "applying creative accounting in the form of dubious performance metrics such as number of visitors or unique users". This was the case of Napster, which was offered completely free, with no advertising in the client application, and never managed to articulate a way in which it could make any profit, yet still managed to raise several rounds of funding (Ante, 2000a). Other entrepreneurs tried to be a bit more explicit about their plans for creating a profit. For example, KaZaA, whose founders already

20. http://freenetproject.org/philosophy.html (last checked 3/10/10).

had significant business experience, created not only a complex legal structure engineered to escape legal prosecution, but also included advertising in the client application based on analyzing users' search patterns, technology licensing, and support for downloading paying content with embedded DRM mechanisms.

When the tasks of searching and sharing were separated, P2P entrepreneurs focused on providing their services around the search function, whereas the sharing and the protocol part became a commodity which did not allow for economic value creation. However, this shift did not change the problems with the copyright holders, who continued to sue the entrepreneurs behind sites like Demonoid, Mininova or The Pirate Bay.

In fact, the dramatic court battle and eventual closure of Napster signaled very clearly to the creators-to-be of P2P networks that a centralized network was not legally viable anymore. Thus, with a few early exceptions, all the other subsequent networks moved towards various decentralized architectures. The early Gnutella represented a swing towards an egalitarian and completely flat network, which corresponded as well to the ideological views of hackers like Frankel, who believed that everybody should be sharing information on equal terms. However, some sort of centralized management was still required by entrepreneurs that wanted to control the technology in order to obtain revenues to capitalize their investments. The first proposal to include advertising in the P2P client, though, proved not to be a lasting one. Even if KaZaA and eDonkey worked with advertising for some time, both saw alternative advertising-free applications get developed by hackers that eventually replaced the official ones.

All in all, the controversy has seen an extraordinary use of very diverse strategies by the different sides and actors, on a global level and unprecedented scales, in order to impose their views. These strategies have been aimed at establishing particular interpretations of the technology involved and thus forcing specific uses (or non-uses) of it. Some of the actions have been addressed to change the context of use —as in the legal prosecution of users or P2P companies and the involvement of political institutions; others have taken a semiotic character in order to impose negative meanings on opponents —as in the use of concepts such as 'pirate', 'mafia'[21] or 'terrorist'[22]— in order to discredit them. However, most notably, some strategies have been aimed at building particular meanings and values into the very technical design in order to compel users to follow them in a more implicit and subtle way —as in the changes introduced in BitTorrent, the DRM systems, or the swarming mechanisms in Mojo Nation. This move echoes the *inscription* process described by actor-network scholars (Latour, 1992) and proves to be a more solid and lasting strategy to achieve change. Whether the next chapters of this controversy will be written in legal or software code remains to be seen.

21. The terms mafia and MAFIAA, a parody acronym for a hypothetical Music And Film Industries Association of America, are used widely on the Internet by supporters of free sharing of media files.

22. Jack Valenti, president of the MPAA: "We're fighting our own terrorist war" (Harmon 2002).

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EXAMEN DES TRAITÉS *SUR LA SECTION DU CYLINDRE* ET *SUR LA SECTION DU CÔNE* DE SÉRÉNOS D'ANTINOÉ À LA LUMIÈRE DE LA TRADITION DE L'OPTIQUE GÉOMÉTRIQUE ANCIENNE

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Résumé : *Sérénos d'Antinoé (III siècle après J.C.) était l'auteur de deux traités Sur la Section du cylindre et Sur la Section du Cône. Dans la deuxième partie de la Section du cylindre, Sérénos examine une définition par observation des droites parallèles donnée par son ami le géomètre Python, en s'appuyant sur une série des propositions (29-33). Nous allons examiner la façon de Sérénos de défendre cette définition par observation avec les outils de l'optique géométrique euclidienne et la tradition des coniques d'Apollonios. Les termes et les notions qu'il utilise étaient valides et légitimes pour les mathématiciens qui travaillaient à l'aide des outils mathématiques hellénistiques, bien que sa période d'activité soit éloignée des grands géomètres, Euclide et Apollonios.*
D'après nous, la nature des propriétés traitées dans son traité Sur la Section du Cône reste en partie un problème ouvert pour les historiens. Tous sont d'accord sur le fait que son but est de faire une étude comparative des sections produites dans un cône par l'intersection d'un plan qui passe par son sommet. Nous allons examiner quelques propositions et jeter de la lumière à un deuxième aspect du traité qui consiste à fonder géométriquement des propriétés optiques.

Mots clès : *Sérénos d'Antinoé, section du cylindre, section du cône, optique géométrique ancienne*

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Examination of the treatises *On the Section of a Cylinder* and *On the Section of a Cone* of serenos of Antinoeia under the light of the Ancient Greek tradition on Optics

Summary : *Serenos of Antinoeia (III Century A.C.) was the author of two treatises On the Section of a Cylinder and On the Section of a Cone. In the second part of the treatise On the Section of a Cylinder, Serenos examines one definition by observation of parallel lines given by his friend the geometer Python. He bases his approach on a series of propositions (29-33). We are going to examine the way that Serenos defends this definition by observation with the tools of Euclidean geometrical optics and the tradition on conics of Apollonios. The terms and the notions Serenos uses were valid and legitimate for mathematicians working with the aid of the tools of Hellenistic mathematics.*
In our point of view, the nature of the properties in the treatise On the Section of a Cone is an open problem for historians. All of them agree on the fact that the goal is to do a comparative study on the sections produced in a cone by an intersection from a plan passing by its vertex. We are going to examine some propositions and throw light on a second aspect of this treatise which consists of basing geometrically optic properties.

Key words : *Serenos of Antinoeia, section of a cylinder, section of a cone, ancient geometrical optic*

Introduction

Sérénos d'Antinoé est l'auteur de deux traités mathématiques *Sur la section du cylindre* et *Sur la section du cône* que la tradition manuscrite nous a transmis à la suite des quatre premiers livres du traité des *Coniques* d'Apollonios de Pergé. Les historiens de l'Antiquité ne nous ont laissé aucun renseignement sur la personnalité et sur les circonstances de la vie de ce géomètre alexandrin.

Sérénos avait également écrit un commentaire (qui ne nous est pas parvenu) de cet ouvrage (*Les Coniques*) dans lequel il nous renvoie dans la proposition 17 de la *Section du cylindre*. Il ecrit : car tout ce qui a été démontré comme étant propre à cette section est également propre à l'ellipse du cône, ainsi que cela a été démontré dans les *Coniques*, au quinzième théorème, pour ceux qui sont à même de constater la justesse de ce théorème, et ainsi que nous l'avons démontré géométriquement nous-même dans des commentaires relatifs à cette proposition (Ver Eecke, 1929 : 29-30).

Certains historiens des mathématiques (P. Ver Eecke, J. L. Heiberg, Halley, P. Tannery, Bretschneider, Montucla, M. Chasles, F. Hultsch, F. Muller) donnent dans leurs oeuvres des petits passages sur la période de son activité en faisant des conjectures.

Une récente recherche de Micheline Decorps-Foulquier a démontré que Sérénos est postérieur au philosophe du moyen platonisme Harpocration et situe sa période d'activité au début du IIIe siècle (Decorps, 2000 : 38-39).

Ses arguments sont basés sur le témoignage du *Parisinus graecus* 1918. Ce manuscrit qui est consacré aux commentateurs d'Aristote, Philopon, Themistios et Michel Psellos présente, aux folios 144-148, après une série de notes anonymes sur des problèmes aristotéliciens, une discussion sur l'âme, la matière et la nature du mal qui abonde en références néoplatoniciennes. La discussion est menée par un chrétien qui confronte ses vues à celles des Grecs. La transition est assurée par une référence à l'interprétation d'Harpocration du mythe de la réincarnation. Le philosophe Harpocration est désigné de la manière suivante (f.145v°) : « Harpocration, le commentateur de Platon auquel se rapporte d'ordinaire le géomètre Sérénos en ce qui concerne la pensée platonicienne ». J. Whittaker (1979) a suggéré que ces notes avaient été rassemblées dans le proche voisinage de Michel Psellos qui fut l'artisan de la renaissance néoplatonicienne du XIe siècle à Byzance. Harpocration, nous le savons par Proclos, fut un disciple de Numénios et un éléve d'Atticos (vers 180 A.C.) et il était un philosophe du moyen platonisme. Ce témoignage est précieux puisqu'il nous apporte la seule chose dont nous puissions être sûrs : le géomètre Sérénos était un philosophe platonicien postérieur à Harpocration. La question est de savoir s'il en était éloigné dans le temps. Ensuite Micheline Decorps-Foulquier essaie de rapprocher Sérénos d'Harpocration en insistant sur le fait que le premier dépend si étroitement d'Harpocration dans sa lecture de Platon, ce qui rend a priori peu probable l'appartenance du premier aux cercles néoplatoniciens des Ve et VIe siècles, en général assez peu enclins à se réclamer des philosophes du moyen platonisme. En revanche, au IVe siècle, leurs oeuvres étaient lues et citées, comme l'attestent encore les fragments de Numénios et d'Atticos qui figurent dans la *Préparation évangélique* d'Eusèbe de Césarée. Par conséquent, Micheline Decorps-Foulquier propose à titre d'hypothèse le rapprochement de Sérénos à Harpocration et situe sa période d'activité au début du IIIe siècle de notre ère.

Le traité *Sur la Section du cylindre* peut être divisé en deux parties dans lesquelles Sérénos résoud deux problèmes de nature assez différente. Dans la *Section du cylindre* nous trouvons deux préambules. Au début du livre, juste avant les définitions, il définit le but de son essai en disant qu'il veut corriger la conception fausse qu'ont beaucoup de gens qui font de la géométrie et qui s'imaginent que la section transversale du cylindre est différente de celle du cône qu'on appelle ellipse. Dans les 19 premières propositions il essaie de démontrer que la courbe fermée déterminée par la section transversale d'un cylindre quelconque, non parallèle ni antiparallèle aux bases, est identique à l'ellipse déterminée dans les mêmes conditions dans un cône quelconque. Dans les propositions 20-28, il examine certains problèmes relatifs à la propriété démontrée dans la première partie que nous avons déjà évoquée.

Après la proposition 28, Sérénos pose dans un petit préambule le but de ses 5 dernières propositions dans lesquelles il expose son deuxième problème (propositions 29-33) selon lequel il veut examiner géométriquement une définition des parallèles, différente de celle d'Euclide, émise par son ami le géomètre Python. À ce point il faut signaler que nous ne savons rien sur Python.

Le géomètre Python, après avoir expliqué ce que sont les parallèles dans un de ses écrits, ne s'est pas contenté de ce qu'en avait dit Euclide. A ce point il fait implicitement référence au cinquième postulat qui s'énonce comme suit : « Si une droite tombant sur deux droites fait des angles intérieurs du même côté plus petits que deux droits, ces deux droites prolongées à l'infini se rencontreront du côté où les angles sont plus petits que deux droits » (Peyrard, 1809 : 6). Le cinquième postulat, ou postulat des parallèles, dont l'énoncé présente la particularité d'avoir la forme d'un théorème a, dès le IIIe siècle av. J.C., troublé les commentateurs. Les tentatives de démonstration de ce postulat ont joué un rôle particulièrement important dans l'histoire de la géométrie et la littérature sur le sujet est immense. Jusqu'au XIXe siècle, toutes les tentatives connues de démonstration du postulat 5 reviennent bien évidemment à remplacer, explicitement ou pas, ce postulat par un (ou plusieurs) postulat plus ou moins équivalent, sans remettre en question un certain nombre de propriétés considérées comme allant de soi (infinité et continuité de la droite, axiome d'Archimède, axiome de Pasch, homogénéité de l'espace). Ces tentatives sont souvent couplées avec la tentative de définir le parallélisme des droites par l'équidistance (ce que fait Posidonios, IER siècle av. J.C.), ce qui pour Proclos et Geminos est contestable du fait qu'il existe des lignes asymptotes qui se rapprochent indéfiniment sans se couper. Ce postulat a ouvert une discussion dans le monde hellénistique mais aussi arabe et latin jusqu'à la naissance des géométries non-euclidiennes.

Python définit les parallèles au moyen d'un exemple plus rationnel comme suit : « les parallèles sont des droites telles que celles que nous voyons se former sur les murs ou sur le sol par les ombres de colonnes à l'opposé desquelles brûle un flambeau » (Ver Eecke, 1929 : 54).

Il continue dans ce préambule en citant que :

Bien que la chose eût parue fort risible à tout le monde, elle n'est cependant pas ridicule pour nous qui respectons l'écrivain, car cet homme est un ami. Mais il faut examiner comment cela se présente mathématiquement, et cet examen se rattache aux choses que nous avons considérées ici précédemment; car c'est au moyen d'elles que nous démontrerons ce que nous avons proposé (Ver Eecke, 1929 : 54).

La démarche de Sérénos s'inscrit dans cette tradition des travaux sur la théorie des parallèles et enrichit les demarches effectuées dans le monde grec. Pour cette raison nous allons examiner la façon de Sérénos de défendre cette définition *par observation* avec les outils de l'optique géométrique Euclidienne[1] et la tradition des coniques d'Apollonios. Nous allons examiner minutieusement une par une les propositions, les termes et les notions qu'il utili-

1. L'*Optique* d'Euclide est une collection de 58 propositions, qui sont précédées par 7 définitions. Ces propositions nous donnent une description géométrique de la formation des apparences du point de vue des dimensions de formes et de positions relatives. Dans l'*Optique* d'Euclide, nous trouvons un groupe de six propositions (XXVIII à XXXIII) (P.V. Eecke, Bruges, 1938, p. 20-25). Ces propositions se rapportent aux cylindres et aux cônes à base circulaire regardés de divers points successivement considérés. Elles montrent d'abord quelles sont les parties de ces solides vues par l'oeil placé dans le plan de la base ou dans les plans parallèles à la base. Elles démontrent ensuite comment ces parties vues varient en réalité et en apparence suivant que l'oeil s'approche ou s'éloigne de ces solides dans ces mêmes plans. Enfin, elles déterminent deux plans passant par les tangentes menées de l'oeil à la base et par deux génératrices du cône, dont l'intersection est le lieu géométrique sur lequel l'oeil se déplace sans faire varier la partie réelle ou apparente du cône qui est vue.

se et leur validité et légitimité pour les mathématiciens qui travaillaient à l'aide des outils mathématiques hellénistiques, bien que sa période d'activité soit éloignée des grands géomètres, Euclide et Apollonios.

Le but du deuxième livre *Sur la section du cône* est l'étude comparative des sections produites dans un cône par des plans qui passent par son sommet. Ce livre est entièrement fondé sur la proposition 3 du premier livre des *Coniques* d'Apollonios, proposition qui permet dans tout cône coupé par le sommet d'obtenir comme section un triangle.

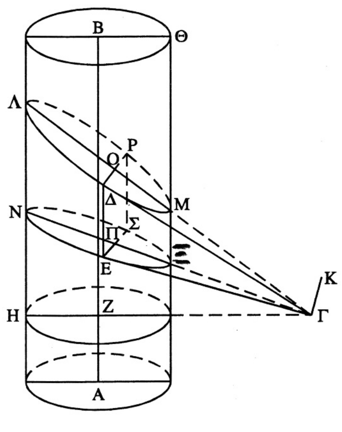
Dans ce traité Sérénos cherche à trouver les triangles maxima et minima établis par un plan qui passe par le sommet d'un cône oblique, c'est-à-dire qu'il cherche les valeurs minima et maxima d'une fonction selon que les sections sont axiales, parallèles ou isocèles.

Le livre de la *Section du cône* de Sérénos peut être partagé en trois parties relativement indépendantes. Dans la première partie, les propositions 1-14, il travaille sur les cônes droits; dans la deuxième, les propositions 15-57, il travaille sur les cônes obliques. La dernière partie propositions 58-69, constitue une section séparée du livre où il regarde les rapports entre les volumes de deux cônes droits en relation avec les hauteurs, les bases et les aires des sections triangulaires qui passent par l'axe. Dans chaque partie Sérénos se pose plusieurs problèmes et donne leur solution.

La nature des propriétés traitées dans son livre *Sur la Section du Cône* reste en partie un problème ouvert pour les historiens. Ensuite nous allons examiner quelques propositions (proposition 3, propositions 50-51), les comparer avec des propositions du traité d'*Optique* d'Euclide et jeter de la lumière à un deuxième aspect du traité qui consiste à fonder géométriquement des propriétés qui sont équivalentes avec des propriétés du domaine de l'optique géométrique.

1. Sur la Section du Cylindre

1.1. *Proposition 29*



« Les droites issues d'un même point et tangentes de part et d'autre à une surface cylindrique opèrent le contact le long des côtés d'un seul parallélogramme » (Ver Eecke, 1929 : 54-56).

Sérénos commence par la partie de l'*ekthesis* où il prend le cylindre de bases A, B et d'axe AB. D'un point **Γ** extérieur il mène les droites **ΓΔ**, **ΓΕ** qui touchent la surface du cylindre dans la même partie, aux points **Ε**, **Δ**. Dans la partie *diorismos* (diorisme) il écrit que ces points de contact sont situés sur une seule droite, c'est-à-dire sur une seule et même arête du cylindre.

Ensuite, dans la partie de la *construction-kataskeve* il construit pas à pas le cylindre des bases B, Z d'axe BZ et le plan axial **ΗΘ**. Ensuite, il mène la droite **ΓΚ** perpendiculaire sur la droite **ΓΖ** et dans le plan du cercle **Ζ**.

Par les droites **ΓΚ**, **ΓΔ** il détermine la ligne **ΛΔΜ** et par les droites **ΓΚ**, **ΓΕ** il détermine la ligne **ΝΕΞ** toutes les deux dans la surface du cylindre. Les droites **ΛΜΓ**, **ΝΞΓ** sont dans le plan du parallélogramme dont les droites **ΛΜ**, **ΝΞ** sont les diamètres des sections. Il abaisse les droites **ΔΟ**, **ΕΠ** d'une manière ordonnée sur les diamètres **ΛΜ**, **ΝΞ** (**ΔΟ//ΓΚ**, **ΕΠ//ΓΚ**) et il les prolonge aux points **Ρ**, **Σ**.

De ce point commence la partie de la *démonstration (apodeixis)* où par la proposition 36 du Livre I des *Coniques* d'Apollonios (il fait seulement référence au livre I des *Coniques* d'Apollonios, **ως δέδεικται τω Απολλωνίω εν τω α′ των Κωνικών**, c'est-à-dire ainsi que cela a été démontré par Apollonios dans le premier livre des Coniques) il établit qu'une telle section est une ellipse et non un cercle; il s'ensuit que **ΛΟ/ΟΜ** est égal au **ΛΓ/ΓΜ** et **ΝΠ/ΠΞ** est égal au **ΝΓ/ΓΞ**.

Les triangles **ΝΓΛ**, **ΞΓΜ** sont semblables (Euclide, *Les Eléments*, Livre VI, proposition 4) donc **ΝΓ/ΓΞ** est égal au **ΛΓ/ΓΜ** d'ou **ΝΠ/ΠΞ** est égal au **ΛΟ/ΟΜ** (Euclide, *Les Eléments*, Livre V, proposition 11).

La droite **ΠΟ** est située dans le plan **ΗΘ** (**ΠΟ//ΒΑ**, **ΠΟ//ΘΜ**).

Par construction il a **ΔΟ//ΓΚ**, **ΕΠ//ΓΚ** donc **ΔΟ//ΕΠ** (Euclide, *Les Eléments*, Livre I, proposition 30) et le plan **ΠΕΔΟ** est un parallélogramme. Il fait référence explicitement à son propre proposition 3 en écrivant que : ainsi que cela a été démontré au troisième théorème (Sérénos, *Sur la Section du Cylindre*, proposition 3). La droite **ΕΔ** est côté d'un parallélogramme. De façon pareil, pour toutes les tangentes, **ΡΣ** est une droite parallèle à la droite **ΕΔ** et **ΡΣ** est côté d'un parallélogramme.

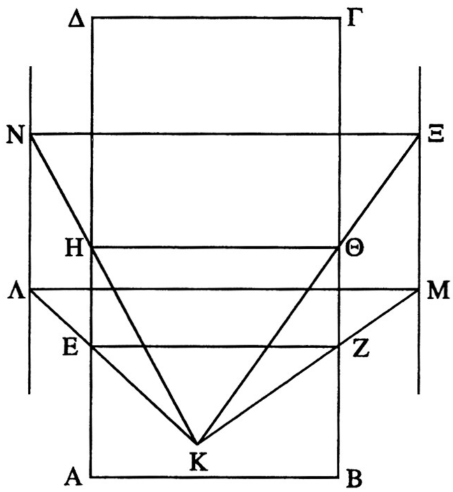
À la partie *conclusion* il écrit qu' « En conséquence, toutes les tangentes font leurs contacts suivant les côtés d'un parallélogramme; ce qui démontre la proposition ».

Dans la proposition 28 du traité d'*Optique*, Euclide démontre que « Si un cylindre est *regardé* d'un seul oeil *de quelque manière que ce soit*[2], on verra moins que le demi-cylindre » (Ver Eecke, 1938 : 72-73). Sérénos, dans sa proposition 29 démontre que « le lieu des

2. Nous avons mis les mots en italiques

points de contact d'un faisceau de tangentes menées d'un point de l'espace à la surface d'un cylindre est un parallélogramme dont le plan est parallèle à celui d'un parallélogramme axial ». Ce parallélogramme est plus petit que le parallélogramme axial qui correspond au parallélogramme du demi-cylindre (Euclide, *Optique*, proposition 28). Il utilise implicitement cette proposition d'Euclide dans sa démonstration, et pendant la procédure démonstrative il fait référence à la proposition 3 de son propre traité (*Sur la section du cylindre*) où il avait démontré que « lorsqu'un cylindre est coupé par un plan parallèle au parallélogramme passant par l'axe, la section sera un parallélogramme ayant des angles égaux à ceux du parallélogramme passant par l'axe ».

1.2. *Proposition 30*



Cette proposition commence sans enoncé avec l'expression « Cela étant démontré ». Donc Sérénos applique la propriété qu'il a démontré dans la proposition 29 dans cette proposition (Ver Eecke, 1929 : 56-58). Il prend un parallélogramme ΑΒΓΔ et il mène des droites ΕΖ//ΑΒ et ΗΘ //ΑΒ. Il prend un point K non situé dans le plan du parallélogramme. Il mène les droites ΚΕ, ΚΖ, ΚΗ, ΚΘ et en les prolongeant elles rencontrent un plan parallèle au plan ΑΒΓΔ aux points Λ, Μ, Ν, Ξ. Il mène un plan par les droites ΚΛ, ΕΖ qui coupe le plan ΛΜΝΞ et détermine la droite (section commune) ΛΜ//ΕΖ. Pareillement il détermine ΝΞ//ΗΘ.

Par Euclide (*Les Eléments*, Livre XI, proposition 16) il obtient ΝΛ//ΗΕ, ΞΜ//ΘΖ et par Euclide, (*Les Eléments*, Livre VI, proposition 2 et Livre V, proposition 18) il obtient ΗΚ/ΚΝ est égal au ΕΚ/ΚΛ.

Mais ΗΘ/ΝΞ est égal au ΗΚ/ΚΝ et ΕΖ/ΛΜ est égal au ΕΚ/ΚΛ (Euclide, *Les Eléments*, Livre VI, proposition 4) donc ΗΘ/ΝΞ est égal au ΕΖ/ΛΜ (Euclide, *Les Eléments*, Livre V, proposition 11) et il applique la propriété de la permutation (Euclide, *Les Eléments*, Livre V, proposition16) **ΕΖ** est égale à **ΗΘ** donc **ΛΜ** est égale à **ΝΞ** mais **ΛΜ//ΝΞ** (Euclide, *Les Eléments*, Livre XI, proposition 9), donc **ΜΞ//ΛΝ** (Euclide, *Les Eléments*, Livre I, proposition 33).

Jusqu'a ce point il a démontré que les droites **ΞΜ, ΛΝ** et **ΛΜ, ΞΝ** sont parallèles et **ΛΜΞΝ** est un parallélogramme. De ce point il passe au domaine de l'optique en écrivant que :

Si nous supposons que le point K est un point lumineux, et que le parallélogramme **ΑΓ** est ce qui intercepte ses rayons, qu'il soit autonome ou situé dans un cylindre, il se fera que les rayons lumineux émis par le point K aboutiront à la droite **ΜΛ** et à la droite **ΝΞ**; et ce qui se trouve entre les parallèles **ΜΛ**, **ΞΝ** sera ombrage.

À ce point il commence à examiner la définition des parallèles donnée par Python en soulignant que la droite **ΔΑ//ΓΒ** et la **ΝΛ//ΞΜ**. Ces droites n'apparaîtront cependant pas de cette manière car celle des distances **ΛΜ, ΝΞ** qui est plus rapprochée de la vue semblera être plus grande. Ici il fait référence explicite aux Optiques (Euclide, *Optique*, proposition 6[3]). Il écrit que **ταύτα δε παρειλήφαμεν εκ των Οπτικών**, c'est-à-dire que nous empruntons d'ailleurs ces choses aux Optiques.

La proposition 6 de l'*Optique* d'Euclide se fonde plutôt sur la définition 4 et considère les deux lignes parallèles sur le même plan en s'éloignant de l'observateur, mais aussi les lignes parallèles qui s'éloignent dans un autre plan. Dans ces deux cas, la démonstration de cette proposition prend en considération l'apparence des lignes parallèles pour les deux et les trois dimensions. En termes de perspective linéaire, elle établit que les lignes parallèles qui s'éloignent d'un oeil sur un plan apparaîtront en convergence (Ver Eecke, 1938 : 1).

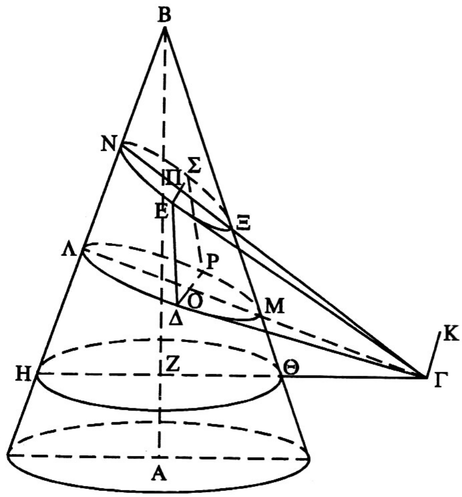
Nous remarquons que Sérénos dans ces propositions 29 et 30 démontre une propriété géométrique. Il prépare une figure qui se base sur cette dernière proposition (proposition 29) et applique sur celle-ci l'optique géométrique (deuxième partie de la proposition 30) pour examiner la définition des parallèles de Python. Pour le faire il utilise plusieurs propositions des *Eléments* d'Euclide (Livre I, propositions 30, 33; Livre V, propositions 11, 16, 18; Livre VI, propositions 2, 4; Livre XI, propositions 9, 16), Il fait référence explicite au livre I des *Coniques* d'Apollonios de Pergé et à l'*Optique* d'Euclide. Il ne mentionne pas explicitement pour les deux cas le numéro de la proposition. Il utilise aussi une autoréférence explicite sur une de ses propositions en soulignant le numéro de cette proposition qui se trouve dans le même traité (proposition 3).

Ensuite, Sérénos écrit « puisque l'ellipse est commune au cône et au cylindre, il y a corrélation à considérer dans le cône la parité de ce que nous avons observé dans le cylindre, considérons-la maintenant dans le cône ». Donc, il démontre les mêmes propriétés dans le cône (propositions 32 et 33). Pour la proposition 32, Sérénos a besoin de la propriété d'un rapport harmonique qu'il a établie dans la proposition précédente (proposition 31).

3. Euclide, *Optique*, proposition 6, Des intervalles de même dimension, vus à distance, paraissent d'inégale longueur. (Ver Eecke, 1938: 4-5)

1.3. *Proposition 31*

« Si l'on prend un point à l'extérieur d'un triangle; si, de ce point, l'on mène une droite qui coupe le triangle, et si l'on amène, du sommet sur la base, une autre droite coupant la droite ainsi menée transversalement, de manière que le grand segment découpé à l'intérieur du triangle soit au petit segment adjacent à celui qui est à l'extérieur du triangle comme la droite entière menée transversalement est au segment situé à l'extérieur du triangle; toute droite qui, menée du point qui a été pris, coupe le triangle, sera coupée dans le même rapport par la droite amenée du sommet sur la base. Et si toutes les droites ainsi menées du même point sont coupées dans le même rapport, la droite qui, menée dans le triangle, les coupe, passera par le sommet du triangle » (Ver Eecke, 1929 : 58).

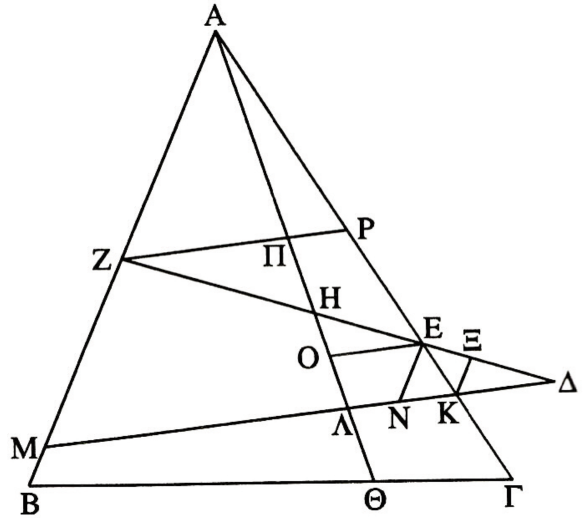


Le faisceau des droites menées du point Δ extérieur au triangle sera divisé dans un rapport harmonique par la droite ΑΘ qui coupe la droite ΖΔ de telle sorte que ΖΗ/ΗΕ est égal au ΖΔ/ΔΕ et menons transversalement une autre droite ΔΚΛ, c'est-à-dire ΜΛ/ΛΚ est égal au ΜΔ/ΔΚ.

En regardant minutieusement la procédure démonstrative nous remarquons que l'auteur utilise les propositions des *Eléments* d'Euclide (Livre VI, 2, 4; Livre V, 18, définition 20) et aussi sa propre proposition 23.

1.4. *Proposition 32*

« Toutes les droites qui, partant d'un même point, touchent de part et d'autre sur une surface conique, font leur contact suivant les côtés d'un seul triangle » (Ver Eecke, 1929: 61-63).



Il commence dans la partie de l'*ekthesis* où il prend le cône de base A, du sommet B et d'axe AB. D'un point **Γ** extérieur il mène les droites **ΓΔ**, **ΓE** tangentes qui touchent la surface du cône dans la même partie, aux points **E, Δ**. Dans la partie *diorismos (diorisme)* il écrit que ces points de contact sont situés sur une seule droite.

Ensuite, dans la partie de la *construction-kataskeve* il construit pas à pas le cône de base Z, du sommet B et d'axe BZ. Ensuite il construit le plan axial **BHΘ** et il mène la droite **ΓK** perpendiculaire sur la droite **ΓZ** est dans le plan du cercle **Z**.

Par les droites **ΓK**, **ΓΔ** il détermine la ligne **ΛΔM** et par les droites **ΓK**, **ΓE** il détermine la ligne **NEΞ**, toutes les deux dans la surface du cône. Les droites **ΛMΓ**, **NΞΓ** sont dans le plan du parallélogramme dont les droites **ΛM**, **NΞ** sont les diamètres des sections. Il abaisse les droites **ΔO**, **EΠ** d'une manière ordonnée sur les diamètres **ΛM**, **NΞ** (**ΔO//ΓK**, **EΠ//ΓK**) et il les prolonge aux points **P**, **Σ**.

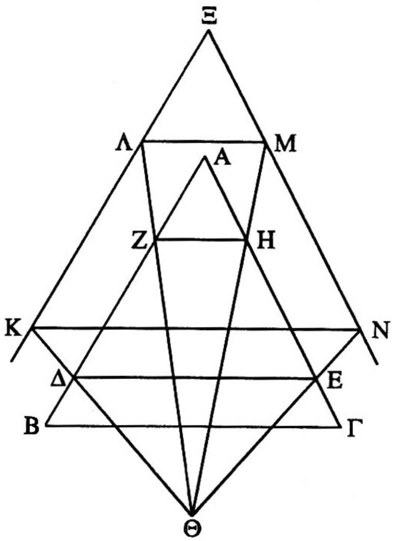
De ce point commence la partie de la *démonstration (apodeixis)* où par la proposition 36 du Livre I des *Coniques* d'Apollonios (sans référence au livre I des *Coniques*) il établit qu'une telle section est une ellipse, et non un cercle; il s'ensuit que **ΛO/OM** est égal au **ΛΓ/ΓM** et **NΠ/ΠΞ** est égal au **NΓ/ΓΞ**.

Par la proposition 31 de la *Section du Cylindre* de Sérénos (il fait autoréférence, **δια το προ τούτο**, c'est-à-dire en vertu de la proposition qui précede) la droite réliant les points **O** et **Π** prolongée passera par le sommet. Les droites **ΛΓ**, **NΓ** étant divisées dans un rapport harmonique, la droite **OΠ** prolongée passera par le sommet B du triangle axial du cône. Après, il mène la droite **OΠB**.

Les droites **EΣ** et **ΔP** sont parallèles à la droite **ΓK** donc **EΣ** est parallèle à la droite **ΔP** (Euclide, *Les Eléments*, Livre XI, proposition 9). Par Apollonios, sans référence *(Les Co-*

niques, Livre I, proposition 3[4]), le plan étendu par la droite ΒΠΟ et par les droites ΕΣ, ΔΡ détermine un triangle comme section dans la surface du cône. Donc les points Δ, Ε situés dans la surface du cône, sont sur le côté du triangle qui coupe le triangle ΒΗΘ suivant la droite ΒΠΟ. Les mêmes choses se présentent pour toutes les tangentes aux points Ρ, Σ. Dans la partie conclusion il écrit que « Par conséquent, toutes les droites issues du point Γ, qui touchent la surface conique tombent sur les côtés d'un seul triangle; ce qu'il fallait démontrer » (Ver Eecke, 1929 : 63).

1.5. *Proposition 33*



La partie démonstrative (Ver Eecke, 1929 : 63-64) commence avec l'expression « Cela étant démontré ». Donc Sérénos applique la propriété de la proposition 32 dans la proposition suivante. Il prend un triangle ΑΒΓ et il mène des droites ΔΕ//ΒΓ et ΖΗ//ΒΓ. Il prend point Θ non situé dans le plan du triangle. Il mène les droites ΘΔ, ΘΖ, ΘΗ, ΘΕ et en les prolongeant elles rencontrent un plan parallèle au plan ΑΒΓ aux points Κ, Λ, Μ, Ν. Il mène un plan par les droites ΕΔ, ΚΘ qui coupe le plan ΚΛΜΝ et détermine la droite (section commune) ΚΝ//ΕΔ (Euclide, *Les Eléments*, Livre XI, proposition 16). Pareil, il détermine ΛΜ//ΖΗ et ΚΛ//ΔΖ et ΝΜ//ΗΕ. Les droites prolongées se rencontrent au point Ξ. En conséquence, ΚΞ//ΔΑ, ΞΝ//ΑΕ. L'angle situé au point Ξ sera égal à l'angle situé au point A (Euclide, *Les Eléments*, Livre XI, proposition 10). Par la même proposition des *Eléments*

4. Si un cône est coupé par un plan passant par son sommet, alors cette section est un triangle rectiligne (Rashed, 2008: 266).

d'Euclide et par le fait que ΞK//ΑΔ, KN//ΔE l'angle compris sous les droites ΞK, KN est égal à l'angle compris sous les droites ΑΔ, ΔE et par Euclide (*Les Eléments,* Livre VI, proposition 4) les triangles ΞKN, ΑΒΓ sont semblables entre eux.

Jusqu'à ce point il a démontré que les droites KΞ, AB et ΞN, ΑΓ sont parallèles et KΞN est un triangle semblable au triangle ΑΒΓ. De ce point il passe au domaine de l'optique en écrivant que :

Si nous supposons que le point Θ soit un point lumineux, et que le triangle ΑΒΓ soit ce qui intercepte ses rayons, que ce triangle soit autonome ou situé dans un cône, il se fera que les rayons lumineux émis par le point Θ non retenus par le triangle ΑΒΓ, détermineront le triangle d'ombre KΞN, semblable au triangle ΑΒΓ (Ver Eecke, 1929 : 64).

Au contraire de la proposition 30 dans la proposition 33 il ne discute pas la définition des parallèles et donc Sérénos finit à ce point la démonstration de cette proposition.

Nous remarquons que Sérénos dans ces propositions 32 et 33 démontre une propriété géométrique. Il prépare une figure qui se base sur cette dernière proposition (proposition 32) et applique sur celle-ci l'optique géométrique (deuxième partie de la proposition 33) pour examiner la définition des parallèles de Python. Pour ce faire il utilise plusieurs propositions des *Eléments* d'Euclide (Livre VI, proposition 4; Livre XI, propositions 9, 10, 16), Il ne fait pas référence explicite au livre I des *Coniques* d'Apollonios de Pergé et à l'*Optique* d'Euclide. Il utilise les propositions 3 et 36 du Livre I des *Coniques* d'Apollonios. Il utilise sa proposition 31 que nous avons examiné précédemment.

Il finit ce traité avec le paragraphe suivant où il souligne que

> « Quoique ces choses appartiennent à la théorie optique et qu'elles paraissent de ce fait être étrangères au présent ouvrage, il est clair cependant, qu'à défaut de ce qui a été démontré ici concernant la section du cylindre et du cône, et notamment concernant l'ellipse et les droites qui lui sont tangentes, un problème de ce genre-ci eut été impossible à établir; de sorte que ce n'est pas sans raison, mais pour leur usage, que la mention de ces choses a été introduite » (Ver Eecke, 1929 : 64).

Nous pouvons remarquer que Sérénos veut discuter une définition des parallèles par observation et il termine sa discussion en disant qu'il a entrepris à la démontrer car ces choses sont importantes pour leur usage.

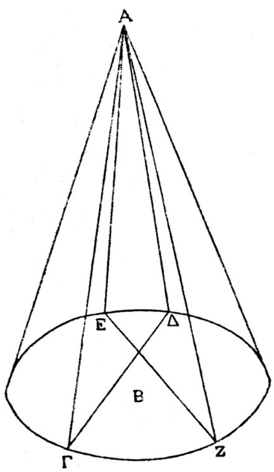
2. Sur la Section du Cône

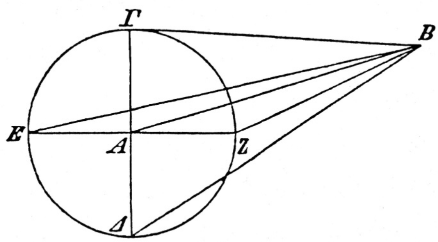
Dans la définition 2 (Ver Eecke, 1938 : 1) du traité de l'*Optique* Euclide introduit les cônes de vision comme des figures géométriques qui se sont formées par les rayons visuels qui relient le point d'observation (sommet du cône) et les points qui se trouvent sur le contour de l'objet visé. La proposition 3 (Ver Eecke, 1929 : 67-68) de *la Section du Cône* peut être optiquement parlant équivalente à l'idée des cônes visuelles d'Euclide étant donné que le sommet du cône est au centre de l'oeil, donc le point d'observation et toutes les propositions suivantes utilisent ce résultat.

Ensuite, nous allons comparer la proposition 3 (Ver Eecke, 1929 : 67-68) de la *Section du Cône* et la proposition 34 (cas 1 de la démonstration, Ver Eecke, 1938 : 26) de l'*Optique* et les propositions 50-51 (Ver Eecke, 1929 : 137-140) avec la proposition 34 (cas 2 de la démonstration). Par cette comparaison nous allons constater l'existence de propositions qui ont des rapports étroits avec des propositions qui traitent des mêmes propriétés mathématiques et qui appartiennent aux propriétés du domaine de l'optique géométrique.

2.1. *Comparaison de la proposition 3 du traité Sur la Section du Cône de Sérénos d'Antinoé avec la proposition 34 du traité d'Optique (cas 1) d'Euclide*

Sérénos d'Antinoé Sur la Section du Cône Proposition 3	Euclide Optique Proposition 34 (cas 1)
Lorsqu'un cône droit est coupé par des plans passant par le sommet, les triangles qui, déterminés dans les sections ont des bases égales, sont égaux entre eux.	Si on élève, du centre d'un cercle, une droite à angles droits sur le plan de ce cercle, et si l'on place l'oeil sur celle-ci, les diamètres menés transversalement dans le plan du cercle apparaîtront tous égaux.





Sérénos dans la démonstration de cette proposition, détermine l'égalité des triangles, donc il démontre l'égalité des angles ΕΑΖ et ΓΑΔ (figure 6). La procédure démonstrative suit les pas suivants : Soit un cône de sommet A et comme base le cercle de centre B. Le cône étant coupé par des plans passant par le sommet, il obtient les triangles déterminés par la section car il a été montré ailleurs que de telles sections déterminent des triangles (Apollonios, *Les Coniques,* livre I proposition, 3). Il obtient des triangles ΑΓΔ, ΑΕΖ ayant des bases égales **ΓΔ**, **ΕΖ**; il dit que les triangles **ΑΓΔ**, **ΑΕΖ** sont égaux. En effet, puisque les bases sont égales entre elles et que les droites **ΑΓ**, **ΑΔ**, **ΑΕ**, **ΑΖ** sont aussi égales, il s'ensuit que le triangle **ΑΓΔ** sera aussi égal au triangle **ΑΕΖ** (Euclide, *Les Eléments*, Livre I, proposition 8)·

Euclide démontre dans la première partie de cette proposition que les diamètres sont vus égaux. Il écrit qu'étant donné un cercle avec centre **Α**, on mène une droite perpendiculaire à ce point au niveau du cercle **ΑΒ** sur laquelle il mène l'oeil au point **Β**. Il dit que les diamètres seront vus égaux. Soit deux diamètres **ΓΔ**, **ΕΖ** et il mène les droites **ΒΓ**, **ΒΕ**, **ΒΔ**, **ΒΖ**. La droite **ΖΑ** est égal à la droite **ΑΓ**, la droite **ΑΒ** est commune et les angles droits. Il conclut que la base **ΖΒ** est égal à la droite **ΒΓ** et les angles aux bases sont égaux. Donc, l'angle ΖΒΑ est égal à l'angle **ΑΒΓ**. Pareillement l'angle **ΕΒΑ** est égal à l'angle **ΑΒΔ**, donc l'angle **ΓΒΔ** est égal à l'angle **ΕΒΖ**. Les choses vues par des angles égaux seront vues égaux. Donc la droite **ΓΔ** sera vue égale à la droite **ΕΖ**.

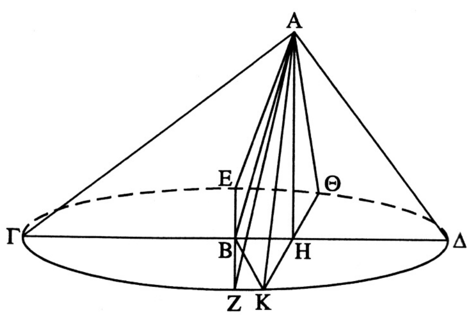
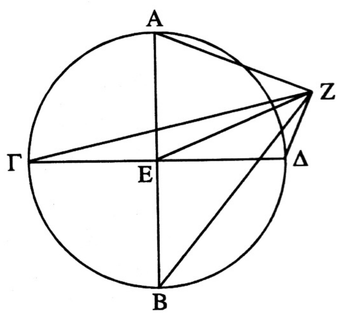
Euclide démontre dans la première partie que les diamètres sont égaux. Dans cette proposition, il démontre un cas qui n'a pas été précisé dans l'énoncé et il prouve exactement la même propriété entre les diamètres. À la fin de la proposition, il généralise en écrivant que: « Si l'on a un cercle qui est le plus grand dans une sphère, et si l'oeil se transporte n'importe où à la surface de la sphère en regardant la circonférence du cercle, les diamètres seront vus égaux » (Ver Eecke, 1926 : 26).

Quand Euclide écrit que les diamètres seront vus égaux, il veut dire que les angles qui regardent les diamètres sont égaux. Après cette remarque, nous constatons que Sérénos et Euclide démontrent la même propriété. Sérénos montre un cas plus général qu'Euclide parce qu'à la place des diamètres (cas d'Euclide) il prend des droites égales dans un cercle. Euclide démontre sa proposition pour le cas du cône droit (première partie de la démonstration) et du cône oblique (seconde partie de la démonstration).

Euclide démontre cette proposition dans le groupe des lemmes qui lui serviront de base pour la proposition 36. Les propositions 34, 35 et 36 considèrent la manière dont se présentent à la vue les diamètres d'un cercle regardé d'un point extérieur à son plan. La première de ces propositions démontre que les diamètres apparaissent égaux dans trois cas : 1. celui où l'oeil est placé en un point quelconque de la perpendiculaire élevée au centre du cercle; 2. celui où l'oeil est placé à l'extrémité d'une droite qui, menée par le centre du cercle, obliquement à son plan, est égale au rayon; et enfin 3. celui où l'oeil est placé en un point quelconque de la droite qui, menée obliquement par le centre du cercle, forme des angles égaux avec deux diamètres perpendiculaires entre eux. La seconde de ces propositions (proposition 35) traite le cas génèral de l'oeil placé sur la droite qui, élevée obliquement au centre du cercle, est plus grande ou plus petite que le rayon, et ne fait pas, comme dans le cas de la proposition précédente, des angles égaux avec de diamètres à angles droits. Il démontre que ces diamètres apparaissent toujours inégaux. Dans la proposition 36, il démontre que « les roues des chars apparaissent tantôt circulaires tantôt oblongues » (Ver Eecke, 1938 : 34). Cette proposition vise d'ailleurs un phénomène plutôt théorique car, en regardant obliquement un char, le concept de la roue dissipe l'illusion visuelle tandis que le phénomène se matérialise lorsque l'ombre de la roue est projetée sur le sol sous la forme d'une ellipse par les rayons obliques du soleil. La démonstration nous indique que si l'oeil ne se trouve pas sur le même plan que la roue et qu'il est localisé perpendiculairement en dessus de son centre, le contour de la roue va apparaître rond. Si l'oeil est placé à une distance égale au rayon de la roue (il n'est pas placé sur le même plan) la roue apparaît ronde. Pour toutes les autres positions en dehors du plan de la roue, le contour apparaît oblong. Cette proposition est une application des propriétés que nous trouvons bien développées dans les propositions 34 et 35. Ces propositions sont fondées sur la définition 4 et produisent des relations de dimensions relatives.

La proposition 3 de la *Section du Cône* démontre la même propriété que la proposition 34 d'*Optique*. De plus, ces deux propositions sont indispensables pour la démonstration de plusieurs propositions qui suivent et qui résolvent d'autres problèmes.

2.2. *Comparaison des propositions 50-51 du traité Sur la Section du Cône de Sérénos d'Antinoé avec la proposition 34 (cas 2) du traité d'Optique d'Euclide*

Sérénos d'Antinoé **Sur la Section du Cône** **Proposition 50-51**	**Euclide** **Optique** **Proposition 34 (cas 2)**
Si l'axe d'un cône oblique est égal au rayon de la base, le plus petit des triangles passant par l'axe sera au triangle isocèle qui est à angles droits sur la base comme le plus grand triangle passant par l'axe est au plus petit.	Si on élève, du centre d'un cercle, une droite à angles droits sur le plan de ce cercle, et si l'on place l'oeil sur celle-ci, les diamètres menés transversalement dans le plan du cercle apparaîtront tous égaux.
	

Dans la proposition 49 (Ver Eecke, 1929 : 136-137), Sérénos démontre que « parmi les triangles qui passent par l'axe, ceux qui sont symétriques sont équivalents et semblables ». Dans les deux énoncés, propositions 50-51 de Sérénos et proposition 34 (cas 2) d'Euclide, nous constatons que la relation entre l'axe et le rayon de la base dans un cône oblique détermine les relations entre triangles semblables chez Sérénos et entre angles égaux donc diamètres vus égaux chez Euclide.

Dans la démonstration Sérénos suit les pas suivants : Soit un cône oblique avec sommet A et axe AB égal au rayon de la base du cercle avec centre le point B. Soit, parmi les triangles passant par l'axe, le triangle ΓΑΔ à angles droits sur la base et le triangle isocèle EAZ. Le triangle EAZ est le plus grand de ceux qui passent par l'axe et le triangle ΓΑΔ est le plus petit (Sérénos, *Sur la Section du Cône,* proposition 24). Il mène la perpendiculaire du point A sur la base; elle tombera sur le diamètre ΓΔ (Euclide, *Les Eléments*, livre XI, définition 4). Que ce soit la droite AH; il mène la droite ΘΗΚ à angles droits sur la droite ΓΔ dans le cercle et il étend un plan par les droites AH, ΘΗΚ déterminant le triangle ΘΑΚ qui sera isocèle et à angles droits sur la base (Sérénos, *Sur la Section du Cône*, proposition 22 et Euclide, *Les Eléments,* Livre XI, proposition 18). Sérénos dit que le triangle ΓΑΔ, le plus petit de ceux qui passent par l'axe, sera au triangle isocèle ΘΑΚ comme le triangle EAZ, le plus grand de ceux qui passent par l'axe, est au plus petit triangle ΓΑΔ. Donc, triangle EAZ /triangle ΓΑΔ est égal au BA/AH et triangle ΓΑΔ/ triangle ΘΑΚ est égal au ΓΔ/ΘΚ qui est égal au EZ/ΘΚ et égal au BK/KH. Or, par hypothèse, BA est égale au rayon BK, d'où l'égalité des triangles rectangles BAH, BKH; donc BA/AH est égal au BK/KH, d'où triangle ΑΔ / triangle ΘΑΚ est égal au BA/AH, donc triangle ΓΑΔ/ triangle ΘΑΚ est égal au triangle EAZ/triangle ΓΑΔ.

La proposition 51 est la réciproque de la proposition 50 et à la fin Sérénos écrit :

Dès lors, il est clair que si l'axe d'un cône oblique est égal au rayon de la base, le triangle isocèle, qui est à angles droits sur la base sera semblable au triangle isocèle passant par l'axe; et réciproquement, que si le triangle isocèle, qui est à angles droits sur la base, est semblable au triangle isocèle passant par l'axe, l'axe du cône sera égal au rayon de la base; car cela aussi est facile à comprendre en vertu des choses déjà démontrées (Ver Eecke, 1929 : 140).

Dans l'examen du cas 2 de la proposition 34 Euclide prend la droite menée par le centre qui n'est pas perpendiculaire au niveau, mais égale au rayon, donc tous les diamètres seront vus égaux.

Dans la procédure démonstrative il prend le cercle ΑΒΓΔ et il mène deux diamètres AB, ΓΔ. Soit ZE droite menée par le point E sur laquelle on met l'oeil au point Z. Cette droite n'est pas perpendiculaire mais égale au rayon et il mène les rayons visuelles ZA, ZΓ, ZB, ZΔ. La droite BE est égale à la droite EZ, la droite EA est égale à la droite EZ d'où il s'ensuit que les droites EZ, EA et EB sont égales. Le demi-cercle décrit autour du diamètre AB au niveau qui passe par les droites AB, EZ passe par le point Z, donc l'angle AZB est droit. Pareillement l'angle ΓΖΔ est droit. Les angles droits sont égaux et les objets vus sous les angles égaux sont aussi égaux. Donc la droite AB sera vue égale à la droite ΓΔ.

Euclide démontre cette proposition comme réponse à l'énoncé dans lequel il examine la question suivante : « Si une droite tombant de l'oeil au centre d'un cercle n'est pas perpendiculaire au plan de ce cercle, ni égale à son rayon, mais est plus grande ou plus petite que celui-ci, les diamètres du cercle paraîtront inégaux » (Ver Eecke, 1926 : 26).

Dans cette partie de notre travail, nous pouvons constater que les figures des propositions sont identiques, mais que les propriétés démontrées ne coïncident pas. L'équivalence des propositions se fonde sur le fait que Sérénos et Euclide travaillent sur les inégalités des segments et les inégalités des triangles, ainsi que sur la relation entre l'axe et le rayon de la base dans un cône oblique, d'où ils déterminent les relations entre triangles et entre angles.

3. Conclusion

Dans la *Section du cylindre* et la *Section du cône* de Sérénos d'Antinoé nous avons vu l'existence, parmi les sujets traités, des propriétés qui traitent des problèmes optiques et des propriétés qui sont équivalentes aux propriétés du domaine de l'optique géométrique. Dans la deuxième partie de *la Section du Cylindre*, on pourrait dire que Sérénos essaie de représenter les parallèles par les moyens de l'optique géométrique. L'effort de Sérénos s'inscrit dans le mouvement des mathématiciens de l'Antiquité qui tentent de définir d'une manière différente d'Euclide les droites parallèles.

La nature des propriétés traitées dans son traité *Sur la Section du Cône* reste, selon nous, en partie un problème ouvert pour les historiens et il va rester car le matériel concernant cette tradition de recherche dans le domaine des mathématiques ou de l'optique mathématique est disparu. A part l'accord sur le fait que le but de Sérénos est de faire une étude comparative des sections produites dans un cône par l'intersection d'un plan qui passe par son sommet dans ce traité, nous avions montré que la proposition 3 peut être optiquement parlant équivalente à l'idée des cônes visuelles d'Euclide étant donné que le sommet du cône est au centre de l'oeil. L'analyse textuelle a montré que cette proposition est équivalente à la proposition 34 (cas 1) de l'*Optique* d'Euclide; cette démonstration est la contribution principale de cette partie de notre article.

Sérénos dans ces traités montre une connaissance de l'*Optique* d'Euclide. Il utilise aisément quelques propositions du domaine de l'optique dans le domaine des sections coniques et cylindriques pour examiner un problème lié à la théorie des parallèles. A ma connaissance c'est la prémière fois que cela est traité avec les moyens d'optique géométrique et des coniques. La perspective des traités de Sérénos et de l'*Optique* d'Euclide est différente et ces derniers résolvent des problèmes de nature différente.

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THE "RULE OF QUANTITY" IN SPANISH ALGEBRAS OF THE 16TH CENTURY. POSSIBLE SOURCES

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The "rule of quantity" in Spanish Algebras of the 16th century. Possible sources.

Summary: *The paper analyses the treatment of the systems of equations by the more notable authors of the Iberian Peninsula in the 16th Century. The works studied allows us to show the evolution of these systems. It was very important in the process of algebraization of the mathematics. Some hypotheses about the sources that inspired the authors studied have been proposed.*

Key words: *Algebra, Systems of equations, Iberian Peninsula, 16th Century.*

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Introduction

In 16th century Europe, mathematics underwent deep changes whose diffusion was favoured by the invention in the previous century of printing, which completely changed the way that culture was transmitted. One of the main changes was the progressive development of algebra from practical arithmetic, in which the symbolism played a relevant role. It is important to remark that at that time algebra was entirely rhetorical: equations and operations were expressed in words, written out in full.

Between the time of Leonardo de Pisa[1] (1170-1250) and the 16th century, scholars developed abbreviations for many of the words, such as writing *m* for "minus", but they did not introduce any kind of standardized notation.

The best known and probably the most influential Renaissance algebra, was the *Summa de Arithmetica, Geometria, Proportioni and Proportionalità* (1494) by Luca Pacioli (1445-1514). It was written in the vernacular and was a compilation of unpublished works that the author had composed earlier, as well as of general mathematical knowledge at the time. It was, together with Euclid's *Elements,* a reference work for Iberian authors in the 16th century.

The *rule of quantity* or the *rule of the second quantity* are the expressions used in the first treatises on algebra to refer to a procedure of solving problems in which more than one unknown were involved. The first appearance of the second unknown in Western culture was probably around 1373 in the *Trattato di Fioretti* by Antonio de Mazzinghi.[2] The use of more than one unknown would lead to the solution of simultaneous linear equations, whose discussion represented a big step forward in the process of algebraization of mathematics.

Kloyda[3] gave a bibliography of primary printed sources from 1550 to 1660, which contained what nowadays are known as simple and quadratic equations, in order to contribute to a knowledge of mathematical progress in the 16th and 17th centuries. Referring to this bibliography, Grcar[4] points out that in only 4 of the 107 texts mentioned were simultaneous linear equations discussed, none of them from Spanish authors.

The works I refer to in this paper are written by the following Iberian authors: Marco Aurel (fl. 1552), Juan Pérez de Moya (c.1513-c.1597), Pedro Núñez (1502-1578) and Diego Pérez de Mesa (1563–c.1633). The criteria for selecting these authors are based on the relevance of their works, in the cases of Aurel and Núñez, his popularity, in the case of Pérez de Moya, and finally the step forward represented by the resolution of simultaneous linear equations in the unpublished manuscript by Pérez de Mesa.

1. Leonardo de Pisa, better known as Fibonacci, was the autor of *Liber Abaci* (1202). This book contains the arithmetic and algebra that Fibonacci had accumulated during his travels. It is especially important because it introduced the Hindu-Arabic place-valued decimal system and the use of Arabic numerals into Europe.
2. See Franci, 1988, 7.
3. Kloyda, 1937.
4. Grcar, 2011, 169.

The aim of this article is to show the treatment given to the second unknown in these Spanish algebras of the second half of the 16^{th} century. This analysis will provide new information about the algebraization process. Thus, from the first text analyzed, in which the procedure to solve problems that involved more than one unknown is rhetorical, to the last analyzed work in which the author sets up the system of equations and solves it by operating with equations. I also will advance an hypothesis about the main sources from which the authors drew their inspiration.[5]

Marco Aurel. The first algebra treatise in the iberian peninsula (1552)

The first book to be printed in the Iberian Península that could be regarded as an algebraic treatise is:[6] *Libro Primero de Arithmetica Algebratica,*[7] which was published in Valencia in 1552. Its author, Marco Aurel, was born in Germany and settled in Valencia, where he taught practical mathematics. This book consists of 24 chapters. In the six first chapters the author exposes the properties of whole numbers, fractions and proportions, the rule of three, currency exchanges and progressions. From the 7^{th} to the 12^{th} chapter, Marco Aurel deals with square roots and cube roots, binomials and apotomes and their square and cube roots. In the 13^{th} chapter he states the characters that he will use afterwards. From the 14^{th} to the 21^{st}, and in the 23^{rd} and 24^{th} chapters, he deals with the rules to solve the different types of equalities. In the 22^{nd} he addresses the binomials and apotomes, which he had already dealt with, by using the rule of thing.

In Chapter XVI of his algebra, Aurel addresses the systems of equations thus: "It deals with the rule of quantity, with some rules and requirements by which they are done, also known as the rule of the second thing".[8]

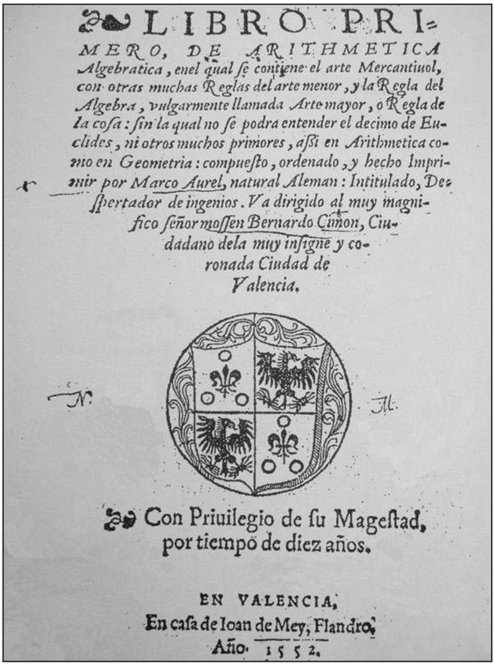
In this chapter the author solves eight problems with quite different wordings. The method to solve these problems consists in putting the second unknown in terms of the first one, the same for the third unknown, and so on. x being the first unknown, he puts *q* for

5. We must first take into account that Spain's communication with the rest of Europe almost ceased to flow from 1557, when groups of protesters were arrested in Seville and Valladolid. One year later, King Philip II presided over the first of a series of autos-da-fe that culminated in the burning of the Spanish Protestants. This ideological repression was a strict control of intellectual activity by royal power and the Inquisition, limited in principle to theology, but soon spreading to other fields. (for further information, see López Piñero, 1979). These circumstances made the availability of European reference works much more difficult in Spain.

6. This was the first algebraic treatise to appear in print, but not the first treatise in the Iberian Peninsula containing algebra, as Docampo has shown (Docampo, 2008).

7. The complete reference of this text is: *Libro Primero, de Arithmetica Algebratica, en el qual se contiene el arte Mercantivol, con otras muchas Reglas del arte menor, y lanRegla del Algebra, vulgarmente llamada Arte Mayor, o Regla de la cosa: sin la qual no se podra entender el decimo de Euclides, ni otros muchos primores, assi en Arithmetica como en Geometria: compuesto, ordenado, y hecho imprimir por Marco Aurel, nautral Aleman: Intitulado, Despertador de ingenios.*

8. *Trata de la regla de la quantidad, con algunas reglas, y demandas que por ella se hazen, que por otro nombre se puede llamar, regla de la segunda cosa* (Aurel, 1552, 108r).


LIBRO PRIMERO, DE ARITHMETICA Algebratica, enel qual se contiene el arte Mercantiuol, con otras muchas Reglas del arte menor, y la Regla del Algebra, vulgarmente llamada Arte mayor, o Regla de la cosa: sin la qual no se podra entender el decimo de Euclides, ni otros muchos primores, assi en Arithmetica como en Geometria: compuesto, ordenado, y hecho Imprimir por Marco Aurel, natural Aleman: Intitulado, Despertador de ingenios. Va dirigido al muy magnifico señor mossen Bernardo Cimon, Ciudadano dela muy insigne y coronada Ciudad de Valencia.

Con Priuilegio de su Magestad, por tiempo de diez años.

EN VALENCIA, En casa de Ioan de Mey, Flandro. Año. 1552.

Figure 1. The cover of Aurel's *Algebra.*

the second unknown, and when the second is expressed in terms of the first he also puts *q* for the third, and so on.

In the first problem, the expression 1x+14Q[9] has to be divided in two parts according to certain conditions.

The second problem concerns three companions who want to buy a horse and each of them individually has not got enough money.

The third one[10] is about four people who owe money and we need to know how much money each of them has when the sum of the debts of every group of three is known. The fourth problem is the same as the third but is solved in a different way.

The fifth concerns three people who want to buy a vineyard and none of them has enough money. The way to afford it is by asking the others for a part of the purchase price.

The sixth has an over-elaborate wording. It is about three people who have a book, a scarf and a pair of gloves. They are given six stones and each of them has to take one, two, or three stones, but we do not know how many stones each person takes. Afterwards, 20

9. Q is the symbol that Marco Aurel links to the independent term. Two of the authors studied in this paper, Marco Aurel and Pérez de Moya link a symbol to the independent term. But this is not the case for Núñez and Pérez de Mesa.

10. There is a small mistake in the text and this problem is numbered with a 2 equal to the second one.

more stones are put on the table. One of the people has to take the same number of stones as before, another one double and the third four times the number of stones he took before. If the person who has the book takes as many stones as before, the one with the scarf takes double, and the one with the gloves four times as many stones, and knowing how many stones are left on the table, we have to work out how many each person took at the beginning.[11] In order to solve this problem, the author puts the number of stones that each person took in terms of the *x* quantity assigned to the first one, and also uses an auxiliary unknown *q*, which is assigned to the second one. Finally, he has to give *x* some values until he finds a consistent solution. The seventh problem is the same as the sixth, but the author solves it in a shorter way.

The eighth deals with four people who have money. Using information about how quantities would change if some of them gave money to the others and about the total amount, one has to find out how much money each person has.

Let us consider the second problem:

> *Three friends have a certain amount of money and they want to buy a horse whose price is 34 ducats. The first friend tells the others to give him the half of money they have, and with the money he has he will buy the horse. The second tells the others that if they give him a third of the money they have, he could also buy the horse. Finally, the third friend wants the others to give him a quarter of the money they have, and by adding his own money he will buy the horse.*[12]

This problem is a classic one of several men buying a horse,[13] where each man needs a share of the other's money to buy the horse. Several of these kinds of problems can be found in the 2nd part, distinctio 9, treatise 9 of the Pacioli's *Summa* (1494). Although Pacioli has used a second unknown in two different occasions, he introduces it in distinctio 8, treatise 6 under the heading: *quantum essential notandum*, for solving a problem of two numbers, when the sum of their squares and their product are known. He calls the unknowns *surd quantities*[14] and he proposes to call the first *co.* and the second *1 quantita.*[15]

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11. Aurel, 1552, 110r-110v.

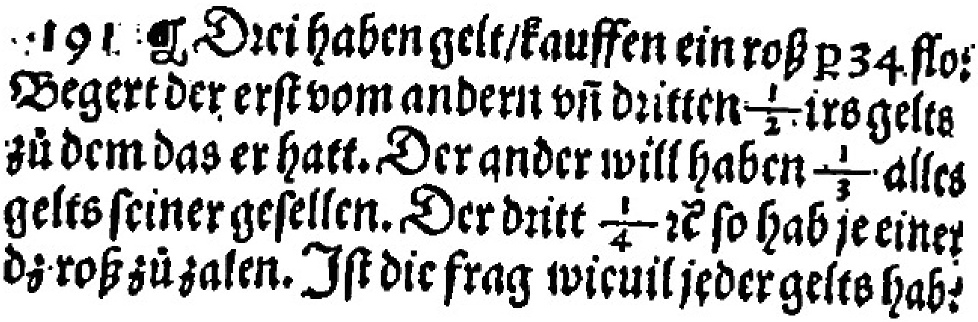
12. Tres compañeros tienen dineros, y quieren comprar vn cauallo por 34 ducados. Dize el primero a los otros dos que le den la $\frac{1}{2}$ de lo que tienen, y con lo q̃ el tiene podra pagar el cauallo: el segundo demanda a los otros dos, que le den $\frac{1}{3}$ de lo que tienen, y con lo que el tiene tambien podra pagar el cauallo: el tercero demanda a los otros dos el $\frac{1}{4}$, y que podra justamente pagar el cauallo (Aurel, 1552, 108v).

13. A comprehensive history of the problem is given by Tropfke (1980, 608).

14. The expression *surd quantity* is used by other authors to refer to irrational quantities.

15. It is also the first problem that Cardano (1501-1576) solves in Chapter IX of his *Ars Magna* (1545). In this case, Cardano speaks about the quantity of money that the three men have. Indeed, it is also question IIII in the first book of the algebra of Pelletier (Pelletier, 1554, 107), with a similar wording to Cardano's.

The way that Aurel solves the second problem is similar to how Pacioli[16] solved the first problem of the 2nd part, distinction 9, treatise 9 of his *Summa.*[17] The procedure is also the same that used Rudolff to solve the problems in the "regula quantitatis" chapter, and in this case we find a problem with similar wording.[18]



·:191.: ❡ Drei haben gelt/kauffen ein roß p 34 flo: Begert der erst vom andern vñ dritten ½ irs gelts zů dem das er hatt. Der ander will haben ⅓ alles gelts seiner gesellen. Der dritt ¼ etc so hab je einer dz roß zů zalen. Ist die frag wieuil jeder gelts hab?

Figure 2. The wording of a "men buying a horse" problem from Rudolff.

Let us now analyse the resolution of the second problem of Aurel, step by step. If we translate the wording of the problem into current algebraic language, we obtain:

$$\begin{cases} x + \frac{1}{2}\, y + \frac{1}{2}\, z = 34 \\ \frac{1}{3}\, x + y + \frac{1}{3}\, z = 34 \\ \frac{1}{4}\, x + \frac{1}{4}\, y + z = 34 \end{cases}$$

Aurel solves the problem in the following way: let us assume the first of the companions has x ducats. So, he needs *34Q-1x* to be able to buy the horse, a quantity that has to be equal to half of the other two.[19] Therefore, the other two have *68Q-2x* ducats, and altogether

16. Although the work of Pacioli was a reference for the authors from this period, the direct influence for Aurel was mainly Rudolff's algebra, especially when using the second unknown, as well as the notation and the concrete wording of problems (See Romero, Massa, forthcoming).

17. The wording ot this problem is: three (men) have d (denars). The first says to the other two: if you give me half of your sum, I will have 90. The second says to the other two: If you give me a third of your sum, I will have 84. The third says to the other two: If you give me a quarter of your sum and plus 6, I will have 87. I demand the quantity that every man has.

18. Rudolff, 1525, 221.

19. Aurel is working with the expression that would correspond to a first equation in the system.

the three of them have *68Q-1x*. If the second one has *q* ducats, then the first and the third together have *68Q-1x-1q* ducats. A third of this quantity is, $22\frac{2}{3}Q-\frac{1}{3}x-\frac{1}{3}q$ to which we have to add the quantity that the second one has, so that we get:[20] $22\frac{2}{3}Q-\frac{1}{3}x+\frac{2}{3}q$. Then Aurel makes this quantity equal to 34 ducats, which is the price of the horse, and with this expression the quantity of the second friend can be expressed in terms of the quantity of the first.

He then assumes that the third friend has *q* ducats; that is, he has used *q* to find the quantity that the second one has in terms of the first one's quantity. Then he uses the same unknown again to indicate the money that the third one has. As before, the first and the second friend together have *68Q-1x-1q* ducats. One quarter of this quantity is $17Q-\frac{1}{4}x-\frac{1}{4}q$,[21] to which he adds the quantity *q* that the third one has and obtains $17Q-\frac{1}{4}x+\frac{3}{4}q$, which must be equal to the price of the horse, that is, 34 ducats. From this equality he gets the expression of the third friend's quantity in terms of the first one: $\frac{1}{3}x + 22\frac{2}{3}Q$. When he joins the three quantities together, he obtains: $1x + \frac{1}{2}x + 17Q + \frac{1}{3}x + 22\frac{2}{3}Q = 1\frac{5}{6}x + 39\frac{2}{3}Q$, which must be equal to *68Q-1x*, from which he gets *x=10*, and *22* and *26* for the second and third one's quantities of ducats.

Thus, with Aurel's procedure it is not possible to work with all the conditions at the same time when solving problems in which more than one unknown are involved.

Juan Pérez de Moya. The most popular algebra treatise on the iberian peninsula

Aurel's book seems to have been one of the main sources for the most popular algebra book written in the Iberian Peninsula in the XVI century, *Arithmetica practica y speculativa*[22] (Salamanca, 1562) by Juan Pérez de Moya (Santisteban del Puerto, ca. 1513 - Granada, ca. 1597), which went into about 30 editions. Pérez de Moya studied in Salamanca and he was a priest in his home village.

The *Arithmetica* consist of nine books. In the three first books, the author exposes the operations with whole numbers and fractions, the rule of the three and proportional distribution of benefits. The fourth is about practical geometry and the fifth about theorical arithmetic. In the sixth book, Pérez de Moya deals with the currency exchanges, and the seventh is devoted to the rule of thing. The eighth is about ancient currencies and mobile holidays. The last book is very different from the others and is written as a dialogue about arithmetic between two students, to convince the reader about the importance of this discipline.

20. Now Aurel is working with the expression that would correspond to the second equation of the system.

21. Aurel is now working with the expression that would correspond to the third equation in the system.

22. Fifteen Chapters of this book were published previously in his work *Compendio de la Regla de la Cosa o Arte Mayor* (1558). More information about the Arte Mayor in Spanish texts in 16th century can be found in (Massa, 2012).

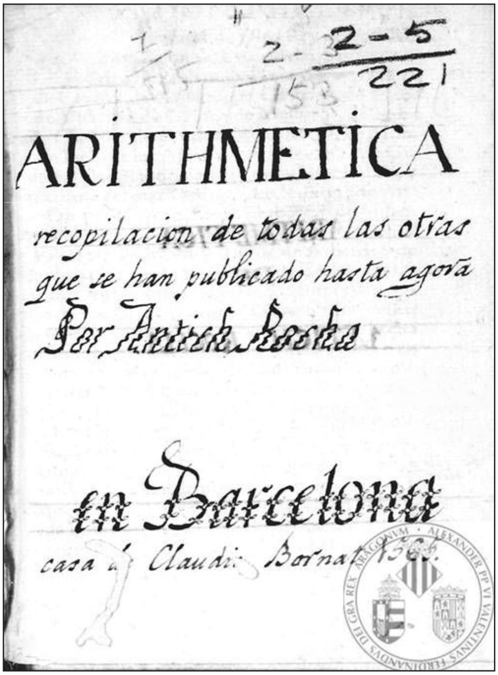


Figure 3. The cover of Antic Roca's *Arithmetica*

The *rule of quantity* is the 9th article of the 13th chapter of the 7th book, and in it the author solves only three problems, the first of which is similar to Aurel's first problem and is resolved with the same procedure. A quantity has to be divided in two parts according to certain conditions.[23]

The other two examples are problems that we would solve nowadays by a three equation system with three unknowns, and these problems are formulated with numbers; they have no concrete context dealing only with relations between numbers.

The wording of the second one is as follows:

> *Give me 3 numbers such that by adding the first number and the second one with half of the third the sum equals 30, and the second number with the third added to a third of the first number equals 30, and the third number with the first added to a quarter of the second number equals 30. It is required that ..., etc.*[24] (Pérez de Moya, 1562, 601-602).

23. Pérez de Moya, 1562, 600-601.

24. *Dame 3. numeros de tal condicion, que summando el primero y el segundo con la mitad del tercero la summa sea 30. y el segundo y tercero con el tercio del primero hagan 30. y el tercero y primero con el quarto del segundo hagan 30. Demando ¿&c.*

Pérez de Moya solves this problem in a similar way to Aurel, with an auxiliary unknown assigned to both the second quantity and the third. Unlike Aurel, he does not assign an *x* to the first quantity but rather *co.* and neither does he use the signs "+" and "-" to indicate addition and subtraction, but rather "p" and "m" , and the sign that Pérez de Moya links with the independent term is *n.*[25] However, he does use *q* to indicate the second and third quantities.

Regarding the resolution of these problems, it is clear that the author follows Aurel, although he does not quote him as a source.

In a later edition, published in 1573,[26] the author extended his work, particularly in the chapter devoted to the second quantity. In the preface addressed to the reader, Pérez de Moya states that there are improvements to be made in his works, and that this latest edition is a compilation of what he had written in vulgar language and also what he had written in a booklet in Latin. In his own words:

> *Therefore, recognizing that in my works there were things which required revision, I agreed to provide such amendments as seemed necessary: by amending myself many things from my works printed so far and adding to each subject what I thought should be made known to improve it. And thus this book appears as a sum of what has been written in the vernacular, and the best and most important things that we included in a booklet in Latin entitled* Sylua*, with the addition of more than two hundred written pages.*[27]

In this new edition, the rule of quantity also appears in the seventh book, but in this case it is the sixth article of the 51st chapter. Six problems are solved, and with important differences from the previous edition. In the 1562 edition, the fractions were expressed in a rhetorical way and the only fraction written explicitly was numerical, at the end of the first problem. In the 1573 edition, fractions that contain unknowns are explicitly expressed. Another more important novelty is the assignation of a letter to the third unknown, different

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The author left the question unfinished, probably taking for granted that the reader knew how this sentence ended. For the resolution of this problem, on the eighth line of page 602 there is a slight error: where it says "the other 2 will be 30.n.p.media.co.", it should say "the three will be 30.n.p.media.co.".

25. Another author that should be taken into account is Antic Roca , whose *Arithmetica* is written along the lines of Marco Aurel and Pérez de Moya . He deals with the Major Art in the fourth book, but there are no references to the rule of quantity. Although this author makes no reference to the *second quantity,* his algebra is interesting for a panoramic view of algebraic Spanish works in the 16th century. For further information, see (Massa, 2008).

26. The work was written at least two years before, since the King's permission to print it is dated 9th December 1571.

27. *Por tanto, conociendo que en mis obras avia cosas que requerian censura, acorde proveer a lo que me pudieran emendar: emendando yo mismo muchas cosas de mis obras hasta agora impressas, y con mejoria añadido sobre cada materia lo que me pareció que bastava saberse. Y assi va agora este libro como una summa de lo que se ha hecho en lengua vulgar, y lo mejor y mas importante de las cosas naturales que pusimos en un librillo de Latin intitulado* Sylua*, y añadidos sobre todo mas de docientos pliegos de escriptura (*Pérez de Moya, 1573, introduction without pagination*).*


TRATADO DE
MATHEMATICAS EN
QVE SE CONTIENEN COSAS DE ARITHMEtica, Geometria, Cosmographia, y Philosophia natural. Con otras varias materias, necessarias a todas artes Liberales, y Mechanicas. Puestas por la orden q̃ a la buelta de la hoja veras.
Ordenado por el Bachiller Iuan Perez de Moya, natural de Sant Estevan del Puerto.
DIRIGIDO A LA S. C. R. M. DE DON Phelipe Rey de España nuestro señor.
Con licencia, y priuilegio Real de Castilla y Aragon.
EN ALCALA DE HENARES.
Por Iuan Gracian. Año de 1573.

Figure 4. The cover of Pérez de Moya's *Tratado de Mathematicas.*

from the letter assigned to the second one. Although he still refers to the second unknown as "quantity", he does not assign to it the letter *q* but rather the letter *a.* To the other unknowns, the author assigns the letters *b, c, d,* and so on.

The first (p. 597) and the second problem (p. 598) solved by Pérez de Moya are numerical and very similar to each other. The translation of their wording into current algebraic language would be as follows:

$\begin{cases} 6x + 2 = 7y - 14 \\ xy = 90 \end{cases}$ for the first problem, and $\begin{cases} 4x + 16 = 3y + 10 \\ xy = 60 \end{cases}$ for the second one.

In both cases, the aim is to solve what are currently known as nonlinear systems, which the author does by isolating the second unknown from the first equation, replacing the second one and solving the obtained quadratic equation.

The third problem (p. 598) is also about relations among numbers: the goal is to find two numbers whose sum is 12, the quotient between the biggest and the smallest being 17, which at present we would translate into symbolic language like this: $\begin{cases} x + y = 12 \\ \dfrac{x}{y} = 17. \end{cases}$

Pérez de Moya solves this problem in different ways. First of all, he isolates the second unknown, from the expression that corresponds to the one I have written in the system as the first equation. He then replaces it in the second one and solves the obtained equation. Let us see how the author expresses the replacement:

> *Divide the thing (which is one number) by twelve numbers minus one thing (the value of the quantity that you put for the other one), and we put the partition above the ___ with a line between them as a fraction, like this* $\frac{\text{1co.}}{\text{12n.m.1co.}}$ *(as shown in Chapter 29), which you make equal to 17n. as you wanted. Then reduce this equality to whole numbers, multiplying twelve n. m .1 co. by 17 (as stated in the fourth article of Chapter 44), and make the result equal to one co. (which is the numerator of this fraction), and you will obtain the equality 204 n. m. 17 co. yg. to one co. Take 17 co. away (which has a minus sign in one part) and add them to the thing (which is in the other part) and 204 n yg. to 18 co. will remain. This means that 18 things are equal to 204 numbers.*[28]

The second way to solve the problem consists in assuming that the biggest term is the second one, so when we make the replacement in the expression that we have written in the second equation, the division will be $\frac{\text{12n.m.1co.}}{\text{1co.}}$ that in modern notation would be $\frac{12-x}{x}$. He later considers other ways that would correspond to the isolation of the unknowns in the second equation and the replacement in the first equation. In these cases, Pérez de Moya also considers different possibilities for finding the other unknown.

The fourth (p. 601) and fifth (p. 603) problems are similar and both of them have a concrete context. They concern three people who have money and the part they need from each other to obtain a specific quantity. In the fourth problem the relations that are expressed refer only to two of the people. However, in the fifth problem each relation includes the three people. In these cases, Pérez de Moya does not speak about quantity when referring to the second unknown. He assumes that the first person has "one thing of *reals*",[29] the second "one *a*" and the third "one *b*". In both problems he works with the relations between quan-

28. *Parte una cosa (que es el un numero) por doze numeros, menos una cosa (que es el valor de la quantidad que pusiste por el otro) poniendo la partición sobre el partidor con una raya en medio como quebrado, deste modo* $\frac{\text{1co.}}{\text{12n.m.1co.}}$ *(como se mostro en el capitulo 29) lo qual ygualaras a los 17n. que quisieras que fuera. luego reduze esta ygualacion a enteros, multiplicando doze n. m .1 co. por 17 (como manda el quarto articulo del cap. 44) y lo que montare ygualalo a la una co. (q es numerador deste quebrado) y quedara la ygualacion 204 n. m. 17 co. yg. a una co. quita la 17 co. (que vienen menos en la una parte) y juntalas con la cosa (que viene en la otra) y quedaran 204 n yg. a 18 co. quiere decir, que 18 cosas, son tanto o valen tanto, como 204 numeros* (Pérez de Moya, 1573, 599).

29. A real was an unit of currency at that time.

tities and sets the second and third unknowns in terms of the first one. Then when he has an equation with only one unknown, he solves it and then finds the unknowns with the relations he has obtained. When he solves the fifth problem, he makes an interesting generalization that is important to point out, with the aim to explain how to solve systems of linear equations, whatever the number of unknowns they have, that is, a general procedure:

> *And if we had four companions, you would proceed in this way: We would put for the fourth a quantity under this letter* c. *and we would do with this what has been done with* a. *and* b.*, and this way we can proceed infinitely.*[30]

The sixth and last problem in the 1573 edition is the first one in the 1562 edition:

> *From two and two thirds thing, plus 18, make two parts so that when taking twelve of the second part and adding it to the first one, the first one is the triple of what is left on the second one, and plus three.*[31]

In this case, the result cannot be given in numbers because it depends on the value of the unknown in the wording.[32]

In both editions, Pérez de Moya checks the solution by giving the unknown the value of 6, although he states that it can have any value. In the 1573 edition he adds this paragraph at the end:

> *From which it can be seen that, without knowing the value of the thing, these requirements can be met and putting as we see fit the value that we wish.*[33]

Although Pérez de Moya's quotes Cardano and Núñez in this edition,[34] he follows none of these authors in their treatments of the second unknown.

---

30. *Y desta manera procederas, si fueran quatro compañeros, poniendo por el quarto una quantidad debaxo desta letra c. y haziendo con ella lo que se hizo con la a.y la b. y asi se puede proceder en infinito* (Pérez de Moya, 1573, 604).

31. *Haz de dos y dos tercios cosa, mas 18 numeros, tales dos partes, que quitando doze de la segunda parte, y añadiendola a la primera, sea la dicha primera el triplo de lo que quedare a la segunda, y mas tres* (Pérez de Moya, 1573, 604).

32. The difference between the ways of solving this problem in the two editions is not the procedure itself, which is the same, but the fact that in the 1573 edition there are more explanations, and fractions are expressed with the current notation rather than in the rhetorical way used in the first edition.

33. *De lo qual se sigue, que sin saber el valor de la cosa, se pueden hazer estas demandas, poniendole a nuestra volutad el valor que nos pareciere* (Pérez de Moya, 1573, 605).

34. *Otras varias, y diversas ygualaciones ay que dexo de poner, porque para sabios no son menester, y para principiantes no se entenderan. Quien quisiere ver algo, lea el decimo d Arithmetica de Cardano. Y en las ygualaciones que el cubo y cosa, se ygyualaren a numero. Lea al doctor Pedro Nuñez, al fin del tratado de Algebra que lo trata mas discretamente, que ninguno de los que antes del lo inventaron* (Pérez de Moya, 1573, 589).

Pérez de Moya’s work contains an important step forward as opposed to Aurel’s work, thanks to the fact that he kept the symbol *co.* for the first unknown and he assigned the letters *a, b, c,* etc. to the following unknowns. This type of assignation, which gives relevance to the first unknown, is also highlighted in the Stifel’s[35] comment of *Die Coss de Rudolff*.

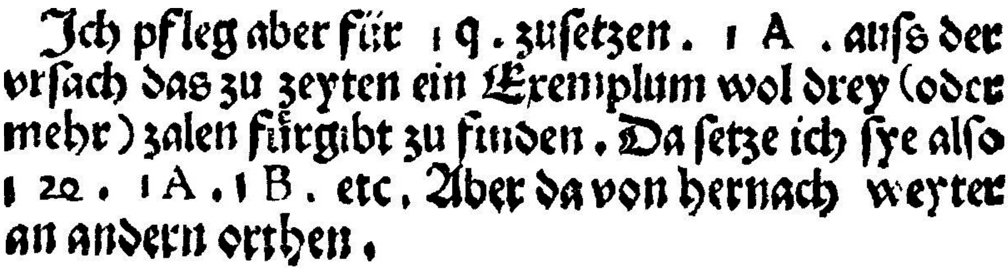

Ich pfleg aber für 1 q. zuſetzen. 1 A. auſs der vrſach das zu zeyten ein Exemplum wol drey (oder mehr) zalen fürgibt zu finden. Da ſetze ich ſye alſo 1 20. 1 A. 1 B. etc. Aber da von hernach weyter an andern orthen.

Figure 5. Notation of the unknowns in the comment of *Die Coss* of Rudolff by Stifel

Here Stifel states that he prefers to use *1A* instead of *1q* because sometimes there are examples with three or more numbers, and we will use *1A, 1B*, etc.[36] However, there is no evidence that Pérez de Moya was inspired by Stifel’s comment in *Die Coss.*

The assignation of the same letter to two or more unknowns makes it impossible to work with all the unknowns at the same time, preventing the system from being formulated explicitly.

Thus, as regards the second quantity, one may see the development of algebra in the work of Pérez de Moya. Assigning different values to different unknowns constitutes an important step, even though the method used does not lead him either to operate with the equations or to make the system explicitly.

Pedro Núñez (1567)

However, probably the most outstanding mathematician in the Iberian Peninsula during the XVI century was the Portuguese Pedro Núñez (Alcácer do Sal, 1502- Coimbra, 1578), who studied medicine and mathematics and worked as a mathematics lecturer at the universities of Salamanca and Coimbra. His book *Libro de algebra en arithmetica y geometria,* to which we will refer, was written in Portuguese during the 1530s, but was not published until 1567 and in Spanish.

35. In Heeffer, 2010, 77-82, the autor points out the innovation that Stifel introduces for the notation of the second and other unknowns.

36. Stifel, 1553, 186r.

It is a very long work, and different from Aurel's and Pérez de Moya's algebras. The interval between when it was written and its publication makes it difficult to analyze. There are many references to works after the 1530s that could not have been in the original version and which shaped the work.

In the 5th and 6th chapters of the third[37] main part, Núñez solves some problems that we would nowadays solve using systems of equations.

In the 5th chapter: *De la Practica de las Reglas de Algebra en los casos de Arithmetica,* Núñez solves 110 problems in which more than one unknown is involved. Some of them are linear and some others not. The resolution is rhetorical and the author makes no reference to a *rule of quantity*.

In the 6th chapter: *De la regla de la quantitat simple o absoluta*, Núñez solves three problems.[38] The author says that the rule of simple or absolute quantity is different from the others and that we use it in two ways. The first way is a substitution of the *rule of the thing* to perform the equality with the help of the term *quantity,* while the second is to do « position » over « position ». With this last expression, Núñez refers to problems whose wording contains one unknown, as we will see in the second problem of this chapter.

The first problem he poses is akin to the one we have given above as an example of Aurel's resolution, the classical problem about the three men who want to buy a horse but without any context.

Nuñez's wording is as follows:

> *We have three numbers, the third number with half of the second equals 32, the second number with a third of the other two equals 28, and the third number with a quarter of the other two equals 31. We want to know what each of them is.*[39]

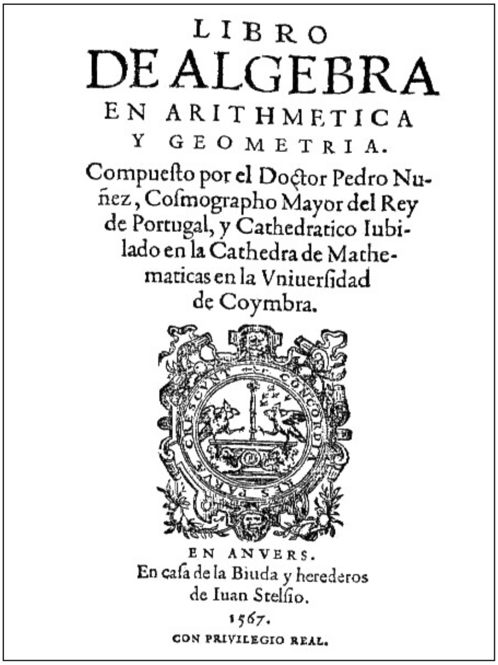
In modern notation we could write:

$$\begin{cases} x + \dfrac{1}{2}\ (y+z) = 32 \\ y + \dfrac{1}{3}\ (x+z) = 28 \\ z + \dfrac{1}{4}\ (x+y) = 31 \end{cases}$$

37. This work is divided into three main parts. For further information about its structure, see (Bosmans, 1908, 4-19).

38. According to Bosmans, this chapter is one of the most interesting in the *Algebra* of Pedro Núñez (Bosmans, 1907, 168).

39. *Tenemos tres numeros, que el primero con la mitad de los otros, haze 32 y el segundo con el tercio de los otros dos, haze 28 y el tercero con el quarto delos otros, haze 31 y queremos saber quanto es cada uno dellos* (Núñez, 1567, 224v).



LIBRO
DE ALGEBRA
EN ARITHMETICA
Y GEOMETRIA.
Compuesto por el Doctor Pedro Nuñez, Cosmographo Mayor del Rey de Portugal, y Cathedratico Iubilado en la Cathedra de Mathematicas en la Vniuersidad de Coymbra.

EN ANVERS.
En casa de la Biuda y herederos de Iuan Stelsio.
1567.
CON PRIVILEGIO REAL.

Figure 6. The cover of Pedro Núñez's *Algebra.*

Núñez assumes that the first number is 1 *co*. Then, half of the second number and third will be[40] 32m̃1co. and this expression doubled, that is 64m̃2co., will be the second and third number together. As the first number is 1 *co*., all three numbers will be 64m̃1co. Here Núñez is working with what I set as a first equation of the system, and he has expressed the sum of the three numbers in terms of the first one. After that, he assumes that the second number is "1 quantity", and from what he obtained with a reasoning based on the second equation, he arrives at the *quantity* in terms of the *thing* and also the third number in terms of the *thing*. It is important to point out that Núñez uses no abbreviation for the *quantity,* and that he gives no name to the third unknown. Núñez's reasoning is as follows: As the three numbers together make 64m̃1co., the first and the third together will make 64m̃1co.m̃1 quantitat. If we take the third part of this expression and we add to it 1 quantity, which is the value of the second number, we obtain $21\frac{1}{3}$ m̃. $\frac{1}{3}$ co.p̃ $\frac{2}{3}$ of quantity, from where he obtains for the value of 1 quantity 10p̃$\frac{1}{2}$ co. and for the value of the third one[41] 54m̃ 2co.$\frac{1}{2}$. Once he has expressed the three numbers in terms of the first one, he works with the rela-

40. m̃ is the symbol that Núñez uses to indicate the subtraction, and p̃ is the one he uses to indicate the addition.

41. I would like to remark that to find the expression of the third number in terms of the first, Núñez gives no name to the third number.

tion that we have written as the third equation of the system, and obtains the values 12, 16 and 24 for the first, second and third numbers, respectively.[42]

The second problem he addresses in this chapter is an example of what he calls "position" over "position", the second way of using the absolute quantity. After solving this problem, he gives an alternative method without using the quantity, but rather the proportion theory from Euclid's *Elements*.

The problem is as follows:

> *Let us divide* 1 co. p̃.3. *into two parts so that when adding 4 to the first number and subtracting 5 from the second, the first number would be six times more than the second.*[43]

In order to solve this problem, he assumes that the first part is 1 *quantity* and therefore the second one will be 1 co. p̃.3.m̃.1 *quantity*. Next, he adds 4 to the first part and takes 5 from the second one. Since the result of the first part is six times more than the result of the second one, he concludes that 6co.m̃.6 quantitats m̃.12. must be equal to 1 quantitat p̃.4., and from this he arrives at the value of the *quantity*, which is the first part, as well as the value of the second part.

He then solves the problem in another way without using the *quantity*. He says that if we add 4 to the first part and subtract 5 from the second one, then one unit of the sum will be lost and thus the sum will be 1 co.p̃.2. As the ratio of the result of the first part to a result of the second part is the same ratio of 6 to 1, the ratio of the total amount to the second part will be the ratio of 7 to 1,[44] and by the rule of three we can determine this second part:

If 7 gives us 1, how much will we obtain with 1co.p̃.2? The author says that the second one should be multiplied by the third and the result should be divided by 7, obtaining $\frac{1}{7}$co.p̃.$\frac{2}{7}$, which is the result of the second part when we subtract 5 units from it, so the second part will be $\frac{1}{7}$co.p̃.5$\frac{2}{7}$. On subtracting this quantity from the total amount, we obtain the first part.

42. Núñez had already solved this problem in the previous chapter without using the "quantity", but rather using the relations between the quantities involved, and in a clever way. This is problem 51 in the 5th chapter. At the beginning, the way to solve it is the same one we use in the following chapter: Núñez assumes that the first number is *1 co*. Then half of the second plus the third together make 32m̃1co. and two times this expression, that is 1co., will be the second and the third altogether. Then he takes into account the relation between the quantities that we have written as the second equation in the system, in order to say that if one third of the first, that is $\frac{1}{3}$ co. is taken from 28, we obtain the value of the second number and a third of the third number. On subtracting this quantity from 64m̃2co., that is, the value of the 2nd and the 3rd, we are left with 36m̃1co. $\frac{2}{3}$, which is the value of $\frac{2}{3}$ from the third number. If we add this quantity to its half, we obtain the value of the third number: 54m̃2co.$\frac{1}{2}$, If we subtract this quantity from 64m̃2co., then we are left with 10p̃$\frac{1}{2}$ co. He then continues as in the following chapter.

43. *Partamos* 1 co.p̃.3. *en tales dos partes, que dando ala primera 4 y sacando dela segunda 5 resulte la primera seis veces mas que la segunda* (Núñez, 1567, 225v).

44. Here Núñes refers to a "joint ratio" that Euclides proved in the 5th book of his *Elements,* but he does not quote the proposition.

The third and last problem leads to the solving of a quadratic equation. Núñez says that Cardano solved this problem by *the* "rule of quantity", but it can be solved in an easier way by *the* "rule of the thing". In fact, the first problem from Chapter X in Cardano's Ars Magna is as follows:

> *Inuenias duos numeros, quorum quadrata iuncta, sint 100 & productum unius i alterum duplumsit aggregato eorum.*

This he solves from the rules that he had previously demonstrated, and by using the properties of proportions.

Núñez says:

> *Let us find two numbers, the sum of whose squares is 100 and whose product is twice the sum of both.*[45]

This solution is more elegant than that of Cardano's. Nuñez's skill lies in the choice of the unknown. Let us assume that the two numbers together are 1 *co.*, so its square will be *1 ce.* Then he uses the equality that we would express as: $(x+y)^2 = x^2 + y^2 + 2xy$,[46] and concludes that 100p̃.4co. equals *1 ce*. Then by applying the rules of compound conjugations, he finds the value of the thing, that is, the sum of the two numbers, 2.p̃.R.104.[47] Therefore, he also knows their product, which is 4.p̃.R.416. The author then says that it is necessary to divide 2.p̃.R.104 into two parts so that the product makes 4.p̃.R.416, and that is why the rule of proportional means or algebra can be used. Núñez assumes that one of these parts is *1 cosa,* so the other will be 2.p̃.R.104m̃.1co. By multiplying these two parts and making their product equal to 4.p̃.R.416, he obtains the solution and then checks it.

Núñez's sources are mainly Pacioli and Cardano, although he looks for alternative methods to the ones they used. As already mentioned above, Núñez appears somewhat sceptical about the use of this rule, which may be seen from some of his statements:

> *And in all cases where Fray Lucas works by the quantity, we work by the rule of the thing, without the help of this term quantity.*[48]

Further:

> *Ieronymo Cardano found many rules of quantity, by which he solves many questions which could have been solved with greater ease by the rule of thing.*[49]

---

45. *Busquemos dos numeros, cuyos quadrados juntos en una suma, sean 100. y que lo que se haza multiplicando vno por otro, sea duplo de la suma de entrambos juntos* (Núñez, 1567, 226v).

46. Núñez sets out this equality rhetorically by referring to the 4th proposition in the 2nd book of Euclid's *Elements*.

47. In current language $2+\sqrt{104}$.

48. *Y todos los casos que Fray Lucas practica por la quantidad, practicamos nos por las Reglas de la cosa, sin ayuda deste termino quantidad* (Núñez, 1567, 225v).

49. *Ieronymo Cardano hallo muchas Reglas de quantidad, por las quales resuelve muchas questiones que trae, podiendose muy bien resolver por las Reglas de la cosa, y con mas facilidad* (Núñez, 1567: 226v).

Diego Pérez de Mesa (1598)

The algebra of Pérez de Mesa forms the second part of Manuscript 2294 dated 1598, which can be found in the Library at the University of Salamanca. It is a double faced 100 page treatise entitled: *Libro y tratado del arismetica y arte mayor y algunas partes de astrología y matematicas compuestas por el eroyco y sapentisimo maestro El Licenciado Diego perez de mesa catredatico desta Real ciudad de Sevilla del año de 1598.*[50] The part dealing with algebra is named by the author *Tratado y Libro de arte mayor o algebra.*[51] It begins on page 60 and is composed of an introduction and 23 chapters. In the three first chapters, Pérez de Mesa deals with numbers and their properties. In the fourth, he exposes his idea of algebra and the fifth is about proportional numbers. The sixth to the tenth are about the whole numbers and their operations, and the eleventh to the sixteenth about fractions and their operations. The seventeenth is about rational and irrational numbers and in the last six the author deals with equations.

Pérez de Mesa solves the systems of equations[52] in the last chapter, which he entitles "On the Rule of Quantity". He says that "the writers"[53] talk about the rule of quantity when they cannot arrive at a solution with only one unknown, and that to solve the situations involved, not all the writers use the same method". He adds that since assigning the same symbol to every unknown might give rise to confusion, the name of "thing" is usually given to the first unknown, while the other unknowns are called "quantities"[54] and are given different names. For the second unknown these authors put "a", for the third, "b", for the fourth, "c", and so on. Sometimes none of them is assigned the name "thing", and the first is called "a", the second "b", the third, "c", and so on.

Pérez de Mesa's way of solving the systems of equations is different from that of other Spanish authors. Rather than taking an auxiliary unknown, he puts « a » for the first unknown, « b » for the second, and so on. To solve a system, Pérez de Mesa reduces the expressions until he obtains a single expression that consists of one unknown on one side and a number on the other side. Then by applying the "simple canon[55]" he obtains the value of the unknown. After obtaining the first value, he obtains the others by replacing the value obtained in the other equations. This is the method that now is known as back-substitution.

The first exercise that he solves is as follows:

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50. Book and treatise on arithmetics and great art and some parts of astrology and mathematics written by the heroic and very wise master, the graduate Diego Pérez de Mesa, professor of this royal city of Sevilla in the year 1598.

51. Treatise and book of great art or algebra.

52. For a more detailed study of the work of Pérez de Mesa related to equations and the treatment of the *second quantity*, see (Romero, 2008).

53. Pérez de Mesa refers to something that is done for the "authors" or to the "writers", to speak about some rules that are assumed to be well known.

54. Pérez de Mesa, 1598, 99.

55. Pérez de Mesa calls "canons" the rules for solving equations.

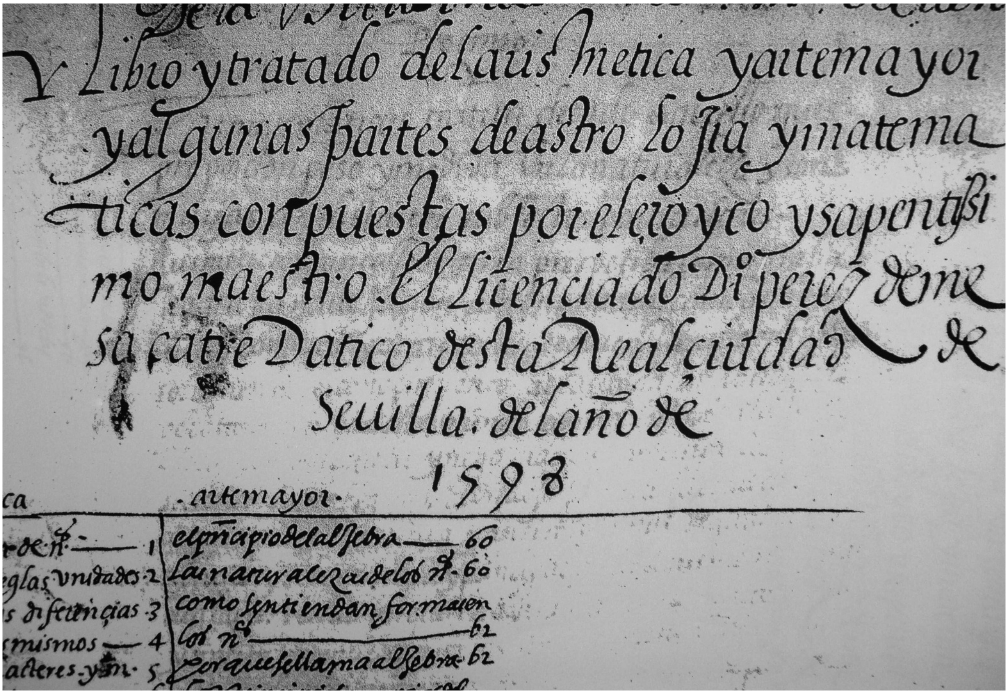

Figure 7. The title in the first page of manuscript 2294 by Diego Pérez de Mesa.

> *Two numbers are given, so half of the lower with the higher is 15 and the lower with a third of the higher is 10*[56].

Pérez de Mesa calls the larger number « a » and the smaller « b ». He says that the fractions should first be reduced to whole numbers and then obtains the equation that would nowadays be written: 2a+b=30 and a+3b=30. He solves the system by using the reduction method and by multiplying the second equation by 2 and subtracting it from the first equation, thereby obtaining 5b=30, and thus the value 6 for « b ». He replaces this value in the first equation and finds 12 for « a ». Then he sets out the system as follows.

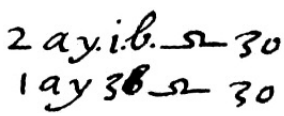

Figure 8. System of equations with 2 unknowns set up in Pérez de Mesa's manuscript.

56. Dénse dos n^{os} que sumando la mitad del menor con el mayor hagan 15 y el menor con el tercio del mayor haga diez (Pérez de Mesa, 1598, 99).

Although he solves this problem in a rhetorical way, I would like to point out the symbols he employs. He uses the letter "y" for the "plus sign", as he did when operating with polynomial expressions, and puts **Ω** for the equality. This is the first time in his manuscript that he uses a symbol to indicate the equality. We would do well to remember that neither Bombelli (1572) nor Stevin (1585) nor Viète (1590) used any symbol[57] to indicate the equality.

Let us see the solution to the second problem:

Three numbers are given such that the largest together with a third of the other two make 17; the second number with a third of the others make 15, and finally the last number with a third of the others make 13.

First, he removes the denominators and he writes the system as follows.

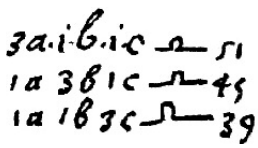

Figure 9. System of equations with 3 unknowns set up in Pérez de Mesa's manuscript.

In modern notation:

$$\begin{cases} 3a + b + c = 51 \\ 1a + 3b + c = 45 \\ 1a + 1b + 3c = 39 \end{cases}$$

Then he multiplies the second equation by 3 and subtracts it from the first. After that, he multiplies the third equation by 3 and also subtracts it from the first, thereby obtaining the system that we write as follows:

$$\begin{cases} 8b + 2c = 84 \\ 2b + 8c = 66 \end{cases}$$

He also solves this system by what is now known as the reduction method and obtains 6 for the value of « c ». Then, by substitution in the first equation, he obtains 9 for the value of « b ». Finally he puts the values obtained in the equation: 3a+b+c=51 and obtains the value 12 for « a ».

The author tackles the last problem that leads to what we now call a nonlinear system and solves it by the method of substitution.

When referring to the solution of the system of equations, Pérez de Mesa is probably taking Buteo[58] as his source, since on the first page of his manuscript devoted to alegebra he

57. For further information about the evolution of algebraic symbolism, see (Cajori, 1993).

58. Joannes Buteo is the Latinized name of Jean Borrel, a French mathematician (ca. 1492-ca. 1570).

quotes this author, first calling him “Triputeon”[59] and later “Puteon”. Buteo[60] makes the system of equations explicitly thus operating with them in the same way that Pérez de Mesa will subsequently do.

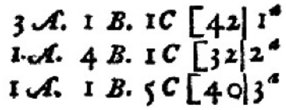

Figure 10. System of equations with 3 unknowns set up in Buteo’s *Logistica* (Buteo, 1559, 190).

We would also like to point out the fact that Pérez de Mesa considers all the unknowns in the same rank by assigning them different letters in alphabetical order.

Concluding remarks

The introduction of the second unknown was very important in the process of algebraization of mathematics, because it constituted a step forward in the development of the concept of an equation. The use of a second unknown contributed to the evolution of the idea of an equation, from a tool for solving some kinds of problems to the understanding of an equation as a new object of algebra with which one can operate, and thus also to the consideration of algebra as a discipline in its own right.

As we have shown, the stages in this process are reflected in the Spanish 16th century algebraic texts.[61] We can observe the different methods, starting with that by Aurel, in which the second unknown is called “quantity”, like the third one; Núñez’s algebra, a work by a somewhat sceptical author with the use of the second unknown, to Pérez de Moya’s work, published in 1573, in which different signs are assigned to different unknowns, although the first one retains a special name. Moreover, we have seen the furthest stage in the process in Pérez de Mesa’s manuscript, where the author writes the simultaneous equation system explicitly, treats the unknowns with equal rank and operates with the equations.

Although Núñez is quoted in the fourth chapter of Pérez de Mesa’s algebra, when refers to the names for the unknowns, and even though Pérez de Mesa follows Núñez in some aspects of his work, he is not followed by Pérez de Mesa in his skepticism about the use of the second unknown.

Thus, an important step referring to symbolic reasoning in the Iberian works studied is taken in the resolution of systems of equations in the algebra of Pérez de Mesa, when the method of reduction that involves operating with equations is used. It is probably not by chance that Pérez de Mesa introduces the sign Ω for the equality when he considers the equations, consciously or not, as a new object in the algebra.

59. Pérez de Mesa, 1598, 61.

60. Buteo, 1559, 160.

61. It is not easy to follow the thread of the second unknown throughout the different texts. A deep study on the early occurences of the second unknown in European texts can be found in (Heeffer, 2010).

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DOSSIER ESCOLA DE PRIMAVERA

EUROPEAN SPRING SCHOOL DOSSIER

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REPRESENTACIONS VISUALS EN CIÈNCIA. MÉS QUE IL·LUSTRACIONS, IMATGES CARREGADES DE SIGNIFICAT

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VISUAL REPRESENTATIONS IN SCIENCE

MORE THAN ILLUSTRATIONS, IMAGES FULL OF MEANING

Special issue coordinated by Alfonso Zarzoso and Josep Simon

Resume: *Images, like words, explain science. They have been decisive and continue to play a major role in scientific practice and in producing new knowledge. But who produces and how are produced these images? Who sees them and who is capable of reading them? Visual representations, like texts, allow the construction and communication of scientific concepts. Their omnipresence in the daily tasks of science does not guarantee however an understanding of their key role. They have often had a secondary status, limited to a decorative function, or are ignored as scientific, historical and philosophical sources. There is indeed an academic bias shaped by textual culture, and the circumscription of knowledge and awareness of the visual world to the strictly demarcated domain of art.*

The celebration this year of a new edition, the sixth, of the European School Spring of Mahon has addressed this problem and opportunity. In his 10 years of existence, the School, a unique initiative at the confluence of history and the popularization of science, has addressed a number of issues combining outreach, education and research in a unique biennial event in international context. This year, we proposed to reap the benefits of the previous editions of the School that had considered visual representations in science just as another kind of knowledge, along with the oral or written. The wealth of approaches around the image in science, produced in recent years within different academic disciplines has enabled us in this School to develop an *interdisciplinary area space aimed at clarifying the complexity of the visual cultures that participate in the forms of scientific knowledge.*

As a forum open to all, this edition has consolidated the international dimension of the School, with lecturers and students both from Europe and the Americas. It has also strengthened the teaching nature of the proposal, with the inclusion of different types of communication and classroom and online discussion <http://schct.iec.cat/school_11/spring11_index.htm>, producing results such as this dossier, and also a special issue for the journal Endeavour.

Les imatges, com les paraules, expliquen ciència. Han estat i continuen tenint un paper decisiu en les pràctiques científiques i en la producció de nou coneixement. Però, qui fa o com es fan les imatges? Qui les veu i qui és capaç de llegir-les? Les representacions visuals, com els textos, permeten construir i comunicar conceptes científics. La seva omnipresència en el quefer quotidià de la ciència no garanteix però la comprensió del seu paper cabdal. Sovint han tingut un estatut de segona divisió, limitat a una funció decorativa, o bé són menystingudes com a font científica, històrica i filosòfica, arraconades pel biaix acadèmic de la cultura escrita i la circumscripció del coneixement i la consciència del món visual al territori estrictament de l'art.

L'estudi de les representacions visuals ha esdevingut un dels àmbits de treball més productius en la història de la ciència de les últimes dècades. No obstant això, els estudis visuals de la ciència formen part d'una sòlida tradició interdisciplinària. Aquest àmbit de recerca ha experimentat un notable creixement i diversificació, fins al punt d'incloure no només objectes visuals tradicionals, tals com els retrats, sinó també un gran ventall de representacions visuals produïdes per la pràctica científica, de tècniques de representació i de pràctiques de producció de significat. L'anàlisi de la producció, circulació i ús de les representacions visuals s'ha beneficiat de la interacció entre disciplines com la història, la sociologia i la filosofia de la ciència, la història de l'art, la història del llibre, la història de l'educació, la divulgació científica i l'antropologia. Els estudis visuals de la ciència han estat a més un punt de trobada dels debats actuals dirigits a superar la distinció tradicional entre la producció i la comunicació del coneixement científic.

Les imatges ocupen un lloc central en aquest context, atesa la seva força per aglutinar coneixement científic, la seva capacitat per comunicar a públics diversos i la seva flexibilitat en el moment de crear significats a partir de la interacció entre productors i usuaris. Les imatges contenen també codis especials i formes de representació que constrenyen els seus significats. Alhora, una gran diversitat de cultures visuals contribueix a modelar el coneixement científic mitjançant processos d'apropiació visual que transformen la seva mateixa producció. El coneixement visual es diferencia del coneixement textual per les seves especials característiques. No obstant això, les interaccions i interseccions entre ambdós tipus de coneixement existeixen i són rellevants.

La pràctica científica produeix un gran ventall de representacions visuals de la natura, les quals esdevenen alhora eines per a la producció de nou coneixement. Les representacions

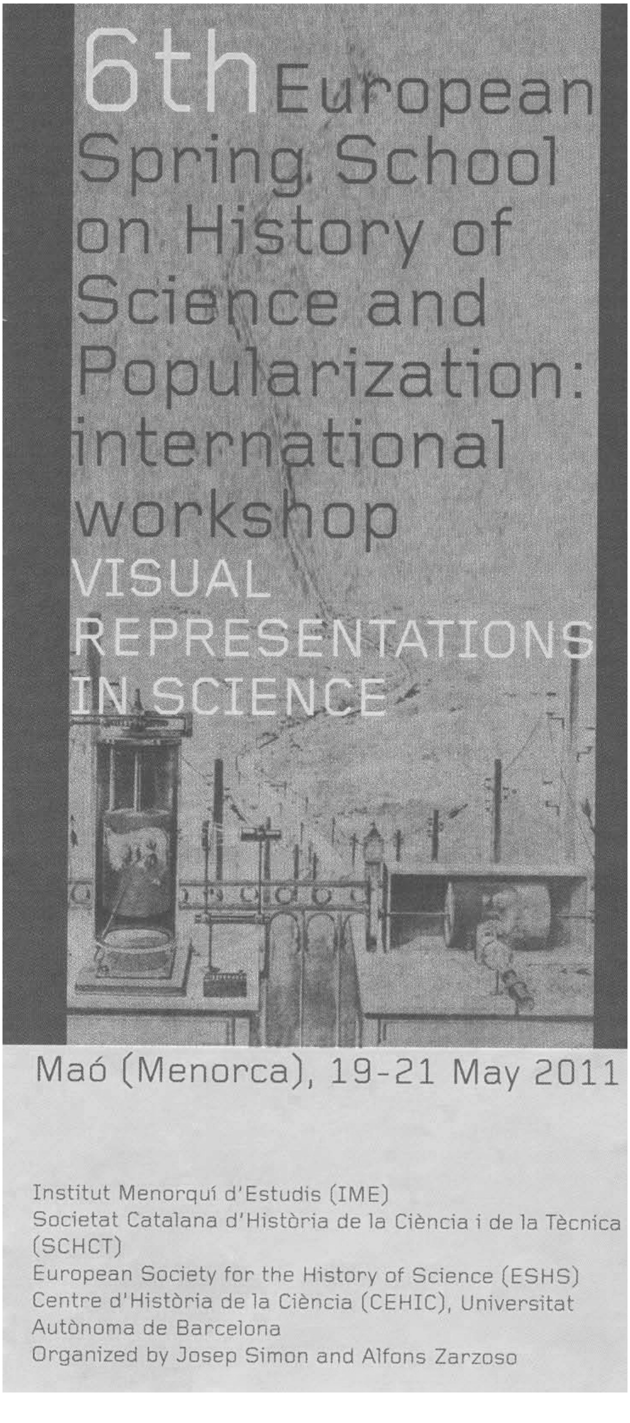

Figura 1. Portada del Tríptic de la VI Escola de Primavera d'Història de la Ciència i la Divulgació, dedicada a les Representacions Visuals en Ciència, Maó, maig, 2011.

visuals en ciència travessen les categories de recerca, ensenyament i divulgació. L'estudi de la reproducció, circulació i apropiació de les imatges ofereix una base excel·lent per comprendre com es forma el coneixement científic. L'anàlisi de la producció i la manipulació de les imatges, els debats al voltant d'aquestes pràctiques i la creació d'estàndards visuals poden tenir un paper clau en la nostra comprensió del canvi disciplinari i en el disseny de noves narratives en la història de la ciència. Paradoxalment, malgrat la centralitat de les representacions visuals en la producció de la ciència, la seva anàlisi apareix endarrerida davant el tradicional biaix acadèmic cap a les fonts escrites.

La celebració enguany d'una nova edició, la sisena, de l'Escola Europea de Primavera de Maó ha abordat aquest problema i oportunitat. En els seus 10 anys d'existència, l'Escola, una iniciativa única en la confluència entre la història i la divulgació de la ciència, ha abordat un gran nombre de temes combinant divulgació, educació i recerca en una cita bianual singular en el panorama internacional. En aquesta edició es va proposar recollir els fruits de les escoles anteriors que havien considerat les representacions visuals en ciència com un coneixement més, al costat de l'oral o l'escrit. La riquesa d'aproximacions al voltant de la imatge, ens ha permès en aquesta Escola de posar en marxa un espai d'interacció interdisciplinari dirigit a esclarir la complexitat de les cultures visuals que participen en les formes del coneixement científic.

En el marc d'un fòrum obert a tothom, aquesta edició ha consolidat el tret internacional de l'Escola, amb professorat i assistents europeus i americans, així com el caràcter docent de la proposta, amb la inclusió de diferents tipologies de comunicació i discussió presencial i en línia <http://schct.iec.cat/school_11/spring11_index.htm>, tot produint resultats com aquest dossier o un monogràfic per a la revista *Endeavour*.

Els textos d'aquest dossier representen els diferents participants que protagonitzaren l'Escola —professors, estudiants de doctorat i estudiants de màster— com a ponents, com a comentadors i com a públic. Aquests materials constitueixen una proposta resumida d'alguns dels elements de reflexió que foren analitzats i debatuts en aquest context. Així, mitjançant la intersecció que els estudis de cas permeten fer entre les imatges i les paraules, els articles del dossier discuteixen les preguntes clàssiques de «qui», «per què», «com» i «per a qui». És a dir, es demanen per l'autoria de les representacions visuals i el seu estatus en la ciència, plantegen com les tècniques de representació visual i les pràctiques d'apropiació visual han modelat la producció de coneixement científic, analitzen les imatges i formulen preguntes sobre com classificar-les, com descriure-les i com lligar-les amb els objectes que representen. En aquest dossier resta ben palesa la importància del coneixement visual, textual i oral en ciència i els autors dels textos destaquen interaccions i formes de circulació i d'apropiació d'aquest coneixement. En definitiva, el dossier mostra com a resultat de l'Escola com podem millorar la nostra comprensió de les representacions visuals en història de la ciència, la tecnologia i la medicina.

Nota. Els articles de Katy Barrett, Nick Hopwood, Sebastian Pranghofer i Jose Ramón Marcaida han estat traduïts pels coordinadors del dossier/monogràfic.

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PRIMAVERA VISUAL: UNA HISTÒRIA DE CIÈNCIA I ART

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Mapes, paisatges, gràfics, fotografies, espectres químics, radiografies, models d'ADN, simulacions computeritzades, traces de partícules elementals... La ciència produeix una gran quantitat de representacions visuals sense les quals no es poden comprendre les pràctiques científiques. L'estudi de la cultura visual de la ciència és, doncs, un repte fonamental per comprendre la creació i l'evolució del coneixement al llarg del temps. Aquest desafiament fou entomat per la 6a Escola Europea de Primavera d'Història de la Ciència i Popularització celebrada entre el 21 i el 23 de maig a Maó. Investigadors i estudiants de diferents nacionalitats i disciplines es van reunir a l'Institut Menorquí d'Estudis (IME) per aprofundir en l'anàlisi de les imatges en ciència, tecnologia i medicina. A més de l'IME, la Societat Catalana d'Història de la Ciència i de la Tècnica (SCHCT), l'European Society for the History of Science (ESHS) i el Centre d'Història de la Ciència (CEHIC) de la Universitat Autònoma de Barcelona van recolzar aquest esdeveniment coordinat per Josep Simon i Alfons Zarzoso. Les jornades van oferir tres conferències d'experts internacionals sobre cultura visual, tres tallers i, conseqüentment amb el contingut de l'escola, una sessió de pòsters. Un programa ambiciós quant a temàtica i abast històric que desgranarem a continuació, seguit d'algunes reflexions sobre l'impacte de la cultura visual en la història de la ciència després de tres dies d'immersió en aquest àmbit.

Els tres ponents principals van afrontar amb les seves xerrades períodes i enfocaments molt variats. Daniela Bleichmar, de la Universitat del Sud de Califòrnia, va obrir l'Escola amb la ponència «Visual Epistemology and Multimedia Knowledge in Early Modern Science», on analitzà les característiques dels mapes administratius de l'Imperi Espanyol a l'Amèrica del segle XVIII, formats per textos i dibuixos. L'estudi d'aquestes representacions mostra la col·laboració entre científics, il·lustradors i indígenes, així com la importància de les imatges com a transmissores de coneixement. El segon ponent va ser l'historiador de la Universitat de Cambridge Nick Hopwood, amb una sòlida reconstrucció de la vida d'una sèrie d'imatges d'embrions. Un recorregut històric des de la seva creació per Ernst Haeckel al segle XIX, fins a la seva reutilització contemporània. «Copying Pictures, Making Icons: from Alleged Forgeries to Textbook Illustration» és un cas paradigmàtic per entendre per què algunes imatges desapareixen ràpidament i altres, com les de Haeckel, sobreviuen durant dècades i es converteixen en autèntiques icones del coneixement, tot superant les acusacions de frau que el van envoltar. L'últim ponent va ser Klaus Hentschel, de la Universitat de Stuttgart, amb la conferència «Towards a Comparative History of Visual Science Cultures». Hentschel, amb el seu estil emfàtic, i basant-se en el seu estudi del desenvolupament de l'espectrografia del segle XIX, va ressaltar la importància de la prosopografia de comunitats científiques, defensant l'estudi de les biografies com una eina per explicar el desenvolupament visual de cada especialitat.

D'altra banda, vuit estudiants postdoctorals, seleccionats entre més de 60 candidatures, van protagonitzar el taller principal de l'escola. Els seus articles van circular prèviament entre els assistents i van ser sotmesos als comentaris dels tres ponents i al debat públic amb la resta d'assistents. L'objectiu va ser polir els treballs per preparar un número especial de la revista d'història de la ciència *Endeavour*. En aquesta sessió un dels articles destacats va ser el de Mirjam Brusius de la Universitat de Cambridge sobre la balbucejant introducció de la fotografia en l'arqueologia del segle XIX, molt allunyada de la imatge d'objectivitat que envolta aquesta pràctica en l'actualitat. Tant aquest taller com la sessió de pòsters van mostrar les nombroses opcions que es poden obrir en posar el focus en les representacions visuals durant l'anàlisi històrica. Com a exemple d'aquest ventall es podria ressaltar l'article d'Aaron Wright (Universitat de Toronto) sobre la influència dels diagrames de Penrose en el desenvolupament de la teoria de la relativitat general, el treball de Tom Schilling (MIT) sobre la rellevància de la geoinformàtica en l'acceptació de les exploracions d'urani a l'Àrtic durant el segle XX o la recerca de José Ramon Marcaida (CSIC), presentada en format pòster, sobre les diferents representacions, amb peus i sense peus, de l'au del paradís durant l'edat moderna. Els altres dos tallers van ser oberts a tot el públic: una sessió lliure on es proposava presentar imatges per discutir-les amb la resta de participants i, finalment, un taller dirigit per Nick Hopwood sobre com abordar l'estudi dels models tridimensionals en la ciència.

Amb tots aquests arguments, l'Escola va deixar patent la rellevància de la cultura visual per entendre les pràctiques científiques. Com es podrien explicar els mapes del segle XVIII estudiats per Daniela Bleichmar sense conèixer les convencions, moltes vegades de caràcter artístic, que s'empraven? Es podrien comprendre aquests mapes o altres representacions visuals com els atles del segle XIX sense aprofundir en les complexes relacions entre científics, il·lustradors i els seus editors? S'encertaria el valor científic de les incipients fotografies sense adonar-se de les limitacions tècniques dels primers daguerreotips? Es podria explicar el desenvolupament de disciplines com l'embriologia o la geologia sense valorar el rol de les imatges en la seva comunicació i comprensió? L'escola va respondre aquestes preguntes sobre la història del coneixement i va obrir moltes altres incògnites on sembla que l'estudi de les representacions visuals i l'apropament a altres disciplines com la història de l'art tenen molt a dir.

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LA DEMOSTRACIÓ DE LA LONGITUD I EL «LLUNÀTIC» DE LA LONGITUD AL *A RAKE'S PROGRESS* DE HOGARTH

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L'any 1735 William Hogarth publicà la seva segona sèrie de gravats sobre «subjectes morals moderns», *A Rake's Progress*, on mostrava la corrupció de Tom Rakewell, fill d'un ric comerciant, en el context de la societat londinenca. A l'última d'aquelles làmines, Rakewell embogia i era tancat a Bedlam —nom amb què es coneixia l'hospital medieval per a bojos Bethlem Royal Hospital—. Un dels interns destaca al centre del gravat, tot dibuixant a la paret i mirant de trobar una solució al problema de com mesurar la longitud en el mar.

El 1714, la British Longitude Act ja havia establert que la determinació de la longitud en el mar constituïa un problema important, de resolució valuosa, per a la ciència, la navegació i el comerç. Amb la llei es dotà un notable premi econòmic i es crearen uns administradors d'aquest fons. El problema va atreure una gran atenció i un gran nombre de mitjans se'n feren ressò: pamflets i diaris, obres de teatre, poemes i impresos. D'aquestes fonts materials van sorgir diversos trops emprats pels coetanis com a mitjà per pensar sobre la longitud, tot utilitzant aquesta per tal d'articular les seves preocupacions socials.

El gravat de Hogarth recull tots aquells trops i es presenta com un marc ideal per discutir no només quin fou el problema de la longitud, sinó també què féu la longitud diferent d'altres problemes contempora-

Figura 2. William Hogarth, *A Rake's Progress*, gravat 8 (1735). Trustees of the British Museum.

nis. Envoltat de tipus genèrics de bojos, el «llunàtic» de la longitud presentat per Hogarth fa referència a un problema científic molt específic. Què fa l'intern esmentat? El podem entendre com un dels «iconotextos» expressats per Peter Wagner o, encara millor, com un «iconotrop» que, analitzat a fons, ens ajuda a desentranyar les qüestions principals sobre ciència, societat i canvi polític i cultural que preocupà els coetanis de Hogarth. La longitud fou així molt més que un problema científic per ser resolt «en el mar»: constituí un veritable problema en terra ferma, els protagonistes del qual lluitaren per tal d'afirmar una autoritat visual, intel·lectual i social que permetés fer plausibles les seves propostes.

Així doncs, hi ha tres dimensions més a tenir en compte per entendre històricament el problema de la longitud: visual, mental i social. En aquell període, un gran nombre de pamflets discutien la longitud i la latitud com quadrícules artificials aplicades per l'home al món.

Diferents mitjans, com ara la cartografia, les il·lustracions d'instruments proposats per resoldre el problema de la longitud i els diagrames d'aquells que tingueren una aplicació amb èxit, tots venien a debatre des d'una perspectiva visual el com articular solucions sobre el paper. Així doncs, el «llunàtic» de Hogarth està de fet presentant aquest debat visual sobre els murs de l'hospital del Bedlam. El mateix intern-dibuixant resulta problemàtic: és retratat com un boig, però també com un «projector», un promotor d'idees ingènues o malvades, nocives per a la societat. Però alhora, aquest boig i tots aquells dedicats en cos i ànima a la resolució del problema de la longitud, apareixen sovint representats com genis excepcionals. Aquest era, de fet, el *problema mental* de la longitud: on s'havia de col·locar la línia separadora entre geni, bogeria i expertesa? En el gravat, el malalt de longitud és observat per dues dones que representen la societat en general. La representació d'un mapa o quadrícula de longitud i latitud fou emprada també per configurar els estrats de la divisió social. Els nous instruments inventats per determinar la longitud acabaren formant part dels debats sobre l'exhibició(nisme) i el consum en la societat, com s'havien de definir les fronteres del coneixement i qui tenia l'autoritat de fer-ho. Aquest fou el problema social de la longitud.

Figura 3. Detall del «llunàtic» de la longitud, a: William Hogarth, *A Rake's Progress*, gravat 8 (1735). Trustees of the British Museum.

Els gravats de Hogarth palesaven la desorientació creada per la societat moderna. En última instància, el problema de la longitud tractat a la darrera il·lustració de la sèrie actuà com a mitjà orientador d'aquella societat des d'una perspectiva visual, mental i social. El llunàtic de Hogarth mostrava als seus coetanis l'escriptura al mur.

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LA POLÍTICA DELS MODELS

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Els vells models anatòmics han cobrat un protagonisme creixent en museus i exposicions de renom al voltant de la febre pels temes «art i ciència». Els departaments universitaris estan traient dels armaris i exposant antigues preparacions de cera, guix o cartró pedra. Però si bé aquests models tenen la capacitat d'impactar-nos més que un llibre, davant la llum i la brillantor de les imatges digitals poden semblar més aviat pesats, foscos i estàtics. Podem gaudir de l'habilitat dels seus modeladors i ser esperonats a reflexionar sobre la vida, la mort i la tradició anatòmica; els models fomenten fàcilment la nostàlgia d'un temps anterior al que arribà per establir que «3D» es referia a una imatge en una pantalla plana. No obstant això, interpretar-los més enllà resulta difícil. La recerca sobre aquest tipus d'objectes ha de fer front al món secret dels modeladors i de les etiquetes perdudes i, sobretot, al gran obstacle que és l'assumpció que els models s'explicaran *per se*. De manera paradoxal, però, els models podran plantejar millor qüestions actuals, com més coses sabem sobre llur producció i ús en el passat.

Les qüestions que estan en joc es manifesten clarament si comparem dos modeladors de mitjan segle XIX procedents de les terres germàniques que en aquell període estaven en vies d'esdevenir centre mundial per a la ciència mèdica. Si bé tots dos van nàixer el mateix any i començaren amb projectes estretament relacionats, les seves carreres exemplifiquen els extrems de la confrontació i la cooperació amb l'autoritat mèdica. Els models de Paul Zeiller (1820-1893) conservats en l'actualitat són pocs i dispersos, mentre que els embrions de cera d'Adolf Ziegler (1820-1889) es troben àmpliament representats a les

Figura 4. Model en cera magnificat d'un embrió humà d'un mes «disseccionat» a partir d'una reconstrucció selectiva d'una sèrie de seccions. Procedent d'una sèrie feta el 1888 per Friedrich Ziegler segons els originals de Wilhelm His. Museu Anatòmic de Basilea.

col·leccions d'arreu del món. El treball d'ambdós connecta amb els debats actuals sobre alternatives a les disseccions. Té fins i tot una rellevància més general, perquè en una època marcada per exposicions com *Body Worlds* de Gunther von Hagens o iniciatives com el *Visible Human Project* de la Biblioteca Nacional de Medicina dels Estats Units, les relacions entre productors, mecenes, clients, usuaris i cadàvers continuen condicionant la política del suport visual anatòmic.

Els models anatòmics en cera van assolir un primer pic de perfecció en les col·leccions italianes de finals del segle XVIII. Aquelles anatomies artificials tenien l'objectiu de crear una enciclopèdia tridimensional del cos per complementar, i fins i tot substituir, la dissecció de cadàvers, sempre escassos. Però després de la Revolució Francesa l'autoritat mèdica es basà cada vegada més en l'experiència directa amb el cos humà, viu i mort. Així, a començaments del segle XIX, la majoria dels anatomistes consideraven que tot i la utilitat dels models, sobretot per als profans, mai no haurien de substituir el paper de les disseccions damunt del cadàver en l'educació mèdica. No obstant això, els professors acceptaren que els models podien tenir un paper auxiliar, sempre que foren produïts sota llur supervisió experta, especialment en el cas d'espècimens rars, petits, efímers o difícils de conservar, i de preparacions complexes o laborioses.

Afavorits per la popularitat dels mitjans visuals, els modeladors més emprenedors presentaren els seus productes com a recursos més adients per a la comprensió de l'anatomia humana que les preparacions naturals o els dibuixos, però van haver de negociar amb cura l'autoritat mèdica dels seus objectes. La majoria dels modeladors concentraren la seva clientela en el públic profà o escolar, o bé treballaren per encàrrec, a les ordres de l'agenda professional de certs metges. Però així i tot, l'autoritat dels modeladors romangué vulnerable a la possibilitat d'atacs frontals. L'anatomia era una matèria delicada pel caràcter controvertit de la dissecció, derivat de pràctiques com les disseccions públiques de criminals executats, i posteriorment el trasllat d'aquesta activitat a l'àmbit privat i als cossos dels pobres. El poeta i ocasional anatomista germànic Johann Wolfgang von Goethe, impactat per les notícies britàniques de robatoris de cadàvers en cementeris i fins i tot d'assassinats amb finalitat dissectiva, va defensar de manera romàntica l'ús dels models com a pràctica més humana i més efectiva, ja que «construir ensenya més que destrossar, unir més que separar, animar allò mort més que no pas acarnissar-se de nou en allò que ja és mort».

La crida de Goethe es féu ressò i fins i tot es radicalitzà a la universitat bàvara de Munic, sobretot després de la revolució de 1848. L'artista Paul Zeiller havia estat designat «preparador de ceres» per a la universitat, per l'alta consideració guanyada en el context acadèmic pels models anatòmics que havia dissenyat per acompanyar un atles d'embriologia. Però en abdicar el rei Lluís I, Zeiller s'uní a altres artesans en la reivindicació dels seus drets professionals, i una disputa personal que tingué amb el catedràtic d'anatomia a Munic acabà transformant-se en una virulenta brega pública sobre l'estatus dels modeladors i el valor dels seus models. Si bé no estava graduat en medicina, Zeiller considerava que treballava millor i amb major cura sent autònom que sota la supervisió d'un metge. Al seu parer, els seus models proveirien la síntesi visual d'allò que la putrefacció i el desmembrament havia destruït, i salvaria els cadàvers del proletariat d'un final que molts ressentien i temien. En resposta, el professor atacà aquestes idees amb un discurs caracteritzat per un esnobisme extrem: com les efígies de cera, els models pertanyien al món de les fires i no tenien cap paper científic. Alhora, acusà Zeiller d'insubordinació i de foment del desordre públic. Malgrat fracassar la revolució, Zeiller gaudí de suport suficient per part del col·lectiu mèdic per mantenir el seu lloc de treball durant el següent decenni, fins que el deixà voluntàriament per desenvolupar la seva visió pròpia, i la de la seva dona i també modeladora, Franziska, en un museu privat d'anatomia i antropologia. Els visitants més freqüents del museu foren públic profà i estudiants de l'escola d'art més important a Alemanya, però també hi anaren alguns estudiants de medicina. Els anatomistes van continuar donant suport als modeladors, atesa la utilitat del seu treball, però sempre rebutjaren la idea que els models pogueren substituir la dissecció i veieren de desacreditar aquells que no havien estat produïts sota el seu control. Només resten avui uns pocs models executats per Zeiller —irònicament un d'aquests s'exhibeix a l'Institut Anatòmic de Munic— i per molt de temps semblà que el seu treball estava destinat al fracàs. Avui, quan els models es troben entre les eines que han reemplaçat el cadàver en algunes escoles de medicina, les lluites, llargament oblidades, tornen a mostrar-se rellevants.

Mentre Zeiller maldava per introduir els seus models en l'ensenyament mèdic, el seu coetani Adolf Ziegler féu que els models de cera tingueren un paper important en la recerca acadèmica. El seu primer gran projecte fou també una col·laboració amb un catedràtic al voltant d'un atles embriològic. Treballaren plegats, en la dècada de 1850, a Friburg, i l'anatomista considerà l'experiència com un aprenentatge rellevant i encoratjà altres col·legues d'arreu del país a comprar els models de Ziegler. Els professors s'adonaren aviat que els models magnificats d'objectes diminuts, complicats i sotmesos a ràpids canvis, eren indispensables per ajudar els estudiants a entendre una matèria especialment difícil. L'embriologia humana depenia de la disponibilitat de material procedent d'avortaments que les sèries de models de Ziegler sobre el desenvolupament permetien reinterpretar i representar de forma vívida.

Ziegler inaugurà un taller propi el 1868. A banda del seu treball amb embrions, el seu èxit es fonamentà en la cura amb què conreà les seves relacions amb els científics, així com la seva disposició per no reclamar l'autoria exclusiva dels seus models. El fet que aconseguís graduar-se com a metge reforçà el seu estatus, però fou una altra estratègia la que el consolidà com a professional: apropiant-se de tècniques habituals en el món de la impremta, Ziegler s'autodefiní com a editor «plàstic» o escultural. Els metges, que podem considerar com els seus «autors», li proporcionaven dibuixos i espècimens a partir dels quals ell produïa models i enviava els primers conjunts de models als autors tal com si foren «proves d'impremta». La «correcció» i aprovació d'aquestes proves li permetien anunciar el seu treball sota la rúbrica «segons el Prof. X», i així aconseguí establir un lligam entre models, llibres i articles de revistes. Les seves ceres van esdevenir «estàndards»; tant, que els llibres de text sovint van reproduir imatges no dels espècimens, sinó dels seus models.

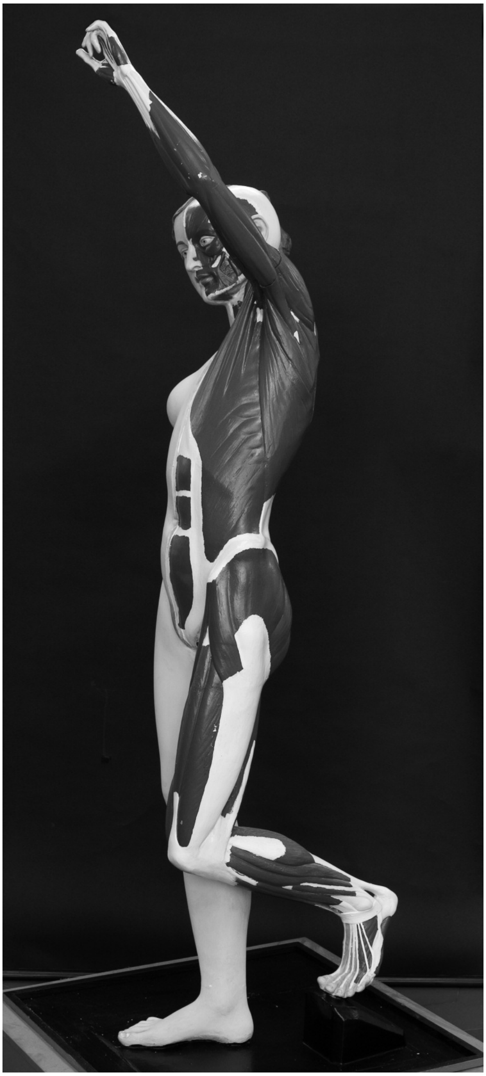

Figura 5. Estàtua femenina de cos sencer, en guix, en part anatòmica, signada per «P. Zeiller». Institut Anatòmic de Munic; fotografia del Prof. Dr. Rainer Breul.

L'estudi de Ziegler guanyà encara més importància amb la introducció a finals de la dècada de 1860 del seccionament rutinari de sèries en embriologia. Ajudà els embriòlegs de vertebrats a «donar cos» a embrions que només existien en fins talls. En la dècada de 1880, aquells embriòlegs van traslladar els seus dissenys magnificats d'estructures seccionades, a planxes de cera del gruix adequat, eliminant l'excés de cera i apilant-les —un antecedent de les tècniques de reconstrucció digital—. Aleshores el treball de l'anatomista consistia habitualment a fer un conjunt de models a partir d'espècimens seccionats i escriure un article descrivint-los i representant-los. Ell, o rarament ella, enviava llavors el manuscrit a un editor de llibres o de revistes i els models originals a Friedrich, el fill de Ziegler, que dirigí l'estudi des de la meitat de la dècada de 1880 fins a la dècada de 1930. Aquest no només reproduïa els models, sinó que abans els donava la seva forma final tot afegint-hi colors i etiquetes. Els models tingueren així un paper crucial en refinar la visió embriològica del curs de l'embaràs i, mitjançant la doctrina evolucionista de la recapitulació, en la història de la vida a la terra.

La fi del segle XIX fou una gran època per a la impremta, i període fundacional per a molts llibres de text, manuals i revistes mèdiques. Els anatomistes mantenien la idea que els mo-

Figura 6. Fotografia del taller de Friedrich Ziegler; ell apareix en primer pla. Procedent del seu llibre *Embryologische Wachsmodelle* (Freiburg, *c.* 1924). Medizinhistorisches Institut, Universität Bern.

dels devien ser tractats com a publicacions amb dret propi o, si més no, com a parts fonamentals de publicacions complexes. No es pot igualar al concepte actual d'«Open Access», però allí on l'embriologia era ensenyada, els articles o textos eren llegits amb els models al costat. Esdevingueren així tan importants com avui ho són el conjunt de dades lligades a una publicació en línia, si bé la barreja de mitjans i la pròpia fisicalitat dels models resta lluny de la capacitat d'accés complet que hom pot tenir avui des del seu ordinador.

Malgrat la continuïtat de mètodes i qüestions, els suports visuals han canviat molt al llarg del temps, i les seves estratègies polítiques també. Tanmateix, tot i que les imatges d'embrions es troben arreu en el ciberespai i que milions de persones fan cua per veure cadàvers plastinats, l'èxit o el fracàs roman lligat a les relacions de producció i d'ús. La interpretació històrica permet analitzar les polítiques dels models del segle XIX i d'aquesta manera promoure'ls com a objectes vàlids per alimentar la reflexió sobre problemes actuals.

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IDENTITAT ABANS DE NÀIXER? IMATGES DEL NONAT EN L'ÈPOCA MODERNA

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Les il·lustracions anatòmiques dels segles moderns (ss.XVI-XVIII), no foren simplement imatges pràctiques, sinó que sovint estimularen als seus públics a reflexionar sobre llur pròpia identitat. En aquestes il·lustracions, l'home i la dona eren sovint representats amb símbols de la Caiguda de l'Home, com ara la fruita prohibida o les fulles amagant els genitals. Segons la Bíblia, Adam i Eva només esdevingueren conscients de la seva identitat sexual en cometre el pecat original:

> «Llavors a tots dos se'ls obriren els ulls i es van adonar que anaven nus. Van cosir fulles de figuera i se'n feren faldars» (Gènesi 3, 7)

Aquest relat fou recollit en una sèrie de quatre gravats publicats per Adriaan van der Spiegel (1578-1625), catedràtic d'anatomia a Pàdua. Els gravats representen figures de dones embarassades nues, amb l'abdomen obert i les diferents capes de l'úter descobertes de manera successiva fins l'aparició del fetus a la darrera planxa (Figura 7). En aquesta sèrie, els atributs de la Caiguda de l'Home només apareixen en aquest últim gravat, tot palesant que la identitat del nonat era incerta fins que arribava al món.

En el pensament cristià, el pecat original comés per Adam i Eva esdevingué una condició prèvia per a tenir una consciència pròpia i féu l'home mortal, tot creant així els fonaments de la condició humana. El

Figura 7. Dona amb l'abdomen obert que mostra el fetus dins de l'úter. Adriaan van der Spiegel, De formato foetu (Amsterdam, 1645, orig. 1626), gravat. Wellcome Library, Londres.

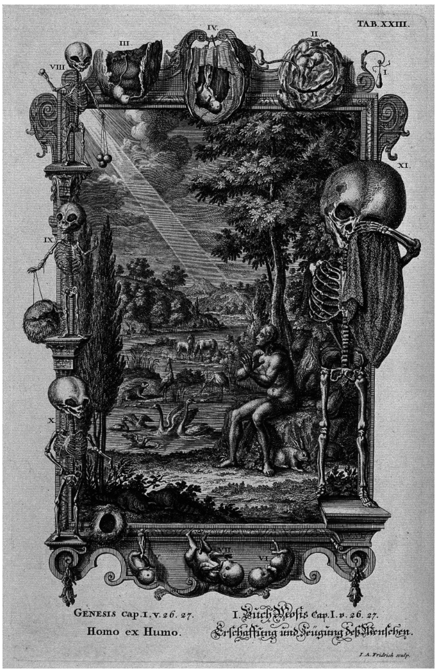

Figura 8. Adam al Paradís. Johann Jacob Scheuchzer, Physica Sacra (Augsburg, 1731-1735), gravat. Wellcome Library, Londres.

naixement, és a dir, el moment en què el nounat quedava subjecte al pecat original de l'home, esdevingué un fet únic en la creació de certesa sobre la identitat. Durant els segles moderns, la manca de certesa sobre la identitat del nonat va ser també objecte de debat sobre la durada de l'embaràs i la qüestió sobre quan l'ésser humà adquiria l'ànima.

El metge i filòsof natural suís, Johann Jakob Scheuchzer (1672-1733), manifestà una altra opinió en la seva història natural de la Bíblia, la Physica sacra (1731-1735). El gravat on apareix Adam al paradís estava emmarcat per òvuls humans fecundats i per esquelets fetals en diferents etapes de creixement (Figura 8). En aquesta proposta, Scheuchzer suggeria que tots els éssers humans foren creats originàriament tal i com es diu al Gènesi i encapsulats dins dels seus pares com si es tractés d'una nina russa. Aquesta idea de preformacionisme fou molt popular al darrer terç del segle XVII i la primera meitat del segle XVII. Segons aquesta doctrina, els conceptes de la mortalitat i la pecaminositat humanes formaven part de la creació divina des dels orígens. Aquesta visió s'acordava amb la doctrina protestant de la predestinació, segons la qual el destí de l'home està predeterminat per la voluntat divina. En emmarcar l'escena d'Adam abans de la Caiguda amb els petits esquelets com a memento mori o recordatori de la mortalitat, Scheuchzer deixà clar que el destí humà estava ja determinat en el moment de la creació del món.

Abans de la Il·lustració, els problemes sobre la generació foren interpretats amb freqüència amb relació al llibre bíblic del Gènesi. En el cas de les il·lustracions publicades per Spiegel, hom es referia de manera general a com la Caiguda de l'Home havia definit la naturalesa humana. En contrast, les representacions publicades per Scheuchzer constituïen el resultat de la combinació d'una posició teològica (predestinació) amb una teoria específica de la generació (preformació). D'aquesta manera, la història natural del nonat permetia donar suport a l'argumentari religiós i vice versa.

Lectures recomanades

Per a més informació sobre imatges històriques del nonat i lectures relacionades, vegeu: BUKLIJAS, T. and HOPWOOD, N. (2008), *Making Visible Embryos*, Cambridge, University of Cambridge. Exposició en linia, http://www.hps.cam.ac.uk/visibleembryos/ (últim accés 08/10/2011).

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L'AU DEL PARADÍS EN LA CULTURA VISUAL MODERNA

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Des de l'arribada dels primers exemplars dissecats a Europa, al voltant de la segona dècada del segle XVI, l'ocell del paradís esdevingué un dels objectes més preuats pels naturalistes i col·leccionistes de curiositats. Al costat de l'extraordinària bellesa del seu plomatge i dels seus exòtics orígens —sobretot de Nova Guinea—, el motiu de tanta expectació es trobava en la pretesa naturalesa àpoda d'aquelles criatures. Un tret carregat de simbolisme que desafiava obertament la teoria aristotèlica segons la qual totes les aus estaven dotades de potes. D'aquesta manera, segons la creença transmesa per viatgers i comerciants europeus, i confirmada pel tipus d'exemplars amb els quals traficaven —ocells morts, mai vius, als quals se'ls havia amputat les potes—, l'au del paradís fou presentada com una au «divina» (*manucodiata*, «ocell diví»), que romania en vol constant, sense posar-se mai enlloc, aliena als afers mundans.

La notícia d'aquest prodigi natural no només va generar un encès debat entre els naturalistes, sinó que també despertà un gran interès des d'una perspectiva visual. Entre les formes de visualització dedicades a l'ocell van destacar els gravats inclosos en els tractats d'història natural, les il·lustracions d'exemplars conservats en col·leccions, els llibres d'emblemes i heràldica —on la *manucodiata* era interpretada com a símbol de diligència o d'austeritat— i, sobretot, a començaments del segle XVII, la pintura. La representació pictòrica de l'ocell trobà un espai en un ampli ventall d'obres, des d'aquelles basades en

Figura 9. Au del paradís a: NIEREMBERG, Juan Eusebio (1635), *Historia naturae*. Antuerpiae, Ex officina Plantiniana Balthasaris Moreti, p. 210.

escenes bíbliques, com ara l'entrada dels animals dins de l'arca de Noè, fins a les composicions al·legòriques dedicades als quatre elements —amb Jan Brueghel el Vell, decidit a plasmar en els seus quadres el coneixement naturalista més actualitzat, com el seu més gran impulsor.

La imatge que mostrem aquí procedeix del llibre *Historia naturae*, del jesuïta madrileny Juan Eusebio Nieremberg. Publicada a Anvers el 1635, aquesta obra és el resultat d'un projecte teologiconaturalista d'interpretació de la natura —exòtica i pelegrina, en especial— que cercava articular un lligam entre coneixement natural i doctrina religiosa. Nieremberg, defensor de la naturalesa àpoda de la *manucodiata*, discuteix aquí el cas de manera exhaustiva com un exemple de la presència d'actituds virtuoses en el món animal.

La xilografia, la primera de les tres incloses en *Historia naturae*, és una versió semblant a l'oferta per Conrad Gesner al tercer volum de la seva obra *Historiae animalium* (1555). De fet, el gravat original de 1555, reproduït en nombroses obres posteriors entre les quals s'inclouen des de les del mateix Gesner fins a les d'autors com Belon, Boaistuau, Paré o Aldrovandi, va constituir durant molt de temps la referència visual més coneguda de l'au del paradís. La xilografia de *Historiae naturae*, possiblement obra del gravador i col·laborador de Rubens Christoffel Jegher, ens resulta útil per il·lustrar allò que William Ashworth ha caracteritzat com la presència «persistent» de certes representacions paradigmàtiques al llarg de la història natural moderna. La imatge, dissenyada i impresa vuit dècades després de la versió original publicada per Gesner, exemplifica el pes de la tradició, textual i visual, en el camp de la història natural. Una gravetat, el d'aquestes representacions «persistents», que contrasta amb la prestesa d'altres formes de visualització com la pintura, on molt aviat l'ocell del paradís començà a ser representat amb potes.

Lectures recomanades

GARCÍA ARRANZ, J.J. (1996), «Paradisea Avis: La imagen de la naturaleza exótica al servicio de la enseñanza didáctico-religiosa en la Edad Moderna», *Norba-Arte* XVI, 131-152.

MASON, P. (2009), *Before disenchantment. Images of exotic animals and plants in the early modern world*, London, Reaktion Books.

B. ASHWORTH Jr., W. (1985), «The persistent beast. Recurring images in early zoological illustration», a Ellenius, A. *The natural sciences and the art*, Stockholm, Almqvist, 46-66.

NECROLÒGIQUES

ACTES D'HISTÒRIA DE LA CIÈNCIA I DE LA TÈCNICA
NOVA ÈPOCA / VOLUM 4 / 2011, p. 149-150
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JOAN VERNET GINÉS

Joan Vernet, arabista, historiador de la ciència i de la literatura àrab i traductor de l'Alcorà, va morir el dia 23 de juliol de 2011 entrant el signe de Lleó en el qual, si no m'equivoco, havia nascut l'any 1923. Em sembla recordar-ho d'una assignatura de doctorat que impartia el curs 1983-84 a la Universitat de Barcelona. L'assignatura es deia «Astrologia Àrab» i consistia en la traducció i comentari del *Tafhīm* d'al-Bīrunī, els *Principis de l'Art de l'Astrologia*.

Entre els anys 1975 i 1980, els anys dels meus estudis de llicenciatura a la Facultat de Filologia, el professorat de l'Àrea d'Àrab estava format, a més d'ell, per Leonor Martínez (la seva esposa), Joaquín Vallvé, Teresa Losada, Manuel Grau, Maurice Kaplanian, María Victoria Arrabal i Francesc Castelló. El Dr. Vernet feia les classes a quart i a cinquè de carrera. La meva promoció érem dues alumnes: l'Íngrid Bejarano, actualment professora a la Universitat de Sevilla, i jo.

Era una persona sàvia. El seu posat i la seva erudició feien respecte, sobre tot quan obria aquells parèntesis encadenats, farcits d'informació i amanits amb un bon grapat de preguntes.

Ens explicava poesia clàssica i mètrica. Conservo «xerocòpia» dels seus quadres de metres, així com un recull de poemes curosament copiats en àrab de la seva pròpia ma: ^{c}Abbās b. Nāsih, al-Gazāl, al-Mutanabbī, al-Ramādī, entre d'altres autors. En aquella època, de tot Barcelona, només al CSIC hi havia una màquina d'escriure en àrab! Entre els poemes ens hi va intercalar dos textos extensos, un sobre el matemàtic Maslama de Madrid i l'altre sobre quadrats màgics, i un de més breu sobre l'origen de la trigonometria plana, que van ser el nostre primer contacte amb la història de la ciència àrab.

L'any 1978 va sortir publicada la seva obra *La cultura hispano-árabe en Oriente y Occidente*, reeditada el 1999 i el 2006 amb el títol *Lo que*

Europa debe al Islam de España. Traduïda a l'àrab amb el títol *Fadl al-Andalus ᶜala thaqafat al-garb*, ho ha estat també a l'alemany (1984), al francès (1985) i al polonès (2007). El curs 1979-1980, l'assignatura «Història de la Ciència Àrab» que impartia a cinquè va consistir en l'actualització i ampliació d'aquest llibre, amb un èmfasi especial en la onirologia, els calendaris, la partició d'herències, els instruments astronòmics i la fabricació del gel ... des de l'antiga Babilònia al Montseny!

No recordo bé si el mateix curs o el curs anterior llegíem l'Alcorà. Varem comentar les sures II, *La Vaca* (aleies 102 al 180); XII, *Josep*; XIII, *El Tro*; XIV, *Abraham*; XV, *Al-Hijr*; i 47 versicles de la XVI, *Les Abelles*.

Ens va iniciar en la metodologia de la recerca i en la catalogació de manuscrits, fent-nos identificar el contingut dels manuscrits microfilmats i fotografiats que hi havia al Departament.

Com a investigador els seus interessos eren molts i diversos i en molts aspectes avançats a la mitjana del seu temps. Deixeble de Millàs Vallicrosa va consolidar a la Universitat de Barcelona la tradició dels estudis històrics sobre la ciència àrab i la va deixar en herència al seu deixeble Julio Samsó. Tots dos van rebre l'any 1995 la medalla «Alexandre Koyré» juntament amb la resta d'integrants de l'Escola de Barcelona d'historiadors de la ciència àrab. Uns anys abans, el 1991, havia rebut la medalla «Sarton» i uns anys després, el 2004, va rebre el premi «Sharjah de Cultura Àrab», entre d'altres distincions com ara la «Creu de Sant Jordi» de la Generalitat de Catalunya, el 2002.

És autor d'una extensa producció bibliogràfica tant en història de la ciència com en pensament islàmic i en literatura àrab. El febrer de 1991, la revista *Anthropos* (no.117) li va dedicar un número monogràfic on s'hi pot trobar, a més de moltes dades biogràfiques, una relació detallada de la seva bibliografia. Una bona part d'articles difícils de localitzar han estat reimpresos en els volums d'homenatge que els seus col·legues i alumnes li van dedicar els anys 1979 i 1989: *Estudios sobre Historia de la Ciencia Medieval* i *De ᶜAbd al-Rahman I a Isabel II*, respectivament.

D'horaris matiners i dotat d'un bon sentit de l'humor, sempre recordarem el mestre i la persona. Rebi un homenatge afectuós i ben sentit.

Roser Puig

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ROGER HAHN
(PARÍS, 1932 - NOVA YORK, 2011)

Roger Hahn, professor emèrit d'història de la ciència a la Universitat de Califòrnia (UC) a Berkeley, pioner en l'establiment de l'àrea acadèmica d'història de la ciència, va morir inesperadament el 30 de maig de 2011 a la ciutat de Nova York.

La història de la ciència és ara una àrea d'estudi reconeguda, però en 1953, quan Hahn estava entre els primers estudiants que es graduaven a la Universitat de Harvard en les dues especialitats de Ciències (Física) i Història, era una disciplina emergent.

A través dels seus estudis a l'École Pratique d'Hautes Études, com a becari Fulbright, i després a la Universitat de Cornell, on es va doctorar, Hahn va desenvolupar un gran interès per la relació entre ciència i societat que va marcar la seva carrera i va ajudar a formar l'àrea d'estudi.

Allunyant-se de l'enfocament establert d'ensenyar el desenvolupament científic com una sèrie cronològica aïllada de descobertes, Hahn va perseguir una visió integral del desenvolupament de les idees i institucions científiques com un reflex de les dimensions sociopolítiques, filosòfiques i humanes del seu temps.

«Ha deixat la seva empremta en l'àrea en una línia que esdevé modèlica», diu Cathy Carson, professora associada d'història de la ciència a la UC Berkeley. «Des del principi, es va ocupar de les qüestions de ciència i societat que es van posar de moda des d'aleshores, i va tractar aquest tema amb molta cura, amb profunditat acadèmica i rigor intel·lectual.»

Un dels seus primers treballs més destacat i influent, *The Anatomy of a Scientific Institution. The Paris Academy of Sciences 1666-1803* (Berkeley, University of California Press, 1971), va proporcionar una anàlisi completa de l'elit de l'Acadèmia de Ciències de París des de la seva fundació sota el ministre de finances Jean Baptiste Colbert fins a la seva dissolució com a institució reial durant la Revolució Francesa, i la posterior restauració durant l'Etapa Napoleònica. A partir de la seva visió de la ciència i de la institució científica en aquell període històric, Hahn va descriure l'Acadèmia com «l'enclusa on els valors sovint conflictius de la ciència són forjats en una forma visible».

Hahn va néixer a París, França, el 5 de gener de 1932. La seva família va fugir de França a Nova York el 1941 per tal d'escapar de l'opressió nazi. Després de graduar-se *magna cum laude* a la Universitat de Harvard el 1953 i obtenir un MAT (Master of Arts in Teaching) en educació per la Universitat de Harvard l'any següent, Hahn va servir després a l'exèrcit americà a l'Estat Major Suprem de les Forces Aliades d'Europa (SHAPE), als voltants de París.

En 1961, Hahn va acceptar una plaça al Departament d'Història de la UC Berkeley. Erudit, professor i col·lega de gran vàlua, Hahn va fer recerca, va fer publicacions, va impartir classes i va participar a bastament tant en la vida acadèmica com en la universitària per un període de 50 anys.

«El traspàs de Hahn és una pèrdua irreparable per a la comunitat universitària», remarcava Erich Gruen, professor emèrit d'història i col·lega durant molt de temps de Hahn. «Ell era un ajut imprescindible i tenia una gran consideració cap als col·legues joves, era un home discret que mai no demanava res, sinó que sempre generava respecte. A les reunions de departament tenia una veu independent, responsable, ben informada amb judici equilibrat mai dirigida per ideologies ni opinions inflexibles.»

Els seus interessos acadèmics el portaven habitualment cap a Europa, on el domini de cinc llengües li facilitava la recerca a través del continent. Va mantenir forts lligams amb investigadors, biblioteques i institucions acadèmiques i va aprofundir en les seves relacions anteriors a la guerra amb familiars, supervivents i amics. Hahn va ser condecorat pel govern francès amb l'alta distinció acadèmica d'Oficial de França per la seva promoció cultural i intercanvi acadèmic entre França i els Estats Units i pels seus estudis clàssics sobre l'Acadèmia de Ciències francesa.

Hahn és igualment conegut pels seus treballs sobre el científic i polític francès de finals del segle XVIII i principis del segle XIX Pierre Simon Laplace. Com a estudiant de postgrau, Hahn va fer veure la manca d'una biografia definitiva de Laplace, un dels grans científics de la Il·lustració considerat com el Sir Isaac Newton de França. El patrimoni ancestral de Laplace s'havia cremat a principis del segle XX i es va destruir la major part dels documents personals i la biblioteca. Hahn, a pesar de les dificultats, es va embarcar en una llarga recerca per descobrir la correspondència de Laplace amb altres científics i col·legues a través d'Europa. Així va reconstruir l'evolució del pensament i les descobertes de Laplace. Treballant com un detectiu Hahn va seguir les pistes que el van portar cap a racons polsosos de les biblioteques, arxius i innombrables col·leccions personals. En una ocasió va descobrir documents importants en una capsa de cartó que havia estat «arxivada» en el lavabo d'homes d'una biblioteca parisenca.

La publicació de Hahn de la tan esperada biografia de Laplace, *Pierre Simon Laplace 1749-1827: A Determined Scientist* (Editions Gallimard, 2004 – French version; Harvard University Press, 2005 – English), va ser a bastament reconeguda. Això no va significar, però, el final de la seva interpretació detallada i investigació. A la seva mort, el treball de Hahn sobre la correspondència de Laplace estava a punt de ser publicat (Brepols, Bèlgica).

En la dècada de 1970, quan l'exdirector de la biblioteca Bancroft, James D. Hart, va proposar la creació d'una col·lecció d'història de la ciència, Hahn es va convertir en el seu conservador i en l'assistent especial. Va perseguir els llibres rars, els manuscrits i els documents personals dels científics notables i va obtenir destacades aportacions a la col·lecció. El més important de l'actuació de Hahn va ser haver-se responsabilitzat d'organitzar l'adquisició d'articles de Laplace encara existents.

D'acord amb el subdirector actual de la biblioteca Bancroft, Peter Hanff, «Hahn tenia un gran interès per l'adquisició de llibres i manuscrits d'història de la ciència en antiquaris. Llegia els catàlegs i contínuament en feia referències, fins i tot després de la seva jubilació». Hahn va col·laborar amb l'Associació d'Amics i amb la Junta Assessora de l'Oficina Regional d'Història Oral de la biblioteca Bancroft, i va reunir i preservar històries orals d'importants científiques de l'àrea de la badia de San Francisco.

Hahn estava profundament compromès amb l'ensenyament, així com amb l'assessorament de nombrosos treballs postdoctorals, de llicenciatura i d'estudiants universitaris. Els seus cursos van abastar un ampli ventall d'història de la ciència, des d'Aristòtil fins a la bomba atòmica. Entre d'altres coses, les interessants conferències i seminaris de Hahn sobre Newton, Galileu i Laplace han deixat una empremta duradora. Un dels seus projectes més innovadors va ser la creació d'un curs interdisciplinari sobre els enginyers del Renaixement, impartit per quatre professors de la Universitat de Berkeley: un historiador, un arquitecte i dos enginyers.

Fou sempre un participant actiu en la vida universitària i acadèmica. Hahn era membre fundador i va exercir com a director de l'Oficina d'Història de la Ciència i la Tecnologia a Berkeley des de 1993 a 1998. També va exercir com a codirector del programa d'estudis francesos de 1987 a 1990, i de president del Comitè de Selecció per al Fons de França-Berkeley. A França, va impartir classes al Collège de France, a la Sorbona i a l'École des Hautes Études en Sciences Socials. Al llarg de la seva carrera Hahn va participar en nombrosos comitès del Departament d'Història, de la Facultat de Lletres i Ciències, del Senat Acadèmic, del complex de la Universitat de Califòrnia, i de l'oficina del Rector.

«Roger era el company perfecte. Hem treballat i ensenyat en estreta col·laboració durant trenta anys sense discussions», va dir John Heilbron, professor emèrit d'història i vell amic i col·lega de Hahn. «Ell era també un molt bon acadèmic; que s'interessava pels temes, de tracte amable, molt culte i interessat pels reptes. Era el tipus de persona, ara cada vegada menys habitual, que ajuda a fer la universitat més gran que la suma de les parts.»

Entre els molts honors i nomenaments, Hahn va ser dues vegades investigador de la National Science Foundation i membre del consell de la History of Science Society. Va ser elegit investigador de l'American Association for the Advancement of Science i membre de l'Académie Internationale d'Histoire des Ciències, on va participar com a vicepresident el 2005. Hahn era també membre de l'American Council of Learned Societies i va ser president de l'American Society for Eighteenth-Century Studies i de la West Coast History of Science Society. Va ser un participant actiu i membre de l'Advisory Council for Humanities West.

«Per sobre de tot, Roger era un veritable erudit, un excel·lent professor, un ésser humà meravellós, de tracte càlid i amb un fi sentit de l'humor, i un amic molt estimat», va dir James Casey, professor d'Enginyeria a UC Berkeley que estava col·laborant amb Hahn en una publicació relativa a la teoria de la plasticitat dels metalls. «El seu profund estudi de la història li havia ensenyat a ser filosòfic sobre la vida, la gent i la política. Tenia una visió realista i equilibrada de la humanitat.»

Ellen L. Hahn.
(Traducció: Francesc X. Barca Salom)

NORMES D'EDICIÓ

ACTES D'HISTÒRIA DE LA CIÈNCIA I DE LA TÈCNICA

Actes d'Història de la Ciència i de la Tècnica és la revista de la Societat Catalana d'Història de la Ciència i de la Tècnica. Està dedicada a la història de la ciència, la medicina i la tecnologia des de l'antiguitat fins al present, i publica articles, notes de recerca, i revisions bibliogràfiques en qualsevol de les llengües de la Unió Europea. La seva periodicitat és d'un volum l'any.

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Els articles inclouran també un resum d'unes 10 línies i les paraules clau, ambdós en anglès.

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- Un autor: (Collins, 1992: 129-130)
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Llibres:

COLLINS, H. M. (1992), *Changing order: replication and* induction *in scientific practice,* Chicago, The University of Chicago Press.

Capítols de llibres, actes de congressos o llibres miscel·lanis:

PRINCIPE, L. (2000), «Apparatus and reproducibility in alchemy». In: HOLMES, Frederic L.; LEVERE, Trevor H. (ed.). *Instruments and experimentation in the history of chemistry*, Cambridge, Mas., London: The MIT Press, 55-74.

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4. Quotations have to be inserted in the text according to the following models:

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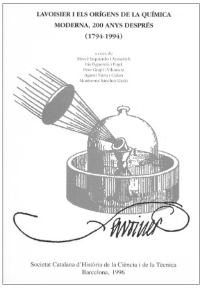

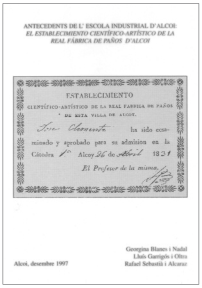

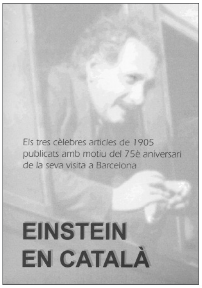

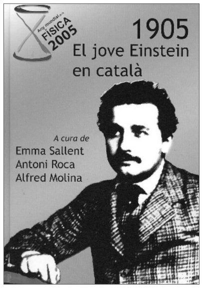

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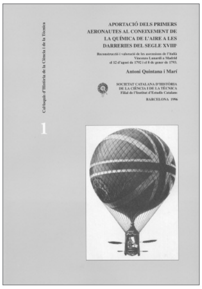

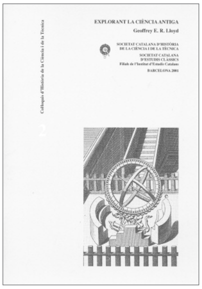

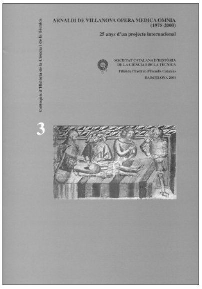

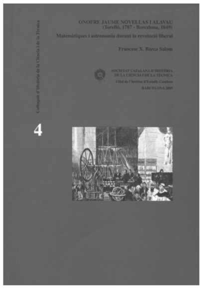

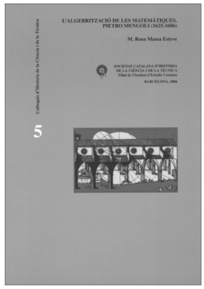

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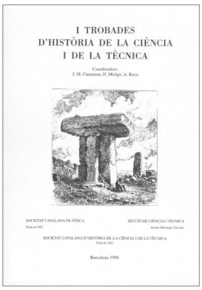
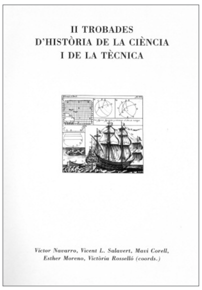
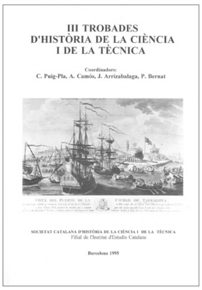
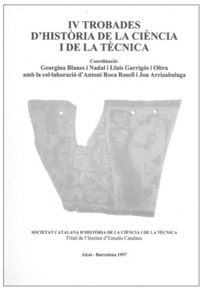
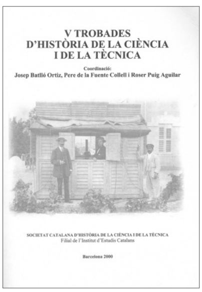
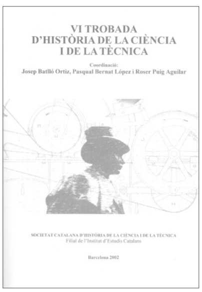


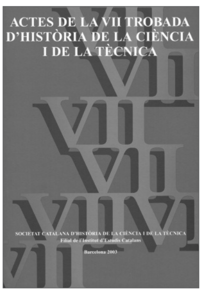


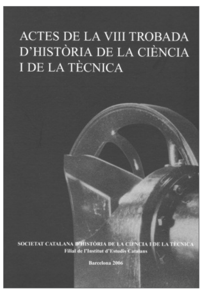




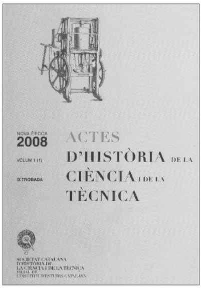


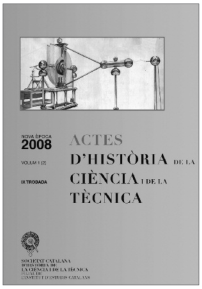


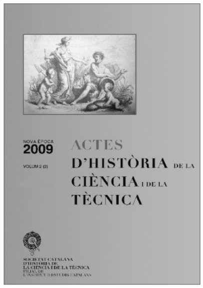


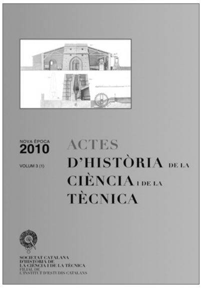


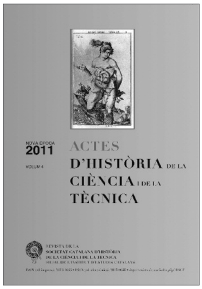
Actes d’Història de la Ciència i de la Tècnica (nova època). Des del 2008



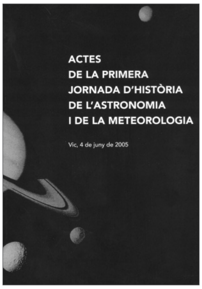




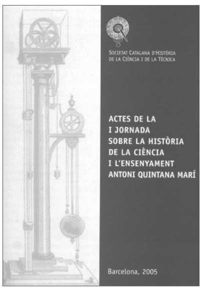


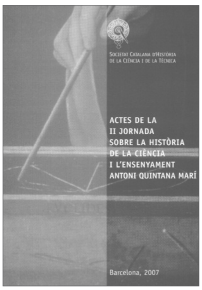


Jornada d’Astronomia i Meteorologia



Història de la Ciència i l’Ensenyament





ACTES
D'HISTÒRIA DE LA CIÈNCIA I DE LA TÈCNICA