CHAPTER 3.4

Observatory mathematics in the nineteenth century

David Aubin

'The value of the service of an Assistant to the Observatory', the Astronomer Royal George Biddell Airy wrote in 1861, 'depends very materially on his acquaintance with Observatory Mathematics'.[1] There is a rather strange ring to this expression. One knows, of course, that mathematics has always been used extensively in observatories. Ever since permanent astronomical stations were set up in Europe during the Renaissance, observers have drawn on the most elaborate mathematical tools available to them to correct the observational data they produced and to come up with theoretical predictions to which it could be compared. Up until the nineteenth century, astronomers played a central role in the development of many parts of mathematics. Indeed, together with geometry and arithmetic, astronomy had always been considered as one of the main branches of mathematics.

Still, in what sense can one talk of 'observatory mathematics'? Should one understand the expression as designating the subset of mathematics that was especially relevant to the scientific activities carried out inside observatories? Or is there—has there ever been—a specific character common to all mathematical

1. 'Remarks on the neglect, by the Junior Assistants, of the course of education and scientific preparation recommended to them' (4 December 1861). Cambridge University Library, Airy's Papers, RGO 6/43, 235.

tools and concepts used in those places? If so, what sense is there in carving out a portion of mathematics on the basis of it being used in a specific institution?

In this paper I want to introduce the question of place and space in the history of mathematics by looking at the various ways in which mathematics was practised at a specific location—the observatory—in a given period—the nineteenth century. My claim is that this exercise will enrich our understanding of changes undergone by mathematics in that period. As Michel de Certeau (1984) has shown, a focus on place as 'practised space'—that is, space where human practices are deployed—can help the historian identify cultural practices that are common to the users of the same space but that are not necessarily talked about. The issue of place in the history of science was in fact inaugurated by a debate about Tycho Brahe's observatory (Hannaway 1986; Shackelford 1993). Numerous studies have since been devoted to the topic, mapping out various spatial aspects of the laboratory and field sciences (Ophir and Shapin 1991; Livingstone 1995; Kuklick and Kohler 1996; Smith and Agar 1998).

At first sight, the history of mathematics, where disciplinary approaches have been dominant for so long, would seem more immune to spatial approaches than any other part of the history of science. What scientific domain could be less tied to a specific place than mathematics? Mathematicians only need pen and paper. And even those, the Bourbaki mathematician André Weil once wrote, could sometimes be dispensed with.[2] Historians, however, have shown that the universality of mathematics was actually forged in large part in the nineteenth and early twentieth centuries (Parshall and Rice 2002). A few institutional surveys underscore the imprint made on mathematics by particular institutions (the École polytechnique, Göttingen, the Institute for Advanced Study . . .). Even Weil would at times concur—if only for opportunistic reasons—that institutional histories may be indispensable:

> It is unthinkable that anyone would write the history of mathematics in the 20th Century without devoting a large portion of it either to the Institute [of Advanced Study in Princeton] as such, or to the mathematics which have been done here, which comes very much to the same thing.[3]

Similarly, I contend that to study the history of mathematics in the nineteenth century it might be useful to pay special attention to the observatory. It has recently been suggested that a tight focus on observatory techniques can provide new insights about the social organization of science for the nineteenth-century state (Aubin 2002; Boistel 2005; Lamy 2007; Aubin, Bigg, and Sibum

2. 'Let others besiege the offices of the mighty in the hope of getting the expensive apparatus, without which no Nobel prize comes within reach. Pencil and paper is all the mathematician needs; he can even sometimes get along without these' (Weil 1950, 296).

3. André Weil, 'Talk to the Trustees of the Institute for Advanced Study, by Professor André Weil, April 1, 1960,' Archives of the Institut des hautes études scientifiques, Bures-sur-Yvette.

forthcoming). Observatory techniques have been defined as the coherent set of physical, methodological, and social techniques rooted in the observatory because they were either developed or extensively used there. Among them, mathematical techniques figure prominently. Whether concerned with astronomy, geodesy, meteorology, physics, or sociology, in their quest for precision observatory scientists were both major consumers and producers of mathematical knowledge and techniques. Most of the founders of the German mathematical renaissance around 1800 had strong ties to observatories (Mehrtens 1981, 414–5). The same is true of other countries. Some observatory scientists, such as Pierre-Simon Laplace or Carl Friedrich Gauss, are even considered among the most outstanding mathematicians of all time. The roster of famous mathematicians who worked in (and often directed) observatories includes Friedrich Wilhelm Bessel, Nikolai Lobatchevski, August Möbius, Adolphe Quetelet, and William Rowan Hamilton. Others, like Augustin-Louis Cauchy, Karl Weirstrass, Henri Poincaré, and David Hilbert, were often passionately interested in celestial mechanics and gravitation theory. So, while mathematical techniques centrally belonged to the arsenal of the observatory, the mathematics developed to serve various observatory sciences equally became prominent areas of mathematics.

The special relation between mathematics and the observatory—or between mathematics and astronomy—is of course in no way a characteristic solely of the nineteenth century. Recall how mathematical analysis has, since Isaac Newton's time, been closely tied with the problems of celestial mechanics. I focus on the nineteenth century because the observatory was, at that time, the place that (as opposed to the Academy of Sciences earlier or the laboratory later) best embodied the intimate link between science, states, and societies in Europe and North America. I do so also because mathematics was then undergoing crucial changes that our look at the observatory will lead us to reinterpret in significant ways.

Indeed, we have a paradoxical view of nineteenth-century mathematics. In historical lectures, Felix Klein said that in earlier times 'independent works of pure mathematics were overshadowed by the powerful creation in which pure and applied mathematics united to answer the demands of the times' (Klein 1979, 2). But in the nineteenth century, mathematics increasingly seemed to be split in two. While the use and application of mathematics went on unabated, pure mathematics—and especially its most abstract and foundational aspects—took centre stage. Mathematics took on larger and larger new territories, providing tools for describing, controlling, and changing the world. In the physical as well as in the social realm, scores of laws expressed in the forms of differential equations were derived by scientists. The number of phenomena that were subjected to precise quantitative measurement increased tremendously. In offices, factories, army barracks, schools, and observatories, people with elementary or advanced mathematical skills multiplied. While the mathematical apprehension of our world progressed, mathematics as an academic discipline became increasingly abstract. Foundational questions started to assume a primary importance for the professional community: 'a revolution [. . .] characterized by a change in the ontological status of the basic objects of study' (Gray 1992, 226). Pure mathematics was detaching itself from the physical world at the very moment when it seemed more applicable than ever.

Most historians of mathematics have focused on the 'revolution' at the expense of the routine expansion of mathematical territories. Even when they have not, historians have found it difficult to deal with both processes at once. By focusing on the observatory, as a specific place where mathematics was intensely used and produced, I hope to throw new light on those two parallel large-scale processes. When examining mathematical practice in observatories, the major role played by numbers is immediately striking. Numbers are the main mediators between the various parts and functions of the observatory. Faced with an 'avalanche of printed numbers' (Hacking 1990) in their practical work, observatory scientists developed tools and techniques that became prominent factors in both mathematics' move towards abstraction and its increasing appeal as a privileged instrument for understanding nature and society.

There are two aspects to my study. First, I examine the specific spatial arrangement of mathematical work *within* observatories. I want to illuminate mathematical practices at this site, including its social organization. In order to do this, I focus on a social history of numbers, tracing their trajectory from their production with instruments to their insertion in observatory outputs. Second, I consider the observatory as the locus of particular mathematical cultures, which had important effects on the development of the field. I pay particular attention to three domains of mathematics: celestial mechanics, geometry, and statistics. In other words, this paper examines first the place of mathematics in the nineteenth-century observatory and then resituates the observatory in the history of mathematics.[4]

THE PLACE OF MATHEMATICS IN THE OBSERVATORY

'Every part of the operations of an observatory is mathematical,' Airy wrote in the 1861 memo quoted above. 'Mathematical Mechanics' was involved in the construction of all instruments. 'The action and faults of telescopes and microscopes require for their understanding a knowledge of Mathematical Optics. Every discussion and interpretation of the observations requires Mathematical Astronomy.

4. Computing aspects will be slightly downplayed here in order not to overlap too much with Mary Croarken's Chapter 4.4 on human computers.

The higher problems, such as the discovery of the elements of a comet's orbit from observations, require the high Mathematics of Gravitational Astronomy.' In a word, mathematics was omnipresent in the observatory.

Five years earlier, Airy had spelled out the mathematical knowledge he thought was indispensable at each level of the strict hierarchy he had devised for the workings of Greenwich. A first draft was drawn up on 20 November, 1856 and a slightly revised version was adopted on 10 May, 1857.[5] At the bottom of the scale, according to this scheme, were supernumerary computers. In addition to being able to 'write a good hand and good figures' and 'to write well from dictation, to spell correctly and to punctuate fairly', computers were to have rudimentary mathematical knowledge, essentially restricted to arithmetic, including vulgar and decimal fractions, extraction of square roots, use of logarithms, and the use of $\pm$. Next came the Assistant, first grade, who was required to read French and to understand geometry (equivalent to the first four books of Euclid), plane trigonometry, and simple and quadratic equations. Assistants, second grade—like, at the time, Hugh Breen (who had been first been hired as a teenager computer in 1839)—needed to read Latin and speak a little French. In mathematics, they ought to understand simple algebraic rules such as the binomial theorem, spherical trigonometry, and differential calculus ('to Taylor's theorem, and applications to small variations of plane and spherical triangles, &c.'), as well as to have some notions in integral calculus. Beyond pure mathematics, they should have elementary knowledge of mechanics and optics and be able to master applications of plane and spherical trigonometry to astronomy. Long-time associates of Airy's had then achieved the higher level of Assistants, third grade. Supposing they conformed to the requirements spelled out by their boss, Edwin Dunkin and James Glaisher would then have understood analytical geometry, conic sections, integrations for surfaces and solids, advanced mechanics, optics, analytical mechanics 'especially in reference to Gravitational Astronomy'. More specifically, they would be conversant in the complete theory of telescopes and microscopes: object glasses, mirrors eyepieces, micrometers, etc. They would be able to apply methods for computing orbits of comets and planets. They should also read ordinary German. Clearly the skills required to work in an observatory were many.

Airy's memo not only sketched a relatively well defined perimeter of the knowledge required for working in an observatory, but also set up a scale of value in mathematical knowledge. While analysis and mixed mathematics (mechanics and optics) clearly stood at the top of his scale, geometry, elementary algebra, the first notions of calculus, and arithmetic especially, lay at the bottom. One may note that contemporary non-university mathematical textbooks reflected such

5. Several slightly different copies of this memo are extent in Airy's papers in Cambridge University Library. Above and in the following I quote from RGO 6/43, pp. 170–175.

scales, paying much attention to its lower parts and almost none to the top levels (Rogers 1981). In such textbooks, practical astronomy rather than analysis and rational mechanics often occupied the last and more difficult chapters, thus being set up as the ultimate goal of the 'practical mathematician'.

In addition to establishing a scale of mathematical knowledge, Airy also put mathematics above all the rest in a general scale of knowledge. Recognizing that mathematical skills were not the only ones required from an observatory assistant—he cited foreign languages, 'general or photographic chemistry', 'telegraphic galvanism', and so on—the Astronomer Royal nonetheless insisted on the special value of mathematical knowledge. Routine telescopic observations, 'which a lad acquires in two months, and which a man scarcely improves in many years', Airy added parenthetically, required few mathematical skills. Beyond those, the operations of an observatory required one to expand one's knowledge 'mainly in the mathematical direction'.[6] And this alone allowed one to rise up the hierarchy at the observatory. Astronomers with a more democratic bent similarly concurred that mathematics was what blocked the masses from familiarity with observatory sciences. In his public lectures, the director of the Paris Observatory François Arago (1836) tried to introduce the subject without using advanced mathematics. Like him, Alexander von Humboldt, John Herschel, Auguste Comte, and Laplace were much praised for presenting the public with treatises that circumvented mathematical technicalities.

But, as Yves Gingras (2001) has shown, mathematization also had an important social role as a technology demarcating insiders from outsiders (on this point, see also Schaffer 1994a). Over the course of the nineteenth century, dozens of new observatories were set up all over the world and the number of staff working in major national observatories increased significantly. Conflicts over the best ways to organize collective work inevitably arose. During the French Revolution, observatories in Paris were placed under the direction of the Bureau of Longitudes, a collegial body of astronomers, mathematicians, seamen, and instrument makers specially set up for that purpose. Others opposed the view that collegiality would ensure that national observatories carried out their regular tasks properly. In the early twentieth century, the American astronomer Simon Newcomb drew the following lesson from those discussions: 'The go-as-you-please system works no better in a national observatory than it would in a business institution' (Newcomb 1903, 332). More than a century earlier, in a memoir presented to the Revolutionary Comité de Salut Public (Committee of Public Salvation) in June 1793, the former head of the Paris Observatory Jacques-Dominique Cassini had similarly explained why, as far as the working of an observatory was con-

6. Airy, 'Remarks on the neglect' (note 1), RGO 6/43, pp. 235–236.

cerned, he thought it necessary to go against the Republican principle of 'sacred equality':

In astronomy, one distinguishes between the astronomer and the observer: the former is the one who embraces this science as a whole, who knows the facts, the data, and draws results from them. The observer is the one who is more specifically devoted to observation; he only needs to have good eyes, skill, strength and a lot of energy.[7] (Cassini 1810, 207).

At the Paris Observatory as at any national observatory, Cassini went on, a director was needed *pour la même raison que l'on place un pilote dans un vaisseau, un chef dans un bureau* 'for the same reason one places a pilot on a ship, a supervisor in an office' (Cassini 1810, 206–207). There were a whole range of observations that needed to be carried out regularly and without interruption. While young observers could be found with enough zeal to fulfil this task, an experienced astronomer was needed to direct them. His special task would be not only to oversee the work of the observers but also to compile their results in general annual publications, presenting not simply gross observations but nicely reduced ones seamlessly woven into *un narré instructif de l'histoire et des progress de l'astronomie* 'an instructive narrative of the history and progress of astronomy' (Cassini 1810, 207).

In this context, exceptional mathematical skills were often singled out as those most likely to determine who would make a good observatory director. In the memos quoted above, Airy underscored his opinion that mathematical knowledge was what counted most to head an observatory. As an enticement for studying abstract mathematics, he wrote that the 'acquisition of these attainments would be at least as valuable to the Assistants (particularly if opportunities of quitting the Observatory should occur) as to the Observatory'. Later, especially after the emergence of astrophysics in the second half of the century, other types of skill seemed at least as important as mathematics for rising to the directorship of an observatory. But, as is well known, mathematics never completely lost its prominence as a tool for social selection.

The nineteenth-century observatory was a place where the quantitative spirit was valued most highly. Astronomy in particular was *la science où l'on rencontre de plus fréquentes occasions de faire des calculs longs et compliqués* 'the science where one has most frequently the occasion to carry out long and complicated computations' (Francœur 1830, vii). While Gauss's love of numerical calculation

7. On distingue en astronomie l'astronome et l'observateur: le premier est celui qui embrasse l'ensemble de cette science, qui en connaît et approfondit toutes les théories, rassemble et compare les faits, les données, et en tire les résultats. L'observateur est celui qui se livre particulièrement à l'observation; il lui suffit d'avoir de bon yeux, de l'adresse, de la force et beaucoup d'activité

is legendary (Bourbaki 1994, 153), Airy is also known to have had a special obsession with quantitative results. His son Wilfrid once recalled:

> He was never satisfied with leaving a result as a barren mathematical expression. He would reduce it, if possible, to a practical and numerical form, at any cost of labour: and would use any approximations which would conduce to this result, rather than leave the result in an unfruitful condition. He never shirked arithmetical work: the longest and most laborious reductions had no terrors for him, and he was remarkably skilful with the various mathematical expedients for shortening and facilitating arithmetical work of a complex character. This power of handling arithmetic was of great value to him in the Observatory reductions and in the Observatory work generally (Airy 1896, 7).

The observatory indeed was a true factory of numbers and, as such, it needed competent people able to withstand the 'avalanche'. Simon Schaffer (1988) and Robert Smith (1991) have debated the most proper metaphor to describe the observatory—the factory or the accounting office. What is more significant to us right now is that the production and treatment of numbers on a massive, 'industrial' scale was observatory scientists' main business over most of the nineteenth century. And this implied forms of social organization that put mathematics at the centre of observatory scientists' practices (see also Ashworth 1994; 1998). But which part of mathematics?

A FACTORY OF NUMBERS

At the end of the eighteenth century, it appeared clear that an astronomical observatory should be built around its instruments and that the most important of them should generate numbers, accurate numbers. *On ne peut s'occuper de la distribution d'un Observatoire qu'après avoir fixé le nombre, la grandeur, la forme et l'usage des instruments dont on se propose de le meubler.* 'The proper distribution of an observatory can only be addressed after the number, size, shape and use of the instruments intended for it has been fixed' (Cassini 1810, 74). While some of those instruments were portable, others needed to be precisely and firmly positioned in a well-designed, controlled environment. Astronomers in Paris always complained that their observatory, built by Claude Perrault around 1667, was too monumental for this purpose. In a memoir he wrote to make explicit the demands an astronomer wished to place on architects eventually engaged designing a new observatory, Cassini insisted on two special types of telescope, respectively called the transit instrument and the mural quadrant (or circle) (for a detailed architectural memoir on how to design an observatory around 1800, see Borheck 2005).

Both instruments consisted in a combination of telescope and graduated limb, and both were used in conjunction with other instruments (Chapman 1995). Fixed to a wall precisely oriented along the North–South line, the mural quadrant was usually larger and more finely graduated than the transit instrument. On a quadrant, special microscopes on the limb and wire nets, called micrometers, enabled a more precise reading of the graduations. Transit instruments were used together with highly precise astronomical clocks. While the former combination of instruments was used to determine the right ascension of a star or planet above the celestial equator, the second measured the exact time at which it crossed the meridian.

Using mural quadrants and transit instruments, both angular coordinates of a celestial body at a specific moment could therefore be measured to a remarkable degree of precision. In the eighteenth century, graduation had been improved by a factor of 200, from 20 seconds to a tenth of a second, and progress over the nineteenth century was no less spectacular (Frangsmyr et al. 1990, 6). Achromatic lenses and clock regulators further improved the precision of the measurements made in observatories. In the first half of the nineteenth century, quadrants and transit instruments were combined and significantly transformed by German instrument makers (Chapman 1993), but the telescope's function as observatory scientists' main purveyor of numerical data was unchallenged until the last decades of the century brought the advent of photography, polarimetry, and spectroscopy. At the transit instrument, the astronomer 'listens in silence to the ticking of the clock, and [. . .] notes exactly the hour, minute, second, and fraction of a second when the star passes each wire' in the telescope (Biot 1810–1, I 55). In the dark, the observer at his eyepiece jotted down a few numbers on a paper slip (Lesté-Lasserre 2004). From then on, those numbers would be copied into large registers, preserved for centuries, averaged, combined with scores of other numbers, and transformed through various computational procedures, tabulated, printed in large folio volumes, distributed across the globe, and eventually picked up by seamen or theoreticians.

While meridian observations, even routine ones, required great manual skill, the level of mathematical sophistication involved at each step of these processes varied greatly. Barely literate teenagers could spend weeks ticking each number in long columns just to make sure that no mistake was made when they were copied from one register to another. But reductions were not trivial computations. In fact it was argued that since the reduction of other people's observations only inspired 'boredom and disgust', it was the observers' task to 'compute' their own observations: 'being the only one to know well the circumstances that go with them, he knows more than anyone else how to choose those that are most trustworthy' (Cassini 1810, 190). This is why any observatory scientist needed to be at least conversant in mathematical techniques.

The mathematical treatment of data served various purposes. Raw data was of little use to the outside community. Reductions made observations at different locations and times comparable with each other. Because a variety of factors affected the interpretation of data, that data was corrected using various mathematical algorithms. Corrections increased the precision of the observation, for example by taking into account flaws in the construction or alignment of the telescope, by compensating for differences in individual perception (the personal equation), or by compensating for aberration (itself dependent on the angle of observation, but also on temperature and atmospheric pressure at the time of measurement). Logarithms were said to be *artifice admirable qui, en abrégeant les calculs, étend pour ainsi dire la vie des astronomes (comme) le télescope avait aggrandi (sic) leur vue* 'admirable artifice that, by shortening computations, extends astronomers' lives (just as) the telescope has increased their sight' (Biot 1803, 26).

In this sense, mathematics was therefore just another instrument in observatory scientists' panoply.[8] As such, mathematics was accordingly taken into account in the spatial arrangement of observatories. At Greenwich, around 1850, it was highly symbolic that, between the transit room, where most transit observations were made, and the east room, previously devoted to Bird's now derelict quadrant and where jotting books and correspondence were stored, stood the computing room—'the grand scene of labour of the whole Observatory':

> It is only by exception that the astronomer or his assistants are to be found *using* the instruments, even during the regular hours of observatory work; but they are nearly sure to be found assembled in the Computing Room, busied, at different tables, with their silent and laborious tasks,—the assistants on watch turning an eye now and then to a small time-piece which regulates their task of allowing no celestial object of consequence to pass the meridian unobserved. (Forbes 1850, 449)

Produced in the transit room and stored in the east room, observatory numbers were processed in the computing room located between them. Besides telescopes, there was a wide variety of instruments (thermometers, barometers, magnetometers, polarimeters, and so on), which, alone or in conjunction with clocks, also gave out numbers. In the natural history of numbers churned out by observatories, the operations carried out in the computing room were therefore crucial mediating steps between the instruments producing the numbers and the outside consumers of these numbers.

Prior to the 1830s, the proportion of published numbers with respect to overall production was rather small. When published, numbers often played a part in narratives that underscored difficulties encountered (Terrall 2006). As publication became an indispensable part of the public observatory's mission, the labour that went into preparing such publications was increasingly erased. When Airy endeavoured to make old Greenwich observations public, he coped with the amount of work involved neither by relying on mathematical innovations nor by having recourse to technical advances (contrary to Charles Babbage's hopes, see Schaffer 1994b), but by organizing work hierarchically. A senior wrangler at

8. A similar argument is made by Switjink (1987) without making explicit what is owed to observatory culture.

Cambridge, Airy owed much of his professional success to his mathematical talents, and these he attributed in large part to his 'high appreciation of order'. He sometimes went as far as to consider mathematics 'as nothing more than a system of order carried to a considerable extent' (Airy 1896, 6). For reducing the lunar and planetary observations of his predecessors, the Astronomer Royal designed several printed skeleton forms which were used by lower level staff to carry out computations. Mathematical operations involved in data reductions were therefore for the most part reduced to elementary operations. They mainly consisted in carrying out additions and subtractions in decimal and sexagesimal forms, and in using numerical tables. Mathematical tasks were split in two: the execution of computations was rendered as mechanical as possible, while the algorithmic part of the work—deciding on the computations that needed to be done and in what order—remained the Astronomer Royal's responsibility.

In the nineteenth century, observatory mathematics was therefore characterized by the same paradox as the one already mentioned for the whole of mathematics. Obsessively quantitative, it nevertheless put non-numerical practices at the top of its hierarchical scale. Publications streaming out of observatories spread were overfilled with numbers. This type of production was a tremendous boost to the widespread diffusion of mathematical practices not only among physicists and statisticians, but also among craftsmen (such as instrument and clock makers), military officers, and seafarers. Nevertheless, forced to manipulate great quantities of numbers, observatory scientists became famous, won prizes and medals, and were elected to academy seats not because of their computations but for the ingenious ways they devised for avoiding them. 'To the astronomer' belonged the task of 'looking for ways to shorten [computations], since by his constant practice, he is better placed than anyone to perceive the shortcomings of methods and resources to be drawn on to make them more bearable' (Delambre 1810, 100).[9] These methods also played a role, which remains to be studied carefully by historians, in widening the number of mathematically literate people in the nineteenth century. We will now examine in more details the various methods they developed, by focusing on a few famous instances where observatory mathematics had a great impact on the field as a whole.

THE OBSERVATORY IN THE HISTORY OF MATHEMATICS

Up until the end of the eighteenth century, it went without saying that astronomy had its place in any book on the history of mathematics. Mathematics and astronomy were so close to one other that they were for all purposes united.

9. C'est à l'astronome à chercher les moyens de les abréger, puisque, par un usage continuel, il est plus à portée que personne d'apercevoir les inconvéniens des methods, et les resources qu'on peut avoir pour le rendre plus supportables.

> 'An astronomer, in order to be skilful, must be a Geometer [that is, a mathematician]; a Geometer, to deal with grand topics, need to have some of the Astronomer's knowledge. [. . .] The Astronomer in his observatory, the Geometer in his cabinet—this is always the same man who observes and meditates, who applies to the heavens either his senses or his thought' (Bailly 1785, III 208).

As James Pierpont's address to the Saint-Louis International Congress attests, by 1904 the situation had completely changed. As the title of this talk made explicit, 'the history of mathematics in the nineteenth century' could now be written by focusing exclusively on the pure domain (complex variables, algebraic functions, differential equations, groups, infinite aggregates, non-Euclidean geometry, and so on) without even mentioning applications, let alone the observatory sciences.

Meanwhile it seemed that 'mathematics [had] separated from astronomy, geodesy, physics, statistics, etc.', a fact that Klein (1979, 3) attributed to the professionalization and specialization of the sciences that were consequences of the social and cultural upheavals unleashed by the French Revolution. The increasing autonomy of the mathematical field, as well as the growing number of mathematicians earning a living as teachers, had important effects in shaping the evolution of the field towards foundational and structural aspects of mathematics (Mehrtens, Bos, and Schneider 1981). Nevertheless, for most of the nineteenth century the observatory remained one of the central scientific institutions of every nation that wished to be called 'civilized'. It was, as we have seen, a place where mathematics was its workers' daily bread. That observatories went on to play major roles in the development of the physical and mathematical sciences therefore comes as no surprise.

The question, however, is whether observatory mathematics left a specific imprint on nineteenth-century mathematics. In this second section, I would like to suggest that the 'values of precision' (Wise 1995) so dear to observatory culture had in fact everything to do with some of the evolutions of mathematics in that period. In 1846, the alliance between precise observation and precise computation was fully realized when it became possible to predict the presence of a missing planet just by taking into account anomalies in the orbit of its neighbour. Urbain Le Verrier and John C Adams acquired instant universal fame when they computed the orbit of Neptune to explain why Uranus was deviating from the orbit assigned by Newton's gravitational theory. The uncanny fit between theory and observation was a product of the extreme precision that characterized observatory culture. In the second half of the nineteenth century, confidence in the value of Newton's law of gravitation, in observational accuracy, and in the analytical methods brought to perfection by Laplace (the so-called 'French Newton'), was so high that some astronomers actually spent decades of their lives computing numerical tables, developing a single function, or trying to determine the value of a single number such as the solar parallax (Aubin 2006). If a discrepancy was found, instruments, theory, or both were usually blamed. Other times, extreme precision also provided a test for the efficiency of mathematical methods themselves, and even for the soundness of the foundations of mathematics.

In the following I will use an abundant secondary literature to discuss three famous instances where observatory culture seemed to be pushing known mathematics to its limits with considerable impact on its future development: (1) Gauss and non-Euclidean geometry; (2) Quetelet's uses of statistics and his theory of the average man; and (3) Poincaré's solution to the three-body problem. All three episodes have given rise to controversies among historians, and underscore the difficulty of discussing the relationship between mathematical innovations and their social environments. My claim is that by considering each of these contributions as anchored in observatory culture, we may gain insight into how conceptions of space, time, and society are related to the foundations of mathematics.

GEODESY, GEOMETRY, AND THE CONCEPT OF SPACE

There has been much debate about the exact relationship between Gauss's unpublished anticipations of non-Euclidean geometry and the commission he received in 1820 to carry out the geodetic survey of the state of Hanover. Director of the Göttingen Observatory since 1807, Gauss was a natural choice for this task. For most of the eighteenth and nineteenth centuries, geodesy was closely associated with observatories, since the precise measurement of the earth and that of the heavens were interdependent. Careful astronomical observations of fixed stars were crucial in any geodetic survey, while it had always been important for the purpose of comparing observations to know the exact geodetic position of observatories with respect to one another. In the early nineteenth century, moreover, the skills needed to carry out a geodetic survey were close to those developed in observatories. The lengthy trigonometric computations involved were exactly of the type observatory scientists were well equipped to carry out, intellectually as well as materially. When repeating circles and theodolites were introduced to geodetic practice, the observatory scientist's special skill with the telescope became so indispensable that, even in the turbulent times of the French Revolution, only astronomers could be sent out, at great risk to themselves, to survey the country from Dunkirk to Barcelona (Adler 2002).

Gauss's correspondents, however, thought that the director of the Göttingen observatory could have made better use of his precious time than to spend days and nights crisscrossing the countryside for up to six months a year. His friends' and colleagues' opinions notwithstanding, Gauss seems to have relished this exercise in high numerical and instrumental precision. In September 1823, to link up his triangulation of Hanover with existing ones to the east and the south, Gauss, together with Christian Ludwig Gerling, measured the angles of a large triangle between Brocken, Hohehagen, and Inselsberg (BHI). Since that time, it has often been said that Gauss undertook the task just to be able to check whether the sum of the angles would add up to 180°, as expected in Euclidean geometry (Miller 1972).

The claim that Gauss made this measurement only to test Euclidean geometry is of course ludicrous. But a detailed examination of his geodetic work concluded that Gauss was bothered enough by the axiom of parallels to bring it up in frequent conversations, sometimes making mention of this large triangle: 'The myth of the BHI triangle as a deliberate test of Euclidean geometry appears a fanciful embroidery upon indubitable fact, encouraged possibly by reports made by Gauss in his inner circle' (Breitenberger 1984, 289). The precision of Gauss's trigonometric surveys was indeed extraordinary (Scholz 2004). In other contemporary surveys (that of Baron von Krayenhoff in the Dutch Counties, for instance) the error in closing triangles was often of the same order of magnitude as the correction that needed to be made to account for the curvature of the earth surface. In Gauss's survey, however, the closing error was smaller than the latter correction. In this context, Scholz wrote, it was imaginable for Gauss to provide a lower bound for the curvature of physical space, although he never expressed it that way—and for good reason, if we are to follow Gray (2006), since we have no indication that the key concepts of three-dimensional Euclidean geometry were ever truly achieved by Gauss.

If one had no need for non-Euclidean geometry to carry out a precise geodetic survey, nor did one need to be immersed in the tedium of measuring angles in the field to breed doubts about the validity of the parallel postulate, the fact is that to discover—or invent—non-Euclidean geometry one needed to spend much time developing a logically coherent edifice, not checking whether numbers added up. Mathematicians who were versed in observatory techniques knew only too well that absolute precision was not achievable. But they were also acutely aware of whether errors were significant or not. In his geodetic survey (as well as in his magnetic experiments, see Aubin 2005), Gauss used observatory precision technologies to extend the limits of what could be explained mathematically.

Indeed, what may be more significant for the invention of non-Euclidean geometry is the realization that physical and mathematical spaces need not coincide. Neither Girolamo Saccheri, Johann Heinrich Lambert, nor Adrien Marie Legendre, who had tried to show before Gauss that contradicting the parallel postulate led to inconsistencies, ever harboured doubts about the fact that they were working with physical space (Alexander 2006). By contrast, having served as head of the observatory in Kazan, Lobatchevski thought that the nature of physical space could be tested by precisely measuring the angles of a large stellar triangle. An alumnus of the Royal Engineering College in Vienna and a sub-lieutenant in the army engineering corps, Janós Bolyai was certainly familiar with geodetic techniques. When a correspondent of Gauss's, Ferdinand Karl Schweikardt, came up with the basic idea of a geometry where the sum of the angles of a triangle was not equal to 180°, he named it 'astral geometry', because he conjectured that one might be able to observe this departure from Euclidean geometry in triangles drawn in the heavens between stars. It is also highly significant that despite his qualms Gauss expressed his ideas about non-Euclidean geometry quite freely to other observatory directors such as Bessel and Schumacher.

To the scientist working in the observatory and in the field, the difference between physical and mathematical space perhaps went without saying. To illustrate the way in which observatory scientists might be drawn to special ideas about space, let me quote from Emmanuel Liais, the French astronomer who founded the Rio de Janeiro Observatory and extensively surveyed Brazil:

> In 1862, I was travelling through the Brazilian campos [...]. Constantly admiring the various but indefinite panoramas in front of me, my thoughts inexorably drifted towards immensity and my attention was caught by our ideas relative to space [*l'espace*]. [...] From the physical point of view, space indeed is another thing than from the point of view of mathematics.[10] (Liais 1882, 6–7)[11]

Liais went on to explain that physical space had many more properties than mathematical space, that even the fact that it could measured away from the earth was debatable and that mathematical space was a mere abstraction. Experience of space in the Brazilian wilderness or on top of German hills was certainly different to experiencing it in one's armchair. With theodolites, clocks, and numbers, observatory scientists constructed spatial networks. In these networks, observatories were crucial nodes that Bruno Latour (1987) has, for good reason, called 'centres of calculation'. Observatory scientists were thereby reconstructing physical space in a manner that went hand in hand with the reconstruction of the mathematical concept of space.

QUETELET AND STATISTICAL THINKING

From the perspective of the conceptual history of mathematics, the geodetic experience is less significant as an inspiration for non-Euclidean geometry than

10. En 1862, je circulais dans les campos brésiliens (...). En voyant continuellement des tableaux variés mais indéfinis se succéder, ma pensée se reportait invinciblement vers l'immensité, et mon attention se fixait sur nos idées relatives à l'espace. (...) L'espace, en effet, au point de vue physique est autre chose qu'au point de vue mathématique.

11. Although this comment was made long after Gauss's measurement, it is roughly contemporary with Bernhard Riemann's famous *Habilitation* lecture that brought non-Euclidean geometry to a large public (Gray 2005).

as a field where the least-square method was directly and systematically applied, in particular by Gauss (Rondeau Jozeau 1997). If errors in measurement were distributed according to the bell curve, Gauss showed that the most probable value for the 'true measure' was the mean value. In the history of statistics, observatory scientists are quite prominent (Sheynin 1984; Stigler 1986; Porter 1986; Armatte 1995; Desrosières 1998). But pride of place is often given to Quetelet, whose work, it was claimed, 'helped create a climate of awareness [. . .] that was to lead to truly major advances in statistical methods' (Stigler 1986, 215). With his book *On man* (1835), Quetelet tried to develop statistical methods in order to found sociology. As such, it was a major step in the development of mathematical tools for the social sciences, as well as in the design of general strategies for making mathematics relevant to the social realm. As the founding director of the Brussels Observatory, Quetelet drew extensively from an array of analogies he found in his daily practice. He introduced the central concept of the 'average man' as the formal analogue of the average position of a star deduced from several measurements. The distinction made by Laplace in the study of planetary motion between periodic and secular motion was also mobilized in Quetelet's work on social phenomena.

My claim is that Quetelet's debt to observatory culture is perhaps less deep but much wider than historians have usually been willing to admit. While historians have fallen prey to the temptation of over-interpreting the meaning of his formal analogies, they have neglected to consider the full range of observatory techniques he drew on. In the domain of number manipulation, especially, Quetelet mobilized the whole array of table construction, averaging, corrections, and data standardization. Observation was also organized in ways taken from observatory culture, with standardized instruments distributed across a network of trained observers. In my view, Quetelet therefore had ambitions to understand and perhaps manage the sublunar world (meteors, the weather, plants, animals, and humans) by applying to it the observatory techniques that helped to understand and manage time and space (Aubin, forthcoming).

The heuristic value of analogies with celestial mechanics first occurred to him at the time of Belgian independence in 1830. He later explained the growing importance such analogies would assume for him:

> At a time when passions were vividly excited by the political events, I sought to distract me by establishing analogies between the principles of mechanics and what was happening in front of my eyes. These rapprochements I had made without at first attributing more value than to a spiritual game later came to take the character of truth.[12] (Quetelet 1848, 104)

12. Dans un moment où les passions étaient vivement excitées par les événements politiques, j'avais cherché, pour me distraire, à établir des analogies entre les principes de la mécanique et ce qui se passait sous mes yeux. Ces rapprochements que j'avais faits, sans y attacher d'abord plus de valeur qu'à un jeu de l'esprit, me parurent ensuite prendre le caractère de la vérité.

This retrospective account is corroborated by several other documents, such as the letter Queletet sent to the minister Sylvain Van de Weyer on 22 August 1834:

The most interesting part of my work will be, I think, the theory of population. I was able to import it entirely into the domain of the exact sciences [. . .]. The great problems of population motion will be as solvable as those concerning the motion of celestial bodies; and what is most remarkable is the astonishing analogy that exists between the formulas that are used for the computations. I think I have partly realized what I have been saying for a long time about the possibility of making a social mechanics, just as we have a celestial mechanics.[13] (quoted in Delmas 2004, 57–58)

In his unpublished thesis, Michel Armatte (1995) also quoted portions from this interesting letter and discussed the way its author was clearly conscious of the analogical transfer of methods from celestial mechanics that he was operating. The question is: what exactly was transferred and how? At the conference organized for Quetelet's bicentennial in 1996, the historian of statistics Stephen M Stigler (1997) opposed the received wisdom according to which it was necessary to insist on Quetelet's astronomical training in order to understand the intellectual sources of his social thinking. Canonical thinking was that Quetelet had sought to repeat in the social sphere what Newton had achieved for the planetary spheres. Stigler thought that this was 'misleading':

The problem, as I see it, is that astronomy, as it was conceived in the 1820s, encompassed a much richer variety of mathematical and empirical problems that can be captured by any simple description; certainly it was much more than Newtonian or Laplacian celestial mechanics. It is quite proper to associate Quetelet with astronomy, but with which part?

The solution offered by Stigler deserves a closer look. According to him, Quetelet was neither the mechanician, nor the physicist, nor even the astronomer of the social, but its 'meteorologist'. It is true that at the Brussels observatory, which for many years lacked proper instruments, Quetelet spent as much—if not more—time working in meteorology and climatology than in either astronomy or the social sciences. He moreover published several books on Belgian meteorology and climatology compared to that of the world. But his scientific practice was intimately linked with the site he was establishing, that is, an observatory. To Quetelet, as far as scientific practice went, the meaningful category was not astronomy,

13. La partie la plus curieuse du travail sera, je crois, la théorie de la population. Je suis parvenu à la *transporter* entièrement dans le domaine des sciences exactes (. . .). On pourra résoudre les grands problèmes des mouvements de population comme ceux des *mouvements des corps célestes* ; et ce qu'il y a de plus remarquable, c'est l'étonnante *analogie* qui existe entre les formules qui servent à ces calculs. Je crois avoir réalisé en partie ce que j'ai dit depuis longtemps sur la possibilité de faire *une mécanique sociale comme l'on a une mécanique céleste*.

physics, or meteorology, but the observatory sciences. And while the misconception that astronomical practice in the 1820s could be reduced to Laplacian celestial mechanics has been a block to a proper understanding of Quetelet's thinking, there is no doubt that ,for someone like Quetelet, underestimating the unity of the observatory sciences and overvaluing disciplinary boundaries would not be of much help either. Like many of his colleagues and correspondents in observatories around the globe, Quetelet was not set on enlarging the dominion of Laplacian determinism. Rather, he was trying to adapt what he perceived as a coherent set of knowledge and techniques that characterized the practice of the observatory sciences to the needs of the world outside the observatory, whether physical or social.

Quetelet's practice in the social sciences is characterized by a strong faith in the quantification of the sciences. One should remember here that the quantification of statistics—that is, the 'science of the state', as it was still understood etymologically—was no trivial business and faced fierce resistance (Quetelet 1830). To him, numbers seemed more objective, less controversial, and less prone to betraying political and ideological a priori opinions than other types of description (on the history of objectivity, see Daston and Galison 1992). But Quetelet could draw on the observatory tradition for material and conceptual techniques to manipulate numbers in large quantities. Tables, equations, averaging, and graphical tools all figure prominently in his social physics, as well as probability theory.

In the 1830s and 1840s, Quetelet's network of collaborators in the physical and in the social sciences, in Belgium and abroad, expanded steadily. Standard instruments were distributed, procedures were shared. By mid-century, it seemed clear that greater coordination was needed. In 1853, Quetelet welcomed two international congresses to Brussels, within two months of one another. The first was devoted to navigation and climate science, under the inspiration of Admiral Matthew Fountain Maury, the head of the US Naval Observatory, while the second founded a series of International Statistical Congresses that is uninterrupted to this day. In both cases, the ideals of the observatory sciences were held in high respect. The aim was to set up vast instrumental networks covering the whole globe and churning out standardized numerical data. Historians have shown the major impact of this vision on the future development of mathematical statistics as well as the social sciences (Armatte 1995; Desrosières 1998).

In this story, the powerful influence of observatories would quickly wane. Quetelet's role in the history of mathematics was therefore not so much to use astronomical analogies at a conceptual level, as it was to adapt the very wide arsenal of tools he had found and developed in the observatory tradition in order to make them pertinent to the sciences of man. In so doing, he mobilized probability theory to an extent rarely done before by physical scientists, leading to important innovations by James Clerk Maxwell and Ludwig Boltzmann, who set out the foundations of statistical physics (Porter 1986). Similarly, this use of statistical and probabilistic tools led to the further development of mathematical statistics (Stigler 1986; Hacking 1990). As the nineteenth century unfolded, it became less and less a characteristic of the observatory to insist on the precise production of numbers, while the mathematical techniques developed for manipulating data were increasingly used outside the observatory. Mathematical statistics was no longer typically associated with the observatory (though some observatory scientists did contribute to it). But, significantly, it was again through the exact quantitative confrontation of mathematics with observations that techniques were developed for standardizing data on an international scale. Numbers extended their empire to society and, by the same token, so did the mathematical techniques for producing and manipulating numbers (Porter 1995).

POINCARÉ, ANALYSIS, AND CELESTIAL MECHANICS

International congresses similar to those Quetelet presided over in Brussels—the Congress for establishing a Prime Meridian, in Washington in 1882, the Geodetic International Conference in Rome in 1883, the Solvay Congresses, and so on—loom large in Peter Galison's account of the origins of relativity theory (2003). The close alliance of precision technology (clocks, telegraphs, and theodolites) with numerical precision, in short everything I have associated with the observatory culture of the early nineteenth century, are described as the basis for the material cultures of Albert Einstein and Henri Poincaré. At the beginning of the twentieth century, they had independently developed similar ideas about time and space—although claims in favour of Poincaré's contributions to relativity theory have been greatly exaggerated (Gingras 2007). But a clerk in a Bern patent office could not see things identically to someone sitting on various councils and bureaus. The worldview of a young theoretical physicist in the German cultural sphere was different from that of an established professor of mathematics, physics, and mechanics at the Sorbonne.

What Poincaré's story illustrates well in my opinion is that the extreme precision of observatory science provided incentives to re-examine the inner workings of its mathematical technologies. Poincaré had no intention of revolutionizing physics or mathematics. Instead of questioning Newtonian tenets, he wished to fill the blanks in the picture. In the process, he developed his own philosophy of science, conventionalism. Conventionalism proposes that the statements with which we choose to express the laws of physics, mechanics, and astronomy are used not because they are real but because, due to their simplicity, they are the most convenient we can think of. This was a very different attitude from Einstein's, who thought that new principles were needed to replace old ones.

In Galison's assessment, there thus was a form of 'optimistic modernism' in Poincaré's conventionalism.

Lately, our understanding of Poincaré's work has had to be reconsidered. One reason has been the recent discovery of an error he made. In 1889, he submitted a fundamental essay to a prize competition organized by Gusta Mittag-Leffler on the three-body problem. When Edvard Phragmén started to edit the paper and found the error, Poincaré was devastated. Reworking the argument, he was led to discover 'homoclinic' points, 'the first mathematical description of chaotic motion in a dynamical system' (Barrow-Green 1997, 71).

As opposed to the first draft of Poincaré's prize-winning essay, which 'conveys a sense of optimism about the ultimate resolution of the problem', the tenor of the second draft was 'quite different: the future progress of the problem has lost its air of inevitability' (Barrow-Green 1997, 75). 'Chaos' is of course the second reason why Poincaré's work is now seen in a different light (Aubin and Dahan-Dalmedico 2002). While some scientists and popularizers have hailed chaos as a new scientific revolution—the third of the century after relativity and quantum mechanics—others pointed out that it had first been explored towards the end of nineteenth or the beginning of the twentieth century (Hirsch 1984; Diacu and Holmes 1996). Most people, however, have agreed on one point—namely, that a new look at many parts of Poincaré's work (his memoirs on curves defined by differential equations, his study of the three-body problem in celestial mechanics, his pioneering work in dynamical systems theory and topology, his contributions to ergodic theory, and so on) played crucial parts in the emergence of chaos theory in the mid-1970s. While it is no doubt true that Poincaré's work foreshadowed concerns, and introduced key concepts and methods used in chaos theory, it is hard to explain why the great burst of activity only took place several decades after his death. This problem has given rise to various attempts to account for this 'nontreatment' (esp. Kellert 1993), but most have eschewed the admittedly arduous task of placing Poincaré among contemporary observatory scientists.

When he submitted his paper in 1889, Poincaré was not directly involved with the observatory.[14] But through his training at the École polytechnique he was fully aware of its scientific culture and trained in the use of theodolites and of the least-square method. Poincaré shared with Cauchy, Le Verrier, and Weierstrass a strong interest in the problem of the stability of the solar system. Further, his main sources very much belonged to the observatory: Hugo Glydén was director of the Stockholm Observatory; Andres Lindstedt had observed at Hamburg and Dorpat; George W Hill worked for the US Nautical Almanac Office.

14. Poincaré was nominated as a member the Bureau of Longitudes in 1893, joined the editorial board of the *Bulletin astronomique* published by the Paris Observatory in 1897, and the Paris Observatory Council in 1900.

A source for Poincaré's optimism may be found in observatory culture. Although the social history of celestial mechanics in the nineteenth century remains to be written, there is little doubt that it constitutes one of the most optimistic branches of science at a time when there was particular optimism about science. After the discovery of Neptune, the highpoint of celestial mechanics was perhaps Charles-Eugène Delaunay's publication of his *Moon theory* (2 vols, 1860; 1867). In these books, Delaunay pushed to the extreme the formal analytical expansion of a single function. He spent twenty years of his life developing it to the seventh order (and sometimes even to the ninth order), computing over 1259 terms in the expansion series for the moon's longitude and 1086 for its latitude. Although this extraordinary effort has sometimes been ridiculed, Delaunay's work is emblematic of the tremendous optimism invested both in the precision of the measurements made in the observatory and in the precision of the analytical method.

In the 1860s, however, mathematicians at the university and astronomers in the observatory were already starting to move apart from one another. The rise of astrophysics implied great changes in observatory culture (Le Gars 2007). New instrumentation had given rise to new problems about the physical nature of celestial bodies. To provide answers to these questions, mathematical tools seemed less useful than those taken from physics and chemistry. Similarly, the now fully professionalized mathematical community was shifting its focus (Lützen 2003). Unlike earlier generations of observatory mathematicians, Poincaré was no computer. 'The mathematical style of Poincaré was intensely modern. [...] Few of his results depend on long or difficult computations. He said of himself with a furtive touch of humor [...] that he was poor at arithmetic' (Veblen 1912, viii). Mathematicians were now emphasizing rigour, which led them to reconsider the concept of convergence. Where astronomers had been content with series whose terms decreased rapidly, mathematicians insisted that convergence had to be proved formally (Barrow-Green 1997, 18). For someone like Poincaré, rigour held the key to the elusive proof of the stability of the solar system.

If we follow Galison (2003), we recognize in Poincaré's conventionalism the technical world of diplomats, scientists, and engineers, where international conventions, telegraphy, and maps were used by modern states and businesses to control time and space. My account suggests that it was this same enterprise that required the foundations of mathematics to be opened up and examined anew. But for this task, a new generation of mathematicians, with few ties with the observatory, was coming along: they would focus more on the implications of Poincaré's work in logic, geometry, and philosophy than in old-fashioned celestial mechanics. A product of the mathematical culture of the observatory, Poincaré's homoclinic points did not seem fundamental enough to modern mathematicians, yet too mathematically rigorous to the observatory community. This is probably why very few people at the time were able to understand their significance.

CONCLUSION

For all the inaccuracies he is known to have perpetrated in his historical work, Eric Temple Bell was drawing attention to an interesting characteristic of nineteenth-century mathematics when he wrote:

> Too often for comfort, mathematics in the nineteenth century followed the same formula of glut without digestion as the rest of civilization in that heroic age of expansion at any cost. But according to the abstractionists of 1940, the discarnate spirit of simplicity was then about to descend and bless all mathematics, and the more rococo masterpieces of the nineteenth century were to be preserved only in museums frequented exclusively by historians. (Bell 1992, 410)

Like Bell's abstractionists, historians of mathematics have paid greater attention to the foundational aspects of mathematics than to the bulk of the mathematical work done in the period. By examining the place of mathematics in a specific but significant site, we have been able to grasp the significance of some of the 'rococo masterpieces' of observatory mathematics. Computing astronomical tables, eliminating errors in geodetic surveys, compiling social data, and analytically expanding solutions of differential equations represented massive efforts that led to impressive results. Other sites, like accounting offices, army training grounds, or engineering projects would similarly unveil interesting aspects of the mathematical practice of the period.

In the course of the nineteenth century, precision instruments, mathematical techniques of number manipulation, and social techniques for establishing standardized conventions became ubiquitous. Because of the prominent position occupied by the observatory in the nineteenth-century worldview, it had a special effect on mathematics as a discipline, and many mathematical innovations came out of the work of observatory scientists. But my study has shown that, more than what it directly contributed in terms of mathematical concepts or theories, the importance of observatory mathematics may lie in what it teaches us about transformations in the relationship between mathematics and the world. Or rather, observatory mathematics is an especially good platform from which to look at the way in which mathematics was transformed between 1800 and 1900 so as to become an autonomous logical construct—a construct that was actually made to account for the physical and social worlds that shaped each other.

An anonymous reviewer wrote in 1900 that:

> A really good history of mathematics in the nineteenth century has yet to be written; it would probably require the combined labour of an organised body of experts. [...] For the history of modern mathematics is not mainly that of individual discoveries, however brilliant; but that of the systematic investigation of mathematical notions such as 'number', 'continuity', 'function', 'limit' and the like. (GBM 1900, 511)

I hope to have shown that the intense examination of the abstract, foundational, and structural aspects of mathematics that was to characterize the next half-century was a direct consequence of collective efforts made by observatory scientists to construct *both* a world that could be mathematized *and* a mathematics whose basic concepts were precise enough to account for increasingly large chunks of that world.

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