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A Commerce of Letters: Astronomical Communication in the 18th Century

1. Introduction

The 18th century has often been regarded as a less exciting period in the history of science, a phase of Newtonian consolidation perhaps, before the next great stride forward in the following century. Astronomy between Newton and Herschel has been characterized as "patient and devoted routine work", albeit "a routine that continually renewed itself and struggled to attain greater accuracy through perpetual improvement of instruments and methods" (Pannekoek, 1961: 280). The most detailed historical treatment of the subject has been given by Jean-Baptiste Joseph Delambre, himself a major character in the cast of 18th-century astronomy. His *Astronomie au dix-huitième siècle* seems to strengthen the view that 18th-century astronomy was rather lacking in originality. It does *not* treat Laplace and Herschel, and as for Newton, he is dealt with in a brief section of 22 pages.[1] Two astronomers are however given much more space than their colleagues, which is telling of Delambre's views but also of the character of 18th-century astronomy. Nicolas-Louise de Lacaille and Joseph-Jérôme Lefrançais de Lalande are allotted 86 and 74 pages respectively. Lacaille held immense prestige for being the most reliable, honest, and hard-working among practical astronomers; he did great services to his compatriots and to his colleagues by geodetic surveys for the Cassini map and by producing reliable catalogues of the sun and stars. Lalande's name is however not associated with even minor scientific breakthroughs; neither was he a paragon of scientific virtue — Delambre assures us that love of fame was his main driving force (Delambre, 1827: 547; Gillispie, 1980: 104—5). Lalande himself made the credible claim that his best pupil, Delambre, also was his greatest scientific achievement (Cohen, 1971: 14). Delambre justified the extensive treatment of his mentor in the following manner:

> Lalande has by no means renewed the foundations of astronomy, like Copernicus and Kepler; he has by no means, like Bradley, been immortalized by two bril-

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> liant discoveries; he has by no means, to the same degree as Lacaille, been an exact, clever, industrious, scrupulous and indefatigable observer and calculator; he has by no means, like Wargentin, had the persistency to attach himself to a unique goal, in order to set himself matchlessly apart; but if he, in all of these respects, is nothing but an astronomer of the second order, he has been the first among teachers: more than anybody else, he has known how to spread the teaching of and the taste for science. He wanted to be useful and famous, and he knew how to succeed through his work, through his activities, through his influence and his instigating; finally, through the most widespread correspondence; he ceaselessly strived to do Astronomy good (Delambre, 1827: 566—67)

In short, Lalande was not a great scientist but he was a great promoter of astronomy, a *"Gros Papa aller Astronomen"*, as one of his colleagues put it (Zach, 1984: 53). Delambre's high regard of Lalande and Lacaille should indeed be taken seriously. Astronomy in the 18th century may largely have been a routine activity; but then again, the establishment of routines may have been its greatest achievement. Characters like Lalande, Lacaille, Zach, Wargentin, Delisle, and, not least, Delambre himself helped change the face of science by organizing systematic and, what is more, nationally and internationally coordinated precision measurement as a routine business. In doing so they laid the foundation of an infrastructure of science which bore fruit already in the 18th century and which gained wider currency within astronomy and other sciences during the decades after the Napoleonic Wars. In order to grasp developments like this, other scientific activities than making discoveries, and other scientific characters than great conceptual innovators, must be studied.[2]

In this paper the communication among 18th-century astronomers will be considered, and its importance for the development of a system of practical astronomy — a technically oriented international profession — will be stressed.

A Republic of Letters

Historians commonly use the correspondence of scientists as a source of information about who did what when, about the intellectual development of individuals, about the work-a-day practices of scientists. Granting its importance as a source for historical understanding, however, surprisingly little has been done by way of analyzing the written letter as a medium of communication in its own right. Journals have received much more attention and are today subject to specialized treatment by scientometricians, while letters are often treated as something given, as sources to be mined for facts (Edge, 1979; Kragh, 1987: ch. 17; Meadows, 1974: ch. 3). Scientific correspondence should, like scientific publishing, be studied as a subject in its own right. It is an important means for disseminating knowledge, for initiating cooperation, and for evaluating science (Woolf, 1959; Daston, 1991; Baumgarten, 1988; Hall, 1965). Further, in the early modern period, correspondence networks helped constitute a semi-professional association of scientists: a Republic of Letters. Some exponents of the art of scientific or learned letter writing, like Mersenne or Oldenburg, have been studied and the importance of their organizational activities has been confirmed. Here the specialized correspondence of a more technical nature will be considered, but with emphasis on its organizational character.

Scientific correspondences are not of one kind. There is a variety of different genres of letters, ranging from the very personal to the more or less standardized official ones. Historically, scientific correspondence has been used as a multiple purpose tool with many functions besides the communication of hard facts. Among the several functions of 18th-century astronomical correspondence two will be singled out as especially important for this discussion.

First, correspondence had, in a broad sense, a *collaborative* function. Through the mail, scientists planned and carried out joint projects; they exchanged information and services. It should be stressed that astronomers had a particular need for collaboration over long distances. In the 18th-century, astronomers paid much attention to the problem of measuring positions and distances; especially longitude determination was problematic (Waters, 1983). But, in the absence of reliable catalogues of stars, the moon, or the Jovian Satellites, such measurements depended on simultaneous observations of celestial phenomena, like eclipses or occultations, by observers at different locations. Simultaneous observations were also needed in order to measure the parallax of planets or of the moon — another major interest of 18th-century astronomers (Williams, 1983). Observations of this kind could be agreed upon beforehand (in letters), but, if they were subject to routine exchange, as they often were, they could also be searched out in the flow of information which passed through a correspondence network. Through a regular correspondence — a *commerce de lettres* — the 18th-century astronomer was able to extend his observatory in space: two observatories, geographically separated from one another, but connected by the exchange of letters, made possible stereoscopic vision of celestial phenomena, and this was necessary for the exact determination of positions and distances.

Second, correspondence had a *mediating* function. The astronomers argued and negotiated over the status of scientific results or instruments, they canvassed for scientific projects, and they checked their mutual standing in the scientific community. This activity, which of course went on at other fora as well, guaranteed a measure of standardization of procedures and values which facilitated technical cooperation and promoted an esprit de corps.

In the following, examples will be drawn from the letters and the activities of most of the leading Swedish astronomers in the period *c.* 1730—1810: Anders Celsius, Olof Hiorter, Pehr Wilhelm Wargentin, Daniel Melanderhielm, and Jöns Svanberg. Under the aegis of these men Swedish practical astronomy was first established, then reached a high level of excellence, and finally plunged into obscurity.

2. Beginnings

The Merits of Correspondence

Practical astronomy hardly existed in Sweden before Anders Celsius, professor of mathematics/astronomy, managed to obtain support from Uppsala University for building and equipping an observatory around 1740. Celsius belonged to the first generation of Swedish Newtonians; he had collaborated with Pierre-Louis Maupertuis in the famous geodetic expedition to northern Sweden/Finland in 1736/37 (aimed to furnish support for the theory of gravitation by proving the "flatness" of the earth toward the poles), and, during five years of travel in the 1730s, he had spent much time in Italy and visited Germany as well as England (Nordenmark, 1935). When he finally settled in Uppsala he was qualified to introduce modern practical astronomy into Sweden. In this he was fairly successful, even though his own contribution to science was modest.

Celsius' foreign correspondence reflects the needs of a pioneering observatory director to gain information about practical matters, like calibrating and determining the exact position of his instruments. Correspondence was an integral part of these activities, which is illustrated by the fact that the letters often dwell on the subject of letter writing. In the early 1730s Celsius engaged in a commerce of letters with Jean-Nicholas Delisle, the well-known French astronomer who taught at the Collège Royal and was director of the observatory at Saint Petersburg 1725—47 — where, according to Delambre, he "did little or nothing to advance science or his reputation" (Delambre, 1827: 320; Chapin, 1971). Delisle was nevertheless an important figure: he introduced and taught modern astronomy in Russia; he was successful also as a teacher, with e.g. Lalande and Messier as pupils; he was a prolific (and very demanding) letter writer and a keen organizer of collaborative work. As an expert in the art he frequently deliberated on the merits of scientific letter-writing:

> Often it is not convenient to publish, all at once, everything that one has accomplished and that which one proposes to do. Everything should not be divulged, and some people may put it to bad use. There is much more liberty in an individual correspondence, when the persons who entertain it sympathize in temperament and share a taste for the same kind of investigations. The mail is regular enough, and it does not cost much from here to Sweden; but as for books and other things that we both want to obtain, it is necessary to take expenses into consideration. (Delisle to Celsius, 8 Nov. 1737.)

Delisle also stressed the importance of astronomers forming correspondence networks: "Which astronomers do you entertain a correspondence with? Are they very communicative with regard to you?" He himself had useful contacts with a number of astronomers in other countries, including Jesuits in China and Fellows of the Royal Society. He was willing (1 Aug. 1738) to exchange lists of correspondents with Celsius if he believed that it would aid their mutual "commerce".

Pierre Charles Le Monnier (co-member of Maupertuis' expedition) also saw the mail service as an instrument for scientific research. He even suggested studying it empirically in order to facilitate its scientific use. In 1737 Celsius requested corresponding observations on the eclipse of some mountains on the moon — the timing of these he hoped would be a useful method for determining longitudes. Delivery of mail between the Stockholm/Uppsala region and continental Europe was as speedy (or slow) in the early 18th century as today — seven to ten days (Lindqvist, 1984: 341) — but this letter by Celsius reached Paris too late, so Le Monnier thought (27/16 Dec. 1737) that the efficiency of the postal service ought to be checked in order to avoid similar accidents in the future.

Celsius' main interest was to receive corresponding observations — especially on the Jovian satellites — that would help him to determine the longitude of Uppsala, necessary for any kind of serious astronomical work at the new observatory. Delisle emphasized this point. He wrote (1 Aug. 1738) that Celsius must determine the position of his new observatory with the utmost accuracy, and that he should also try to find the exact coordinates for other places in Sweden where astronomical work had been carried out in the past. These coordinates Celsius should then determine more accurately, "through new observ[ations] that you make in concert with the other astronomers in Europe". Le Monnier, on his part, wanted corresponding observations that would be of use for his research on refraction, a perennial problem for all precision measurement of celestial phenomena. These astronomers thus had practical considerations in mind when conducting their commerce of letters — it served instrumental ends. There was no better way of carrying out such investigations, Le Monnier wrote (24 March 1738), than through a "good correspondence".

Collaboration and Mediation

Matters of collaboration are frequently the subject of discussion in Delisle's and Celsius' long letters to one another. Delisle was the more demanding party, however. He urged Celsius to send copies of books and maps; he wanted information about all astronomical observations ever carried out in Sweden; he asked for details about Maupertuis' geodetic measurement, and he expected Celsius to carry out observations that were needed for his own geodetic work; he had many questions and gave plenty of advice concerning the purchase and use of instruments.

The occupation with geodesy and the longitude problem are typical for the concern with exact determinations of position and distance. The ability to make these with some precision was central to astronomy's claim to be an exact science; it was the foundation for the comparability of all sorts of observations between observatories, and it was likewise fundamental to its practical application. Celsius and his correspondents exchanged data from their own observations, and also from those of colleagues within their correspondence networks. Hence, correspondence made it possible for the individual astronomer to include the scientific equipment of several colleagues with his own. In as far as the work at different observatories could be coordinated, their respective telescopes in effect functioned as one large instrument, extended in geographical space through the communication network of correspondence.

Sometimes observations were transmitted through the correspondence network without restrictions, but sometimes they were shared on condition that they be kept secret, presumably because they were to be included in forthcoming publications (Delisle to Celsius, 12 Sept. 1738; Le Monnier to Celsius, 2 June 1738). The exchange of information was not open and free on principle, only when it suited the purposes of astronomers, which it quite often did. But information could become a commodity in negotiations concerning professional status or the value of scientific research programs.

This is shown in a quarrel between Delisle and Le Monnier concerning the publication of astronomical tables at Paris, in which Celsius became involved and where the mediating character of correspondence is illustrated. Le Monnier was to publish the French academy's semi-official *Histoire celeste* (1741), and the academy demanded that Delisle send him copies of observations carried out in Paris in the days before he was employed in Czar Peter's academy. Delisle stingily refused. The reason, he explained to Celsius, was that the *Histoire* would not be any good unless it was edited by Jacques Cassini. In this quarrel, Delisle optimistically tried to recruit Celsius as an ally. He asked Celsius (17 July 1739) to tell him every piece of news that he could pick up from "those with whom you correspond in Paris", especially things concerning Le Monnier's *Histoire*, "if you continue to receive his letters".

Le Monnier likewise pressured Celsius for support. He had the advantage of residing at Paris, rather than scientifically peripheral Saint Petersburg, but the still powerful Cassini — whom he marked out as the "Tyrant of astronomy" and the main obstacle against the acceptance of Newtonian cosmology in the Paris academy — made scientific life difficult for him (Le Monnier to Celsius, 7 July 1742). Le Monnier was therefore in need of political support, and he asked Celsius (8 Sept. 1738) to make Delisle more favourably disposed towards his editorship of the *Histoire* by writing and telling him how much he, Le Monnier, admired Delisle's work. Le Monnier then wanted Celsius to write back to him about Delisle' reaction. To Delisle, Celsius diplomatically wrote (5 Jan. 1742) that the *Histoire* was not as good as it would have been, had Delisle participated.

Without probing deeper into this disagreement we can note that attempts at mediation were going on and that these involved more than just the question of who should edit the *Histoire celeste*. Delisle presented himself as an ally of the Cassini camp at the Paris academy; he supported their geodesy in face of severe accusations of inadequacy from the young Newtonians, to which Le Monnier (but also Celsius) belonged.[3] The quarrel hence must be seen in the light of deep running differences concerning scientific style and theoretical commitments.

When Celsius passed away in 1744, Delisle turned (5 June 1744) to Olof Hiorter — Celsius' collaborator and brother-in-law — for a replacement. He claimed (12/1 March 1745) that Hiorter was obliged to fulfil different promises that the deceased had made vis-à-vis Delisle. Hiorter responded (4 July 1744) that he was willing to "take on the correspondence which you have deigned to offer me", and to supply Delisle with the kind of astronomical information he requested, as well as books and journals. Delisle stated (10/21 Aug. 1744) the terms of the future exchange of letters: the correspondents should inform one another of their scientific plans and of what they claimed as their own intellectual properties in view of eventual publishing. Hiorter was not a very assiduous correspondent, however, and in 1748, two years before his death, the commerce of letters stopped or rather, as we shall see, was transferred to Wargentin.

Through Celsius the technology of practical astronomy was transferred to Sweden. He established an observatory (though he positioned it so disadvantageously that it could never be used for serious research), he furnished it with British instruments, engaged on topical research, and educated able astronomers, most importantly Wargentin. In the decades after Celsius' death Swedish astronomy would become fully integrated with the continental system of astronomy — through Wargentin's work on the Jovian Satellites, and through the participation by him and his colleagues in cooperative projects. When Celsius came with Maupertuis to the Torne Valley, he travelled as a foreigner in his own land, a member of a *French* party, with no backing from the local scientific community. Wargentin would collaborate on equal footing with continental colleagues, supported by government and a flourishing scientific academy, and aided by several competent compatriots.

3. Routine collaboration

Because of his early death, Celsius never had the chance to accomplish what he had hoped for — to produce catalogues of the zodiac stars and the moons of Jupiter. The latter goal was to be attained by his pupil Pehr Wilhelm Wargentin, Permanent Secretary of the Royal Swedish Academy of Sciences 1749—83, and Director of the Academy's observatory (opened in 1753). By force of his office and his personality, Wargentin became the central organizer of Swedish science — and also the main international authority on the moons of Jupiter, his exclusive field of astronomical specialization (Nordenmark, 1939: 202—31).

Wargentin's single-minded devotion to the Jovian Satellites was nurtured by Celsius' and Delisle's commerce of letters. In 1742, Celsius had sent Wargentin's dissertation on the Satellites to Delisle (10 Dec. 1742), and in 1745 Wargentin wrote to Delisle asking for more data on these. The Frenchman replied (4/15 Jan. 1745) that he hoped that Wargentin would continue working on this very important subject; in order to encourage him he enclosed a table of all the observations of the moons that he had carried out over the past six years. During the following two decades Wargentin and Delisle exchanged around 50 letters in a correspondence which soon became officially sanctioned, as Wargentin was assigned to Delisle as foreign correspondent of the Académie des Sciences (1748). During these years, as Wargentin's international reputation grew, the scope of his correspondence increased; after mid century he had become one of the more important participants in the European network of astronomical correspondence. Wargentin's correspondence with colleagues like Delisle, Lacaille, Lalande etc. reflects the day-to-day life of 18th-century practical astronomy: it was a matter of coordinating observations of celestial phenomena, of discussing the reliability of observation techniques and of astronomers, of obtaining information about books, journals, and maps, and — naturally — of acquiring as much information as possible concerning topics where one had professional interests to protect, like that of the moons of Jupiter.

The Cape of Good Hope, the Jovian Satellites, and Venus

A pioneering effort at organized international collaboration occurred in connection with Lacaille's voyage to the Cape of Good Hope 1751—53, where observations to determine the parallaxes of Mars, Venus, and the moon were to be made. Delisle sent Wargentin messages about the expedition in late 1750, together with a printed announcement by Lacaille (18 Dec. 1750); some months later (19 Feb. 1751) he sent another circular together with a reprimand, complaining that Wargentin had not answered the first letter. Collaboration from Swedish astronomers was deemed urgent as Sweden was close to the meridian of the Cape, and the country's northern situation made observations there especially valuable. Delisle actually suggested going to Sweden himself in order to see that the work was done properly; the Paris Academy decided, however, that a local party of astronomers, led by Wargentin, were capable enough to carry out the task. Eventually the Swedish Academy of Sciences, prompted by Wargentin, mobilized all the astronomical expertise in the country in order to arrange observations coordinated with Lacaille's — besides Wargentin in Stockholm, the astronomers at the universities at Lund (observing at Härnösand, believed to be on exactly the same meridian as the Cape), Uppsala, and Åbo (Turku) participated, and also the competent amateur astronomer Anders Hellant in Torneå. The government paid for the expenses — salaries as well as instruments (clocks and micrometers). Astronomers from several countries had been asked to participate but the Swedish effort was, next to the French, by far the most important (Delisle to Wargentin, 17 Aug. 1752; Lindroth, 1967, Vol. I:1: 393—99; McClellan, 1985: 202—5; Woolf, 1959: 36—48). Swedish astronomy was now an integral part of the continental system of practical astronomy.

The Jovian Satellites were always a major topic in Wargentin's correspondence. Delisle had encouraged him to pursue this line of research from early on, and he helped Wargentin to gather data from other astronomers in his extensive network of correspondents. By the mid-1750s Wargentin had emerged as a serious contender in this particular field of astronomy, and Delisle, who planned to publish a book on the subject, was very curious to know what he would come up with next by way of improved tables (Delisle to Wargentin, 2 Jan. & 30 June 1755, 4 June 1756). Wargentin, however, changed his allegiances, and in 1755 he made Lalande the primary recipient in Paris of his desirable tables of the Jovian Satellites (Lalande to Wargentin, 29 Nov. 1755, 27 March 1756). The reason was that Lalande offered him a possibility to get his data "printed with fine characters under royal imprimatur" (4 March 1759). They appeared in Lalande's edition of Halley's tables (1759), in the *Connoissance des temps*, and in Lalande's very successful astronomical textbook, *Traité d'astronomie* (1764; 1771; 1792). Lalande stressed (1 Jan. 1761) that Wargentin's data should appear "under his name"; he offered Wargentin (21 July 1760) to print his work "for your glory and for the utility of astronomers" (Nordenmark, 1939: 225—31).

This marks an important contrast to Delisle's repeated assertions of secrecy: to Delisle, the correspondence network was sufficient as a means of "publication" — there, he was in charge and could transmit whatever information he deemed appropriate and keep the rest to himself. Some astronomers complained about this selectiveness. Lacaille wrote to Wargentin (5 Sept. 1759, 1 Dec. 1754) that Delisle "loves to keep for himself what the others do not have", and that he "pesters all and sundry to make them communicate their observations, though he is not at all communicative himself". Lalande described him (4 March 1759) as "a devouring gulf which yields back nothing". Through Delisle, it was possible to receive and transmit important information, but this channel of communication was indirect and uncertain: it was definitely not a reliable way to gain general acknowledgement and *gloire* for one's work. Lalande promised to bring it all out into the open; through the medium of print. Sound work, which benefited all, should be honoured. What we see in Lalande and other leading astronomers after *c.* 1760 is a realization of the need to make scientific or technical results quickly available in print: for practical reasons and in order to establish the intellectual property of astronomers. Lalande was a forerunner in the business of specialized astronomical publishing (Chapin, 1988), an activity which would supplant some of the functions earlier upheld by networks of communication — which lacked royal or any other kind of imprimatur.

As journals and books proliferated after mid century, the usual functions of scientific correspondence did not immediately become obsolete, however (some of those functions of course never did). Lalande's letters to Wargentin contain much the same material as those of Delisle; they do for example forward communications from other scientists (e.g. 15 Dec. 1771, 12 May 1772). As specialized publishing grew in importance correspondence became a vehicle of salesmanship. Lalande, Delisle, Lacaille, and also the Berlin astronomer Jean Bernoulli, asked Wargentin to advertise or commission their work among colleagues (Lalande, 10 July 1771; Delisle, 2 March 1752; Bernoulli, 13 July 1776; Lacaille, 14 June 1757).

The most important collaborative project discussed in the Lalande-Wargentin correspondence was the observing of the transits of Venus in 1761 and 1769. Lalande and also Delisle constantly prodded the astronomers of Europe in order to remind them that, when the time came, the transit of Venus over the sun's disc *had* to be observed in a common international effort, if the ancient question of the sun's parallax finally was to be solved. This time Wargentin and the Swedish Academy joined the collaborative venture at their own initiative, and their effort was of major importance for the outcome. Swedish observations were of special significance in 1769 when immersion *and* emersion could be observed nowhere in Europe except in the very north of Scandinavia. During both transits the work of the Swedish astronomers was of high quality and impressive as to quantity. In 1761 Sweden was second only to France with respect to the number of successful observations carried out, a position which in 1769 had changed to third (as a result of the huge British effort now made). In preparing and evaluating the observations of both transits Wargentin discussed methods and results with colleagues in other countries — France, Britain, and Russia. Swedish astronomers, above all Anders Planman and Melanderhielm, made important contributions to the general interpretation of the collective result of the measurements (Lindroth, 1967, Vol. I:1: 401—11; Woolf, 1959: 135—43, 182—9).

In the 1760s Swedish astronomy reached its peak. It was endowed with a fine observatory in Stockholm which was provided with instruments by John Bird of London, the master among contemporary makers of telescopic equipment; Wargentin was a central figure in European astronomy; Swedish astronomers contributed in a measure next only to that of France in international collaborations; several competent theoreticians were at work in the country as well. All in all, Swedish astronomy had succeeded in becoming a *part* of the European system of astronomy. This was achieved by adhering to the technical standards and to the general goal orientation of international astronomy, and through specialization within this framework. Wargentin's concentration on the Jovian Satellites is one case in point; another is the utilization of the country's position on the geographical periphery. When observations from northern latitudes were needed, Wargentin managed to arranges so that they were provided. Scientifically, Swedish astronomy was therefore *not* any longer on the periphery. We should note that the overall system of practical astronomy increased its efficiency by including Swedish astronomy. The gains from incorporating a northern subsystem like the Swedish one may be described as a result of division of labour: continental astronomers no longer had to travel to Sweden in order to produce measurements that had to be made there; that could be safely left to the locals. When necessary, their work could be checked and directed through established channels of communication.

4. Getting out of touch

The last two or three decades of the 18th century were a period of decline in Swedish science (Johannisson, 1981). In Sweden, unlike in some other countries, the universities were focal points of the scientific movement when it reached maturity around 1740. Toward the end of the century academics, especially mathematicians and astronomers, complained loudly about decreasing status, low income, or lack of work opportunities, and many left the university for more lucrative positions e.g. at court or in military education. This was true of Daniel Melanderhielm, who left his professorship in 1788 to write textbooks in military science for the king. In 1796, he became Permanent Secretary of the Academy of Sciences and many of Melanderhielm's activities during the nine years which he stayed in office must be understood in relation to the troublesome situation of mathematical professionals in Sweden, of which no one complained more bitterly than he. Melanderhielm was a hardheaded scientific politician who worked earnestly to elevate the status of the mathematical sciences and its practitioners, especially himself. Zach rightly compared him with Lalande, another expert at the simultaneous promotion of astronomy and self. During the decades after Wargentin's death, when Swedish science was hit by a recess, Melanderhielm's exertions to improve the situation for mathematical specialists was indeed justified by harsh reality (Widmalm, 1990a: ch. 12).

Melanderhielm's most weighty effort to improve the status of the mathematical sciences was the geodetic measurement that the academy carried out in 1801—03: it would "make an Epoch in astronomy" (Widmalm, 1990a: 218). Later geodetic work had shown that the value of the length of a degree of meridian in the Torne Valley, determined by Maupertuis in the 1730s, was too large if the earth's shape was to approach that of a regular ellipsoid. The new measurement was masterminded by Melanderhielm and it was carried out by the young mathematician Jöns Svanberg, who was indeed able to make likely that Maupertuis had made grave errors. Svanberg and Melanderhielm therefore proclaimed that the earth's shape was regular and that Newton had been right all along. They presented this result as one of the greatest victories ever for Swedish science: a triumph, thought Melanderhielm and his academy, which would be the starting signal for a rejuvenation of Swedish astronomy, with Svanberg — who now succeeded his mentor as academy secretary — as its bright new star (Widmalm, 1990a: ch. 14). Nothing of the sort happened, however. When the geodetic work was over and done with, the decline of Swedish astronomy continued. It would show but feeble signs of life before the second half of the century. Svanberg, whose greatest love was mathematics, remained a solitary figure in Swedish science, intellectually isolated from mathematically backward compatriots and out of touch with foreign practical astronomy. His correspondence with Delambre, one of the firmest supporters of Melanderhielm's geodetic project, is a striking example of failure in scientific communication. It reveals the very opposite of Wargentin's correspondence — the estrangement of Swedish astronomy from the continental system.

Failed Mediation

Delambre was co-leader of the survey between Dunkerque and Barcelona 1792—98, carried out in order to establish the length of the metre, and around 1800 he had emerged as the leading geodetic expert in Europe (Heilbron, 1990). No one had a sounder judgement of geodetic matters, theoretical or practical; the Swedes were indeed fortunate to have access to his friendly advice, but Delambre also firmly stated his critical opinions. He did not share the enthusiasm of the Swedes when they claimed to have proven that the earth's shape had been determined according to the gospel of Newton, once and for all. This idea was dear to the hearts of Melanderhielm and Svanberg: it gave their measurement a global and theoretical significance which guaranteed that it would be well publicized among foreign scientists and also locally, in Sweden (Widmalm, 1990a: ch. 13). Furthermore, only if they solved the problem of the shape of the earth was their work of use to navigators and cartographers and consequently of economic value (a condition for the king's financial support). But, as Delambre knew better than anyone, the metric survey had revealed irregularities in the earth's gravitational pull that clearly showed that nothing much could be concluded about the shape of the earth from isolated surveys like the Swedish one. One measurement could add important information, Delambre thought, but it could not be decisive. Here lies the root to the disagreement between Delambre and Svanberg: the latter considered his measurement as a crucial experiment; Delambre regarded it as a form of technology evaluation.

Delambre's judgement of the Swedish effort was however important, as the measurement constituted nothing less than an application of wholesale French geodesy, which had received its modern shape under the hands of above all Delambre himself. Svanberg did not only use a repeating circle by Lenoir — an instrument made popular through Delambre's metric survey — he also adhered closely to Delambre's mathematical methods, and he used the metric system throughout his work. His measurement was French as to method, technology, and in spirit; eventually he would publish his results in French. When Svanberg's book, *Exposition des opérations faites en Laponie* (1805), appeared Delambre's evaluation was however critical (Delambre, 1806). In 1805—06, an inconclusive discussion about these matters was carried out in a series of letters between Delambre and Svanberg (Widmalm, 1990a: ch. 16).

To Delambre, the discrepancy between Maupertuis' and Svanberg's results primarily raised questions of a technological nature: if the difference was as great as Svanberg claimed, was it possible to find out exactly where Maupertuis had gone wrong? Could it be that the instrument (a zenith sector) he had used was at fault, and, if so, how did this reflect on the sectors presently at use (mainly in Britain)? Furthermore, how did it reflect on the repeating circles used by Svanberg and by Delambre himself? The principles of the zenith sector were simple: you used it to measure the height of stars close to zenith, thus minimizing the error caused by refraction; great accuracy was achieved by the sector's long radius and, consequently, large-scale limb. The repeating circle, on the other hand, was an instrument of a new type, easy to transport but technically not very sophisticated. Its accuracy depended on the principle of repetition, that it was possible to make many observations of one position, using the whole 360° limb of the instrument, the inaccuracies of which thus evened out. If the sector was all right, then perhaps the circle was unreliable. If so, the prestige of the metric survey was at stake. This measurement seemed to give evidence to the existence of large irregularities in the field of gravity, hence making the notion of a geometrically well-defined shape of the earth come under suspicion, as Laplace's interpretation of the metric survey indeed showed (Laplace, 1799: 133—45). On the other hand, British measurements by the Ordnance Survey, using a zenith sector, also fuelled the doubts about the usefulness of applying a concept of strict geometrical regularity to the earth (Delambre, 1912: 334, 363—72). The whole geodetical research program was in the balance around 1800: one of the major problems was to find out exactly how reliable was the technology. Hence Delambre's strong interest in Svanberg's work, and hence his frustration in the face of the Swede's complete reliance on the technical excellence of the circle and his concentration on theoretical matters — minute detail as well as grand conjecture.

A related complaint was that Svanberg had not provided detailed information about observations. He did for example not give the original angles, only the corrected ones. According to Delambre (24 Oct.—6 Nov. 1805), no one had been so negligent since the days of Picard. Svanberg protested (undated letter, 1805) that he would have had to publish 600 (instead of 200) pages in order to harbour all the information that Delambre desired. To this Delambre responded, half jokingly, but — considering that he was preparing a three volume work, amounting to 2279 pages, about the metric survey (Méchain & Delambre, 1806—10) — probably also with real vexation:

> to me, nothing seems useless if it can show the reader what degree of confidence he can accord the observations, or serve to instruct those who may be charged with similar operations. You could have produced 600 pages? in that case you owe us 400 more and I invite you to pay the debt (24 Oct.—6. Nov. 1805)

At the same time, Delambre made a general complaint about the unwillingness on Svanberg's part to reveal technical or observational detail: "you must not give the impression of escaping daylight when you have no reason for fearing it". If he did, some might think that the very close agreement between Svanberg's result and the one predicted by theory was too good to be true — a very alarming suggestion indeed.

Delambre's criticism underscores two important features of practical astronomy around 1800: its credibility rested on technological foundations; its work was of a collective nature. The theoretician Svanberg did not appreciate this. He took the reliability of the technology for granted as it had been purchased from France, where people like Delambre himself had tried it out with good results, or so it seemed. To borrow a term used by Simon Schaffer in his analysis of the diffusion of Newtonian prisms: Svanberg regarded the repeating circle as *transparent*, that is, he accepted its technological merits, no questions asked (Schaffer, 1989). On the other hand, his measurement seemed to show (but not unequivocally — this was one of its great weaknesses) that the errors in Maupertuis' work must have been due to the sector. But many people had as much confidence in the sector as in the circle. Delambre would probably have been more pleased with Svanberg if he had *confirmed* Maupertuis' measurement; then the discrepancy between it and the Newtonian geometry of the earth could have been explained with reference to local variations in gravity, a well known phenomenon which did not reflect on the technology in which so much faith, prestige and money had been invested.[4]

The scientific value of Svanberg's work, however, was not meant to rest upon tedious technical detail but on the importance of its major conclusions. He had not spent a year in Torneå in order to check the accuracy of Maupertuis' *instruments*. The purpose of his work was to confirm the Newtonian prediction about the shape of the earth, indirectly to save beautiful theory, for which Svanberg held a Kantian reverence, from the ill effects of shoddy empiricism. Had he not viewed the circle as a "transparent" instrument, he would have been forced to reckon with the possibility that Maupertuis might have been right, in which case the task he had set himself would have been highly dubious.

The scientific approaches of the two astronomers were mutually exclusive; Svanberg therefore distanced himself from the Continental system of practical astronomy, where Delambre's methods and the repeating circle were rapidly gaining currency as they were adopted by the French military cartographers and astronomers attached to the Napoleonic *Dépôt de la guerre* and *Bureaux topographiques*. Svanberg did his best to prolong the correspondence with Delambre (20/6 1806), to transform it into a regular commerce of letters, but the Frenchman coldly refused (12/8 1806). Swedish practical astronomy thereafter ceased to be of international concern.

5. The system of astronomy

In this paper the word "system" has frequently been used to designate the evolving collaborative enterprise of practical astronomy in the 18th century. The same term has been applied by James E. McClellan in order to characterize the network of scientific academies that developed in the same period (McClellan, 1985: 153—4, 173—5). In fact, correspondence and collaboration between astronomers in the 18th century were often taking place under the auspices of one or several scientific academies and it is proper to ask if there was an organizational duality here or if the system of astronomy is reducible to that of the academies. Three points serve to illustrate that the latter is not the case.

First, though academies did help astronomers to develop networks of communication these networks were in many respects autonomous. Contacts were frequently established between scientists who did not belong to academies, though the more prominent ones of course tended to become attached to one or more scientific societies.[5] Second, the professionalization of astronomy owes as much, or more, to the development of a professional identity, centred around practical matters like cartography or navigation, as to the general scientific/professional ethos which McClellan (1985: ch. 7) associates with the academies. With one or two exceptions the 18th-century academy was not a professional organization: it did not train, it employed few scientists.[6] The labour market for astronomers was constituted mainly by educational establishments and observatories, by surveying and shipping; around 1800 more and more astronomical jobs (on routine as well as specialized levels) became available in geodetic/cartographic organizations. Third, international collaboration in 18th-century astronomy was not secondary to that of the academies. McClellan (1985: ch. 6) cites ten examples of academic collaboration in the 18th century: with one exception all of those which came to fruition were concerned with matters of practical astronomy — finding the longitude, measuring parallaxes, and making geodetic measurements. On the other hand there are several examples of international collaboration between astronomers where academies were not directly responsible: the Jesuits Roger Boscovich (Italy/Croatia) and Joseph Liesganig (Austria) collaborated with one another and also with César-François Cassini de Thury (France); Boscovich collaborated with British colleagues; Cassini de Thury worked with Germans and attempted a collaboration with Italian scientists. All of these collaborations concerned geodetic measurements, cartographic but also part of "physical astronomy" (Widmalm, 1990a: 31—4; Widmalm, 1990b: 180—84). Academies helped in bringing collaboration about, but so did monarchs and governments — who actually had to pay for the more expensive schemes cooked up by the academicians — and not least the enterprising spirit of the astronomers themselves.

The evolution of (largely) European systems of scientific academies and of practical astronomy in the 18th century coincided in time and they were in many ways connected. But the astronomical system was not subordinate to the academic one; it had its own dynamic, its own goal, and in important ways (like in the development of international collaboration) it set the direction which the academies, and later other sciences, followed. The letters of astronomers are testimonies of a community of scientific practitioners crystallizing an international profession around the solid core of precision technology.

This reliance on precision technology sets 18th-century practical astronomy apart from much contemporary science and gives the system of astronomy its peculiar characteristics. Some of these are captured by Thomas P. Hughes' well-known model of technological systems (Hughes, 1986; Hughes, 1987; Shrum, 1984). The "innovations" which triggered the development of modern practical astronomy occurred in the late 17th century, when telescopes were fitted with graduated limbs and other precision-measurement accessories and when national observatories were founded. Reverse salients emerged in the areas of dividing limbs or making telescopes providing sharp images (Bennett, 1987; Chapman, 1990; Daumas, 1972). These particular problems were solved with the invention of the dividing engine and the achromatic lens — both of which may be regarded as "conservative inventions", whereas e.g. the marine chronometer may be viewed as a "radical invention", solving the astronomical problem of finding longitude by non-astronomical means. Likewise the system of astronomy may be said to have exhibited "momentum" as its material and organizational base grew with the number of instruments, observatories, and surveying organizations (Howse, 1986).

Other similarities between Hughes' technological system and the system of 18th-century practical astronomy could be mentioned — the division of labour within the astronomical community does for example bear a strong resemblance to Hughes' notion of "diversification". There is however one important difference that must be pointed out. There were tendencies toward centralization in the astronomical system but nothing like the strongly hierarchical structure implied by Hughes' model. Instead we have the networks of communication underpinned by commerces of letters, scientific societies, and publications. There does simply not seem to have existed any means of power strong enough for a centralized organization to emerge in astronomy or any other science in the 18th century. On a national level centralization could emerge as a potent force, as in Wargentin's Sweden or Cassini's France. Some individuals did their best to impose their notions of theory or scientific language on a whole discipline; but in no area do we find an individual or a small group of individuals with the power to direct the practical efforts of an international community of scientists. Only for brief spells, like during the transits of Venus, did something similar to centralized scientific management come into effect.

At the backbone of the system of practical astronomy were the international networks of communication, which made possible the coordination of data production and the perpetuation of professional standards. With time, the networks of correspondents came to reflect a network of precisely determined positions which redefined geographical space (Harley, 1988). The uniformity which was imposed on space by its quantification affected the quantifiers themselves. Their self-imposed task, to measure positions and distances, required international collaboration, efficient communication, and standardized instruments and methods. These procedures paved the way for the emergence of a professional identity of which the ethos of accuracy and the habit of internationalism were integral parts. Aspects of the astronomer's professional identity were, no doubt in large measure because of the academy system, shared by that of other sciences. But precision consciousness and internationalism were developed to a high degree in astronomy before other scientific disciplines, some of which would however follow suit in the next century (Crawford, 1990; Olesko, 1991).

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FOOTNOTES

1. This is probably because Laplace and Herschel were both alive when Delambre wrote (Delambre, 1821: li). Newton does not have a section of his own in this volume, but his work is frequently discussed therein. That is the reason Delambre states for giving him such a brief treatment in the sequel (Delambre, 1827: 1) Hence, with Delambre's periodization, Newton flourishes too early and Laplace and Herschel too late to belong firmly to the 18th century.
2. There are indeed good studies of broader topics in 18th-century science. Of importance for the subject under consideration here are e.g. Chapman, 1990; Daumas, 1972; Dhombres & Dhombres, 1989; Gillispie, 1980; Hahn, 1971; Helden, 1985; Frängsmyr, 1990a; Konwitz, 1987; Lindroth, 1967; McClellan, 1985; Woolf, 1959.
3. Delisle did not share the French Newtonians' (and Celsius') conviction that Jacques Cassini's measurements in France, which indicated that the earth is bulging at the Poles and is flattened at the Equator, were faulty. Delisle reserved his judgement, but he was in favour of the method that Cassini had used, to measure parallels besides degrees of meridian, and he did not like to think that he could have made errors as great as the critics suggested. Delisle intended to use Cassini's method himself. Unlike Maupertuis and his followers, Delisle did however not think that it was possible to find the earth's shape by only a few measurements because it might very well be too irregular for such a simplistic method to be feasible. Delisle to Celsius 1 Aug. 1738. On the controversy concerning the use of parallels or meridians for determining the shape of the earth, see Greenberg, 1983. Cf. Terrall, 1992.

4. When Delambre first received news that Svanberg had localized the error in Maupertuis' measurement to its astronomical part, and that the error was around 10 seconds, he was unwilling to believe it. Delambre to Melanderhielm, 21 May 1803. In the spring of 1806 Svanberg was however awarded Lalande's gold medal, bestowed annually by the Institut national for the best astronomical work of the year. Lalande, now 74 and no longer with much scientific authority, was a supporter of the measurement which he thought would make further investigations about the earth's shape unnecessary. His influence probably lay behind Svanberg's award. Lalande to Svanberg, 30 Sept. 1806.
5. McClellan (1985: 157) does for example regard the correspondence between Celsius and Delisle as an institutional one, but that is incorrect. Their interest in the correspondence was simply astronomical. When Wargentin was elected official correspondent to Delisle by the Paris Academy in 1748, this was done in order to encourage an exchange already established (Nordenmark, 1939: 22).
6. In Sweden there was not "a major community of men ... who were able to make scientific careers within an institutional establishment that included the Swedish Academy of Science[s] as its major part" (McClellan, 1985: 236). A couple of poorly salaried positions were available, one as the Academy's astronomer. Swedish scientists, of which there were many around mid-century, worked at universities, in mining, as physicians, technicians etc. The Swedish Academy of Sciences served these people well in many respects, but not as an employer.

Archival sources

Letters to Celsius from foreign correspondents are at Uppsala Universitetsbibliotek (Uppsala University Library: UUB), A 533. Copies of letters to Celsius from foreign astronomers are at UUB, X255 oa. Originals of the letters referred to in this paper are at the Bibliothèque de l'Observatoire de Paris. Letters to Olof Hiorter, and drafts of his replies, are at UUB, A 553. The Letters to Wargentin are kept at Kungl. Vetenskapsakademien, Centrum för Vetenskapshistoria, Stockholm (The Royal Swedish Academy of Sciences, Centre for History of Science: KVA). Wargentin's own register of letters to foreign correspondents, and a list of all the letters to Wargentin that are preserved at the archives of the academy, are printed in Nordenmark, 1939: 399—449. Melanderhielm's foreign correspondence is preserved at UUB, G 172. A list of these letters has been printed in Nordenmark, 1946: 247—49. Svanberg's letters (drafts) are kept at UUB, A 640, and at KVA (letters to Svanberg). Original letters from Svanberg to Delambre are preserved at the Archives de l'Académie des Sciences, Paris, Dossier Svanberg.

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