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James Lequeux

Le Verrier— Magnificent and Detestable Astronomer

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James Lequeux

Le Verrier—Magnificent and Detestable Astronomer

Edited and with an introduction by William Sheehan

Translated By Bernard Sheehan



James Lequeux
Paris Observatory
Paris, France

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Preface

The name of Urbain Jean Joseph Le Verrier is well known today as that of one of the immortals of astronomy. It was he who discovered a planet, “with the tip of a pen, without any instruments other than the strength of his calculations alone,” as his colleague, François Arago, memorably exclaimed.

The planet was even referred to as “Le Verrier” for a short time though it is now, of course, known as Neptune. The story of its discovery is one of the best known, and most dramatic, in the history of astronomy.

The drama played out over the year 1845–1846 – a remarkable year, in and out of astronomy. Lord Rosse at Birr, Ireland, first made out the spiral arms of the nebula M51 in Canes Venatici at almost the very moment the Irish potato crop failed and brought famine and devastation to his tenants; the year England dedicated itself to laissez-faire economics on the basis of the principles of Adam Smith; the year of the Mexican-American War, opposed by a young congressman from Illinois named Abraham Lincoln; the year that US production of whale oil peaked; the year of the Oregon Trail; the year of Henry David Thoreau’s experiment in “living deliberately” at Walden Pond.

It was also a year in which the ledger sheet of the motions of the last major planet in the Solar System to be discovered, Uranus, was finally put in order. Soon after its unexpected discovery by William Herschel in March 1781, astronomers who calculated the orbit of Uranus and worked out its motions found that it was straying from its predicted path. The foundling on the outermost porch of the Solar System proved to be an obstinate and defiant child: it refused to do as the astronomers bade it. In the early 1800s, it was moving too fast, while after 1822 it began to move too slowly.

Prediscovery observations turned up in astronomers’ log books, including one dating back to 1690 by the Astronomer Royal at Greenwich, John Flamsteed, made while he was preparing a star catalog. These “ancient observations” should have made it easier to compute the orbit. Instead it was found that an orbit could be computed that satisfied either the old observations or the new, but not both. When, in 1820, the French mathematical astronomer Alexis Bouvard tackled the problem, he decided to throw out the old observations. Even this slander to the reputation of the

old observers did not suffice; the planet continued to stray – by 1832, when George Biddell Airy of the Cambridge University Observatory drew up a report to the British Association for the Advancement of Science, the discrepancy of Uranus's motion in longitude from Bouvard's orbit had amounted to 30 s of arc, an unacceptably large discrepancy by the precise standards of positional astronomy. Thereafter, things only proceeded to get worse.

The idea that an exterior planet was probably pulling Uranus off course occurred to a number of astronomers, including Alexis Bouvard himself (according to a statement by his nephew, Eugène, also an astronomer); the English amateur Rev. T.J. Hussey, who wrote to Airy in 1834 on the matter but received a discouraging reply; and Friedrich Wilhelm Bessel, mathematical astronomer extraordinaire at the Königsberg Observatory in Prussia. Bessel even took the step of assigning the problem of computing the orbit of the unknown planet to an assistant, Friedrich Wilhelm Flemming, but Flemming died soon afterward, and Bessel's health was also failing, and he died in March 1846.

Meanwhile, in England, Airy's report to the British Association had come to the attention of a 22-year-old undergraduate at St John's College, Cambridge: John Couch Adams. Adams was a remarkable man. Raised in rural Cornwall, he distinguished himself from an early age in mathematics and earned a place at Cambridge where he took all the mathematical honors in sight. On 3 July 1841, he jotted this memorandum in his notebook:

"Formed a design, in the beginning of this week, of investigating, as soon as possible, after taking my degree, the irregularities of the motion of Uranus, which are not yet accounted for, in order to find whether they may be attributed to the action of an undiscovered planet beyond it."

Adams's interest was entirely self-initiated. Still a student (and a very conscientious one at that), he did not have time to work in earnest on the problem until after his graduation in 1843. He had set himself an extraordinarily difficult problem. Effectively, it was an inverse problem in perturbations: it is always much more difficult to work out a cause from the effects than the effects from a cause. As the Irish astronomy writer Agnes M. Clerke later put it: "The difficulty of determining the perturbations produced by a given planet is small compared with the difficulty of finding a planet by its resulting perturbations. Laplace might have quailed before it; yet it was now grappled with as a first essay in celestial mechanics."

After his graduation, Adams remained on as a fellow at St. John's, but was still charged with many routine duties. Receiving from Airy (through an intermediary, Cambridge University Observatory astronomer James Challis) Greenwich Observatory data on the motion of Uranus and working mainly during the vacations, by September 1845 (using simplifying assumptions) he had wangled a tentative orbit and had produced a theoretical position for the planet which, as it turned out, put it within a degree and a half from where it was actually lurking at the time.

Adams, of course, is an extremely sympathetic figure. He was a brilliant mathematician, though prone to be perfectionistic; he was also, almost fatally, a faint and forgettable personality; one of his fellow students only remembered him as "a rather small man, who walked quickly, and wore a faded coat of dark green." The story is

well known about how he was failed by his superiors and how the British consequently failed to discover Neptune. To some extent, no doubt, his own naivete (and perhaps tendencies to Asperger's syndrome) played a role in this failure. He did not know how to promote his research and, on returning from vacation in Cornwall to Cambridge in October 1845, attempted an (unannounced) visit to Airy's residence at which he was not received. He only had time to drop a thumbnail sketch of his calculations in Airy's letter box. Unorthodox as Adams's method of publication was, Airy, who was the busiest civil servant in the land, did not lack interest and tried to follow up – on 5 November 1845, he wrote to Adams about the “radius vector” problem. An exterior planet would, after all, produce both tangential and radial displacements from the unperturbed orbit. So, Airy was asking whether the same theory that corrected the error in Uranus's longitude also corrected that in the radius vector. The question was a good one, and it also happened to be one to which Airy himself attached the greatest importance (he even called it the *experimentum crucis*). But Adams never replied. (We now know that he started to do so; a draft, dated 13 November 1845, has turned up in the Adams papers in the Cornwall Record office in Truro, which begins: “Sir—I must apologize for having called at the observatory the other day at so unreasonable an hour, the reason was that I had only arrived in town that morning & it was necessary for me to be in Cambridge the same day, so that I had no other opportunity. The paper I then left contained merely a statement of the results of my calculation....” But Adams breaks off before getting around to answering Airy's question. He is known to have disliked writing letters. On the other hand, perhaps he did not quite know how to answer.)

Whatever the reason for it, the lapse proved critical. Airy, nothing if not regular in his habits, would later recount: “I waited with much anxiety Mr. Adams' answer to my query. Had it been in the affirmative, I should at once have exerted all the influence which I might possess... to procure the publication of Mr. Adams' theory.” Not hearing from Adams, however, as Airy afterward explained to the Cambridge geologist Adam Sedgwick (Airy to Sedgwick, 4 December 1846), “not only left the matter in an unsatisfactory state ... but also entirely stopped me from writing again.”

And so, as of November 1845, the trail of the British planet investigation went cold even as an impressive series of results began to trickle in from France. In the *Comptes rendus* of the French Academy for 10 November 1845, which did not arrive in England until December, Le Verrier published a paper summarizing an investigation (instigated, he says, at the request of Arago) on the perturbations of Uranus produced by Jupiter and Saturn. Airy later said of this impressive paper: “I shall only say that, while the correctness of the former theories, as far as they went, was generally established, many small terms were added; that the accuracy of the calculations was established by duplicate investigations, following different courses, and executed with extraordinary labour; that the corrections to the elements, produced by treating the former observations with these corrected perturbations, were obtained; and that the correction to the ephemeris for the present time, produced by the introduction of the new perturbations and the new elements, was investigated, and found to be incapable of explaining the observed irregularity of Uranus. Perhaps it may be truly said that the theory of Uranus was now, for the first time, placed on

a satisfactory foundation.” Here, indeed, was a substantial piece of work and, moreover, *published* work. The contrast (in Airy’s mind at least) between Le Verrier’s accomplished labor and the rather shadowy promissory investigation being pursued by Adams could hardly have been greater.

Nothing more took place until the following June. On 1 June 1846, Le Verrier appeared before the public again with a summary of calculations in the *Comptes rendus* in which he offered a sketch of a theory of an exterior perturbing planet and gave the position where it ought to lie in the heavens. That position, near the Aquarius/Capricorn border was, as Airy noted toward the end of the month when the publication reached England, uncannily close to the one which Adams had casually dropped in Airy’s letter box the previous autumn. About to leave for Europe, Airy took pains to entrust to Challis, who had the large Northumberland refractor of the Cambridge University Observatory at his disposal, a stealthy search to find the planet. Airy himself laid out the parameters of how the search should proceed. Challis cannot be said to have been enthusiastic, but he was conscientious, and at the end of the month, somewhat ploddingly, he embarked on a 2-month survey of the stars in the region indicated, looking for the planet. The search would notoriously miscarry.

In the English-speaking world – to which this translation of James Lequeux’s masterful biography of Le Verrier is addressed – Adams has always been the young man let down by his superiors, the underdog whose brilliance was unrecognized. Le Verrier, though there has never been any doubt as to his great ability, enters the narrative as a rather cold and remote figure. It is usual to picture him as an established scientist, in contrast with the young and inexperienced Adams, the assumption being that he commanded the full support of his superiors as well as all the resources of the French scientific establishment. We must remember, however, that Le Verrier, born in the provincial town of Saint-Lô in 1811, had originally been trained as a chemist and was a protégé of the famed chemist J.L. Gay-Lussac; he had switched to mathematical astronomy in 1837 when Gay-Lussac, having to choose between him and Victor Regnault, a chemist of equal promise, for a single vacant position, neatly solved the dilemma by offering Le Verrier a position of equal rank in astronomy. Malleable to circumstance, he had achieved in 2 years, through a level of concentration and intelligence that still seem little less than marvelous, sufficient mastery over the arcana of celestial mechanics to begin to contribute important and original research. When Arago proposed that he tackle Uranus, he was still only 34.

Apart from the fact that Le Verrier was acting as a professional on an assigned project, rather than as a self-directed student like Adams, he too worked without assistance. The papers that caught Airy’s attention were the tip of the iceberg; they were mere summaries. In fact, Le Verrier’s “Uranus” file in the Paris Observatory Library, which culminates with the discovery of a planet, contains hundreds of pages, full of tables and intricate calculations, only a small part of which has ever been published and not studied thoroughly since Felix Tisserand did so at the end of the nineteenth century. Having held this sheaf of paper in my hands and shuffled through the pages, I can hardly begin to convey the sense of awe they inspire. Adams’s papers are also impressive, but more scattered. Needless to say, no one can

fully appreciate the intelligence required to achieve what Le Verrier and Adams accomplished who has not carefully studied the manuscripts in which they laid out their subtle calculations.

The idea that Le Verrier enjoyed the cooperation of his French colleagues whereas Adams was failed by Airy and Challis is also a myth. Le Verrier himself was not employed at the Paris Observatory at the time; he had a position with the Bureau of longitudes, but even before he embarked on the Uranus problem, his relations with some of his French colleagues were already seriously strained. Despite Arago's undoubted personal brilliance, he was the exception: as a whole, the Paris Observatory was then highly dysfunctional, under the thumb of the Bureau, and possessed of old, unproductive, self-satisfied astronomers patrolling the skies with an array of doddering instruments. It is rumored that a brief search for Le Verrier's planet was mounted in Paris, though the details are vague; if it was, it seems that Le Verrier himself was never made aware of it. Regardless, the French search was soon given up as hopeless. Indeed, Le Verrier was compelled to seek assistance from abroad. After Airy received the 1 June 1846 *Comptes rendus* paper, he submitted to Le Verrier the very same question he had put to Adams about the radius vector. Le Verrier responded decisively and affirmatively and also appealed to Airy to take up the cudgel for his planet by mounting a search for it from one of the well-equipped observatories in England. He even offered to send Airy refinements in his calculated position in due course. Now, however, it was Airy's turn to become unaccountably silent: he would not, in fact, communicate with his French colleague again until after the planet's discovery, a circumstance that, once Adams's so far secret efforts and the extent of the British search became known after the planet's actual discovery, would ultimately prove a matter of considerable embarrassment to Airy and come close, as Airy himself admitted, to provoking a war with France.

Le Verrier was kept completely in the dark about developments in England. If there was anything nefarious about Airy's actions, it was no doubt, as the historian of astronomy Robert W. Smith has pointed out, merely a matter of wanting the planet's discovery to redound to the honor of Cambridge – Airy, Challis, Adams, and Sir John Herschel were all Cantabridgeans. They acted in loyalty to the old school tie. Challis might have been successful if he had possessed an up-to-date star chart of the region where Adams and Le Verrier put the planet. He did not; therefore, he had to go ploddingly through the stars, with the diligence of the maker of a star chart. Adams's investigation, dormant apparently since the previous November, was also reanimated; he began revising his calculations and also produced for Challis a set of ephemerides to guide his search. The first date of the ephemeris is 20 July. It is a duplicate calculation: the first set is based on the assumption that a "lost" planet recorded by Louis Wartmann of the Geneva Observatory in 1831 might have been the sought-for planet; the other calculation is based on Le Verrier's theory. The existence of this paper (in the Challis papers at Cambridge University Library) shows that Adams himself was aware of what Le Verrier had done, and though Challis undoubtedly grasped that both men had put the planet within the search area defined by Airy, the ephemerides he was using were explicitly for Le Verrier's planet, ironically as calculated by Adams. Starting his search on 29 July,

Challis actually recorded the planet twice, on 4 and 12 August. However, he failed to compare his observations.

The rest of the story is well known. While Challis continued his dogged search and Adams continued, at times with an air of desperation, to tinker with his calculations, Le Verrier published another paper – on 31 August – suggesting that the planet might most easily be disclosed by its disk. Other searchers in England were beginning to catch the scent: notably, William Lassell, who had built a 24-in. equatorial reflector and who seems to have heard of the putative planet from his friend the Rev. W.R. Dawes but who was “indisposed” at the moment of opportunity with a sprained ankle. Le Verrier, meanwhile, established communication with Johann Gottfried Galle, an assistant at the Berlin Observatory. Galle was enthusiastic and managed to get the permission of the director Encke for the search (the latter being in a perhaps more congenial mood than usual as he was headed home for a celebration of his birthday). Assisted by a graduate student, Heinrich Louis d’Arrest, who remembered that a star chart of the region had recently turned up and found it in a cubbyhole outside Encke’s office, Galle detected the planet after an hour’s search around midnight on 23/24 September 1846. The memorable exclamation of d’Arrest ranks among the most thrilling words ever uttered: “That star is not on the map!” The “star” was Le Verrier’s planet, and was found within a degree of where Le Verrier’s calculations had put it.

There followed, of course, the international priority dispute, which has been well summarized in James Lequeux’s biography and elsewhere. Laurel wreaths of credit for the co-prediction would eventually be settled on both Le Verrier’s and Adams’s heads, through a carefully worked-out international compromise not unlike the Congress of Vienna that determined the balance of European powers after the Napoleonic Wars. Each was hailed as a codiscoverer. The animus that existed between England and France at the time has long since been forgotten, and the discovery was certainly great enough – the “zenith of Newtonian mechanics” – to cover more than one man in glory (or ignominy, in the case of Airy and Challis). But the great event had very different consequences for each of the two men, based on their very different personalities. Adams remained bashful, retiring, modest, prone to blushing. He took like a fish to water to the quiet life of an eccentric Cambridge don and spent most of his career unwinding the complicated motions of the Moon. Le Verrier, on the other hand, with steely determination, tremendous organizational abilities, and an almost pathological need to impose order on everything and everyone he came in touch with, used his great fame as the discoverer of Neptune to begin a very public, productive, but often acrimonious career as the director of the Paris Observatory. In the end, despite facing opposition that would have broken most men, and being pulled down by his own disgruntled staff (though he was subsequently reinstated), he would leave the mark of his strong personality on every aspect of French science. Among his achievements was not only mastery of the motions of the planets but the introduction of telegraphy to timekeeping and longitude determinations, as well as pioneering work in meteorology.

Though the discoverer of Neptune is presented here in his full glory, the less familiar post-Neptunian giant of French science appears here for the first time, at

least to the English-speaking world. For Le Verrier lived another 31 years to the day after the celebrated discovery of Neptune. His later career, rich with accomplishment, has been comparatively little studied. James Lequeux's long-overdue biography at long last restores some sense of balance.

James Lequeux's book is not only authoritative, and likely to remain so for the far foreseeable future, it is also well timed, as 2011 marked both the bicentennial of Le Verrier's birth and the celebration of Neptune's first complete orbit of the Sun since its discovery. Four times, in 2010–2011, Neptune returned to the same field of stars where, on 23/24 September 1846, Galle spotted it and d'Arrest exclaimed: "That star is not on the map!" We will have to wait another 165 Earth years for it to do so again.

Willmar, MN, USA

William Sheehan

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Chapter 1

The Years of His Youth (1811–1845)



Comet Donati seen above
Notre-Dame in Paris on 4
October 1858

The most comprehensive text on the youth and first works of Le Verrier is °Bertrand J. (1880).

Fig. 1.1 The birthplace of Le Verrier, place du Champ de Mars, Saint-Lô. This house was destroyed in 1944



Urbain Jean-Joseph Le Verrier was born on 11 March 1811 in Saint-Lô (Manche). His father, Louis-Baptiste Le Verrier, was an estates manager, born in Carentan.¹ His mother, Marie-Jeanne-Josephine-Paulie de Baudre, was born in Baudre.² Carentan and Baudre are two communities close to Saint-Lô; the grandparents of Le Verrier were also born in the region. The house in which he was born (Fig. 1.1) was a quite modest bourgeois home, attesting to their limited means.

First a student at the local College of Saint-Lô, Le Verrier entered as intern at the College royal de Caen where he studied mathematics from 1827 to 1830. The latter year, he presented himself at the entrance exam for the École polytechnique, but failed, to everyone's surprise, for he was a very brilliant student. His father decided to send him to the Mayer Institute in Paris, and sold his house to defray the cost. After 1 year of study under the direction of the mathematician Choquet, the director of this institution, Le Verrier obtained the second prize in mathematics in the *Concours général* (a national competition), and was received in the École polytechnique in 1831, at age 20, which was typical of the period. He graduated in eighth place at the Polytechnique 2 years later, in the tobacco engineering corps, and

¹ From the birth certificate of Le Verrier, reproduced in *Centenaire de la naissance de U.-J.-J. Le Verrier* (1911).

² A summary of the life of Le Verrier, placed in the context of the time, is to be found as Appendix 1.

followed for two additional years courses at the school devoted to this industry, situated at the time at the quai d'Orsay in Paris. His studies consisted largely of chemistry, in which our young man interested himself to the point where he began researches in a small laboratory set up at his home. In 1835 he published a first article on chemistry; another followed in 1837.

The Beginning of a Scientific Career

The first two papers of Le Verrier³ were concerned with the combination of phosphorus and oxygen; there was nothing really surprising in that, for phosphorus is a constituent of matches, and as such fell within the purview of the school subserving the Parisian tobacco products industry. It was a very classic and conventional investigation, therefore, even though the experiments were dangerous, carried out under the direction of Louis Joseph Gay-Lussac with whom he had become acquainted at the École polytechnique. His work, however, was of high quality, and would be cited long afterward by chemists. In particular, he attracted the attention of Dulong, a scientist who carried out risky experiments which caused him to lose one eye, and the chemist Balard. The second paper by Le Verrier was written after he had left the École des Tabacs, for he was supposed to accept a position in the provinces: he remained, instead, in Paris, and made a living as a mathematics tutor and by teaching at the Collège Stanislas, all the while continuing to work with Gay-Lussac. He married in 1837 Lucile Marie Clotilde Choquet, the daughter of his former professor. She was only 17 years old, while he at the time was 26.

The Le Verriers were to have three children:

- Leon, born in 1838, a chemist and an engineer with the Western railroads, who died suddenly, without descendants, in 1876, probably by suicide;
- Urbain-Louis-Paul (1849–1911), a mining engineer, professor at the *Conservatoire des arts et métiers* (Conservatory of arts and crafts) and at the *École supérieure des mines*. He was to have a daughter and two sons;
- Marie Geneviève Josephine Lucile (1853–1931), who would marry in 1875 Lucien Magne, inspector general of historical monuments, professor at the École des Beaux-arts (School of fine arts) and at the Conservatory of arts and crafts. She would have two children, both painters. Lucile authored a charming journal, published in 1994, in which one finds information on the day to day life of the Le Verriers.⁴

Besides these, Le Verrier had a sister, whom he employed as his housekeeper after his marriage, but with whom he fell out definitively in 1858. One knows little about her other than that a tobacco shop was attributed to her in November 1846 when she left her brother, and that she was still alive, but poor and ill, in 1878.

³ *Annales de Chimie et de Physique* 60 (1835) pp. 174–194; 65 (1837) pp. 18–35.

⁴ Le Verrier Lucile (1994).

Fig. 1.2 Claude-Louis Mathieu (1783–1875). Brother-in-law of Arago, Mathieu became one of the enemies of Le Verrier



In 1836, two positions as *répétiteur* (private tutor or assistant teacher; someone who explains to the students the professor's lectures) at the École polytechnique became vacant: one in chemistry and the other in astronomy. Le Verrier applied for the former, but it was Victor Regnault who obtained it (he would go on to become a celebrated chemist). As Le Verrier needed money, and was a good mathematician, he then applied for the other post, which he obtained. With a surprising facility, spurred by an ambition already solidified, he completely changed the orientation of his researches. He wrote to his father⁵:

In daring to accept the duties which had successively been fulfilled by Arago, Mathieu [Fig. 1.2] and Savary, I have imposed on myself the obligation not to let the post which they have occupied be depressed in the public esteem, and for this I must not only accept but seek out opportunities to extend my knowledge. [...] I have already ascended many ranks, why should I not continue to rise further?

Le Verrier became the *répétiteur* of Felix Savary, a good friend of Arago, for whom he himself had been *répétiteur* between 1828 and 1830, and who succeeded him as professor. In 1840, Savary became gravely ill and Le Verrier replaced him on December 11. Savary retired to the home of Arago's family in Estagel (Eastern Pyrenees), where he died on July 15, 1841. Le Verrier would not succeed him, contrary to what was frequently the case, and would only cover the course until February 1841: it was Michel Chasles who was nominated professor of Machines [Mechanics] and Hydraulics, Astronomy and Geodesy at the École polytechnique, and would give the course until 1850. It's true that Chasles was already a celebrated geometer, 18 years older than Le Verrier.⁶ The latter therefore remained as *répétiteur*. He must have been affected by

⁵ Letter cited by °Bertrand J. (1880).

⁶ Le Verrier had later another relationship with Chasles concerning an affair of fake manuscripts, see later Chap. 6.

Fig. 1.3 Pierre-Simon Laplace (1749–1827) in old age. Posthumous engraving by Harbivillier (1833)



being passed over, but he had his revenge in the latter year (1850) on being appointed chairman for the mixed commission on the reform of admissions and instruction at the École polytechnique (we shall say more about this in Chap. 3), and besides, Le Verrier was by then involved in astronomy, which was the most important thing.

At this period astronomy concentrated on the positions of the stars, and more especially the movements of the planets relative to them, and made use of the methods of celestial mechanics which predicted these movements. Pierre-Simon Laplace (Fig. 1.3), the greatest representative of celestial mechanics in France and in the world in his time, died in 1827, and there was no person of the same caliber to succeed him. Le Verrier, who hardly had any sense of his limitations, was to try: in the words of Jean-Baptiste Dumas, “Laplace’s inheritance was unclaimed; and he boldly took possession of it.”⁷ It would only take him 2 years before he presented to Academy of sciences the first fruits of his investigations into celestial mechanics.

To understand what was involved, we must backtrack some two centuries. Johannes Kepler had established the laws of motion of a planet in its orbit, and Isaac Newton had explained these laws by means of universal gravitation; in this particular case, by means of the attraction of the Sun on the planet. Newton showed

⁷ Discourse at the funerals of Le Verrier, 25 september 1877.

that planetary orbits are not exactly Keplerian, for the planet is subject to perturbations caused by the forces of attraction of the other planets. It is therefore necessary, if one wishes to know the orbit of a planet with high precision, to take into account these perturbations. It is to this project that Newton's successors, such as Leonhard Euler, Alexis-Claude Clairaut and Jean le Rond d'Alembert, applied themselves. The solution of the perturbations involving the Sun, Moon, and planets led to great progress in mathematics: especially in calculus, both differential and integral, power series, trigonometric series, etc. Synthesizing this work, Joseph Louis Lagrange in 1767 introduced the method that would be perfected by Laplace and used with variations by all their successors: this is explained in Box 1.1. The method requires integrating a set of differential equations; it is in this integration

Box 1.1 Perturbations of a Planet by Other Planets

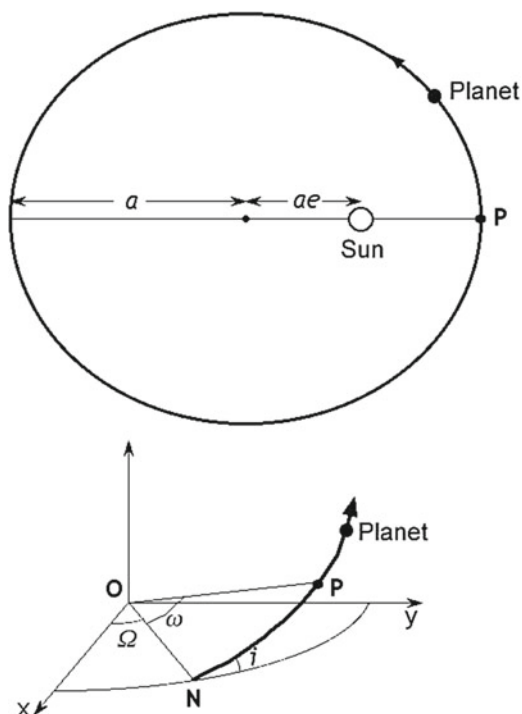
It has been known since Kepler that the planets follow elliptical orbits, in which the Sun occupies one of the foci (Fig. 1.4).⁸ Such an orbit is described by six parameters: the semi-major axis of the ellipse a , the eccentricity e , the inclination i to a reference plane (which could be the ecliptic, that is the plane of the earth's orbit), the longitude Ω of the ascending node (that is the intersection of the ellipse of the reference plane where the planet is ascending or directed toward the north), the longitude ω of the perihelion, the point of the orbit closest to the Sun, measured along the orbit starting at the ascending node, and the instant, τ , the planet passes perihelion. The laws of mechanics allow one to know the position of the planet in its orbit at time t , as a function of $(t-\tau)$.

This is true only as a first approximation: the gravitational attraction of the other planets perturbs the movement of the planet under consideration, and deforms its orbit. The analytical method generally used to solve this problem is as follows: the orbit of the planet at a given instant is regarded as a section of an ellipse (this ellipse is called the *osculating orbit*), which is characterized by the six parameters described above. These are referred to as the *osculating elements*. They would be constant were there no perturbations, but the perturbations cause small variations in the values of these parameters, which can be evaluated for a given instant, given the positions and masses of the other planets. The movement of the planet in the next instant follows from the osculating elements of an orbit being perturbed in this way. If one wishes to know what the movement is during a longer interval, it is necessary to integrate a system of differential equations, consisting of a set of mean elements with secular and periodic terms that describe the variations in time of the osculating elements.

⁸ See C. Wilson (2001) *Celestial Mechanics in the Eighteenth and Nineteenth Centuries*, in *Encyclopedia of Astronomy and Astrophysics* 4 vol., IOP Publications, London, Vol. 1 pp. 276–282. A clear and simple account (in French) can be found in *Tisserand and Andoyer (1912), pp. 267–279; this text dates from 1885 and still ignores the difficulties raised by Henri Poincaré concerning long-term stability of the Solar system.

Fig. 1.4 The parameters of the orbit of a planet. *Top*, the orbit seen from the north. P is the periastron (here the perihelion), a the semi major axis and e the eccentricity.

Bottom, a portion of the orbit in space. i is the inclination of the orbit with respect to a reference plane xOy , Ω the longitude of the ascending node N with respect to the direction Ox and ω the longitude of the periastron P with respect to N , in the plane of the orbit



that the difficulty of the problem consists. One can try to accomplish the integration by an analytical method, as was done for a long time before modern numerical means of calculation became available. This involves expanding as a series the perturbation terms, a strategy that leads to calculations more or less difficult according to the precision required: in effect, the solution deviates less and less from the exact solution as one includes more and more terms in the series. Potentially, there can be hundreds, or even thousands of terms. The power of computers nowadays allows solution of the problem by a numerical technique in which one calculates directly the force $\mathbf{F}$ exerted on the planet due to the various gravitational attractions to which it is subjected; then its acceleration, $\mathbf{a}$, is given from Newton's law $\mathbf{F} = m\mathbf{a}$, where m is the mass of the planet; one then deduces its speed from two successive integrations, and its position as a function of time. The analytical methods, however, still have value for verification, because they allow one to appreciate the contributions of each of the individual forces, the effects of which are not mixed together as in numerical integration.

In his *Traité de Mécanique céleste*, the culmination of all the work in celestial mechanics of the eighteenth century, Laplace used only a relatively limited number of terms for the series describing the perturbations, and everything seemed to work out well enough: the Solar System seemed stable and, even if the orbital parameters of the planets varied slowly over time, these slow variations were periodic, with a period that could be as long as several centuries. But it was realized (notably, by

Siméon Denis Poisson⁹) that if higher order terms were introduced, difficulties appeared: certain elements seemed to grow or increase without limit, even though very slowly, a circumstance which called into question the long-term stability of the Solar System. Moreover, it did not seem impossible that Laplace had omitted certain small terms. Even if Laplace had been convinced of this stability, Le Verrier entertained doubts, and was to renew the attack on the problem of the stability of the Solar System by considering as large a number of terms as possible.¹⁰

The first part of Le Verrier's work was completed in September 1839, as can be seen by the résumé of his published memoir, in the September 16 issue of *Comptes rendus des séances de l'Académie des sciences*.¹¹ One can only admire the mastery acquired in only 2 years of application to a subject so difficult. The Commission charged with reviewing his memoir, composed of François Arago (Fig. 1.5, Box 1.2), Felix Savary and Joseph Liouville, recommended its publication in the *Recueil des mémoires des savants étrangers* (i.e., of scientists who were not members of the Academy of sciences). Finally the memoir was printed in 1840 in the *Connaissance des temps* for 1843 (one may wonder about the difference in these dates, but one must realize that the ephemerides of the *Connaissance des temps* were of no value for preparing observations unless they were published well ahead of the period to which they applied).

Le Verrier next returned to a work of Lagrange, who in 1782 had studied the variations over time of the orbital parameters of the various planets, and had noted the important role that the masses of the planets play in this particular problem. As some of these were still rather poorly known, he repeated Lagrange's calculations by varying the masses. In passing, he pointed out the errors in the *Système analytique du monde* of his contemporary Gustave de Pontécoulant, a mediocre researcher, who immediately became Le Verrier's enemy (and here Le Verrier was in good company, for Arago also had a run-in with Pontécoulant). The following year, Le Verrier published an important new report, summarized in the *Comptes rendus des séances de l'Académie des sciences*. The full report was published in the *Connaissance des temps* for 1844. Le Verrier shows that the third-order terms in the perturbation functions, hitherto regarded as negligible, can become very important because the denominators are close to zero; however, for the same reason they are not easy to take into account. Though hardly affecting calculations of the movements of the giant planets, Jupiter, Saturn, and Uranus, whose orbits are accordingly quite stable, in the case of the inner or terrestrial planets, movements over long time periods cannot be predicted with any precision. Le Verrier affirms: "The direct integration of the equations, including the third order terms, is the only way around these difficulties." This integration, however, was impossible at the time, and Le Verrier thus came to doubt the long-term stability of the orbits of the inner planets.

⁹ Poisson S.-D. *Connaissance des temps* for 1831, additions, pp. 23–48.

¹⁰ Marie-Charles Théodore de Damoiseau in France and Giovanni Plana in Italy made similar studies concerning the motion of the Moon.

¹¹ *CRAS 9 (1839) pp. 370–372.



Fig. 1.5 François Arago (1786–1853), portrait by Charles Steuben (1832)

Box 1.2 François Arago (1786–1853)

Born into a petite bourgeoisie family, François Arago was noticed in 1805 at the École polytechnique by Laplace, who hired him for the Paris Observatory where he would remain for the rest of his life and which he led from 1834 until his death.¹² After interesting researches, carried out alone or under the direction of Biot, he departed with the latter for Spain in 1806, in order to extend as far as the Balearic Islands the measurement of the meridian arc of Paris, which Delambre and Méchain had carried only as far as Barcelona. After numerous

(continued)

¹² See Lequeux (2008) for a detailed scientific biography.

Box 1.2 (continued)

difficulties, and a veritable odyssey across the Mediterranean during the war between France and Spain, he returned to France in 1809 having accomplished his mission, covered in glory, and was elected at 23 years of age to the Academy of sciences. A fruitful period followed in which he discovered chromatic rotational polarization, the gaseous nature of the Sun and stars, and the polarization of the sky, the Moon, and comets. He brought the young Fresnel to wider notice and was associated with him in his work on the undulatory theory of light. He worked also with Ampère on electromagnetism. Elected perpetual secretary of the Academy of sciences in 1830, Arago founded the *Comptes rendus* of the Academy in 1835 and was the most influential French scientist of his epoch. He was also an important political figure who played a role in the ephemeral Second Republic following the Revolution of 1848, in which he, with Victor Schoelcher, abolished slavery and presided for a time over the destiny of France. He died ill and blind in 1853.

Encyclopaedic in his breadth of knowledge, a great promoter and popularizer of science and technology, and an acute discoverer of talent, Arago suggested to Le Verrier that he should study the perturbations of Uranus, which would lead to the discovery of Neptune. However, the two men soon fell out with one another. The reign of Arago at the Observatory was a period teeming with ideas; however, the realization of them sometimes fell short, because Arago was always under the finger of the Bureau of longitudes, and did not surround himself with collaborators worthy of him.

Henri Poincaré's researches much later and recent quantitative measurements have shown that Le Verrier's doubts were indeed justified. The Solar System can be considered stable over the short term; however, over the very long term—we are talking here about billions of years—its behavior becomes unpredictable (chaotic), especially as regards Mars and Mercury and to a lesser degree Venus and the Earth. The orbits of the Giant Planets are by comparison very stable (Fig. 1.6). It is not entirely out of the question that Mercury might someday be ejected from the Solar System.¹³

¹³ See for example Laskar J. *Astronomy & Astrophysics* 287 (1994) pp. L9–L12, and Laskar, J. and Gastineau, M. *Nature* 459 (2009) pp. 818–819.

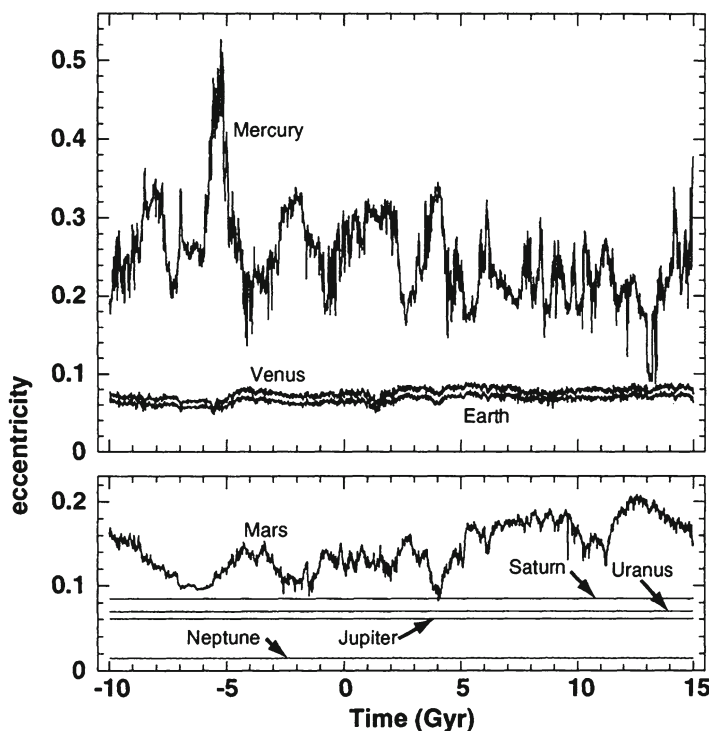


Fig. 1.6 A possible very long term evolution of the Solar system, from a numerical simulation of Jacques Laskar. The eccentricities of the orbits of the different planets are plotted as a function of time. Time zero is the present time. The calculation goes from -10 billion years, which has no physical sense until -4.6 billion years, the age of the Solar system, to 15 billion years in the future. Because the long-term behavior of the Solar system is chaotic, other simulations with slightly different initial conditions give very different results. One can see that the orbits of the four major planets are relatively stable, while those of the minor ones are highly unstable

Early Recognition

This work made Le Verrier well known among astronomers, and led to his nomination for membership in the Bureau of longitudes. This organization, created by the Convention in 1795, took as its mission “to make shipping concerns flourish and to benefit the maritime and commercial interests through the development of astronomy.”¹⁴ Its role was thus, at least in principle, entirely subservient to navigation and geodesy, with minor incursions as well into meteorology. It had at its disposal the Observatory of Paris, also that of the École Militaire, and “all the astronomical instruments belonging to the entire nation, so that in

¹⁴For an history of Bureau of longitudes, see Bigourdan (1928–1932) and Lequeux (2008), Chap. 1.

Fig. 1.7 Jean-Baptiste Biot (1774–1862)



practice its field of action extended to all public-sponsored research in astronomy. The staff of the Bureau consisted of tenured members, at a salary of 8,000 livres at the time of the founding, and of assistant members, at a salary of 4,000 livres. The latter salary sufficed at the time for a modest livelihood. On 25 January 1841, the physicist and astronomer Jean-Baptiste Biot (Fig. 1.7), who was interested in the advancement of Le Verrier's career and would remain for a long time sympathetic to him, wrote to the President of the Bureau, the navigator Louis Claude de Freycinet, as follows¹⁵:

My dear colleague,

Since our last meeting I have learned some details about M. Le Verrier that have been unknown to me, and which might be also to other members of the Bureau. M. Le Verrier is 29 years old, is married, has an infant and, living only by his modest position as répétiteur at the École Polytechnique, is obliged each day to relegate to private lessons a portion of time and effort which he would otherwise employ so well, and with such ardor, toward the perfection of theoretical astronomy. Would it not be a sort of barbarism to let him stay in the present situation, in order to gratify someone already advanced in age, who has not rendered any useful service to astronomy and who is therefore assured of a completely different career? Would not acting in this way pervert the charter of institute of the Bureau of Longitudes? Is this the example that our predecessors have given us? I submit this question to your conscience as well as to your reason.

Your devoted colleague,

J.-B. Biot

Despite Biot's passionate appeal, Le Verrier was not elected, but neither was the candidate who opposed him. Instead the post would remain vacant for two years before being filled by Victor Mauvais.

¹⁵ Reproduced in *Centenaire de la naissance de U.-J.-J. Le Verrier* (1911) p. 10.

This setback at the Bureau of Longitudes did not prevent Le Verrier from zealously continuing his researches in celestial mechanics. He was interested especially in the movement of Mercury, a planet difficult to observe because it is always very close to the Sun, and for which observations were therefore few and far between. Summarizing the history of the subject, Le Verrier would say¹⁶:

No planet has demanded from astronomers more care and pain than Mercury, nor given them as recompense so much worry, or presented them with so many contradictions. The planet has been as problematic as its namesake terrestrial mercury [the metal] was for alchemists. Riccioli had simply stated the opinion of the astronomers of his time, and that of his predecessors, in the words: "If I knew someone, says Maestlinius [Mastlin], who was occupying himself with Mercury, I would feel obliged to write to him and counsel him charitably that he would better employ his time otherwise."

But fortunately this attitude was not shared in France. Le Verrier, therefore, continues:

The theory of Mercury could be taken up again today to advantage. The meridian observations of this planet have been multiplied for the past forty years. Thanks, in particular, to the zeal and persevering skill of its astronomers, the Paris Observatory now possesses more measurements of Mercury than any other observatory in Europe. In recent times, from 1836 to 1842, two hundred useable observations of Mercury have been carried out; it is a prodigious number considering how difficult this planet is to see in our climate....

As to precision, the preeminence again belongs to France. The examination of a large number of observations of the Sun has shown me that the average error of the observations does not exceed 1/17 of a second in time at the Paris Observatory. This admirable result attests to the perfection of the observations.

Owing to the generosity of the illustrious leader of our observatory, M. Arago, I have had the opportunity to draw without reserve from this precious store.

Le Verrier proceeded to gather together all of the modern observations as well as the most useful among the ancient ones in order to work out a new theory of Mercury. He compared his results with those published by previous investigators, and uncovered various errors. In studying the perturbations imposed by Venus on the motions of Mercury, for instance, he deduced a new value for the planet's mass: it was 1/390,000 of the solar mass, a value remarkably close to the actual value, 1/408,500. Le Verrier also calculated the apparent radius of the Sun using observations of Mercury's transits across the solar disk, and arrived at 16'0".01 for this value as seen from the mean distance of the Earth from the Sun. The present-day value adopted for this quantity is 15' 59".68; again the agreement is very close. Nevertheless, a number of outstanding problems remained, and Le Verrier would have occasion, as we shall see, to revisit his work on Mercury some years afterward.

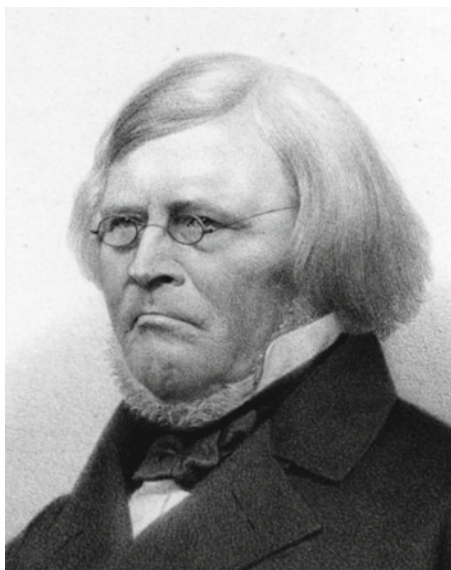
¹⁶ *CRAS 16 (1843) pp. 1054–1065.

First Controversies

In 1842, Le Verrier first occupied himself with the planet Uranus. The then outermost known planet presented marked anomalies: for a number of years, the tables had failed to accurately account for its movement.

Le Verrier was not the only one to work on the problem of Uranus. For example, in Germany and in Denmark, Peter Hansen (Fig. 1.8), who had developed new methods of celestial mechanics, attempted to apply them to Uranus. In France, Liouville charged a young man, Charles Delaunay (Box 1.3, Fig. 1.9), to examine the work of Hansen and to check the results, a task which he carried out by 1841.¹⁷ But Delaunay also found that certain terms in the series expansions not considered by Hansen were far from negligible.¹⁸ Le Verrier did not agree with Delaunay, however. A rather technical controversy ensued which can be followed in detail in the *Comptes rendus de l'Académie des sciences*.¹⁹ In the end, Le Verrier proved to be right, but he ungenerously mocked his more inexperienced colleague. At odds for the first time, the two men would engage in such conflicts often in the future, and come to regard one another as mortal enemies.

Fig. 1.8 Peter Hansen
(1795–1874)



¹⁷ *CRAS 14 (1841) pp. 371–373.

¹⁸ *CRAS 14 (1841) p. 406.

¹⁹ *CRAS 14 (1841) pp. 487–488, 579–582, 660–663.

Box 1.3 Charles Eugène Delaunay (1816–1872)

Delaunay (Fig. 1.9) came from a very modest family. From an early age, he showed a great predilection for mechanics and mathematics, which led him to the École polytechnique where he graduated at the head of his class in 1836. Because of this rank, he was awarded a prize founded by the widow of Laplace, a gift which consisted of an edition of the works of the master. It was reading these works which clinched Delaunay's devotion to a career in astronomy. Arago proposed for him the post of student-astronomer at the Observatory, a somewhat insecure position which nevertheless he preferred over that of student-engineer in the *École des Mines*. Biot chose him as assistant to his course in physical astronomy at the Sorbonne and also for his course at the Collège de France. Delaunay later became répétiteur (tutor) at the École polytechnique, gave courses as professor of physics and mechanics in the Faculty of sciences, and in 1851 rose to the rank of full professor of mechanics at the Polytechnique. He published in 1850 a remarkable *Cours élémentaire de mécanique théorique et appliquée* (Paris: Masson, Langlois and Leclercq), a model of clarity which went through many editions.

The principal work of Delaunay was a new theory of the motion of the Moon, a motion that is extremely complex if one wishes to predict it with a high degree of precision. It was “an enormous work which the most competent judged impossible before him, and which we admire both for the simplicity of its method and the power of its application.”²⁰ It won for Delaunay admission in 1855 to the Academy of sciences.

Delaunay's life was agitated by his disputes with Le Verrier. The battles between these two scientists and between the two institutions each represented, the Bureau des longitudes and the Observatory, were incredibly fierce. “Delaunay,” said Leon Foucault, “only thinks of two things: the Moon and Le Verrier. When he is thinking about the Moon, he has a large, expansive face, like the Full Moon. On the other hand, if he is thinking of Le Verrier, he has the face of a bulldog.”²¹ There were certainly faults on both sides. After the removal of Le Verrier in 1870, Delaunay was nominated director of the Observatory, but his reign was of short duration. Tragically, he was drowned on 5 August 1872, while on a boating expedition undertaken in bad weather on the harbor of Cherbourg.

²⁰ Faye, H. (1872) Draft for a discourse (not delivered) at the burial of Delaunay, *Annales des Mines*, 7^e série, vol. 2., accessible by <http://www.annales.org/archives/x/delaunay.html>, where one can also find a detailed biography of Delaunay.

²¹ From Joseph Bertrand: see BOP, *Documents divers sur l'Observatoire de Paris*, 1854–1872, cote 3567(3), folder AP.

Fig. 1.9 Charles Delaunay (1816–1872), photograph



Between 1840 and 1843, when he was chiefly preoccupied with Uranus and Mercury,²² Le Verrier also concerned himself with the motion of the minor planet Pallas. The resulting memoir²³ attracted particularly the attention of the Academy of sciences, which decided to insert it in the *Recueil des mémoires des Savants étrangers*. Next, in 1844, he worked on the orbits of various comets. This latter work,²⁴ even though it has been overshadowed by his great work on the planets, is of considerable interest. It has been described by one of Le Verrier's successors, the great specialist in celestial mechanics, Felix Tisserand (Fig. 1.10)²⁵:

The comet Lexell had been discovered by Messier on 15 June 1770; six days later it became visible to the naked eye, and on 4 July it was lost in the rays of the Sun. Pingré calculated an ephemeris according to which the comet would soon become visible again. Messier observed it, indeed, on 4 August, and it remained visible to the naked-eye until 26 August.

²² Le Verrier's work on Mercury culminates in a long paper entitled *Théorie du mouvement de Mercure* printed in 1845 in *Connaissance des temps* for 1848, additions, pp. 3–165.

²³ *CRAS 16 (1843) p. 1435.

²⁴ *CRAS 18 (1844) pp. 826–827 and 19 (1844) pp. 559–560.

²⁵ Tisserand F. (1880).

Fig. 1.10 Félix Tisserand (1845–1896), painting by Dupain



Lexell was the first to appreciate that it moved in an elliptical orbit and that the period of its revolution was some $5\frac{1}{2}$ years. By way of objection to Lexell, it was noted that if the comet had such a short period of revolution it ought to have been observed more than once; but Lexell responded that in 1767 the comet must have passed very close to Jupiter, an event which could have changed its motion considerably. He remarked, moreover, that a second approach of the comet by Jupiter would take place in 1779, and that this event might prevent the comet from returning to its perihelion in 1781. Indeed, the astronomers waited in vain for its perihelic return in 1781 and 1782. Le Verrier attempted to find out what had happened to the comet. He reexamined with the greatest care all the observations of 1770; he showed, by a detailed analysis, that the orbit it followed at the time could not be determined with precision; he presented a number of possible orbits that could account for the observations with nearly equal accuracy. By following the comet through each of these possible orbits up to 1779, when it approached closely to Jupiter, he showed that it was impossible for this comet to have become a satellite of that planet; rather, after being perturbed, it might have followed a hyperbolic path, in which case it would be lost to us without the possibility of return; on the other hand, it might have continued to move in an elliptical orbit.

It was possible, therefore, that one of the periodic comets which had recently been discovered was in fact Lexell's comet. To verify this, it was necessary to calculate the

perturbations experienced since 1781 by each of these new comets, in order to work out what their orbits had been at the time. Le Verrier did not hesitate to undertake this enormous labor, and arrived at the conclusion that neither the comet discovered by Hervé Faye²⁶ in 1843, nor the one discovered by the Italian De Vico in 1844 possibly have been Lexell's comet.²⁷ He further showed that the comet discovered by De Vico had already been seen by Tycho Brahe in 1585, and was therefore periodic. Le Verrier's own words concerning this comet are worth quoting:

The comet of 1844 might have, like the others, come to us from the most distant regions of space and been fixed or settled among the planets through the powerful influence of Jupiter. Its returns go back no doubt several centuries; since this time, it has passed certainly many times near the Earth.... After a number of centuries it will again reach the orbit of Jupiter, and its course will certainly again be altered. Perhaps Jupiter will even send it again into the regions of space from which it has been stolen.

In part thanks to Le Verrier, it is now clear what can happen to comets in the Solar System. Originally, they all come from very far away.²⁸ The gravitational perturbations of the planets near which they pass determine their eventual orbits. The orbits can be elliptical, in which case the comet is periodic (Fig. 1.11). The orbit can later be greatly modified over time by other perturbations, and can even become parabolic or hyperbolic, in which case the comet may be ejected from the Solar System altogether.

Following these brilliant researches, Le Verrier was easily elected to the Academy of sciences on 19 January 1846. He had suffered previously a defeat in 1843 when the astronomer Victor Mauvais had been elected in preference to him as well as several other candidates on 20 November, as a replacement for Bouvard. (Le Verrier would never forgive Mauvais for this.)

In 1845, Le Verrier returned to Uranus, whose perplexing behavior continued to intrigue astronomers “who were not accustomed to such large errors in their calculations.” The stakes were very high. Here was a menace that seemed to threaten the very foundations of Newtonian mechanics. In a celebrated passage of the *Exposition du Système du Monde*, Laplace had waxed eloquent about the magnificence of mechanics in explaining all natural phenomena:

In the midst of the infinite variety of phenomena that evolve continually in the heavens and on the earth, we have arrived at unraveling the small number of general laws that matter follows in its movements. Everything in nature obeys them; everything follows from them with the same necessity as the return of the seasons; the curve followed by the light atom that the wind carries away by chance is regulated in the same definite manner as the orbits of the planets.... Geometers ... have at last reduced all of mechanics to general formulas that leave nothing to be desired but the perfection of analysis.

²⁶ An obituary of Faye by Charles Nordmann, in French, can be found in **Revue générale des Sciences pures et appliquées* 13 (1902), pp. 897–898.

²⁷ **CRAS* 19 (1844) pp. 666–670 and 982–984.

²⁸ The Dutch astronomer Jan Oort showed in 1950 that comets, which are residues of the formation of the Solar system, reside in a “belt” located at around one light-year from the Sun, from where they can be ejected by small gravitational perturbations which can send them to the inner parts of the Solar system.

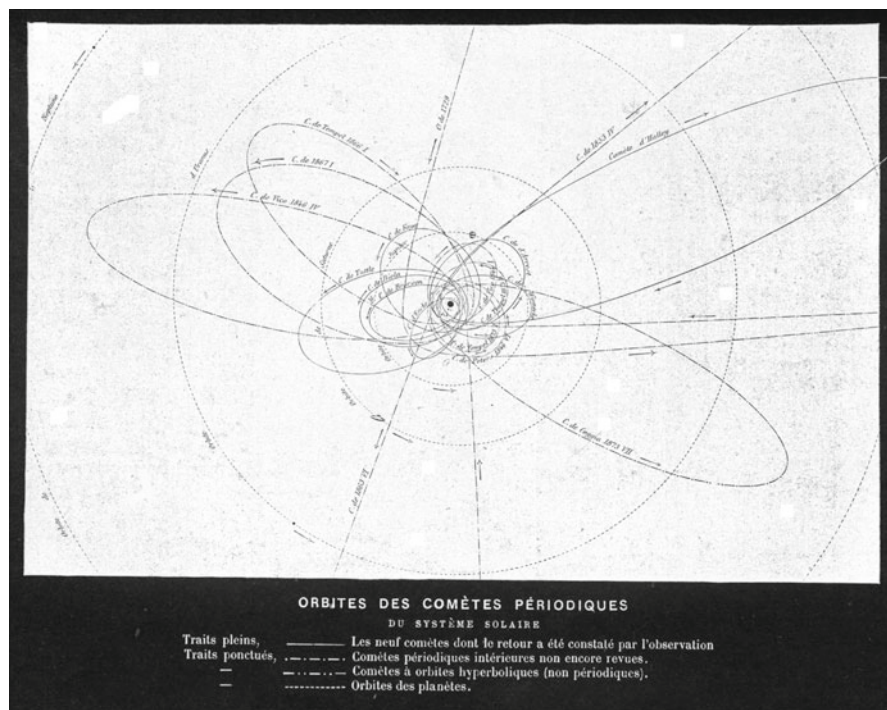
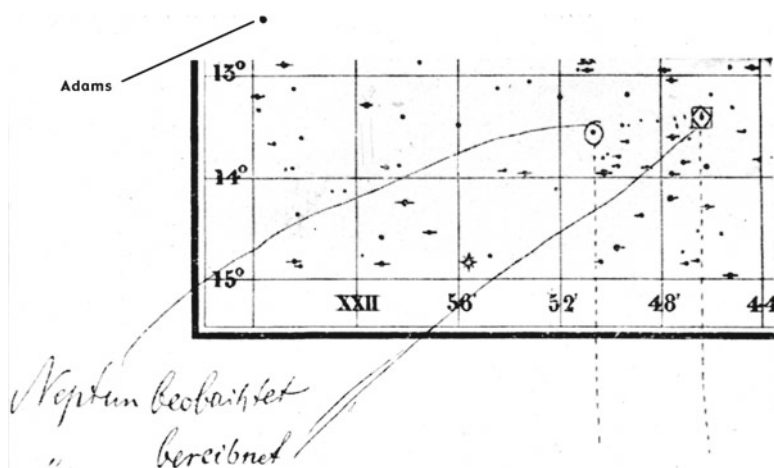


Fig. 1.11 Orbits of the periodic comets known in 1873. For scale, the orbits of Jupiter, Saturn, Uranus and Neptune are also drawn

Laplace's great dream seemed to be in doubt. Uranus had posed a possible exception to the reign of mechanics. It was this problem—which as great as any in the science at the time—which Le Verrier was about to attack.

Chapter 2

The Discovery of Neptune (1845–1846)



A fragment of Bremiker's celestial map on which a German hand (Galle's?) has plotted the position of Neptune predicted by Le Verrier (*Neptun bereibnet*) and the actual position (*Neptun beobachtet*). The position predicted by Adams is also indicated

The discovery of Neptune has been the subject of an immense literature, for it was undeniably one of the major scientific events of the nineteenth century. It is well known that the effort to discover the perturbing planet responsible for the abnormal behavior of Uranus was carried out simultaneously in England and France, and that La Verrier was the first to announce the discovery, John Couch Adams having independently obtained similar results. From these events, interminable controversies followed, in which nationalism played a large role, and even today they are not totally extinguished.¹ But at least it is now possible to look at these matters with cooler heads, and with relative neutrality.

First, let us examine the problem that La Verrier set out to tackle: the problem of the motion of Uranus.

The Problem of the Motion of Uranus

The chance discovery of Uranus by William Herschel (Fig. 2.1) in 1781 had huge repercussions. As soon as it became evident that Herschel had discovered not a comet as first thought but a new planet, earlier records were scoured, and a number of earlier observations began to turn up in which the planet had been mistaken for a fixed star. The earliest, by the Englishman John Flamsteed, went back to 1690; still others were made by James Bradley, Tobias Mayer, and Charles Le Monnier.

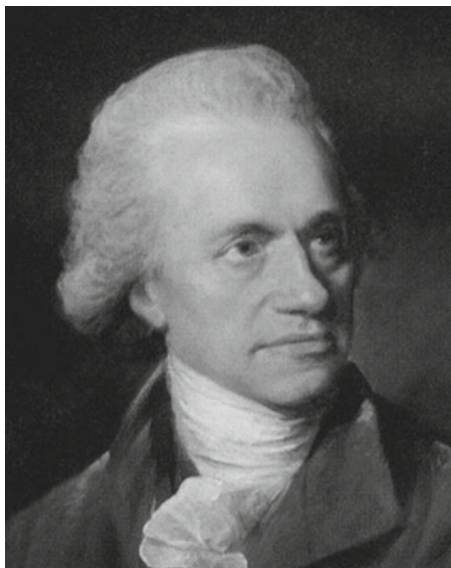


Fig. 2.1 William Herschel
(1738–1822)

¹ In 1880, Gaillot, the only pupil of Le Verrier, wrote (Gaillot 1880, p. 103): “There had been in the past discussions, more passionate than impartial, about the priority of the discovery; today, the question is settled: to each one his due, and the mutual esteem between the two scientists proves that both, at least, were giving justice to each other. But it is to Le Verrier alone that the discovery of the planet is due.” However controversies revived from time to time.

Fig. 2.2 Alexis Bouvard
(1767–1843)



In his celebrated treatise on celestial mechanics, Pierre Simon de Laplace had developed mathematical expressions for the mutual perturbations exerted by the planets as a result of their gravitational attraction. Using these expressions, one could carry out numeric calculations to produce tables of the positions of the planets over time. The responsibility for doing so was claimed by the Bureau of longitudes, headed by Laplace himself, though the work of actually performing these backbreaking calculations was distributed among several astronomers at the Bureau, including Delambre, Alexis Bouvard, and Burckhardt. Bouvard (Fig. 2.2), Laplace's student, was assigned the most thankless task. In 1821, he began the laborious calculation of tables predicting the movements of the three giant planets: Jupiter, Saturn, and Uranus. The calculation of the tables of Jupiter and Saturn proved to be relatively straightforward. Uranus, however, proved to be highly intractable. Even after taking into account the perturbations exerted by the other planets, Bouvard would not derive a set of orbital elements that would successfully account for the movements of Uranus during the entire period over which it had been observed (going back to Flamsteed's observation of 1690).

As he struggled with the problem, Bouvard tried various expedients. First he used only the numerous positions of Uranus measured since its discovery in 1781 (i.e., covering the four decades 1781–1821). He then found, however, that he could not satisfy the earlier ("ancient") observations going back to that of Flamsteed in 1690. The discrepancy for Flamsteed's observation reached more than a minute of arc, and this seemed too great to ascribe to errors of observation. On the other hand, if Bouvard accepted the observations between 1690 and 1781, the more recent observations failed to fit. Resigned to defeat, Bouvard wrote in the introduction of his *Tables of Uranus* in 1821 that it would remain the task of future investigators to determine whence arose the difficulty in reconciling these two data sets: whether the failure of the observations before 1781 to fit the tables was due to the inaccuracy of

Fig. 2.3 George Biddell Airy
(1801–1892)



the older observations or whether they might depend on “some foreign and unperceived source of disturbance acting upon the planet.” Bouvard’s tables were based only on the observations between 1781 and 1821, but soon after their publication, discrepancies began to appear once more. They accumulated over time, until it became impossible to attribute them to the effects of observational errors alone. Following Alexis Bouvard’s death in 1843, his nephew Eugène was charged by the Bureau of longitudes to work on new tables of the planets. He submitted his results to the Academy of sciences on September 1, 1845, but they were never published. By then he had come to regard the discrepancies between observation and theory as irreconcilable without adding another factor, and personally found “entirely plausible the idea suggested by my uncle that another planet was perturbing Uranus.”²

It seems, then, that Alexis Bouvard himself had been the first to speculate that the anomalous motion of Uranus could be occasioned by the gravitational action of a new *planète troublante* (disturbing planet). Others would later claim to have independently come up with the same idea. Perhaps they did, though by then the idea was so much in the air that there was little credit attached to doing so. Indeed, the idea spread rapidly through the scientific world, and began to attract the attention of the larger public, not only in France but elsewhere. For instance, in November 1834, an amateur astronomer, the Reverend Thomas J. Hussey, rector of Hayes, Kent, wrote to George Biddell Airy (Fig. 2.3), Plumian professor of astronomy at Cambridge, about the matter. During a previous visit to Paris, Hussey had met Eugène Bouvard,

² *CRAS 21 (1845) pp. 524–525.

Fig. 2.4 Friedrich Wilhelm Bessel (1784–1846)



and had become convinced that an exterior planet disturbing Uranus from its course was the likely explanation of the discrepancy between observation and theory. He now proposed to Airy, a first-rate mathematician, that if the latter would work out its approximate position, he, Hussey, would gladly take up the search for it.³ In 1835, the astronomer Benjamin Valz, who the following year was appointed director of the Marseille observatory, proposed to Arago carrying out a search for the planet from its possible perturbing effects on Halley's Comet. Moreover, in 1840, the renowned German astronomer Friedrich Wilhelm Bessel (Fig. 2.4), who from 1830 onwards had conjectured about the possible existence of just such a perturbing mass, gave a public lecture on the topic. He also discussed it with John Herschel, the son of the discoverer of Uranus and a well-known astronomer in his own right.

Arago evidently hoped that the problem of Uranus would be taken up at the Paris Observatory, but he lacked confidence in Eugène Bouvard, whose measurements at the eclipse expedition of 1842 had been of poor quality. Since there was no else at the observatory he deemed capable of tackling such a difficult problem, he turned to Le Verrier. He had great faith in Le Verrier's mathematical abilities, and so, at Arago's request, Le Verrier abandoned the investigation of comets in which he was then involved and devoted himself to Uranus. He recalled, in the first notice of this work to the Academy of sciences⁴:

³ These letters are preserved in the archives of the Royal Greenwich Observatory in Cambridge, UK, in *Papers of George Airy*, general ref. GBR/0180/RGO 6. Important extracts are to be found in °Airy, G.B. *Monthly Notices of the Royal Astronomical Society* 7, 121–144, (1846).

⁴ *CRAS 21 (1845) pp. 1050–1055.

During the course of this last summer [1845], Mr. Arago made clear to me that the great importance of this question imposed a duty on every astronomer to contribute, to the utmost of his powers, the clarification of certain points. In response to his plea, I abandoned, therefore, my researches into comets, of which several fragments have already been communicated, in order to occupy myself fully with Uranus. Such, then, is the origin of that work I now have the honor to present to the Academy.

The Work of Le Verrier

Le Verrier scrupulously examined all the available observations up until 1845, notably those made recently at the Paris Observatory, which Arago put in his hands, and which were of excellent quality; and also those made at Greenwich which were sent by the director, Airy. He also examined carefully Alexis Bouvard's calculations (he seems not to have considered those of his nephew, Eugène). He discovered that certain terms had been neglected unjustifiably, and he also turned up several out-right errors, which required him to redo parts of the calculation. Next he undertook to determine the actual location of the perturbing planet.

The problem was entirely novel: hitherto, the position of each planet was determined by taking into account the perturbations of the others whose positions were known by observation. In the present case, it was a matter of determining the position of a planet about which one knew nothing except the perturbations that it exerted on the other planets. In mathematics, this is called an inverse problem. It is both difficult and complex, because there are many unknowns to be determined. Le Verrier simplified the problem from the outset by supposing as known the distance of the planet from the Sun and the inclination of its orbit. He wrote on 1 June 1846⁵:

It would be natural to suppose that the new body is situated at twice the distance of Uranus from the Sun, (Box 2.1) even if the following considerations didn't make it almost certain. First, it is obvious that the sought-after planet cannot come too close to Uranus [since then its perturbations would have been very evident]. However, it is also difficult to place it as far off, say, as three times the distance of Uranus, for then we should have to give it an excessively large mass. But then its great distance both from Saturn and Uranus would mean that it would disturb each of these two planets in comparable degree, and it would not be possible to explain the irregularities of Uranus without at the same time introducing very sensible perturbations of Saturn, of which however there exist no trace.

We might add that since the orbits of Jupiter, Saturn, and Uranus all have a very small inclination to the ecliptic, it is reasonable to suppose, as a first approximation, that the same must apply to the sought-after planet.

By such legerdemain, Le Verrier had reduced the number of unknowns by two: he assumed the semi-major axis of the orbit, a quantity that would have been particularly difficult to determine otherwise, and the inclination of the orbit. Nevertheless, there remained more than enough other unknowns, in part because the orbital elements of Uranus were themselves poorly determined owing to the lack of any solution fitting all the observations. The hypothesis of a perturbing

⁵ *CRAS 22 (1846) pp. 907–918.

Box 2.1 The Law of Titius-Bode

In 1772, the German astronomer Johann Daniel Dietz, called Titius, showed that it is possible to approximately represent the distances of planets from the Sun by the following empirical relation:

$$a = 0.4 + 0.32^{n-1},$$

where a is the semi-major axis of the orbit expressed in astronomical units (the semi-major axis of the Earth’s orbit) and n represents the consecutive integers. At first unnoticed, this relation was later publicized by the German astronomer Johann Elert Bode. Here is how it represents the distances of the planets from the Sun.

	Mercury	Venus	Earth	Mars		Jupiter	Saturn	Uranus	Neptune
n	∞	1	2	3	4	5	6	7	8
a (calc.)	0.40	0.70	1.00	1.60	2.80	5.20	10.0	19.6	38.8
a (real)	0.39	0.72	1.00	1.52	–	5.20	9.55	19.2	30.1

The gap for $n=4$ incited astronomers to search for a planet occupying this position. This effort was crowned with success, when Piazzi discovered in 1801 the small planet Ceres, for which $a=2.77$. Since that time, thousands of asteroids have been discovered, moving between the orbits of Mars and Jupiter. It was once thought that they might have been produced by the rupture of a single planet, but this theory has long been abandoned. Note that the agreement is rather close between the predictions of the Titius-Bode law and the actual values, except for Neptune, which was unknown at the time the law was first enunciated. It is still not clear whether or not there is any physical basis to it, or whether it is merely a numerical coincidence.

planet did not change this. Of course, it is possible to take the orbit of Uranus as given; then one can work out all the perturbations of the other planets except the new one, and finally establish the discrepancies between the calculated and observed positions so as to show the effects of the perturber. An example is given in Fig. 2.5. Nevertheless, it is not possible in this way to obtain a unique solution to the problem since any number of other orbits remain possible for Uranus.

Seeing this, Le Verrier was obliged to determine simultaneously both the orbital elements of Uranus and those of the new planet. This is a problem with 12 unknowns. However, as we have seen, Le Verrier had already settled on two for the unknown planet, and using the same reasoning he settled the same ones for Uranus: the semi-major axis and the orbital inclination. With this simplification, there remained eight unknowns in the orbital elements, to which he added a ninth, the mass of the perturbing planet. We cannot enter here into all the details of the solution of the

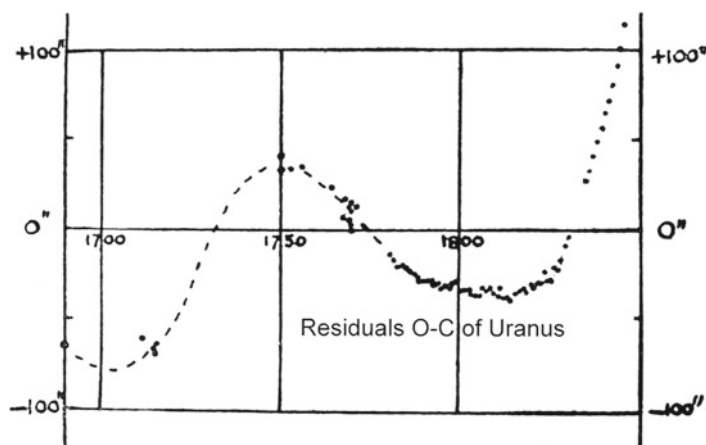


Fig. 2.5 An example of the discrepancies between the calculated longitudes of Uranus C and the observed ones O from 1690 to 1845 (From Le Verrier's paper in *Connaissance des temps* for 1849, additions, pp. 3–254, table pp. 129–136; calculations and drawing by André Danjon (1946).). Here, the calculated longitudes, which take into account perturbations by Jupiter and Saturn, are those of Bouvard, the theory of which, corrected by Le Verrier, uses only the observations between 1781 and 1821. This solution is not necessarily the correct one, because it was equally justifiable to have used the observations before 1781 to calculate the orbit of Uranus

corresponding equations.⁶ Figures 2.6 and 2.7 exhibit two unpublished manuscript pages of Le Verrier's calculations.

So far sure of himself, Le Verrier affirmed, in his presentation to the Academy of sciences on 1 June 1846 (to avoid confusion, we give in Box 2.2 a chronology of developments related to the discovery of the new planet):

I demonstrate that all the observations of the planet [Uranus] can be represented with the exactitude they deserve.... I conclude also that one can effectively model the irregularities of Uranus's movements by the action of a new planet placed at a distance of twice that of Uranus from the Sun; and what is just as important, that one can arrive at the solution in only one way. To say that the problem is susceptible to only one solution, I mean that there are not two regions in the sky in which one can choose to place the planet in a given epoch (such as, for instance, 1 January 1847). Within this unique region, we can limit the object's position within certain bounds.

Next Le Verrier indicates within 10° the possible positions occupied by the perturbing planet for 1 January 1847. The uncertainty was still considerable, and Le Verrier added that he could do no better at the time of his presentation, since the

⁶ Jean-Baptiste Biot attempted to explain Le Verrier's methods in six papers in **Journal des Savants* (October 1846, pp. 577–596; November 1846, pp. 641–664; December 1846, pp. 750–768; January 1847, pp. 18–35; February 1847, pp. 65–86; March 1847, pp. 182–187). Arrived at the third paper, he writes: "As I progress in the task I have undertaken, the difficulty of the subject seems to increase." In order to understand what Le Verrier did, the best thing is to read his own papers. A more elementary account can be found in **Tisserand and Andoyer* (1912) pp. 279–288.

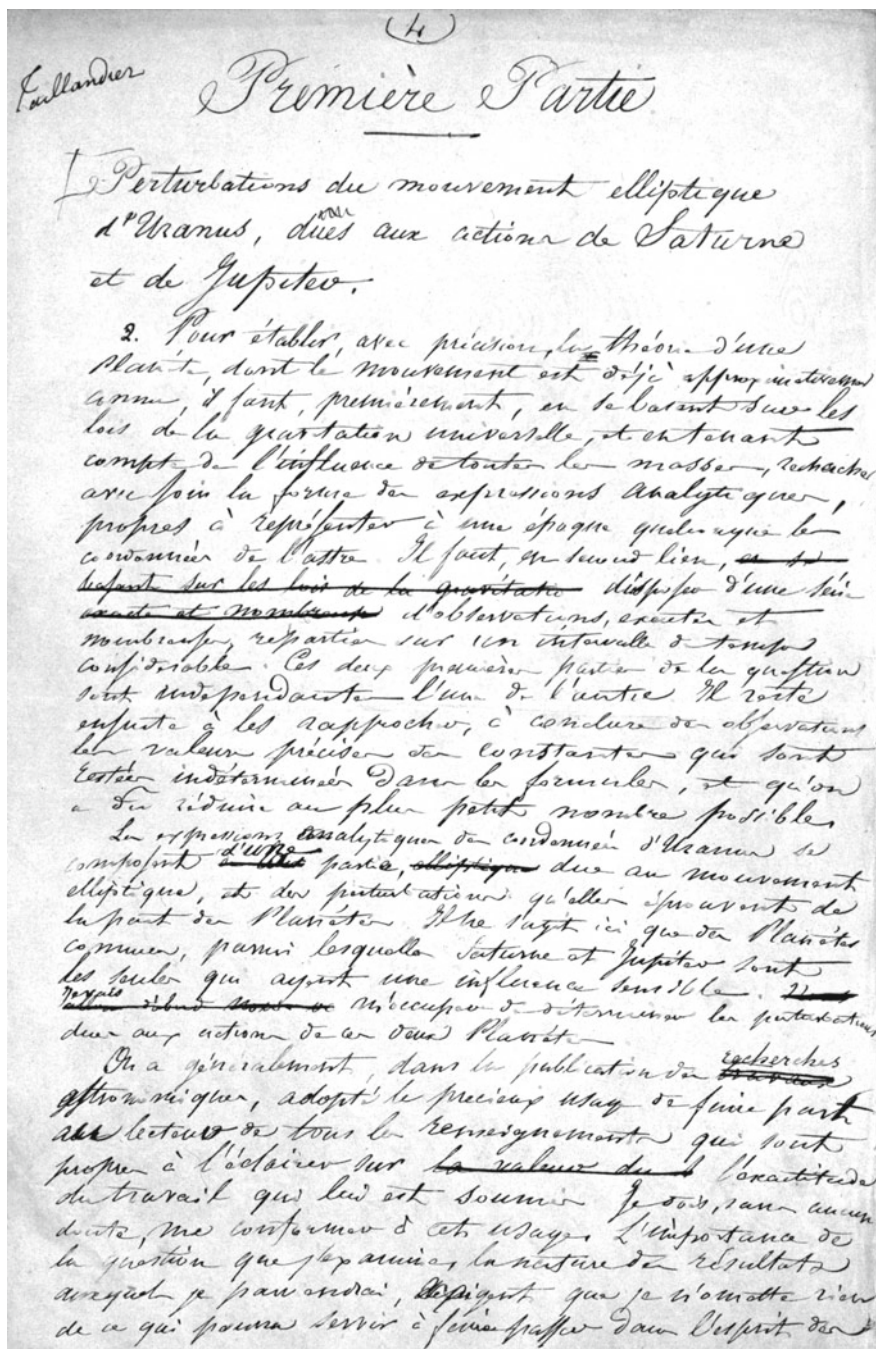


Fig. 2.6 Researches on the movements of Uranus (1846) (Autograph manuscript of Le Verrier [BOP, Ms 1063(27), 1st part, p. 5])

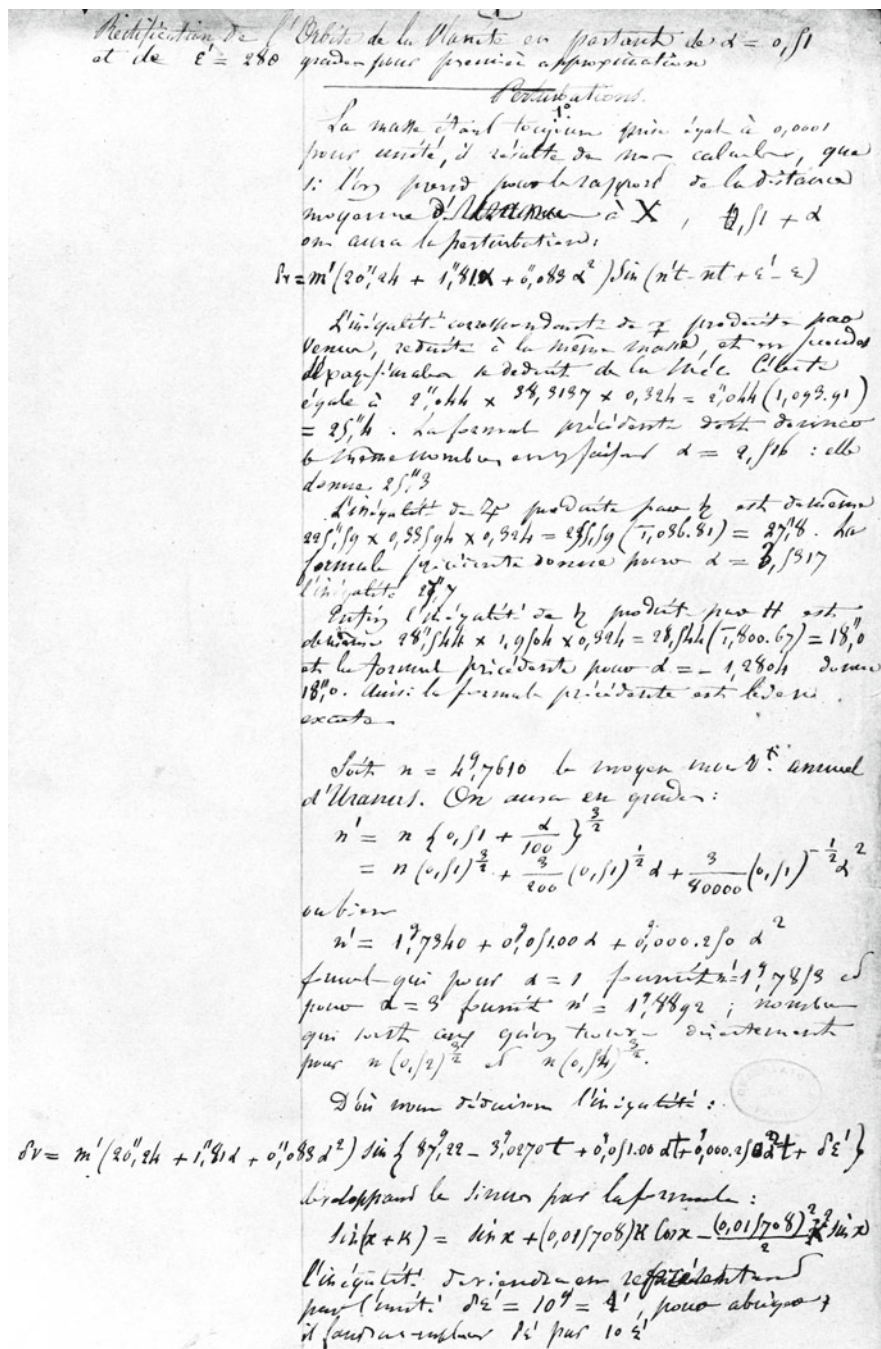


Fig. 2.7 Search for the perturbing body, 2^d approximation (1846) (Autograph manuscript of Le Verrier [BOP, Ms 1063(27), 5th part, p. 2])

Box 2.2 Chronology of the Discovery of Neptune

1821

Alexis Bouvard publishes tables of Uranus.

1845

1 September: Eugène Bouvard mentions that his uncle Alexis Bourvard had suggested the idea of a “perturbing planet.” Arago suggests to Le Verrier that he explore the idea.

21 October: Adams notifies Airy that he has obtained results concerning the perturbing planet.

5 November: Airy asks Adams for clarifications, but Adams does not respond. Adams seems to be no longer interested in the problem.

1846

1 June: Le Verrier presents his method and gives a rough position for the perturbing planet. A search is undertaken at the Paris Observatory, but it seems to have been abandoned by early August, owing to a lack of star maps and appropriate instruments.

26 June: Airy requests clarifications from Le Verrier, which Le Verrier supplies on 28 June. Le Verrier proposes to give Airy a better position for the planet as soon as he calculates one. Airy does not respond to this offer.

9 July: Airy asks Challis to search for the planet.

29 July: Challis commences his search at Cambridge; it lasts 2 months, but he fails to analyze sufficiently his observations, and misses the discovery, even though the planet figures among the stars he observes.

31 August: Le Verrier publishes the elements and a rather precise position for the planet.

18 September: letter from Le Verrier to Galle.

The night of the 23/24 September: Galle discovers the perturbing planet. Immediately thereafter, a number of astronomers, including Le Verrier, see the planet.

30 September: two journals announce the discovery and call the new planet Neptune, a name proposed by Le Verrier.

5 October: the discovery is announced to the Academy of sciences. Arago proposes now the name of Le Verrier for the planet.

10 October: Lassell discovers Triton, satellite of the planet.

14 October: Airy proposes to Le Verrier the name Oceanus, also taken up by Challis and Adams in *The Athenaeum* of 17 October.

End of October: The Bureau of longitudes supposedly decides on the name Neptune, but there seems to be no record of this decision. The name seems to have been adopted, rather, by a sort of international consensus.

13 November: the “predictions” of Adams in 1845 are finally revealed during a meeting of the Royal Astronomical Society.

work for which he had just presented an abstract to the Academy “must be considered a rough draft or outline of a new theory, which [was] only in the initial stages.” The orbital elements he calculated were provisional, but he hoped to extend his labors to provide more precise results. He concluded:

Dating from the year 1758, the illustrious geometer Clairaut declared in his publication to the Academy of sciences, on the subject of the perturbations of the comet of Halley, that an object which traverses the remotest regions might be subject to totally unknown forces, such as the action of planets too distant to ever be perceived. Let us hope that the stars which Clairaut spoke of will not be all of them invisible; that, if Uranus has been discovered by chance, nevertheless, the new planet will be successfully found from the position I have calculated.

Despite Le Verrier’s seeming confidence, skepticism still reigned in certain quarters. Thus Airy wrote on 26 June to Le Verrier to ask for further clarifications, at the same time sending him additional Greenwich observations.⁷ Le Verrier thanked Airy for his assistance, and responded to Airy’s specific questions. He even proposed to communicate the orbital elements of the perturbing planet, if Airy were at all inclined to search for it. Airy was very impressed by Le Verrier’s confidence. Though his skepticism was completely overcome, he declined Le Verrier’s offer, for reasons that remain rather mysterious even today.

Despite the novelty of the problem and the great mathematical difficulties involved, Le Verrier needed only 3 months to specify the orbital elements of the perturbing planet, guess at its mass, and even provide an order of magnitude estimate of the apparent diameter it would present in the telescope. In his note of 31 August 1846, he summarized his methods and gave the predicted orbital elements for the new planet, to a degree of precision that would prove, however, to be entirely illusory.⁸ Table 2.1 compares his values for the orbital elements to the true ones.

Le Verrier modified the semi-major axis of the orbit slightly from the initial hypothesis in which he had simply (and rather arbitrarily) followed the Titius-

⁷ *Centenaire de la naissance de Le Verrier* (1911), pp. 12–13. In a considerably later letter from Airy to Le Verrier dated 20 December 1876 (BOP, MS 1072 (35)), he adds further details:

“The part which depends on calculation of observations was to be divided between Bessel and me. Such work required generally only ordinary powers of judgment. But I believe that both Bessel and I looked, with a doubtful hope, to the possibility that our work would at some time be reconciled with theory. But at that time there was no Le Verrier; I was one of the few persons who might rashly have taken up the enterprise, but my time has always been very heavily subscribed. In discriminating amongst the various persons concerned in this great enterprise, we must give a very high position to Bessel. The failing of his first discussion of Bradley’s observations have been well pointed out by you; but they did not in any case greatly affect the results as for planets: and in proceeding downwards along the course of time by uniform scale, they were practically annihilated. But I must say that Bessel, in the construction of his *Tabulae Regiomontanae*, showed himself as the first man who profoundly felt that Astronomy is a science of connection and comparison. And my perception of this point in his character and the character of his work induced me to undertake (using his work as foundation) the reductions which you state to have been so useful to you.”

⁸ *CRAS 23 (1846) pp. 428–438.

Table 2.1 Values of principal elements of the orbit of Neptune

Value	Le Verrier	Actual value
Semi-major axis of orbit (a.u.) ^a	36.154	30.0690
Sidereal period of revolution (years)	217.387	163.723
Eccentricity	0.10761	0.008586
Mass (solar masses)	1/9,300	1/19,424
Mass (earth masses)	36	17.14

^aThe astronomical unit (a.u.) is equal to the semi-major axis of the Earth's orbit, i.e. $1.496 \cdot 10^8$ km

Bode law, and taken it to be twice that of the orbit of Uranus, i.e., 38 AU. The eccentricity adopted by Le Verrier is important, in that it made the values for the distance between Neptune and Uranus during the period in question differ very little from the actual ones. Le Verrier predicted that the planet lay in the sky about 5° east of the star delta in the constellation Capricorn, and also, as noted, indicated the approximate values of the apparent diameter and brightness of the planet, probably in an attempt to stimulate the imagination and ambition of an observer to look for it. On 5 October (i.e., after the discovery) Le Verrier would work out the actual inclination of the orbit of the planet, which as a first approximation he had taken as zero. He found the orbit inclined at least $4^\circ 3'$ to the orbit of Uranus.⁹

The Discovery

On August 31 1846, Le Verrier presented a paper to the Academy of sciences, containing the elements of the planet and the place where it ought to be found. He then wrote to several foreign astronomers in an effort to enlist a powerful instrument in the search. Sadly, there were at the time no suitable instruments at the Paris Observatory itself (the largest telescope was still a $9\frac{1}{2}$ inch [23 cm] Lerebours refractor, finished in 1823, but of such poor quality that the outer zones of the glass had to be masked with a diaphragm). Furthermore, the observatory did not then have at its disposal any good maps of this part of the sky. Despite all that Arago and Le Verrier between them had done, the planet would not, and indeed could not, be discovered in Paris.

Among the foreign astronomers contacted by Le Verrier was Johann Gottfried Galle (Fig. 2.8), of the Berlin observatory. Le Verrier wrote to him on 18 September. The letter reached Berlin on 23 September; that night Galle, after seeking and receiving permission from the observatory's director, Johann Franz Encke, and being assisted by a graduate student from Copenhagen, Heinrich Louis d'Arrest,

⁹*CRAS 23 (1846) pp. 657–659.

Fig. 2.8 Johann Gottfried Galle (1812–1910)



quickly discovered the planet. On 25 September, Galle wrote to Le Verrier (in French; the latter did not know German)¹⁰: “Monsieur, the planet whose position you had indicated really exists. On the very day I received your letter I found an eighth magnitude star, which did not appear in the excellent chart *Hora XXI* (drawn up by Dr. Carl Bremiker) from the collection of celestial charts published by the Academy of Berlin. The observation of the next night clinched the matter: here was indeed the planet we were looking for. Encke and I found with the great refractor of Fraunhofer (with an objective 9½ inches [23 cm] in diameter) that in brightness it was comparable to a ninth magnitude star.” (Fig. 2.9a, b). Galle himself proposed a name for the planet: “Let it be Janus,” he said, “for the most ancient deity of the Romans, whose two-sided face signifies its position at the frontier of the solar system.” On 28 September, Encke added his congratulations to those of Galle, and did so in impeccable French¹¹: “Permit me, Monsieur, to congratulate you most sincerely for the brilliant discovery by which you have enriched astronomy. Your name shall henceforth be associated with the most glorious imaginable demonstration of the correctness of universal gravitation. I believe that as these few words

¹⁰ *Centenaire de la naissance de Le Verrier* (1911), p. 19.

¹¹ *Centenaire de la naissance de Le Verrier* (1911), pp. 20–22.

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Addition au Mémoire précédent.

La nouvelle Pléïade, dont l'existence était découverte, d'autre la place dans le ciel était fixée dans le ciel par les recherches ~~de~~ Gaby vient de l'être, ayant été trouvée à Berlin, j'écris donc app. le 28 septembre dernier. La femme qui avait trouvé écrit à Paris le 18 septembre à Mr. Galle, pour réclamer son bienveillant concours : et balala offrons-nous à vu les planètes le jour même où il eut ma lettre. Voici ~~Cette~~ que M^r. Galle m'a fait l'honneur de m'adresser :

Berlin, le 29 Sept. 1846

Monsieur,

La planète, dont vous avez signalé la position, (Rellmann'sche) Le même jour, où j'ai reçu votre lettre, je trouvais une étoile de 8^e grandeur, qui n'était pas inscrite dans l'excellente carte Bora XXI (Pessines par M^r. le Dr Bremker) de la collection de cartes célestes publiées par l'Académie Royale de Berlin. L'observation du jour suivant décida que c'était la planète cherchée. Nous l'avons comparée M^r. Locke en moi, par la grande lunette de Fraunhofer avec une étoile de 7^e grandeur (α) Betelgeuse 119. 25° 50' 31" N - 13° 30' 9" E ce nous avons trouvé :

Groupe moy. de Berlin Sept. 23. 12 ^h 0 ^m 14 ^s ,6	Plan. = (α) + 21' 21",5 en R. = (α) + 1. 36,8 en Declin.
Sept. 24. 8.5h. 40,9	Plan. = (α) + 20. 19,9 en R. = (α) + 1. 15,3 en Decl.

Lien apparent de l'étoile (α) Vénus Betel :

Sept. 23. 327° 58' 2" ^s	- 13° 25' 49",0
24. 2h. 24	49,0

Nous avons comparé cette étoile deux fois avec Piazzi XVI. 5hh, dans la position la trouve aussi chez Cayser, ce qui lui, dans la British Association astronomique. D'après ces comparaisons, et la position dans la 32^e assoc. cat. ou a pour (α) :

Sept. 23. 327° 57' 54",5	- 13° 35' 45",0
24. 5h. 4	45,0

Cette détermination sera préférable Bessel ayant marqué pour zone 119, l'air inégal. Ainsi on a pour la planète :

Fig. 2.9 (a) and (b) The letter of Galle to Le Verrier announcing the discovery of Neptune. This is a copy by an unknown hand, with autograph commentaries by Le Verrier

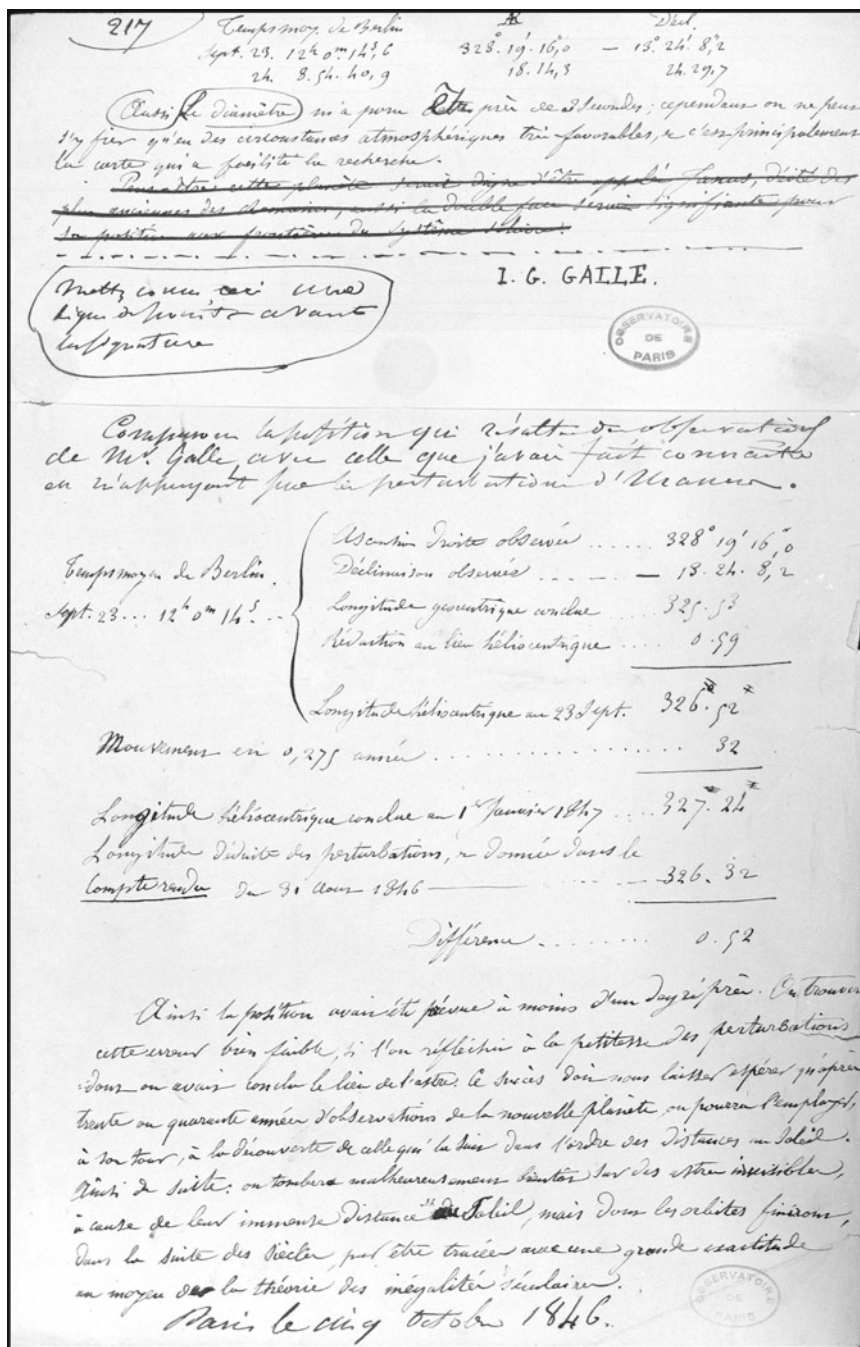


Fig. 2.9 (continued)

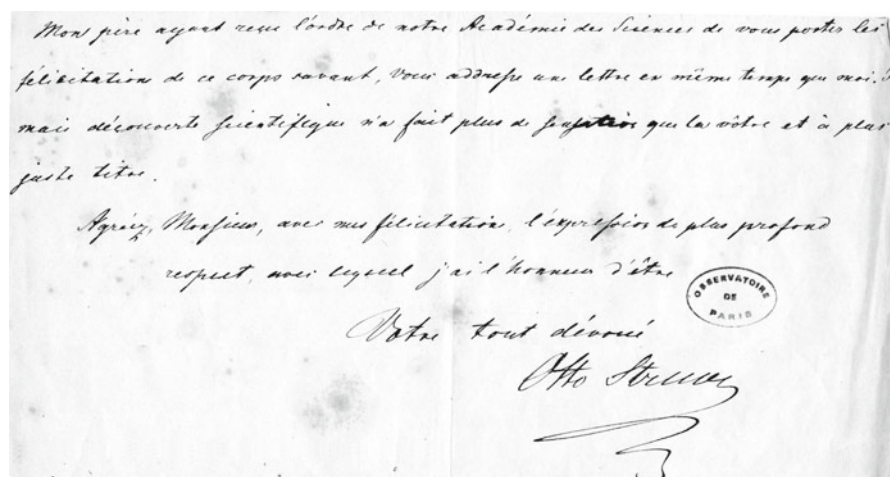


Fig. 2.10 The end of the congratulation letter from Otto Struve to Le Verrier for his discovery of Neptune

encompass all that a scientist's ambition could possibly hope for, it would be superfluous to add anything more.” He nevertheless did add more, as follows:

There was, nevertheless, a great deal of luck in the search. The Academy's chart of Mr. Bremiker, which has perhaps not even yet arrived in Paris but which I shall send out at once, happens to include, close to its lower edge, precisely the region where you have designated the position of the planet¹² [see the figure in the frontispiece of this chapter]. Without the fortuitous circumstance of having a chart containing all the fixed stars down to the tenth magnitude [of this particular area of the sky], I do not believe the planet would have been found. I would add that your position for the planet does not differ from its actual one for noon on 23 September by more than 54 minutes and 7 seconds in longitude; while, if my calculations are correct, the observed retrograde motion is 73.8 seconds [per day], just a bit greater than the 68.7 seconds predicted by your elements. It is possible, therefore, that the planet is not quite as far away as you have supposed, though in any case the difference is truly very small.

Shortly after the announcement of the discovery, the planet was viewed in Paris by Le Verrier himself (thus putting paid to Flammarion's celebrated but doubtful story that Le Verrier, the consummate theoretician, never saw it himself), as well as by several other astronomers, including Otto Struve and his father Wilhelm at the Pulkova Observatory¹³ near Saint Petersburg, by Emil Plantamour in Geneva, by Carl Ludwig von Littrow in Vienna, by John Russell Hind and James Challis in England, and by Carl Friedrich Gauss in Göttingen, etc. Many wrote to congratulate him, notably Otto Struve (Fig. 2.10) at Pulkova and Father Angelo Secchi at the Jesuit Collegio Romano in Rome. A modern photograph is displayed in Fig. 2.11.

¹² Heinrich d'Arrest is the one who gave to Galle the idea of using this map, allowing him to work very fast.

¹³ One finds several spellings for this observatory: Pulkova, Pulkowa, Poulkova, Poulkovo.



Fig. 2.11 Neptune returns to the same field of stars as on the date of its discovery, one Neptunian year (164.8 terrestrial years) later. Imaged by William Sheehan and Michael Conley with a 10-in. Ritchey-Chretien on the night of October 27, 2011. Compare the stars in this field with those in the figure at the head of this chapter

Flushed with enthusiasm, Le Verrier wrote on 5 October¹⁴: “This success leads to the hope that, after observing the new planet for another 30 or 40 years, it will become possible to use it in turn to discover the orbit of the next one in order of distance from the Sun, and so on. Unfortunately, the more distant objects will be invisible because of their immense distance from the Sun. Nevertheless, over the course of centuries, their orbits will be traced out with great exactitude by means of their secular inequalities.” Needless to say, his hope was not fulfilled in the way he expected. Other bodies in the solar system more remote than Neptune, such as Pluto and Eris, have been found; however, they are so remote and their masses are so small that the influence they exert on the orbit of Neptune is negligible. The discoveries of these “dwarf planets,” as they are now known, resulted not from mathematical investigations of the kind that led to Neptune’s discovery but from systematic photographic or CCD surveys.

Though a torrent of salutations rained down on Le Verrier, those of his own colleagues meant the most to him.¹⁵ He became famous overnight, and received countless honors: Officer of the Legion of Honor (though he had only been a Chevalier for 4 months), assistant member of the Bureau of longitudes, chair of celestial mechanics in the faculty of sciences in Paris – the latter was specifically created for him in honor of his achievement. King Louis-Philippe named him preceptor of astronomy for his

¹⁴ *CRAS 23 (1846) pp. 657–659.

¹⁵ Congratulation letters from Encke, Schumacher, Plantamour, Otto Struve, de Vico, Littrow, Valz and Airy are published in *Centenaire de la naissance de Le Verrier* (1911).

grandson, Louis-Philippe d'Orléans. The Royal Society of London awarded him the prestigious Copley Medal, the very same that William Herschel had won for the discovery of Uranus, and inscribed him among its foreign members. Many other learned societies followed suit. The Minister of Public Instruction, Narcisse-Achille de Salvandy, commissioned a bust of him by the celebrated sculptor James Pradier (see Fig. 10.4). This was presented as a gift to Madame Le Verrier on 31 December 1846, with instructions that it be set up in the College of Saint-Lô, in Le Verrier's hometown. (Miraculously, it survived the general destruction visited on Saint-Lô during the Battle of Normandy in 1944 that led the playwright Samuel Beckett to call Saint-Lô "the Capital of the Ruins." It is now displayed in the City Hall).

Among all the exuberant cheers, a few dissenting notes were heard. A congratulatory letter from Benjamin Valz to Le Verrier on 30 October refers to "wicked quibbling." He urges Le Verrier not to let it upset him. "I've seen," writes Valz, "that one of the members of the Academy revives the stars of [Gaetano] Cacciatore and [Louis François] Wartmann." Indeed, in the *Comptes rendus de l'Académie des sciences* of October 12th, on page 716, appear the following words: "Several academicians have examined whether there might be any truth in identifying Le Verrier's planet with two other objects observed several years ago by Messrs. Cacciatore and Wartmann". Wartmann, an amateur astronomer in Geneva, had observed in 1831 an object that, like Neptune, followed a slow retrograde motion,¹⁶ and published an account of it in 1836 in the *Comptes rendus*.¹⁷ The identification of the perturbing planet with Wartmann's object was proposed by Guglielmo Libri, a member of the Academy of sciences. Though Libri was a good enough mathematician, he chiefly devoted himself to polemics and to plundering libraries, and was an avowed enemy of Arago. He wanted to minimize the credit for Le Verrier's discovery by insinuating that the planet had been discovered previously. During the following meeting on 19 October, Arago showed decisively that neither Cacciatore nor Wartmann could possibly have observed the new planet,¹⁸ a conclusion fully corroborated by subsequent research; in particular, though Wartmann's object was, in 1831, 18° from the position where Neptune was found in 1846, the latter's motion in the heavens was too slow for it to be the same object. The daily press, nevertheless, until the end of 1846, tried to stir up controversy by repeating claims or propositions similar to those of Libri.¹⁹

It would later appear that Wartmann may well have recorded a planet – but it was not Neptune. Wartmann recorded his object 9° from where Neptune actually lurked in 1831. Nevertheless, the position he gave – if one assumes a small error in plotting or reading the position from a map, or perhaps a failure to properly apply a correction for precession – agreed closely with that of Uranus!

More serious criticisms began to surface as soon as it became apparent that the true orbit of the new planet differed significantly from that predicted by Le Verrier. It is true that Le Verrier had indicated the planet's elements with deceptive

¹⁶ Wartmann is often cited by the UFO fans as having observed one in 1831.

¹⁷ *CRAS 2 (1836) pp. 307–311.

¹⁸ *CRAS 23 (1846) pp. 741–754.

¹⁹ See *Centenaire de la naissance de Le Verrier* (1911), pp. 51–52.

Fig. 2.12 William Lassell
(1799–1880)



Fig. 2.13 Neptune and his
brightest satellite Triton, as
seen by the Hubble Space
Telescope



precision, when in fact they were necessarily rather uncertain. In his enthusiasm, he had confidently fixed the mean distance within an excessively narrow range, giving 35–38 astronomical units (a.u.) for the semi-major axis of the orbit. After the discovery, the actual value was found to be only 30 a.u. Similarly, he put the sidereal period between 207 and 233 years, when in fact it is only 164 years. Moreover, the discovery of Triton, a satellite of Neptune, on 10 October 1846 by the English amateur astronomer William Lassell (Fig. 2.12), using a 24-in. (60 cm) reflector, led at once to an accurate determination of the planet's mass (Fig. 2.13, Box 2.3). Lassell

Box 2.3 Determining the Mass of a Planet Having a Satellite

A planet with a satellite, such as Neptune with Triton, allows by the measurement of the semimajor axis a of the satellite's orbit and ascertainment of its period of revolution T a determination of the planet's mass. One begins by setting the attractive force of the planet on the satellite equal and opposite to the centrifugal force. Supposing for simplicity that the orbit is circular, with radius a , this becomes:

$$GMm / a^2 = mv^2 / a,$$

where G is the constant of gravitation, M the mass of the planet, m that of the satellite, and v the speed in the orbit. Eliminating m , the orbital speed is $v = 2\pi a / T$. From this one deduces:

$$M = 4\pi^2 a^3 / GT^2.$$

What one directly measures is the angular radius of the satellite's orbit. To deduce from that the linear radius a of the orbit, it is necessary to know the distance of the planet, and at first that produced perplexities in the case of Neptune.

kept Triton under observation for several months, during which he established that the period of revolution around Neptune was just under 6 days. He worked out the orbit of this satellite,²⁰ and using the distance for the planet given by Le Verrier, derived a mass for Neptune of 20 times that of the Earth. Le Verrier had expected the planet to have a mass 36 times that of the Earth. (In fact, even Lassell's value was too high, since he used an incorrect value of the distance; we now know that the mass of Neptune is 17.2 times that of the Earth.) Clearly, the discrepancies between the predicted and actual elements were substantial, and this led the Harvard astronomer Benjamin Peirce to maintain, in a discussion of Lassell's observations, that Galle's discovery had been a matter of sheer luck.²¹ But it was soon noted that Peirce himself had made a mistake: in discussing Lassell's observations, he incorrectly deduced that Triton revolved around Neptune in 21 days, in which case the mass of Neptune would have been much too small to have had any significant effect on the motion of Uranus.²²

All these criticisms melted away, however, in face of the immense success of the discovery of Neptune.

²⁰ *CRAS 25 (1847) pp. 465–466.

²¹ *Proceedings of the American Academy of Arts and Sciences* 1 (1846–48), pp. 286–295.

²² From a contemporary article in **Revue des deux mondes*.

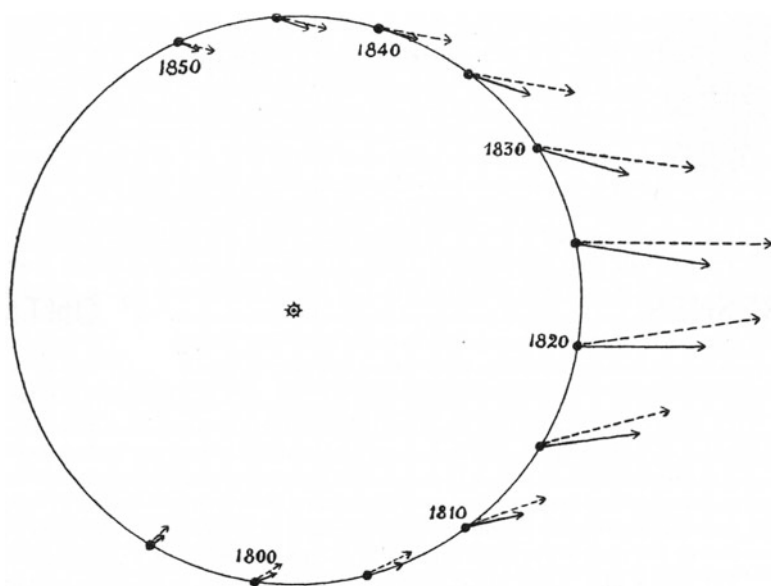


Fig. 2.14 Comparison of the perturbing force exerted on Uranus by Neptune at different epochs (*full arrows*) and by the hypothetical planet of Le Verrier (*dashed arrows*). One sees that the direction of the perturbing force is approximately correct (although the date of conjunction of Neptune with Uranus is too late by 1½ years). However, the intensity of the perturbing force is too large (From Danjon [*Ciel et Terre* 62 (1946) pp. 369–383, Fig. 4])

The solution to these difficulties lies in the fact that the only quantity that can be accurately determined by the study of perturbations is the intensity of the perturbing force. In this case, it was that exerted by Neptune on Uranus near the times of conjunction of the two planets, i.e., when they are closest together, for example in 1821 (Fig. 2.14). In accordance with the law of gravity, the force is proportional to the mass of Neptune and inversely proportional to the square of its distance from Uranus. Figure 2.15 shows that the distance between Uranus and Neptune predicted by Le Verrier for this epoch does not differ greatly from the actual distance: it is, however, a bit too great, an error that is more or less canceled out by the excessive mass he assigned to the planet. Having predicted a too-large semi-major axis for the orbit, Le Verrier exaggerated its eccentricity, which is in fact nearly zero i.e., it is (circular). But these are subtleties that need not concern the non-specialist.

Delaunay, an acknowledged master of celestial mechanics, but not on good terms with Le Verrier, summed up matters in 1868²³:

M. Leverrier [Delaunay always wrote it this way] is certainly a talented individual. He has done excellent work in theoretical astronomy, and has bequeathed to science the best tables we possess concerning the movements of the Sun and of the planets Mars, Venus, and

²³ Letter reproduced by Bigourdan in *Annu. BdL* for 1933, pp. A.30–A.33.

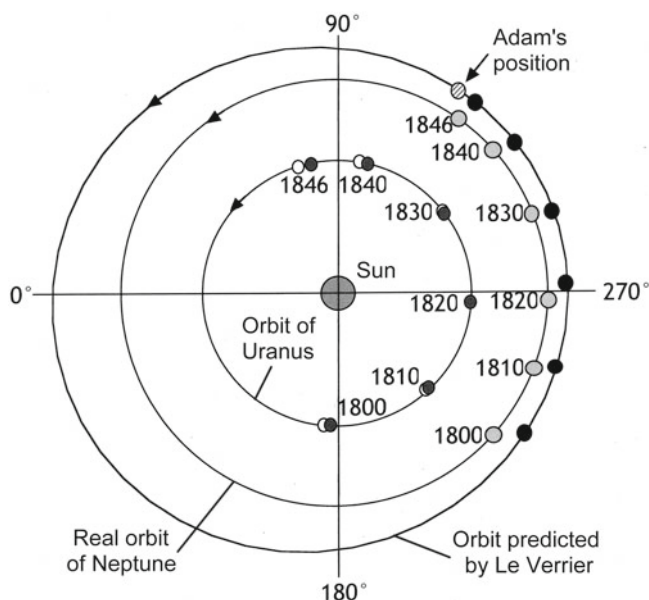


Fig. 2.15 The orbits of Uranus and Neptune. The axes mark longitudes as seen from the Sun. The position of Uranus in its orbit is indicated for different dates from 1800 to 1846 by the *dark grey circles*. The *empty circles* represent the positions that Uranus would have occupied if not perturbed by Neptune (the differences are very exaggerated on the figure). The positions of Neptune in its orbit for the same dates are also marked with *grey circles*. The attraction of slower moving Neptune is accelerating Uranus before the 1821 conjunction, and retarding it afterward. Le Verrier's orbit of Neptune is shown, with positions at the same dates indicated by *black circles*. Seen from the Earth, the position calculated for the date of the discovery is 1° behind the real one. John Couch Adams's September 1845 orbit for Neptune is not very different from Le Verrier's for the same period, but Adams's predicted position for the date of discovery (*hatched circle*) lies more than 2° from the real position, while Adams's later revised orbits from the summer of 1846 put the planet farther and farther from its actual place in the sky

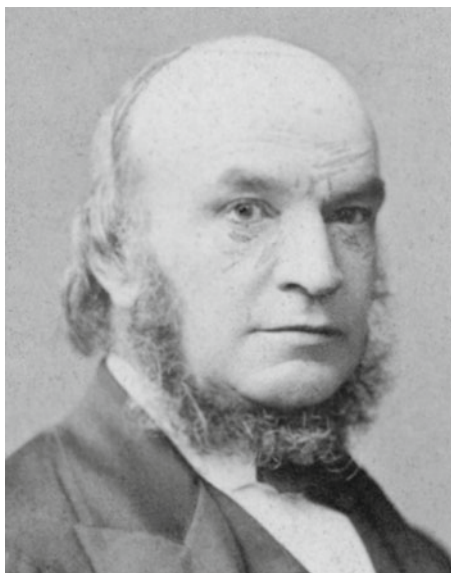
Mercury. It is remarkable that the work leading to the discovery of Neptune which served to establish his immense reputation (with the assistance of M. Arago) was nevertheless the worst of all his works: he did not dare introduce them to the *Annals of the Observatory*, where he has published all his other Memoirs.

This judgment of Delaunay is severe, but it is partly justified by the exaggerated precision with which Le Verrier believed he was able to determine the elements of the new planet. In defense of Le Verrier, one must say that the problem he solved was entirely new. However, he had not been the only one to tackle it.

The Competition

Given that the idea of the existence of planet disturbing the movements of Uranus was very much in the air, it is not surprising that several astronomers attempted, as Le Verrier did, to predict its position through calculations. Who, then, were these competitors?

Fig. 2.16 John Couch Adams (1819–1892)



One of those interested in this problem was none other than the great Friedrich Wilhelm Bessel himself. He had spoken as early as 1840 about the idea of a perturbing planet in a public lecture. About 1845, he wrote to Alexander von Humboldt²⁴:

I believe the moment will come when the solution of the mystery of Uranus will perhaps be furnished by a new planet, whose elements will be ascertained by its action on Uranus and verified by that which it exerts upon Saturn.

As noted before, Bessel, in 1840, had gone so far as to assign his student Friedrich Wilhelm Flemming the task of collecting and reducing the observations of Uranus, so as to compare them with the tables of its motion. Flemming's premature death, and a long illness leading to that of Bessel himself in 1846, prevented their success. Otherwise, it is entirely possible that Bessel, a mathematician of genius, would have arrived at the solution, perhaps even before Le Verrier could do so.

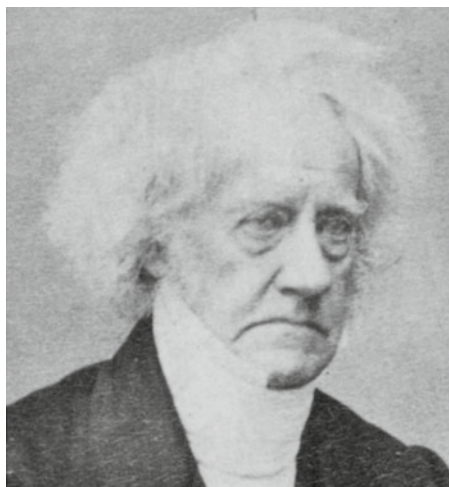
Another competitor actually did succeed: John Couch Adams (Fig. 2.16). Less than 2 weeks after Le Verrier had announced the discovery of the planet to the Academy of sciences, on 5 October 1846, Le Verrier received a letter from Airy who was just back from a trip in Germany. In this letter dated 14 October,²⁵ one reads the following sentences:

I do not know whether you are aware that collateral researches had been going on in England, and that they had led precisely to the same result as yours. I think it probable that I shall be called on to give an account of these. If in this I shall give praise to others I beg that you will not consider it as at all interfering with my acknowledgement of your claims.

²⁴ From Tisserand (1889–1899) t. 1, p. 375.

²⁵ Reproduced in *Centenaire de la naissance de Le Verrier* (1911), pp. 29–30.

Fig. 2.17 Sir John Herschel (1792–1871) in old age. Son of Sir William Herschel, John Herschel was the most popular astronomer of England of his time. His influence was very important



You are to be recognized, beyond doubt, as the real predictor of the planet's place. – I may add that the English investigations, as I believe, were not quite as extensive as yours. They were known to me earlier than yours.

Le Verrier, who knew nothing about any of this, responded sharply to Airy on 16 October²⁶ (mail was at least as rapid in those days as it is today!):

The satisfaction you have given me has been, I confess, disturbed by a letter of Mr. Herschel as communicated to me, which is in very bad taste, and fails to do me justice.²⁷...

What can be his motive? I can't quite understand him, especially when he descends to insinuations which I find mortifying. Of what use, therefore, is it for Mr. Herschel to cry out, before all England, that I was not good enough to deserve his confidence?...

Have I been so wrong in the theory of the secular inequalities? Is the theory of Mercury so far in error...?

... But the story is perfectly clear given that not a single line of serious work had been published [by anyone else] in the course of all my researches. And only now Mr. Herschel belatedly comes around to raise a claim in favor of historical documents!

Why would Mr. Adams have kept silent for four months? Why wouldn't he have spoken from the month of June (onwards) if he had had something to say? Why wait until the object has been seen in the telescope? I could add many other questions as well on this subject. But I will only ask one of Mr. Herschel. How is it that the son of the immortal astronomer who discovered Uranus has the audacity to write that my calculations alone would not have given confidence he showed before the British Association? Why, the day after the discovery of my planet, does he not see that he brought into question his scientific judgment, by placing under an injurious suspicion a labor which in fact had been confirmed in the most spectacular manner?

The incident to which Le Verrier refers here was this. Sir John Herschel (Fig. 2.17) had published a letter in the journal *The Athenaeum*, on 3 October, which became

²⁶ *Centenaire de la naissance de Le Verrier* (1911), pp. 30–33.

²⁷ On the role of John Herschel, see Kollerstrom, N.: John Herschel on the discovery of Neptune, *°J. Astron. Hist. Herit.* 9 151–158 (2006).

known on the other side of the Channel right after the discovery of Le Verrier. In it he stated that he had had confidence in the French astronomer's calculations only because they had been corroborated by those of Adams. After Le Verrier and Airy exchanged several letters, Airy attempted to set out a full account of the matter. He wrote:

I received your letter of the 16th and I am very sorry to find that a letter published by Sir John Herschel has caused you so much pain.... I am certain that Sir John Herschel would be equally sorry, for he is the kindest man, and the most scrupulous in his endeavours to do justice to all persons without giving offence to any, that I ever saw. I am confident that you will find, upon examining closely into the matter, that no real injustice is done to you: and I hope that you will receive this expression the more readily from me, because I have not hesitated to express to others as well as to yourself very strong feeling upon the extraordinary merit of your proceedings in this matter. This I intend shortly to express in a more public manner.... Meanwhile I will state to you a few facts and a few considerations which will enable you to judge of the justice of Sir John Herschel's expressions.

A considerable time ago, probably in the year 1844 or in the beginning of 1845 (I have not had leisure since my return [from Germany] to refer to my papers) I supplied Mr Adams with several places of Uranus, expressly for an investigation into the cause of its disturbance. In October or November 1845 I received from Mr Adams a notification that the disturbances could be explained by supposing another planet to exist, of which he gave me the elements.

Shortly after this I addressed to him the same inquiry which I afterwards addressed to you, namely whether the error of radius vector was explained by the same disturbing planet. I know not whether any accident prevented Mr Adams from receiving my letter: at any rate, he gave me no answer. Had he answered me, I should have urged him immediately to publish his investigations.

In June 1846 the *Comptes rendus* issue containing your investigations was received by me: I was astonished and delighted to find that the elements were nearly the same and the present apparent place of the disturbing planet nearly the same as those given by Mr Adams' investigations.

On June 29th a meeting of the Board of Visitors of the Royal Observatory was held [at Cambridge] at which Sir J. Herschel and Professor Challis were present. At this meeting there was question about the expediency of distributing subjects of observations among different observatories, and I strongly urged the importance of distribution in some such cases, and I specially stated the probability of now finding the disturbing planet if one observatory could be devoted to the search for it. I gave as my reason the very strong evidence afforded by the agreement of the result of your researches and Mr Adams' researches. It was my strong statement upon this that induced Sir John Herschel to express himself at the meeting of the British Association and to write such a letter to *The Athenaeum*.²⁸ It was my statement which (followed by some correspondence) induced Professor Challis to search for the planet.

Professor Challis commenced his search on July 29, and saw the planet first on August 4, and subsequently on August 12 [without comparing his observations and so failing to realize it was a planet]. All the rest of the history is known to you.

Airy's strategy was to attempt to justify Herschel's distrust by insisting on the necessity of verifying the calculations, and by affirming that the English astronomers had waited to make their announcement until Adams's results were confirmed by

²⁸ One researcher (Kollerstrom 2006) has shed doubt on the insistence of Airy during the Board of Visitors meeting. It is however clear that he persuaded Challis on July 9 of the necessity to search for the planet.

those of Le Verrier, rather than the other way around. For his part, Challis wrote on 15 October in *The Athenaeum*, giving details about his observations and maintaining that he had seen on 12 August in the region of the sky where Adams had indicated the perturbing planet, an object that was not where it had been on 30 July:

I undertook to make the search,--and commenced observing on July 29. The observations were directed, in the first instance, to the part of the heavens which theory had pointed out as the most probable place of the planet; in selecting which I was guided by a paper drawn up for me by Mr. Adams... On July 30, I went over a zone 9' broad, in such a manner as to include all stars to the eleventh magnitude. On August 4, I took a broader zone,--and recorded a place of the planet. My next observations were on August 12; when I met with a star of the eighth magnitude in the zone which I had gone over on July 30, --and which did not then contain this star. Of course, this was the planet;--the place of which was, thus, recorded a second time in four days of observing. A comparison of the observations of July 30 and August 12 would ... have shown me the planet. I did not make the comparison ... partly because I had an impression that a much more extensive search was required to give any probability of discovery--and partly from the press of other occupations. The planet, however, was *secured*.... The part taken by Mr. Adams in the theoretical search after this planet will, perhaps, be considered to justify the suggesting of a name. With his consent, I mention *Oceanus* as one which may possibly receive the votes of astronomers.

The publication of these letters from England, notwithstanding that they contained Challis's admission of failure, excited considerable alarm in France. Arago mounted the battlements to defend Le Verrier before the Academy on 19 October, alleging justly that the English had not published anything and that "there exists only one rational and just way to write the history of science: that is, to rely exclusively on publications having a definite date; beyond that, everything is confusion and obscurity."²⁹ The cartoonists were let loose on an orgy of nationalism and proceeded to attack Adams and defend Le Verrier (Fig. 2.18).

Eventually, things would settle down. English astronomers, including Airy and Herschel, recognized Le Verrier's priority, at least for the time being: later they would qualify their position. In particular Airy, in a long presentation given before the Royal Astronomical Society on 13 November 1846, put Adams and Le Verrier on an equal footing, even while showering the latter with praises.³⁰ It is this version by Airy that would long seem the definitive source among historians, planting the seeds from which stereotypical images of the protagonists grew up. Adams was cast as a shy and callow youth who would go on to be acknowledged as "the greatest English astronomer after Newton." Challis, for his part, was considered (not entirely without reason) as an incompetent bumbler who recorded the planet without recognizing the significance of what he had seen. Airy himself was the narrow bureaucrat, etc. Only recently have historians been systematically taking a second look at the affair³¹; here it must be noted that the original English documents actually disap-

²⁹ *CRAS 23 (1846) pp. 751–754.

³⁰ ° *Monthly Notices of the Royal Astronomical Society* 7 (1846) pp. 121–144. There is a long article by Airy on the discovery of Neptune in *Astronomische Nachrichten* 25 (1847) pp. 133–160.

³¹ See Kollerstrom, N.: A hiatus in history: the British claim for Neptune's co-prediction, 1845–1846. *Sci. Hist. Publ.* 44, 1–51 (2006), <http://www.ucl.ac.uk/sts/nk/neptunestory.pdf> and Sheehan, W., Kollerstrom, N., Waff, C.: The case of the Pilfered Planet. *Sci. Am.* 291(6), 92–99 (2004).

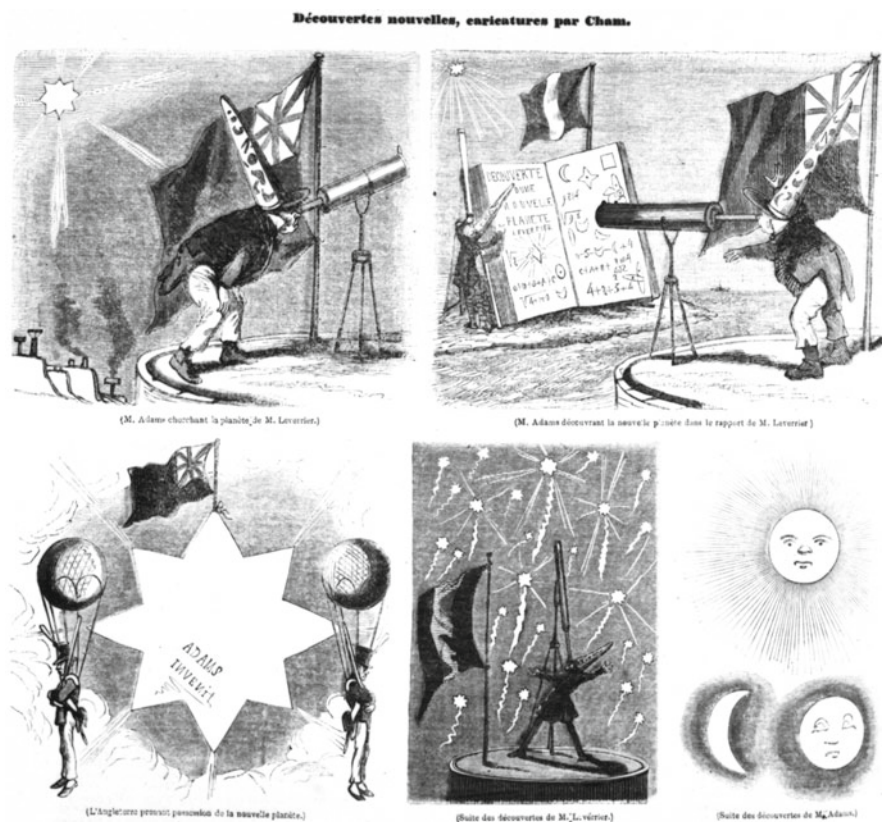


Fig. 2.18 Caricatures from Cham about the discovery of the new planet, published on 7 November 1846

peared in the 1960s, only to be recovered in 1998. In contrast to Le Verrier, Adams, who had nonetheless demonstrated a great ability (his method was similar to Le Verrier's, though he had used Peter Hansen's equations instead of those of Laplace), had hesitated to make known his results. Even though he had calculated orbital elements of the new planet as early as October 1845, he did not give Airy all the information needed to induce the latter to undertake a search for the planet. This is contrary to what Airy seems to affirm in his letter of 19 October 1846, cited above. Still lacking complete confidence in his calculations as late as the summer of 1846, he had rather desperately tried to use the positions of Wartmann's object to narrow the scope of his investigation. Also he had relied on the information provided by Le Verrier on 1 June 1846 to produce for Challis an ephemeris of sky positions to direct the latter's search (this was the "paper drawn up for me by Mr. Adams," which Challis alluded to, somewhat misleadingly, in his letter of 15 October 1846). Adams estimated the uncertainty of the positions in this paper as 20° in longitude, which was much greater than that of Le Verrier and would have reinforced Challis's

expectation that he was undertaking a prolonged siege rather than a brief skirmish. Adams's hesitation, which contrasts with Le Verrier's assurance, seems to have been a function of his personality; perhaps also he suffered from Asperger's disorder, or high-functioning autism, which may have made it difficult for him to communicate his results to others.³² In any case, historians nowadays tend to endorse Airy's verdict on Le Verrier, and see virtue in "the firmness with which he proclaimed to observing astronomers, 'Look in the place which I have indicated, and you will see the planet well.' ... It is here, if I mistake not, that we see a character far superior to that of the able, or enterprising, or industrious mathematician; it is here that we see the philosopher." Adams, though Airy implicitly defends him, is nevertheless placed in a different and inferior category.³³ Challis also had some excuse for his tardy recognition of the planet's presence among the stars he was mapping: he did not have available the Hour XXI map of the Berlin Academy star-map Airy had suggested he use, since it had not yet been sent out from Berlin. But he had – or at least the Cambridge University Observatory had – another chart from the same series, which contained the section of the sky in which the planet was lurking and which would have sufficed for its detection. However, the existence of this chart has been unearthed only by recent investigators. Mercifully, Challis himself probably never realized he had it.

Another footnote to the Neptune discovery story is the fact that John Herschel himself nearly discovered the planet on 14 July 1830, as he informed Le Verrier in a letter written on 9 January 1847. He recognized the object he observed was not a star because it showed a small disk, but he supposed it to a planetary nebula and thought no more of it.

Also, as with Uranus, an observation of Neptune turned up that was made a long time before its discovery: Michel Lefrançois de Lalande, the "nephew" of Joseph Jérôme de Lalande, observed Neptune on 6 and 8 May 1795 with the transit instrument of the observatory at the École militaire. He recorded it as a "star" whose positions differed slightly between two observations. However, believing this to be an observational error, his "uncle" Jérôme de Lalande only gave the second position, that of 8 May, in his *Histoire céleste*. The star is given in the Berlin maps, which were based partly on Lalande's catalog, but it had gone missing when the American astronomer Sears Cook Walker returned to this location on 2 February 1847.³⁴ Walker correctly deduced that the missing star might be Neptune, and using Lalande's position for it in 1795 was able to work out the first high-quality orbit for the planet: the semi-major axis was 30.2 a.u., the eccentricity 0.0088, and the period of revolution of 166.4 years, values which are much closer to the modern values than those of Le Verrier and Adams given in Table 2.1.

³² Sheehan, W., Thurber, S.: John Couch Adam's Asperger syndrome and the British non-discovery of Neptune, *Notes and Records of the Royal Society*, **61**, 285–299 (2007).

³³ ° *Monthly Notices of the Royal Astronomical Society* 7 (1846) p. 142.

³⁴ See Kollerstrom N. (2006) *op.cit.* p. 33.

Janus, Oceanus, Neptune or Le Verrier?

Astronomers have habitually named planets and asteroids after the gods and goddesses of the Greeks and Romans. After the discovery of the new planet, it was necessary to agree on a name for it. Normally, the astronomer who makes the discovery offers a proposal, and a learned scientific society votes on its appropriateness. As we have seen, Galle proposed the name Janus, then Adams and Challis suggested Oceanus. (A brief review of Greco-Roman mythology may not be out of place here: we present this in Box 2.4). Since Le Verrier was considered to be the true discoverer of the planet – as Arago put it so poetically, he had discovered it “at the tip of his pen” – these other suggestions were dark horses at best. Ultimately, it was Le Verrier’s prerogative to name the planet. Indeed, it seemed to have been Le Verrier himself who first proposed Neptune, asserting, moreover, to his correspondents that the Bureau of longitudes had already selected this name and introduced a symbol for the planet, a trident (curiously, transcripts of the society show no record of any of this). To illustrate the confusion, consider this excerpt from Airy’s letter of 14 October 1846 cited earlier:

There is one thing which somewhat disturbs my mythological ideas, namely the name Neptune, which (it is understood,) you propose to fix upon the planet. There seems to be an interruption of order which is unpleasant. If you would consent to adopt the name Oceanus instead, it would, I think, be better received, as more similar in its character to that of its predecessor, Uranus, and more closely related to the mythological ideas of the Greeks. I beg you to think of this carefully, for experience has shewn that a name will not last unless it is well selected. The name of *Stella Medicea* [given by Galileo to the satellites of Jupiter in honor of the Medicis] has perished, and the adjunct of *Ceres Ferdinandeia* [by Piazzi] has perished, and the name *Georgium Sidus* [for Uranus, by William Herschel] has perished, although all these were given by their respective discoverers.

Box 2.4 A Review of Greek and Roman Mythology; Greek Names Are in Italics, Latin Names in Roman Font

Uranus (*Ouranos* for the Greeks) was born from the Earth (*Gaia*). Uniting with his mother, he engendered Oceanus (*Ocean*), the first river, the father of all the others, and six other deities of whom the last was Saturn (*Cronos*). The latter cut off the genitals of Uranus, from which the semen issuing into the ocean led to the birth of Venus (*Aphrodite*), whereas the Giants were born from his blood. United with Rhea, *Cronos* engendered Jupiter (*Zeus*). He later vomited out several infants, of whom Neptune (*Poseidon*) was one. Mars (*Ares*) was son of Jupiter and Juno (*Hera*), and Mercury (*Hermes*) of Jupiter and Maia, one of the Pleiades. Finally Apollo (*Apollo*), god of the Sun, and Diane (*Artemis*), goddess of the Moon (the Greeks considered also *Selene*, daughter of *Hyperion* and *Theia*, as a personification of our satellite), were twin infants of Jupiter and Latone (*Leto*). Janus with two faces was a purely Roman god.

In fact, Oceanus, the name proffered by Challis and Adams and now endorsed by Airy, was never seriously considered outside of England. But now Le Verrier, having first proposed Neptune, seems to have had second thoughts. Unaccountably, he resigned the task of choosing the planet's name to Arago. Arago, in turn, promptly proposed a different name – “Le Verrier.” In a rather fawning letter of 6 October to Encke, Le Verrier pretended embarrassment at this turn of events:

Mr. Schumacher has done me the honor of writing, and asks me to send him a name for the planet.³⁵ I have asked my illustrious friend, Arago, to take charge of this care. I was a bit taken aback by his decision announced before the assembly of the Academy. I would not be able to explain in what my consternation consists if I did not find at the same time an opportunity of paying tribute to your admirable work on Encke's comet.³⁶ The obscure name that M. Arago wants to give to the planet would bestow upon me the same honor as that accorded the illustrious Director of the Berlin Observatory [who was none else than Encke himself], and I do not deserve it.

What Le Verrier says here agrees with the transcript of the meeting of 5 October of the Academy of sciences, during which Arago set forth his ideas about the discovery of the planet³⁷:

... Mr. Arago has announced to the Academy that he has received from Mr. Le Verrier a very flattering assignment: the right to name the new planet. He has accordingly decided to give it the name of the person who has so deftly discovered it, and to call it Le Verrier... How is this! One names comets with the names of astronomers who have discovered them, or of those who have computed their orbits, and does one refuse the honor to the discoverers of planets?!... Is someone preoccupied or worried because this resolution would seem to entail other changes? Fine, I don't subscribe to this alone: Herschel must dethrone Uranus, the name of Olbers will be substituted for that of Juno [a minor planet that Olbers had discovered], etc.; it is never too late to shed the swaddling clothes of old habits. I will commit myself, Mr. Arago concluded, to never call the new planet by any name other than Planet Le Verrier. I believe that in this way I will give an undeniable proof of my love for science, and will follow the inspirations of a legitimate national sentiment.

Not everyone was in agreement with this proposal. The following appeared in the *Revue des deux mondes*:

We will only say one word concerning a minor incident that has arisen regarding the discovery by Mr. Le Verrier: what name will be given to the new planet? Despite the judicious observations of M. [Louis Jacques] Thénard and M. [Louis] Poinsot, M. Arago persists in calling this planet by the name of Le Verrier.

³⁵ Heinrich Christian Schumacher, director of the Altona Observatory near Hamburg, created the most important astronomy journal of the time, the *Astronomische Nachrichten*, and was at a consequence at the center of European astronomy.

³⁶ Contrary to planets, comets are designated by the name of their discoverer; actually, it was Pons who discovered the comet in question, but because Encke found previous observations and showed that this comet had the shortest known period, his name has been attached to it. The same thing had occurred before with Halley's comet.

³⁷ *CRAS 23 (1846) p. 662.

Fig. 2.19 Heinrich Christian Schumacher (1780–1850)



Le Verrier, however, was evidently highly satisfied with Arago's proposal. Moreover, he now attempted to regularize the situation by using for Uranus the name *Herschel*, a name which had hitherto been used only sporadically³⁸:

In my subsequent publications, I will consider it a strict duty to make disappear completely the name Uranus, and to only refer to the planet using the name HERSCHEL. I sorely regret that my already published writings do not permit me to follow the determination that I shall religiously observe henceforth.

Nevertheless, the name "Le Verrier" would encounter more and more fierce opposition, and finally the name Neptune would be adopted. How did this happen? It is not entirely clear. A letter of John Herschel to Le Verrier shows that John Herschel did not wish to give the name of his father to Uranus, and therefore, *a fortiori*, the name of Le Verrier to Neptune. It's interesting in this regard to page through the volumes of the *Astronomische Nachrichten* of Heinrich Christian Schumacher (Fig. 2.19), a publication central to astronomy at this period.³⁹ We have noted earlier that there is no trace of a decision in favor of the name Neptune in the proceedings of the Bureau of longitude's meetings, though it would have been within their purview to make such a decision. Arago, moreover, was there to supervise the matter. He manifested, also, his discontent, by recalling in the *Annuaire du Bureau of longitudes* for 1847 (p. 371) that "he had proposed to call the planet Le Verrier, and that foreigners, leaning on alleged decisions of the Bureau

³⁸ Le Verrier U.-J.-J. (1846) *Recherches sur les mouvements de la planète Herschel (dite Uranus), Connaissance des temps* for 1849, Additions, pp. 3–254. The memoir contains most of Le Verrier's work leading to the discovery of Neptune. The cited text is a note at the bottom of p. 3. See also the letter of Le Verrier to Schumacher in *Astronomische Nachrichten* (1847) 25, pp. 237–238.

³⁹ See in particular *Astronomische Nachrichten* (1847) 25, pp. 192–196 (Encke) and pp. 309–314 (Challis). The name of Neptune appears to have been definitively adopted in May 1847.

of longitudes, call it nowadays Neptune.” Then he complained that he did not find any collaborators to help him write a “detailed history of the new planet,” which is hardly surprising because no one wanted to get mixed up in such a scabrous affair.

Le Verrier was evidently furious about the Bureau of longitudes’ decision to adopt name Neptune, and wrote to Airy on 26 February 1847:

When the planet was discovered, it was proposed by the Bureau of longitudes to call it Neptune. I was not part of the Bureau at the time, and I did not charge it with this decision.... I declared ... to M. Arago that the Bureau was a little too hasty, and that I would specifically entrust him with the task of presenting to the Academy of sciences whatever he judged to be most suitable. Since then have had no further involvement in the matter.

Airy responded two days afterwards that he himself would adopt the name Neptune because of the agreement of the “principal astronomers of Northern Europe,” and, of course, his “English friends.”

In the end, everything about this muddled affair becomes comprehensible as soon as one admits to a certain duplicity on the part of Le Verrier. Still, in retrospect, Arago’s fervidly eulogistic interventions before the Academy are also perplexing. One suspects there may be something missing from the whole account. One perhaps far-fetched possibility hinted in a letter written in 1869 by Delaunay to the Minister of Public Instruction. After complaining about Le Verrier’s willingness to use blackmail to get his way (a subject for later), Delaunay says⁴⁰:

In 1846, in the aftermath of the discovery of the planet Neptune, M. Arago, driven by certain hideous circumstances from which it is not here appropriate to lift the veil, had placed M. Le Verrier on a pedestal, and made him out to be an extraordinary man, one of the greatest geniuses that France had ever produced. Some months later, M. Arago recognized his enormous mistake, but the harm was already done, and he could do nothing to repair the damage. His final years were darkened by his vision of the dreadful consequences which followed inevitably from this.

What were these “hideous circumstances?” It is indeed a shame that Delaunay did not raise the veil; since he died soon after writing this, he took the secret with him to the grave. There was some gossip that Arago had had an affair with Madame Le Verrier. Le Verrier, discovering it, took advantage of the situation by using it to blackmail Arago into supporting a proposition that was clearly indefensible. This, however, is impossible to verify, and seems rather far-fetched. What cannot be denied is that Le Verrier always seemed to feel that Arago never did enough for him, and this attitude would eventually lead to a definitive and fateful rupture.

⁴⁰ Lettre reproduced by Bigourdan in *Annu. BdL* for 1933, pp. A.30–A.33.

Chapter 3

Waiting in the Wings (1847–1853)



Portrait of Le Verrier by Charles Daverdoing (1846)

Le Verrier was now very famous. The discovery of Neptune had won the admiration of the entire scientific community and captured the imagination of the general public. He now consented to have his friend Charles Daverdoing make his portrait, which is reproduced in the front of this chapter. It would be the only painting of him made from the life (except for the drawing of Felix-Henri Giacomotti showing him on his death-bed (Fig. 7.26)). No photograph of Le Verrier exists; for some strange reason he refused to be photographed. Le Verrier and his discovery were the subject of numerous caricatures, of which Fig. 3.1 is an example. He was showered with honors: elected adjunct astronomer to the Bureau of longitudes on 14 October 1846, and on 15 November named professor of Astronomy and celestial mechanics at the Sorbonne, a chair specifically created for him. He was invited by Airy to visit England (Fig. 3.2), and completed the trip in 1847.

The Commencement of Hostilities

Intoxicated by his success, Le Verrier assumed an imperious bearing that was irksome to the scientific community, and even alienated him from the all-powerful Arago. Acrimony began to break out, first, at the Bureau of longitudes, from which Le Verrier resigned in February 1847 because “of the systematic opposition which he encountered there and the persistence with which he was kept away from the Observatory.” Already on 6 October he had complained in a letter to Encke that he “could not put his eye to the telescope, due to disgraceful circumstances.” In his resignation letter to the Count of Salvandy, Minister of Public Instruction,¹ he wrote on 24 February 1847:

When I was first appointed [to the Bureau] four months ago, in early October 1846, I saw immediately that I would encounter very great difficulties due to a certain influence there, and I did not decide to accept my installation at once. The circumstances which followed my installation led me to abstain for a very long time from presenting myself to the Bureau at all. These difficulties have been caused by a systematic opposition to my presence at the Observatory. In the Bureau of longitudes, where everything is done by a secret committee, consisting of twelve individuals of whom two-thirds submit to a single will [obviously that of Arago], resignation is the only recourse for an honorable man.

Le Verrier nevertheless would take up his position again at the Bureau on 8 October 1847, at the request of the Minister.

Joseph Bertrand, a mathematician who knew Le Verrier very well, would later describe as follows both Le Verrier’s behavior and its consequences²:

Absorbed in his own researches, and in no apparent hurry to communicate them or to share the results more widely, he had little curiosity regarding the work of anyone else; he censured others, on occasion, and pointed out the errors they had made, without a thought to tempering the brusqueness of his manner on these occasions. The dispiriting effects on others might have been lessened had he only found it in himself to be more gracious. Severe in the

¹ Archives of Académie des sciences, file Le Verrier.

² Bertrand J. (1880) p. 12.



LE VERRIER (URBAIN-JEAN-JOSEPH), célèbre astronome français, né à Saint-Lô (Manche), le 11 mars 1811. — On raconte qu'il manifesta de très-bonne heure sa vocation astronomique. Encore dans le sein de sa mère, quand elle se promenait le soir, au clair de la lune, il se remuait tant qu'il pouvait pour lui donner des crampes d'estomac; et comme ça la faisait bâiller beaucoup, il en profitait pour lever la tête et étudier

Fig. 3.1 Caricature of Le Verrier in *Le Charivari*

I understood from you that you were likely to visit England in June, probably to attend the meeting of the British Association (at which, ~~as~~ as Colonel Sabine informed me, you were expected). In that case, I would suggest that you should come to Greenwich some time earlier: probably before the end of May, and at any rate before the first Saturday of June (when the annual visitation of the R. Observatory takes place). If you will give me notice of the route which you take, I will give you instructions as to the best way of coming to Greenwich.

OBSERVATOIRE
DE
PARIS

I am, my dear Sir,
Sincerely yours
G. B. Airy

Fig. 3.2 The end of Airy's letter to Le Verrier, dated 12 April 1847, inviting him to England

demands he placed upon himself, he was no less so in those he placed upon others. For this reason perhaps, or perhaps for no reason at all, he had little intercourse with the other astronomers. In consequence, he had few friends among them, and the brilliance of his successes did not make any difference. Through many controversies, the admiration with which he was at first regarded did not last, nor did it produce any sustained good will toward him.

In a letter written by Le Verrier on 3 May 1846 to his friend Emile Gautier, an astronomer in Geneva, he said that he was worn down by "the envy, the sloth, the ignorance, the greed and the other capital sins he encountered among his astronomical colleagues." There may be truth to this. At the same time, the letter testifies to his contempt for others.³

A controversy which broke out in 1847 between Le Verrier and two other astronomers at the Paris Observatory, Ernest Laugier and Victor Mauvais, is an example of his execrable relations with his colleagues.⁴ We saw in the first chapter that in 1844 Le Verrier had established the identity of a comet discovered by the Italian astronomer De Vico with one seen previously by Tycho Brahe. It was, therefore, a

³ Cited by Bigourdan, *Annu. BdL* for 1932, p. A.16.

⁴ *CRAS 25 (1847) pp. 945–953.

periodic comet. Now Laugier and Mauvais had simultaneously claimed to have done the same. Le Verrier would not accept this. Renewing the study of this comet, Le Verrier asserted that the calculations of these men were nothing more than rough estimates; his rivals ought to guard against concluding for the identity of comets based on a mere glance at the tables of their elements, which demanded little effort and testified to the adoption of unrigorous methods. Laugier and Mauvais replied that they had in fact calculated the orbits, and referred, in passing, to “the low estimation with which Mr. Le Verrier seems to hold those whose work involves the painstaking business of observations”; they allowed that, certainly, the method they had used “does not require a great exertion of genius; but it may be quite useful and serve as the inspiration for more happy discoveries, and so we dare to hope that astronomers, even though they run the risk of incurring Mr. Le Verrier’s censures, should continue to employ them just as they always had done.”

Even that, however, was not the end of it; Le Verrier would not let the matter drop, and the controversy, reported in the pages of the *Comptes rendus* of the Academy with an abundance of details which we today find hard to credit, went on interminably, becoming as it did so ever more personal and vindictive.

While having problems with certain of his colleagues, Le Verrier remained extremely popular and highly regarded within the spheres of government. On 8 October 1846, the Count de Salvandy, Minister of Public Instruction, addressed to King Louis-Philippe a letter,⁵ in which he served as Le Verrier’s intermediary in offering thanks to the king for his nomination as an officer of the Legion of Honor. According to protocol, this was an indirect way of passing along the information that Le Verrier desired to personally meet the King. The Minister was especially eager to bring this meeting about, considering “how few mathematicians and geometers there are with the right political opinions”, and believed that in Le Verrier Louis-Philippe “will have a conquest truly worthy of him.”

At this time, Le Verrier was already quite close to Louis-Philippe, who had named him as astronomy preceptor for his grandson Louis-Philippe II d’Orléans. The Observatory preserves a letter from Le Verrier to this child, with the latter’s letter of thanks (Fig. 3.3). The reception between Le Verrier and Louis-Phillipe on the present occasion was, however, a formal event (Fig. 3.4). The King, Salvandy, and also the Prime Minister Guizot, had all wanted to find a way to get rid of Arago as a political opponent. Arago had been deputy, without interruption, since 1831. At first he had been a supporter of the King, who seemed to him, as to almost all the French at the time, much less dictatorial than his predecessor Charles X. But he was soon disappointed. He then joined the opposition, and became a formidable, and a tenacious, adversary. The numerous discourses he gave to the Assembly are well known, particularly that of the 16 May 1840 in which he advocated the return of universal suffrage, abolished after the Revolution. The Observatory, where several astronomers were members of Arago’s family, became a hotbed of opposition. Not being able to discredit Arago in his capacity of deputy, where he was constantly

⁵ Letter cited in Levert, Lamotte and Lantier (1977) pp. 103–107.

Fig. 3.3 Letter of thanks to Le Verrier of Louis-Philippe II d'Orléans, aged 8, dated 1st November 1846

Je vous remercie, Monsieur, de la
 belle carte de la lune que vous
 m'avez envoyée. Je la regarderai
 souvent, car elle m'a fait grand
 plaisir, et en la regardant je
 penserai à toutes les choses in-
 téressantes que vous nous avez
 racontées et qui m'ont donné
 un grand désir de bien appren-
 dre.

Je vous prie d'embrasser
 pour moi votre petit garçon, qui,
 comme vous me l'avez dit, est
 né la même semaine que moi.

Louis Philippe d'Orléans.
 L^e Cloud le 1^{er} 9^{bre} 1846.

Fig. 3.4 Le Verrier paying a visit to the king Louis-Philippe



reelected without difficulty, the Government maneuvered to set up a rival for the position of director-apparent⁶ of the Observatory, hoping that the Bureau of longitudes, to which the Observatory then answered, would choose this rival as Arago's replacement. In the Bureau of longitudes, Le Verrier was the ideal person to fulfill this role. Not only was he celebrated for the discovery of the Neptune, but his political opinions placed him on the Far Right. It takes little imagination to suppose that the idea of replacing Arago at the Observatory would have seemed pleasing to him. Mr. de Salvandy at this point requested from him for a plan for his researches. Coming from the Minister of Public Instruction, this was a very rare distinction.

The research plan of Le Verrier⁷ was delivered to the Ministry on 18 February 1847. It laid out the working principles of the ambitious undertakings that Le Verrier would successfully realize over the course of his lifetime: i.e., the achievement of a complete mechanical theory of the Solar System. In order to construct the theory of a planet, Le Verrier said,

⁶ Arago never held the title of director of the Observatory, because the Bureau of longitudes always wanted to keep control, but from 1834 he was called "director of the observations."

⁷ *Centenaire de la naissance de Le Verrier* (1911) pp. 48–53.

It is necessary, in the first place, to carry out a series of observations, exact and numerous, of this planet, observations which are spread out over a considerable interval of time.

It is necessary, in the second place, basing oneself on the law of universal gravitation and taking into account the influence of all the masses, to investigate the form of the analytical expressions suitable to represent at any epoch the coordinates of the planet relative to the [fixed] stars....

The third task is to deduce from the observations the values of the constants, which are undetermined in the formulas, and which one tries to reduce to the smallest number possible.

The theory of the planet being thus obtained, it is indispensable, in order for it to take a definitive place in practical astronomy, to reduce it to a table of numbers...

To encompass in a single work the entire ensemble of the planetary system, to reconcile and render everything harmonious, if possible, and when this cannot be done, to declare with certainty that there exist causes of perturbations still unknown, whose origins are then and only then revealed, is the definitive project I have the honor to submit to your Excellency.

Le Verrier believed that this work would occupy 10–15 years, and that to complete it would “exceed the powers of a single researcher.” He asked, therefore, that he be permitted to “devote to this end all of his time,” and that he be given, for assistance, a calculator, paid 125 francs a month. He concluded his communication to the Minister: “It would be a great encouragement to me to be summoned before you at the end of each year to give an account of my efforts.”

Certain commentators have seen in this text an implicit appeal on the part of Le Verrier to be granted the directorship of the Observatory. That insinuation, if exists at all, must be regarded as extremely subtle. Certainly the Minister did not require such hints to be convinced of Le Verrier’s ambition. Moreover, the calculator Le Verrier had asked for was provided to him. This was a man named Buisson.

Invited by Airy, as we have seen, Le Verrier headed off to a triumphant voyage to England during the summer of 1847, which caused no little worry to his adversaries at the Paris Observatory. During this voyage, Le Verrier allied himself with several foreign astronomers who were very well known. Among the most eminent was Wilhelm Struve (Fig. 3.5), director of the Pulkova Observatory near St. Petersburg. Le Verrier thus strengthened his position. He could appeal to these eminent astronomers for support, if necessary, of his nomination to direct the Paris Observatory.

Le Verrier also wrote to Wilhelm Struve to discuss the observatory of Marseille, whose status was then being determined. No doubt he already had the idea, realized later, of turning this observatory, duly renovated, into a branch of the Paris Observatory (see following chapter). Struve replied in November 1847 in a very interesting letter written in French, which is reproduced in its entirety in Box 3.1 later in this chapter (see p. 63). He assessed the state of the European observatories, and noted that France was far behind other countries since it could boast only one important observatory, the Paris Observatory itself. He admitted that the latter, at least, had “raised itself to the level of the other great observatories of Europe and had distinguished itself in advancing celestial knowledge” (this remark cannot have pleased Le Verrier, who usually discounted the improvements Arago had carried out). “If the provincial observatories of France still exist,” Struve continued, “they

Fig. 3.5 Wilhelm Struve
(1793–1864)



Box 3.1 Letter of Wilhelm Struve to Le Verrier⁸

This is the letter referred to above (on p.62), in which Wilhelm Struve, the director of the Pulkova Observatory and a close ally of Le Verrier, gave his assessment of the situation of astronomy in France. It is reproduced here because of the great influence it had on Le Verrier's later initiatives as director. Pulkova, 7 December/25 November 1847 [the difference in dates is that between the Russian and Western calendars]

My very dear friend and colleague,

In one of your recent letters, you indicated that there was a question of reorganizing the Marseille Observatory. Permit me to communicate to you some reflections that this important news has inspired in me. France, a country to which science owes the immense progress in theoretical astronomy made during the last century, has fallen behind during this same period, in its participation in observational work, partly due to the confluence of unfavorable circumstances that Mr. Biot has laid out in an article appearing in the *Journal des Savants* for Sept. 1847. The last several years, indeed, the Royal Paris Observatory has put itself on the same level as the other great European observatories, and has contributed in a distinguished manner to our progress in celestial science. France, therefore, possesses a single observatory, whereas the other leading countries in Europe have a large number of them. Great Britain is the richest country in respect of astronomical establishments, for in the United Kingdom there are eight observatories attached to public institutions: those of Greenwich, Cambridge, Oxford, Dublin,

(continued)

⁸BOP, Ms 1072, 12. This letter was partially published in *Centenaire de la naissance de Le Verrier* (1911).

Box 3.1 (continued)

Armagh, Edinburgh, Glasgow and Liverpool, and another seven observatories set up by private individuals: South, Bishop, Dawes, Smyth, Cooper, Lassell, and Lord Rosse. We add to those the observatories found in the Colonies, at Madras and the Cape and at New Holland [Australia], and we find a total of 18 astronomical observatories, more or less active, according to individual circumstances, of which two are the only existing observatories in the Southern Hemisphere. In Germany, the observatories of Königsberg, Altona, Göttingen, Bonn, Munich and Vienna are of the first rank, both by the quality of their instruments and by the nature of the work that they perform. A considerably greater number of observatories of the second rank could be added, such as those of Leipzig, Prague, Mannheim, and Cremsmünster [now Kremsmünster, Austria]. Italy is no less rich in observatoires, although the work they are performing is rather inferior to that being done at the English and German observatories. In Russia nowadays there are eight state-of-the-art observatories, as well as a number of observatories of the second rank. Even in Sweden and Norway, there are the two observatories of Stockholm and Christiania [nowadays Oslo] which are well developed, and there is discussion of setting up a third in the ancient University of Uppsala. Dr. Lindhagen of Uppsala, a young, very distinguished savant, is even now at Poulkova, preparing himself for his duties as director of the new observatory of Uppsala.

In the course of the last century, there were several observatories in Paris, and a number of astronomical establishments in the French provinces, such as Marseille, Toulouse, etc. The other observatories have disappeared, and included in this number also is the one where the Lalandes carried out the titanic labor of the celebrated *Histoire céleste Française* [i.e., the observatory of the École militaire, where Lalande and his “nephew” Lefrancois de Lalande had measured the positions of some 50,000 stars], and even if the provincial observatories of France still exist, they languish without serving any purpose useful to science.

To some extent the Marseille Observatory stands exempted from this criticism, for it was there that Pons and Gambart discovered a large number of comets, though unfortunately, lacking sufficient means to track the paths of these celestial bodies, they could not determine, during their apparitions, their exact positions on the celestial sphere. Even so, for these discoveries made in Marseille, science owes an enormous debt for the brilliant advances of cometary science. If the French government has resolved to build up practical astronomy in the country, it is clear that the Marseille Observatory has the first claim to assistance, and I dare to suggest that this, through your good office, be the recommendation presented to the beneficent, powerful, and enlightened chief of public instruction in France, Mr. le Comte de Salvandy. In the hope that you will not find it too forward in me, I presume to set forth some specific ideas I have for the Marseille Observatory.

Marseille enjoys a climate that is known to be most favorable for observing stars, with an extraordinary transparency of atmosphere, and a remarkable constancy of good weather. These advantages would impart to the Marseille Observatory the first place among all as regards planetary and cometary astronomy, were it in condition to make observations with perfect means.

Look, therefore, at what is necessary to achieve this goal.

A good meridian circle equipped with a refractor of at least 4, and if possible 5 or 6 *pouces* [slightly larger than inches] aperture, in order that planetary observations can be carried out at the same time as stellar observations, and provide a comparison with the equatorial observations.

A large equatorial refractor of at least 6 *pouces* aperture, or better yet a larger aperture, perhaps of 9 *pouces* like that of Dorpat [now Tartu in Estonia]. This telescope must be equipped with a perfect filar micrometer, in which the threads can be illuminated by reflection in the dark field of view, so as to render possible the reliable observation of comets.

(continued)

Box 3.1 (continued)

A comet seeker of the highest quality.

Two pendulums of the highest quality, of which one should be positioned next to the meridian circle and the other designated for use next to the large reflector set up in a revolving turret.

A good box chronometer which will serve to correlate the two pendulums.

This is all, but it is a lot: especially as the success of the Observatory depends on the installation of these instruments. I confess that I have no idea whether the current Marseille Observatory is working to obtain or set up these instruments, but I hope that France will not hesitate to commit to a new, perfect establishment, rather than remain forever hampered by the arrangements of an older one.

Such are my ideas concerning Marseille. One idea, however, gives rise to another. As the protector of Tahiti, would not France like to contribute to Southern Hemisphere astronomy, and set up in the other hemisphere, in a unique climate, a well-equipped observatory where the ideas of the greatest French astronomical observer of the past century, La Caille, could be carried out on a scale worthy of our era and of the glory of France. The English observatories in the Southern Hemisphere still leave much to be desired. It seems that even the one in Sidney [Sydney] has already fallen into a state of lethargy. Receive, my very dear colleague, assurances of my friendship and most sincere esteem, with assurances that I am,

your truly devoted,
W. Struve.

languish rather than serve any useful purpose. To some extent the Marseille Observatory is the sole exception, since [Jean-Louis] Pons and [Adolphe] Gambart have discovered there a great number of comets.”

Le Verrier saw his opportunity, and seized it. At the moment, everything seemed to be breaking in his favor. His plans had the support of the prime minister, Guizot. But then came the Revolution of 1848, chasing both Guizot and the king out of France. Le Verrier’s plans were forestalled. Astounded by the perfidy of a man he still held in esteem and whom he had continued to support, Arago now broke definitively with Le Verrier, as did the rest of his clique. Henceforth, it was war. For the moment, Arago was in a position of strength, since the Revolution carried him to the heights of power in the State.⁹

War!

Arago himself had neither the time nor the interest to take on Le Verrier, but a close associate took up the task. He was Jacques Babinet (Fig. 3.6), secretary of the library of the Paris Observatory, “a scientist who was,” said Joseph Bertrand, “very

⁹ See for example Lequeux (2008) Chap. 2.

Fig. 3.6 Jacques Babinet
(1794–1872)



intellectual, and much loved by the people.” He also had family connections with Arago.¹⁰ In August 1848, Babinet insinuated that in the calculations leading to the discovery of Neptune “enormous errors” had been made. He suggested that another planet, which he called Hyperion, had contributed an additional perturbing action to that of Neptune,¹¹ and asserted that “no one admits any longer the identity of Le Verrier’s planet with that which disturbed Uranus.” At once Sir John Herschel, Wilhelm Struve, the German mathematician Carl Jacobi and other foreign allies of Le Verrier rose to the latter’s defense.¹² But Le Verrier himself easily refuted Babinet¹³. He wrote:

1. Is it true that the direction in which I placed Neptune had an enormous error apart from the epoch of the discovery by M. Galle for a very few years before or after?
NO. This is false.
2. Is it true that there were enormous errors relative to its distance from the Sun?
NO. This is false. I simply placed Neptune a bit too far away.
3. Is it true that the theoretical mass of Neptune differs from the mass derived from the observation of the satellite to such an extent that it would be an irresistible argument against the identity of the theoretical Neptune with the observed Neptune? NO. This too is false.

The abbé François Moigno, a prolific popularizer of science and enemy of Le Verrier, echoed Babinet’s criticisms in the newspaper *La Presse*. Le Verrier demanded the right to respond, and did so with a long letter of rebuttal. Moigno

¹⁰ Babinet married in 1840 the sister of Ernest Laugier, who had himself married Arago’s niece, Lucie.

¹¹ *CRAS 27 (1848) pp. 202–208; a first answer of Le Verrier is pp. 208–210.

¹² Their letters can be found in *Centenaire de la naissance de Le Verrier* (1911) pp. 58–64.

¹³ *CRAS 27 (1848) pp. 273–279.

persisted, alluding to “enormous” differences that existed between the elements of Neptune’s orbit and those of “Le Verrier’s planet” as predicted. He wrote in *La Presse* on 25 September 1848:

As a professional who doesn’t engage in empty rhetoric or mental gymnastics, for whom the identity of two planets is not demonstrated by clever, ingenious, or arbitrary concurrences in the positions in the firmament and the distances from the Sun, but by a comparison, absolute and irrefutable, between the orbital elements themselves, we ask: (1) how can we agree to equate Neptune with the theoretical planet, and (2) how can we agree that the perturbations are equally and sufficiently explained by two totally different objects, especially when M. Le Verrier has taken the trouble to declare to us in advance that such an explanation is impossible, and warned us against the possibility of enormous errors, only now to claim that such enormous errors are an essential aspect of the problem he claimed to have resolved?

By taking this part in the affair, Babinet not only made himself ridiculous but left a legacy of bitter resentments that would take a long time to heal. The ill-will generated is attested to by a pamphlet published as late as 1892 by Emmanuel Liais, an astronomer who had worked under Le Verrier at the Paris Observatory and who had had, as did so many others, a bone to pick with him.¹⁴ Liais attempted to deny all merit to Le Verrier’s search for the perturbing planet. According to him, the planet’s position would have been very easy to establish, by means of a simple method. First Uranus’s unperturbed orbit could be found by utilizing only the observations before 1790. Then the perturber’s orbit have been deduced from the more recent observations, which showed its effect on Uranus’s motions. (Of course, Liais forgot that Le Verrier could not have known in advance where the perturbing planet was, and therefore he could not say at what times it would have the greatest disturbing effect on Uranus).

A Political Career

The discovery of Neptune made Le Verrier a celebrity in his native town. On 21 September 1847, 70 former students of the College de Saint-Lô offered a dinner in his honor. On 9 October, even the Minister of Public Instruction paid a visit to the new college then under construction. On this occasion, a great banquet was offered in the evening, during which the Minister gave a “pompous eulogy.” Le Verrier, who was seated directly in front of him.¹⁵ The deputy Léonor Havin offered a toast to the college, declaring that “the college that had produced the rival of Newton and the Herschells [sic], is hardly an ordinary college in the eyes of the world.” Le Verrier naturally was flattered by the homage paid to him by such high-ranking officials, and encouraged in his hopes that a political career would soon open up to him.

¹⁴ Liais (1892).

¹⁵ For details on the political career of Le Verrier and on festivities in Saint-Lô, see Levert, Lamotte and Lantier (1977), pp. 103–123.

Le Verrier and the Revolution of 1848

The Revolution of 1848 seemed to completely dash these hopes, as well as Le Verrier's ambition to replace Arago at the Observatory. Arago, in fact, played a very important role in the revolution, and found himself for 7 weeks president of the Executive commission that ruled France. The "Second Republic" established the principle of the "right to work," and organized national workshops for the unemployed. The closure of the national workshops, owing to problems with finance, unleashed in June an insurrection of the people of Paris (the "June days") that neither Arago nor his colleagues in the government were able to restrain. As for Le Verrier, he responded to the appeal that the government and the Assembly launched to re-establish the republican order, by attaching himself to the "garde bourgeoise," as the party of national guards levied from the bourgeois quarters of Paris called itself. During this crisis, Le Verrier wrote to Emile Gautier on 26 June 1848¹⁶:

I kept myself safe and intact from the battle, but I am still a bit dazed. We have only to regret the death of one member of my company. None of my friends have been touched. While I was at the foot of rue Saint-Jacques, the cannon thundered in front of the hotel behind my garden. Madame Le Verrier insists, however, that she was not afraid. But what conduct on the part of the Executive commission! It was impossible Friday morning to obtain a written order to retreat. Our four chiefs of the battalion have vainly pressed the executive chief [Arago; no doubt Le Verrier took a malicious pleasure in pointing out his helplessness!], and finally we sounded the retreat ourselves.

Having put down the insurrection brutally on 25 and 26 of June, General Cavaignac provisionally assumed power after the dissolution on the 24 June of the Executive commission. Le Verrier rallied to Cavaignac, and then to Louis-Napoléon Bonaparte, who was elected president of the Republic on 10 December 1848.

Election to the Chamber of Deputies¹⁷

On 13 May 1849 the elections of the legislative Assembly took place. Le Verrier himself did not try for office in his own département of La Manche, but instead was encouraged to join the list of the 13 *amis de l'Ordre et de la Liberté* (Friends of Order and Liberty), headed by Alexis de Toqueville. The journals on the Right were enthusiastic: Le Verrier was portrayed as "an elite intelligence, as well as an honest soul, a man of practical ideas, with positivist principles, like those of the mathematical sciences of which he was one of the princes; the utopists and the socialistic dreamers do not have a more pronounced enemy, a more logically pitiless adversary." Moreover, Le Verrier, in his electoral platform, affirmed that he would like

¹⁶ Cited by Bigourdan, *Annu. BdL* for 1932, p. A.20.

¹⁷ This paragraph and the following one are based on the study by Françoise Lamotte in Levert, Lamotte et Lantier (1977), pp. 101–117.

“religion to flourish, the family to be sacred, property to be inviolable, and the magistrates for life to be guarantors of complete justice.” And he recalled that he had helped to “alert people to the imminent danger of the utopians that would do so much damage to France.” It remained to convince the inhabitants of the countryside, who were not particularly anxious to see this intellectual represent them and said: “Let him go make almanacs!” The confidential report of the Sous-préfet of Valognes was not optimistic: “With perseverance and sympathy for the person of Le Verrier, one could convert, though I doubt it, the rural folk to his candidacy.” Nevertheless, the entire list of the 13 was elected; the first candidate of the opposing list, the Friends of the Constitution, only obtained 24,761 votes, even though he had received 119,817 votes during the elections for the Assembly on 23 April 1848. Tocqueville received 82,404 votes, and Le Verrier 56,674. This is but one example among many others in which notable locals, like Tocqueville and Le Verrier, were elected.

So Le Verrier became a deputy. He was named secretary of the Fourteenth Bureau of the legislative Assembly. Giving speeches was not his forte, but he did present a few reports: three concerned the establishment of new telegraph lines, a subject which particularly interested him; two others concerned the railroads. That was about it. His political opinions were always very firm. He wrote on 22 June 1849 to Emile Gautier¹⁸:

You recommend that I not let the Reds gain the upper hand. I'm working as hard as I can against them, in my little parliamentary sphere. I voted against the investigations—the amnesty—against the request to charge the ministers. But I voted for the authorization to help our friends the Montagnards—for the state of siege—and against the clubs, etc. It is said that they have projects. It may be so. We will defend our interests, if need be even with arms.

From 1849 to 1851, Le Verrier served as secretary of a commission of the Chamber of deputies, charged with the task of “examining the proposal to modify the decree of 19 July 1848 concerning the École polytechnique and the military schools.” Inspired by Arago, who was at that time chief executive, and also by Cavaignac, this decree was intended to reestablish free admission to these schools beginning on 1 October 1850 (a policy that had been suppressed by Napoléon I) as the initial step toward establishing free instruction at all levels. The discussion in the Chamber on this point was part of a protracted debate concerning public instruction. The opponents of free schooling used a rather specious argument:

It is our assertion ... that free tuition would not benefit poor young men as much as the authors of this decree imagine. It would cost a great deal to ready a young man for the competition at the École polytechnique or even at the École militaire.... The result of this is that young men who presented to one or the other of these schools would for the most part belong already to families of ample means. Would free tuition to these schools change the situation? No, surely it would not.... Accordingly, free attendance at these schools would essentially benefit the aristocrats. It would be a privilege that would chiefly profit families of ample means. Those who support free attendance at specialized schools, if they consider the matter logically, ought first to demand free attendance at preparatory schools, as the one is an illusion without the other. However, one sees where that would lead.

¹⁸ Cited by Bigourdan, *Annu. BdL* for 1932, p. A.22.

But that in a nutshell was the problem. Le Verrier was convinced that in the high schools “this kind of instruction, which is a great luxury, would not easily adapt itself to the immense majority of the nation,” and that it would be preferable for most to “proceed in the most direct way towards some honorable profession.” However, he added, the proposal made to the Chamber by the marshal Achille Baraguey d’Hilliers, governor of the École militaire de Saint-Cyr, “would fully protect the interests of youth belonging to families of modest means, because it would fix, at one quarter of the students of each division of the [École polytechnique and the École de Saint-Cyr] the number of scholarships which could be awarded; but that is more than is required by the normal proportion of poor students.” Finally, the Assembly adopted an amendment providing that “all youth [admitted to these schools] who will have established beforehand that they have insufficient resources from their parents will receive scholarships or partial scholarships.” The law, thus amended, passed on 5 June 1850.

In the course of his work on this commission, Le Verrier wrote a detailed and in-depth study of the organization and instruction at the École polytechnique. The commission regretted with reason that too much emphasis was given to theory at the school and not enough to practical applications; also that too much attention was paid “to the solution of subtle problems without any possible utility.” It thought it wrong likewise to completely exclude the study of literature, history, geography, and modern languages. The Chamber of deputies, at Le Verrier’s request, agreed to create a mixed commission “with nominations from the Minister of War, in concert with the Public Works ministry and the Navy,” and charged with reviewing admission policies and the courses of instruction at the École polytechnique. The work of this commission, presided over by Le Verrier himself, led to the reorganization decree of 12 January 1851. Arago, for various reasons, was quite agitated by the publication of this decree. In his eulogy to Gay-Lussac, read before the Academy of sciences on 20 December 1852, and subsequently published as a pamphlet,¹⁹ he defended the École as it had been before the reform and criticized the work of the commission in this way:

The commission charged with reorganizing the École polytechnique includes eminent individuals whose merit is universally recognized.... These personages ought to know that, from the moment these new proposals are published, the illustrious professors and examiners, not wanting to cooperate in carrying them out, whether out of concern for science or for their own dignity, will hand in their resignations.... [Furthermore,] the alumni ... are almost to a man against the proposed reforms.

Evidently Arago regretted that the École would never be free again. He noted that in certain cases students, or rather their families, had financially supported the studies of students less fortunate than themselves:

Particular circumstances have made me aware of the names of several young men who have been maintained in this way at the École at the expense of their fellows. If I were compelled

¹⁹ Arago F. (1852) Éloge de Gay-Lussac, in **Œuvres complètes de François Arago*, ed. par J.-A. Barral, 13 vol. Gide, Paris et T. O. Weigel, Leipzig (1854–1862): t. 3, see pp. 70–112, in particular 70–83 et 109–112. This text seems to suppose that the commission had not finished its job and that the reorganization decree could still be modified.

to make their names known, it would certainly come as a surprise that among their number would be a certain person who nowadays presents the old École polytechnique in a most unfavorable light.

Is the person referred to Le Verrier? It is no longer possible to know for sure, but it certainly seems likely.

Senator

After the coup-d'état of 2 December 1851, Le Verrier was one of the first to rally behind the Empire. He wrote to a friend: "The president of the Republic [who was none other than Louis Napoléon-Bonaparte, the future Napoléon III] has seized all the powers with a firm will to reestablish everywhere the principle of, and respect for, authority." The Emperor was thankful to him for this, and so on 26 January 1852, "M. Le Verrier, member of the Institute, ancient member of the legislative Assembly," was nominated senator (1 of 72). He would remain in this capacity until the fall of the Second Empire in 1870. This position brought him a substantial revenue of 30,000 francs per year, added to what he drew from his posts as astronomy professor at the Sorbonne and at the École polytechnique, being nominated to the latter on 2 February 1849 as a replacement for Biot, 1,200 francs he received annually as a member of the Academy of sciences, and further payments he received from the Bureau of longitudes. For him it marked the end of a financial situation that till then had been rather precarious.

In 1852 the *Conseils généraux* (general Councils) of the *départements* were up for election, and Le Verrier offered himself as a candidate for the canton of Saint-Malo-de-la-Lande (Manche). His revenues were estimated at this time at 10,000 francs per year (no doubt independent of what he would receive in salary as senator). On 1 August 1852, he was nominated president of the general council on the grounds that he was "of upright character, devoted to Prince Napoléon, of exceptional capacity and highly influential both in the département and elsewhere." This mandate was renewed each year until 1869, except for a 3-year interruption from 1854 to 1857. Le Verrier did not seem to show a great deal of zeal in this role. As far as is known, he wrote only one report: it is devoted to the exploitation of *tanguières*, which are sandpits from which one extracts *tangue*, a muddy sand, calcified, and very fine. Found on the shore of the Manche, it is utilized as fertilizer. Over time, he would exasperate his colleagues by his pretentious airs and ill humor, and was viewed badly by many of the other elected officials. Finally, in 1870, he would tender his resignation, and was replaced on 13 June of that year by M. Pignard Dudezert.

In 1853, Le Verrier was nominated Inspector general of higher education. In 1854 he became member of the Imperial council for public instruction and finally a member of the Council for the improvement of the École polytechnique, no doubt in recognition of his efforts to reform that school. All of these activities did not

hinder his researches in celestial mechanics, as attested by what he says in various letters to Emile Gautier. He says, for instance, on 28 November 1852²⁰:

I work ... as if I had to win a chair at the Institute. Every instant that my other duties leave free is consecrated to a great labor, one requires the greatest pains and the greatest care.

An important change was about to take place, however, with the death of Arago on 2 October 1853. The post he so coveted, that of director of the Paris Observatory, was finally available, and Le Verrier could at last see himself installed in this position.

Minor Planets

Le Verrier's main work at this time concerned the minor planets (also called asteroids), of which the first, Ceres, was discovered by Giuseppe Piazzi on 1 January 1801. Heinrich Olbers, the next year, discovered Pallas. The orbits of most of the minor planets lie between Mars and Jupiter (Fig. 3.7), so that they seem to obey the distribution of distances of the planets from the Sun known as the Titius-Bode law (see Box 2.1 in the preceding chapter). On account of this arrangement, Olbers speculated that the minor planets might be debris left over from an ancient planet destroyed in an explosion.

Already Le Verrier had noticed in 1841 a relation between the period of revolution of Pallas and that of Jupiter: 18 times the period of Pallas was precisely equal to 7 times the period of Jupiter.²¹ This is what would later be called a resonance, and resembled the one previously recognized between the periods of Jupiter and Saturn, whose ratio is 2/5. Le Verrier noticed that these resonances are by no means haphazard—there were other cases, such as those involving the satellites of Jupiter. Clearly, they had the effect of producing large perturbations in the heavenly bodies involved.

Taking up the problem again at the beginning of the year 1850, Le Verrier became convinced that Olbers's idea concerning the origin of the asteroids was wrong. He wrote²²:

Far from explaining the existence of the minor planets by an alteration of the original system of the Universe, one is rather inclined to believe now that they have been formed in the usual way as the other planets, and by means of the same laws.

If this view is correct, then one would expect to discover in turn a prodigious number of minor planets according to the zeal with which observers prosecute their research, with ever more powerful instruments. The generous way in which astronomers have lent assistance to these efforts, by publishing their maps of the ecliptic whose construction has been so painstaking, will make the work going forward relatively easy [Le Verrier was no doubt thinking

²⁰ Bigourdan, *Annu. BdL* for 1932, p. A.23.

²¹ *CRAS (1841) 13, pp. 344–348; see also *CRAS (1843) 16, p. 1435.

²² *CRAS (1853) 37 pp. 793–798, see also pp. 965–966.

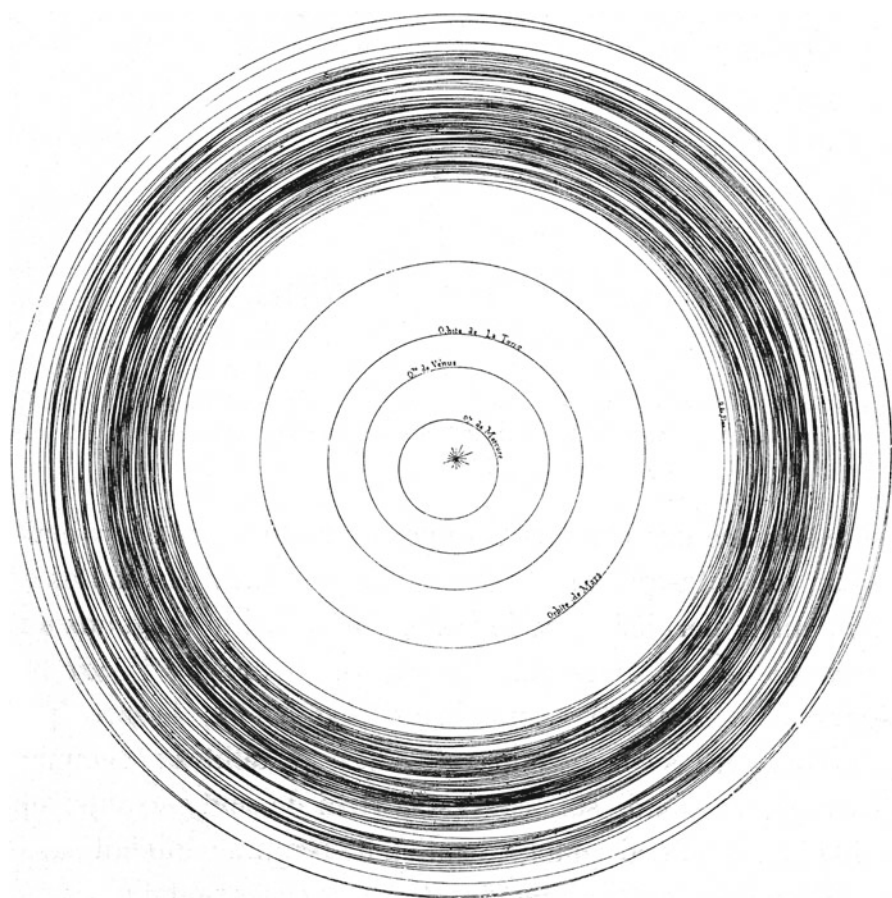


Fig. 3.7 The orbit of the first 108 minor planets discovered. The orbits of Mercury, Venus, the Earth and Mars are indicated, all inside the orbits of the minor planets; the orbit of Jupiter is well outside the limits of the figure

of Jean Chacornac who was doing this work in Marseille, and whom he would hire for the Paris Observatory in 1854]. The numerous discoveries made in this manner, far from diminishing the interest of the subject, will rather increase its importance. For although it has become necessary to renounce Olbers's hypothesis, one can expect that knowledge of a large number of minor planets will lead to the discernment of some law concerning their distribution, and in the end lead to the determination of the configuration of their principal groups. It is hardly to be expected that the minor planets will be indiscriminately and randomly distributed in all parts of the sky: based on the fact that they have been discovered so far only in a single zone [between Mars and Jupiter], one must expect that the same cause that has gathered together material for each of the principal planets will also have clumped them into distinct groups.

What prophetic insight! Indeed, many minor planets were discovered in succeeding years through a program to which Le Verrier attached the utmost importance

(by 1891, 321 had been found²³). Their orbital periods were not randomly distributed; instead they consisted of small groups just as Le Verrier had surmised. But the groups did not always reflect the way in which the minor planets had formed, as he thought. Rather, at least in some cases, they are produced by the gravitational influence of Jupiter (see Box 3.2).

Le Verrier obtained another remarkable result by studying the movement of the perihelion of the orbit of Mars. He was able to estimate the total mass of all the asteroids, and found it to be less than one-fourth of the mass of the Earth. He would go on to carry out a similar investigation regarding the movement of Mercury's perihelion, leading to his discovery of an anomaly which he believed was due to the existence of a swarm of small planets sandwiched between Mercury and the Sun. Though this was not the case, this discovery had far-ranging consequences, and provided the first observational proof of the validity of Einstein's Theory of General Relativity. We will have more to say about this in Chap. 5.

Box 3.2 The Distribution of the Orbits of the Minor Planets

The orbital periods of the minor planets, as well as the semi-major axes of their orbits related to the periods by Kepler's third law, are not distributed randomly. As first demonstrated by the American astronomer Daniel Kirkwood around 1866, they do not remain in orbits whose periods have a simple relation with that of Jupiter. The orbital periods equal to $1/3$, $2/5$, $3/7$, and $1/2$ of Jupiter's define gaps, on the edges of which the asteroids appear more numerous (Fig. 3.8). These are the "distinct groups" Le Verrier had hoped to find, but which could hardly be discerned in 1853, when only 26 minor planets were known. The reason for the distributional effect is, as he had remarked in the case of Pallas, that even an approximate commensurability in the periods causes large perturbations, because the minor planet finds itself periodically in the same relation to Jupiter and the Sun. These perturbations lead to chaos. The semi-major axes of the major planets initially close to resonance find themselves spread out randomly into a large band. Those of the minor planets far from resonance, on the other hand, are stable.²⁴ Only the gap corresponding to the resonance of $1/2$ cannot be explained in this way. It is possible that it is due to conditions prevalent when the Solar System was formed, as Le Verrier himself had conjectured. But this situation would not be fully clarified until after Le Verrier's time, in a study by Henri Poincaré (Fig. 3.9) and by many others following him. Even today, it is a subject of rich and active investigation.

²³ See Fonvielle, W. de (1891).

²⁴ A simple study of the problem can be found in <http://www.physics.udel.edu/~jim/solarsystem/>.

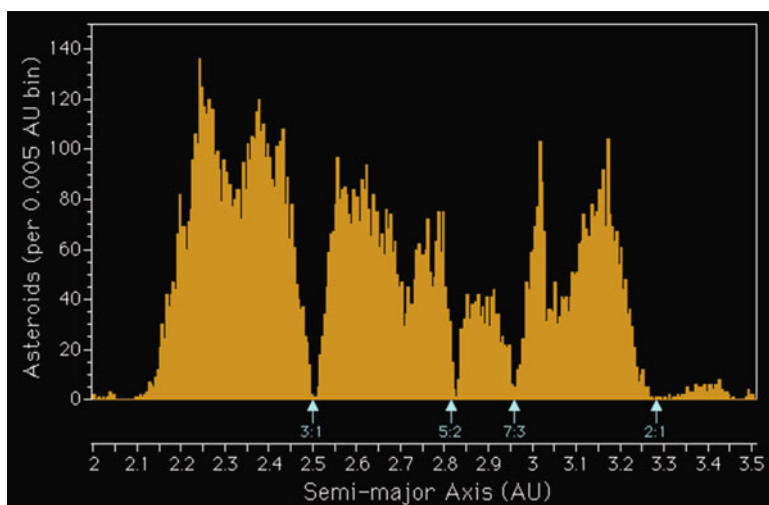


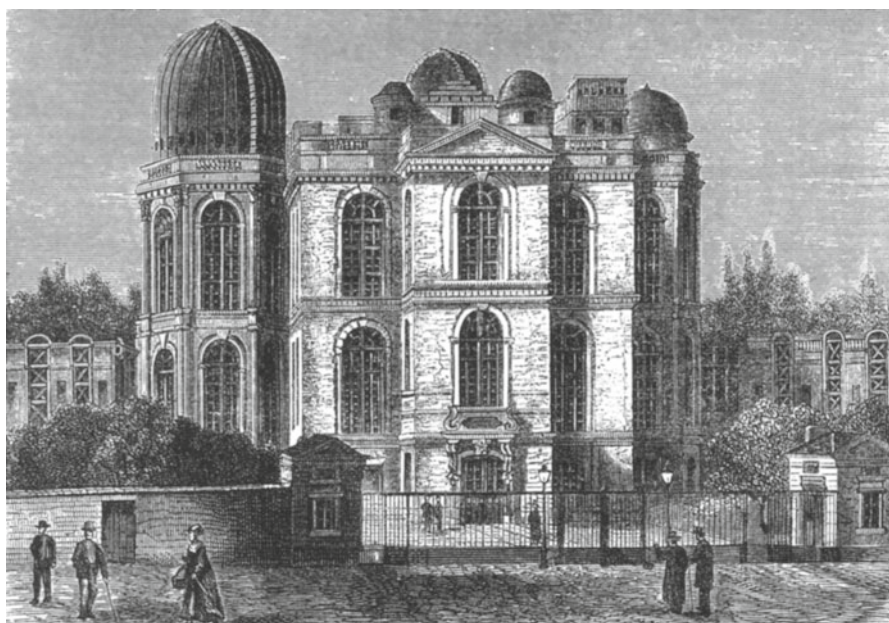
Fig. 3.8 Distribution of the semi-major axes of minor planets. The number of planets per interval of semi major axis a is plotted as a function of this axis (in astronomical units). One observes a lack of minor planets for values corresponding to simple ratios of their revolution period with that of Jupiter, as indicated. These gaps are not visible on Fig. 3.7, because the orbits of the minor planets are generally not circular

Fig. 3.9 Henri Poincaré
(1854–1912)



Chapter 4

The Observatory: At Last!



The Paris Observatory around 1870, seen from the North. On the terrace, to the left, is the large dome of the 38 cm equatorial, to the right that of the 31.6 cm, and the small observatory lies in between. The low wing to the left contains the meridian instruments. Le Verrier's rather grandiose apartment is on the right

The Rupture

Arago spent his last years in declining health. He suffered from diabetes, an incurable illness at the time, which led to progressive blindness and wasting physical weakness (Fig. 4.1). Luckily, his intellectual faculties remained unimpaired, and he dictated to his assistants his celebrated *Astronomie populaire* as well as a number of important scientific monographs and biographical notices. He refused to swear an oath to the Emperor, as required of all government officials. However, he was too famous for this refusal to have any consequences. His final days were especially darkened by the realization that his enemy Le Verrier would probably occupy his place at the Observatory. Indeed, this now seemed practically inevitable.

From 12 October 1853, 1 week after Arago's interment, members of the Bureau of longitudes proposed to create a commission consisting of the astronomer Claude-Louis Mathieu, the astronomer Charles-Louis Largeau, and the instrument-maker Louis Breguet for the purpose of evaluating candidates to succeed him. With this membership, it's clear that this successor would have had to have been someone from Arago's inner circle, probably Mathieu, his brother-in-law, who had been, after Arago himself, the most prominent member. But on 19 October, the Minister of Public Instruction, Hippolyte-Nicolas-Honoré Fortoul, insisted that the Bureau suspend these proceedings. On 28 October 1853, he signed an administrative



Fig. 4.1 Photography of Arago 15 days before his death. Surprisingly, this is the only known photograph of this pioneer of photography

decree,¹ creating a commission charged with the task of “suggesting improvements in the organization and administration of the Paris Observatory and the Bureau of longitudes.” Presiding over the commission was the marshal Jean-Baptiste-Philibert Vaillant, a person completely devoted to the Emperor Napoléon III – (Box 4.1 and Fig. 4.2). Other members were admiral Charles Baudin, acting president of the Bureau of longitudes and former minister of the Navy, the physicists Jacques Binet and Jean-Baptiste Biot, the chemist Jean-Baptiste Dumas (Fig. 4.3), vice president of the Imperial council of public instruction, and of course Le Verrier himself, who was the only active astronomer on the commission. Biot had been active long before, but his current preoccupations were quite different, though he still maintained a great competence in astronomy; moreover, his political opinions favored the new regime. As for Dumas, he was a senator whose loyalty to Napoléon III was absolute. Le Verrier’s views therefore carried the day, and the commission submitted entirely to his desires and those of the government.

The Organizational Decree

The commission’s report was presented on 20 January 1854 by marshal Vaillant to the Minister of Public Instruction.² A draft of the document, which is certainly by Le

Box 4.1 Jean-Baptiste-Philibert (marshal) Vaillant (1790–1872)

Entering the École polytechnique in 1807, Vaillant was to pursue a very active military career. At the same time he took a distinct interest in education, and was nominated in 1838 as director of the École polytechnique. After the coup d’état of 2 December 1851, he enthusiastically threw his support behind Napoléon III, who nominated him as senator and then as marshal of the palace. He was elected to the Academy of sciences as a member at large in 1853. He was also named Minister of War on 11 March 1854, and was twice Minister of Public Instruction, notably after the premature death of Fortoul in 1856. He took a turn as Minister of the house of the Emperor; he served as Minister of the beaux-arts (fine arts) from 1860 to 1870, and reorganized the École des beaux-arts in 1863. He also sat on the Bureau of longitudes beginning in 1862. The combination of all these offices brought him annually the rather tidy sum of 263,000 francs. After the fall of Napoléon III, he judged it prudent to exile himself to San-Sebastian in Spain. He returned to France in March 1871, but died the following year.

¹ This text and the report of the Commission, as well as all legislative texts cited in this chapter can be found in Beauchamp, A. de (1880–1889) *Recueil des lois et règlement sur l’Enseignement supérieur*, Paris, Université de Paris, 4 tomes: t. 1: 1789–1847; t. 2: 1847–1874; t. 3: 1875–1883 and annexes; t. 4: 1884–1889 and tables. Most of them can be consulted in the BOP, in particular in *Documents divers sur l’Observatoire de Paris, 1854–1872*, cote 3567.

² Published in *le Moniteur universel* du 3 février 1854.

Fig. 4.2 Marshal Vaillant
(1790–1872)



Fig. 4.3 Jean-Baptiste
Dumas (1800–1884)



Verrier, is preserved in the Paris Observatory³; the name of Arago is absent, although he figures in the final text, no doubt owing to the efforts of several members of the commission who had known Arago and appreciated him. Here are some extracts from this draft. The final report of the commission differs from it very little:

Above all the Commission was tasked with giving a precise account of the condition of astronomical science in France and overseas. Without entering here into all the details--those the Minister will find in the transcripts of our meetings--it will suffice to say that unfortunately the comparison is not to our advantage. By examining the publications of different observatories, and even more by the exposition by our colleague, the venerable dean of French astronomers [referring to Biot, then 80 years old], we arrive at the regrettable conclusion that we have fallen behind in the progress of science in many respects: in the installation and arrangement of instruments, in the reduction of observations, and in the investigations into celestial physics. The situation of the observatory in the heart of the capital, in a murky atmosphere and on a shaky, agitated ground, is an inconvenience which the observatories of Greenwich and St. Petersburg [Pulkova] avoid by their location; at least this is the case since the St. Petersburg Observatory was rebuilt 15 years ago at a distance of four leagues [16 kilometers] from the city center [note: these observatories would serve as models for Le Verrier, who was well acquainted with their respective directors; see Box 3.1].

The Commission cannot believe that this unfortunate condition lacks any remedy, or that, as has been alleged, astronomy in France must henceforth be considered *an exotic plant*....

The Commission hardly insists at this time that the current location of the Observatory be abandoned. Before coming to that, it would be useful to examine by what useful and convenient means it might be possible to mitigate, at least in part, the inconveniences of its present situation; but as one will not be able to remedy, in all events, the lack of transparency of the atmosphere, it's well to note that the abandonment of the great building, improperly called an observatory, would occasion no regrets. This monumental pile is so inappropriate for observations that not a single one has ever been made from it.⁴

It is only with difficulty that a few uncomfortable lodgings have been set up. They are, however, completely insufficient....

³BOP, Ms 1047, D2.

⁴This is quite true: the instruments were only stored in the observatory, and were taken out when observations were to be made. Several of them were fixed on pedestals in auxiliary observation rooms or in two small domes on the upper terrace of the building, constructed of course long after the completion of Claude Perrault's building. Le Verrier's successor, the rear-admiral Mouchez, was very severe in his criticisms. He wrote in 1878 (Paris Observatory, MS 1059-2): "As a consequence of its very defective construction for an observatory, which did not permit the installation of any instruments nor the lodging of any astronomer, and as a result also of its situation in the midst of a crowded quarter, which has now become industrialized, the old monument of Perrault is and always has been in the most atrocious conditions for astronomical observation: and in fact, since its foundation down to Le Verrier's directorship in this middle of the last century, it has contributed very little to astronomical progress. During the entire eighteenth century one cannot cite a single discovery, or any work of importance. The only two discoveries which have been made there date from the end of the seventeenth century, namely, the satellites of Saturn by Cassini and the speed of light by Roemer, based on his observations of eclipses of Jupiter's satellites, but during the entire eighteenth century the majority of observations made in Paris were from individual houses or private observatories. It was from the École militaire that Lalande observed most of the 48,000 stars in his *Histoire céleste*. During the eighteenth century the Cassinis, who had for the most part presided over the Observatory, were chiefly occupied with the map of France." The discoveries of Arago were made with portable instruments.

The Commission [is] unanimously of the opinion that a permanent director should be placed at the head of the Paris Observatory.... Although the rules impose on the Bureau of longitudes the responsibility of annually designating the director of observations, these functions have been neglected, in effect, for the past twenty years, and without any new designee being named, the control has remained in the hands of one and the same person [Arago].⁵

The arrangements must now, however, be made more regular. If a permanent Director of the Paris Observatory is to be secured, the absolute lack of clear responsibility, which the current absence of regulation and harmony creates for him, must not be allowed to continue.

The Commission thinks that the Director of the Paris Observatory must, like those of other great establishments, derive his authority directly from the head of state. The Commission holds that the Director should be named by the Emperor, upon recommendation of the Minister of Public Instruction and without the need for an introduction [this was added, evidently, to insure the appointment of Le Verrier to the post].

The authority of the Director, so appointed, must be absolute. His actions are not to be inhibited or compromised by the intervention of a deliberative body [such as the Bureau of longitudes!]. The Director alone should preside over the work of the Observatory. He will see that the observations are conducted according to a plan which he has laid out, a plan which, once approved by the Minister, shall be obligatory for all of his collaborators. He will regulate the services of the astronomers, adjuncts, and students placed under his authority. At his disposal will be all the instruments of the Observatory. Of course, this authority will be conferred to the Director on condition that he will use all these resources to the benefit of science. Also, he will have both real responsibility and specific obligations that in part will be new: each year he will publish the observations that have been made in the preceding year, also the reductions of these observations and their comparison with theory. He will annually present to the Minister a report on the work of the Observatory. Finally the Minister, whenever he judges it useful, but at least once every three years [in the final text this was changed to every two years], must take account of the scientific situation and the needs of the establishment through a commission comprised of the most competent judges: the opinion of the scientific world will therefore be called to check and validate the results obtained under the supervision of a strongly established Director.

It does not enter into the plans formulated by the Commission, and it could not enter therein, any decision regarding procurement of equipment necessary for the proper execution of observations. It will be the Director's task, backed by the Government's confidence, to make known the very real needs of the establishment. Now the Commission thinks that not only all the observational means present in other observatories, and also those currently in use in France, ought to be furnished to the Paris Observatory, but also the Director ought to be encouraged to present projects suitable to insure that some superiority exists in the institution he is directing. Finally, it is impossible that the lodgings for real observers, of which part are simply cubbyholes, should not receive at once the improvements that hygiene absolutely requires.

The decree of 30 January 1854 reiterated all the proposals of the Commission without modification, and defined the powers of Le Verrier, who was appointed to the directorship of the Observatory by decree the following day (Fig. 4.4). These powers were very extensive, as can be seen from these extracts of the decree of 30 January:

(Article 10): The Director ... alone directs the observations, their reductions, their publication, and generally all the scientific work done at the Observatory.

⁵ Before the creation of the post of director of observations in 1834, everyone did as he wished: one minister declared that establishment he did not intend to set up a master or supervisor over the scientists who were attached to the establishment; and that he thought that the administrator ought to change hands each year, something which was not however implemented (Débarbat 2005).

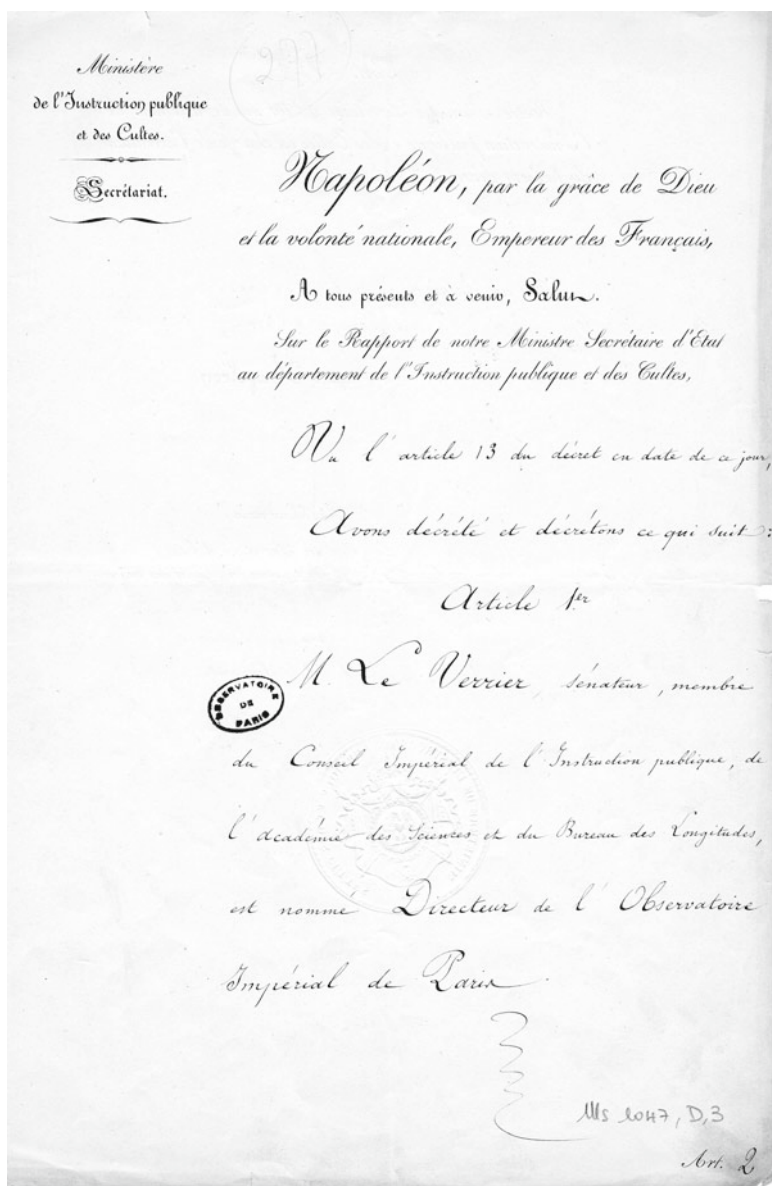


Fig. 4.4 The first page of the decree nominating Le Verrier as Director of the Paris Observatory

He draws up the rules concerning the duties of astronomers, adjunct astronomers and student astronomers; which he submits for approval to the Minister.

He has at his disposal all the resources of the Observatory ... the administration of which is confided to his care.

He proposes to the Minister the distribution of the lodgings among the functionaries and employees.

(Article 14): Each of the astronomers carries out the role which the Director assigns to him.

Like all administrators of importance, the Director, as well as four high-ranking astronomers at the Observatory, were nominated by the Emperor. The Director had the power, to all intents and purposes, to nominate or dismiss the other members of the Observatory. A biennial review of the activities of the Observatory “by a Commission composed of two members of the Council of the admiralty, a member of the Institute, and two members of the Bureau of longitudes, and an Inspector general of higher education and the Director of the Observatory” was provided for (Article 12). But Le Verrier never convened this commission, and it was only in opposition to him that it was to be called back to life in 1867 by the Minister of Public Instruction.

The Divorce from the Bureau of Longitudes

The second part of the decree concerned itself with the separation of the Observatory from the Bureau of longitudes. Since its creation in 1795, the Bureau had always had the upper hand over the Observatory, and had always insisted on its prerogatives. For this reason it had always refused to name a director of the observatory, Arago being referred to simply as “director of observations.” Of course, the chief astronomers were part of the Bureau, but the Bureau also included mathematicians, geographers and navigators (in effect, geodesy, the measurement of the Earth, and the determination of time and longitudes were regarded as among the important duties of astronomers). This supervision of the Observatory by colleagues from another institution caused significant friction, and provided a large part of the motivation for the new decree. However, the express motives pretended to ignore the fact that the Observatory had always been an integral part of the Bureau of longitudes: “Leaving to the Observatory the mission of perfecting observational astronomy, the Bureau has for quite some time, under the inspiration of Laplace and Lagrange, been the sanctuary for theoretical astronomy and calculational astronomy, which is its true role.” It was this “true role” which the Commission was attempting to institutionalize, hypocritically maintaining that “far from diminishing the responsibilities of the Bureau, it is proposed to establish it with an even more complete independence of the Observatory [sic].” This new role was defined in precise terms:

1. Improving the construction of astronomical instruments and the methods of observation, both on land and sea;
2. The publication of instructions concerning the study of physical astronomy, on the tides and terrestrial magnetism.
3. Identification of special missions having as their goal extending the present knowledge of the shape of the physical globe.
4. Advancement in the theories of celestial mechanics and their applications; the perfecting of tables of the Sun, Moon and planets.

5. The analysis and publication of older observations which have remained unedited in the annals of the Observatory and in the manuscripts in its library.

The first four points represent what were certainly regarded previously as duties of the Bureau, but the final point was new, and corresponded to a real need. Most of the observations made at the Observatory since the beginning of the century had not been processed but had simply been published in their raw form. Le Verrier himself had no desire to concern himself with their reduction. But here he would be disappointed. Frustrated by its shrinking responsibilities and the meagerness of its means, the Bureau would do nothing to help him with the reduction of observations. The Bureau would only have a consulting role to play in response to requests from the Government, and this discouraged it from taking initiatives. Of course, the division between the Bureau and the Observatory would unleash a merciless war between two. It was at the Bureau, indeed, that Le Verrier would encounter his fiercest and most persistent enemies.

The decree of 30 January 1854 marks an important date in the history of French astronomy, for the decree would determine for the next century the roles of the Paris Observatory and the Bureau of longitudes. There would be many revisions, in particular regarding the role of the Director, but the main points, at least, would remain in effect. We will see Le Verrier putting them into application.

Le Verrier Reorganizes the Observatory: The Dismantling of Arago's Influence

Four months after Arago's death, Le Verrier moved into the Observatory. His first act was to chase away the astronomers of "Arago's clique" who resided there,⁶ and to begin with a clean slate. Mathieu and his son Charles, a student-astronomer since 1849, and Laugier and other of Arago's close allies had to leave the Observatory with their families, and were therefore deprived of lodgings and instruments, even of their appointed posts. It was the same with Ernest Liouville, student-astronomer for 2 years, who resigned on 15 February 1854, also the adjunct-astronomer Victor Mauvais, who was so distraught that he committed suicide on 23 March 1854. The library of the Observatory preserves a copy of the letter that Le Verrier handed over to Mathieu⁷:

Paris, 17 February 1854

Monsieur,

I cannot defer for long acting upon the instructions I have received, which state that the observations must not suffer interruption.

⁶ Some people who had less ties with Arago would remain at the Observatory: Antoine-Joseph-François Yvon Villarceau, a specialist of instrumentation, who entered the Observatory in 1846 and became full astronomer in 1854, and Hervé Faye, student-astronomer from 1836 then astronomer in 1843, the year when he discovered the periodic comet which bears his name.

⁷ BOP, MS 1060-1-A.

Moreover, I must fix a date with foreign observatories for the determination of differences in longitude.

It will not escape your attention that all of this is not possible in the current state of affairs. I cannot install either the astronomer nominated by the decree of the Emperor nor the one who is coming from Greenwich [for determining the differences in longitude with that observatory] nor myself [until the necessary changes are implemented].

Would you be so kind as to excuse the overriding necessity which obliges me to ask you to decide on a date which will be at your convenience to hand over the part of the Observatory which you occupy, in order that I may begin this work?

Please accept, Monsieur, the expression of my most sincere thanks.

The Director of the Observatory, Member of the Institute

Delivered to M. Mathieu on Saturday 18 [February] at 1 p.m.

And here is Mathieu's response, dated the following day:

Monsieur,

In the position in which I am placed, everyone will understand my keen desire to promptly leave the Observatory. But difficulties beyond the control of my wishes force me, to my great regret, to remain here until 1 May.

Please accept my salutations,

L. Mathieu

The Ministry of Public Instruction offered successively to Mathieu and Laugier, by way of compensation, the chair of astronomy at the Sorbonne, which they refused. Laugier expressed to the Minister his "regrets to not be able to enter into such a scheme."⁸ Mathieu would occupy himself with the publication of the *Connaissance des temps* and the *Annuaire du Bureau des longitudes* which remained the prerogatives of that institute, then turned his attention to industry – he would direct the manufacture of tobacco at Dieppe. Laugier was to live from funds procured in his capacity as examiner at the École navale, and received the satisfaction of being in 1862 promoted to titular membership of the Bureau of longitudes with Delaunay. All three were to become reformers of the Paris Observatory after Le Verrier's revocation in 1870. But neither Mathieu nor Laugier were to have the means to carry out astronomical researches at the high level that could have been expected under more favorable circumstances.

Le Verrier has been much criticized for his dismissal of the members of Arago's circle. But did he have any choice? Forty five years after the events, Joseph Bertrand, who had lived through these events and whose memory of them was still green, declared to the astronomer Guillaume Bigourdan⁹:

As regards the Observatory, one must realize that whoever had been placed in charge at the death of Arago would have found himself engaged in a ferocious battle, because Arago's family had taken for granted that Arago's successor would be Mathieu, and next Laugier. Nothing would have pleased Le Verrier more than to keep these two by his side, but this was impossible: he would have had to cede the first place, because Mathieu was regarded as having been "spoiled."

⁸ See this letter in Bigourdan, *Annu. BdL* for 1932, pp. A89–A90.

⁹ BOP, Documents divers sur l'Observatoire de Paris, 1854–1872, cote 3567(3), folder AP.

On 4 March 1854, Laugier received from his Swiss friend, Emile Plantamour, who was later to become director of the Geneva Observatory, a letter that offers interesting comparisons between the Paris Observatory and that at Greenwich¹⁰:

My dear friend,

For several days now I have wanted to respond to your letter, which supplied more sad details to those the *Moniteur* has given us regarding the changes at the Observatory. There is no point in telling you the extent to which I have been affected.... If I have tarried in writing to you, it is because I clung to some hope of a modification of the original decree; I thought that advisers with more wisdom would [after due reflection] realize the folly of trying to reorganize the Paris Observatory on the model of the Greenwich Observatory, without taking into account the differing histories of these two institutions, the differing goals, and the differing foundations that exist in these two countries regarding the institutions of public instruction and scientific research.

In England, the state does not maintain any institution devoted to instruction or science; the Greenwich Observatory is the only exception, and exists with an eye toward marine affairs; the exclusive goal of this observatory is to supply and improve the measurements and tables necessary for navigation. It has long been understood that this goal can be achieved by placing a director at the head of subordinates, [who are little more than] calculating and observing machines. Pure scientific research is completely excluded from the work that goes on at Greenwich.¹¹

In France, on the other hand, institutions for public instruction and science are in the charge of the state, and not under the control of corporations or individuals. The Observatory ought to serve the interests of science, and not just be concerned with applications. In proof of this statement, one finds among the personnel of the Observatory true scientists, as opposed to mere observers. But now it appears that this must be given up. What scientist of merit would ever consent to make himself a subordinate of M. Le Verrier, and devote all his time to the execution of rote observations and to rote calculations as ordered by this director? The overthrow seems complete: not only has the organization changed, but a director has been named who is an individual completely unfamiliar with observation and the operation of an observatory, and moreover an individual whose character is not such as would allow a circle of collaborators around him, but only subordinates, machines. I find this very sad: sad for the observatory itself, and sad because of the crying injustice committed toward those savants whose investigations have been interrupted and from whom the instruments needed to carry on their work have been withdrawn.

Plantamour had indeed seen that Le Verrier's despotic character would cause serious difficulties. However, even though many researches that had been undertaken in Arago's time were now to be abandoned, Le Verrier did not neglect completely fields that did not have to do with his preferred emphases in astrometry and celestial mechanics. To the contrary, his plan for the development of the Observatory was to be truly remarkable.

¹⁰ Bigourdan, *Annu. BdL* pour 1932, pp. A90–92.

¹¹ This explains why Neptune had been investigated not at Greenwich but at Cambridge, which had instruments other than those destined for astrometry, namely, one of the largest refractors of the world at the time.

Le Verrier's Plan

Arago was more concerned with the nature of the stars than with their movements. He demonstrated the gaseous nature of the surface of the Sun – a pioneering result for astrophysics. He discovered the polarization of light from the Moon and from comets, from the latter deducing the existence of dust in the tails. He also demonstrated the polarization of light from the daytime sky. He was a pioneering investigator of photometry. An admirer of William Herschel, he induced his collaborators to study in turn nebulae and double stars, without however much success. Geodesy, the measurement of the earth, and meteorology and terrestrial magnetism were traditionally responsibilities of the Observatory, and Arago carried out a tremendous amount of work in these areas. He was actively interested in various aspects of physics, oceanography, photography, electric telegraphy, etc. During his tenure, the activities of the Observatory were highly diverse: Fresnel conducted his optical experiments, Fizeau compared the speed of light in air and water, and Foucault introduced his celebrated pendulum. All of this research was very fertile and productive, though it also tended to proceed in somewhat helter-skelter fashion, and the results did not always measure up to expectation. Arago's good qualities were also tied in with his faults: his energies were vast but dispersed while, unfortunately, his astronomical collaborators were not scientists of the first order. Nevertheless, life at the Observatory under Arago was certainly stimulating, and was characteristic of an epoch that would witness striking new developments in science.

But when Le Verrier took over the Observatory, he swept the slate clean, in his rather brutal fashion. Following to the letter the stipulations of the Imperial decree, he prepared and submitted “for the Ministry's approval the plan which he proposed to follow in directing the Observatory,” and noted “the improvements to which the establishment was susceptible.” In December 1854, he completed a report on the state of the institution. This report¹² was the first article published in a new review, similar to those published by other observatories around the world: the *Annales de l'Observatoire de Paris* (Fig. 4.5).

This report is remarkable for its clarity and lucidity. Le Verrier did not, however, write this in a vacuum: he was greatly inspired by what Wilhelm Struve had written and also by a very detailed report by Biot in 1847, to which Le Verrier added many annotations of his own.¹³ Biot certainly reiterated many of the conclusions of his report to the marshal Vaillant's commission. One reads there a description of the Pulkova Observatory, of which Struve was the director, and a comparison of that observatory with those of Greenwich and Paris. Biot acknowledged the improvements due to Arago. Under his active and intelligent leadership, the *cabinets d'observation* [observing rooms] were rebuilt and the instrument park was refurbished.

¹² °Le Verrier: (1855) Rapport sur l'Observatoire impérial de Paris et projet d'organisation. *Ann. OP, Mémoires* 1, 1–68, Mallet-Bachelier, Paris.

¹³ *Biot J.-B.: (1847) Review of the book “Description de l'observatoire astronomique central de Poulkova, by F.G.W. Struve ...” *Journal des savants*, septembre 1847, pp. 513–533.

ANNALES

DE

L'OBSERVATOIRE IMPÉRIAL DE PARIS,

PUBLIÉES

PAR U.-J. LE VERRIER,

DIRECTEUR DE L'OBSERVATOIRE.

TOME PREMIER.

PARIS,

MALLET-BACHELIER,

IMPRIMEUR-LIBRAIRE DE L'OBSERVATOIRE IMPÉRIAL DE PARIS,

QUAI DES GRANDS-AUGUSTINS, 55.

Fig. 4.5 Title page of the first issue of the *Annales de l'Observatoire Impérial de Paris*

But he continued to deplore the observatory's unfavorable situation in a highly urbanized zone. "Mr. Arago was just able to hold off the encroachment of the railroad line [the Sceaux line], which nevertheless managed to build its station right next door. Will they, however, continue to listen when a greatly increased population will grow more active and demanding?... It would have taken admirable courage to have [the observatory] built elsewhere, such as on the summit of Mont Valérien or in the heights of Châtillon." However, "the main difficulty would have been reorganizing the personnel. Supposing a new observatory had been built and provisioned with new and beautiful astronomical instruments, it would have been necessary to assemble there talented, energetic, and hardworking individuals,

resigned (with their families) to living a philosophical existence, as if beneath an abbot, in solitude, and according to a strict regimen of rules. Do we have among us monks for such a [scientific] monastery?" We have seen that Vaillant's commission had not recommended relocating the Paris Observatory, despite a favorable recommendation of the Academy of sciences. Le Verrier himself was in favor at first, before changing his mind.

In his report, Le Verrier began by noting that astronomical researches could not be fruitful except by their long continuation, a point that had been emphasized by Struve and Biot, and he cited the examples they mentioned: the Greenwich Observatory, which had followed a single goal since its founding in 1675; the same with the Pulkova Observatory, founded in 1834, for which it was decided that the "principal purpose ought to be to work toward the advancement of stellar astronomy, without however renouncing other investigations to the extent that these could be reconciled with the principal goal."

For Le Verrier, the Paris Observatory was to become "an observatory of the first rank." As was the case when it had enjoyed the auspices of the Bureau of longitudes, its activities would include "researches in terrestrial physics, including studies of the figure of the Earth and of the laws of gravity; those of astronomical geography; and the application of astronomy to the needs of society; the study of terrestrial magnetism, the properties of light, meteorological phenomena, regular and accidental."

For him, there were two sorts of observations. On the one hand, there were those pertaining to what would now be called astrophysics: studies of the nature of the Sun, Moon, and planets (with regard to which Arago had made important advances, though Le Verrier omitted any mention of them); the distribution of matter in the Milky Way; the secrets of the formation of the nebulae¹⁴; to which he added the discovery of asteroids and comets. On the other hand was astrometry: the measurement of the positions and movements of the planets and their satellites, and the measurement of the positions and movements of stars, including double stars.

These sorts of observations, which taken together belong to precision astronomy, should occupy the first priority in this report, just as they do in all the rest of science. The attentive contemplation and description of the sky has without doubt given, and will yet again give, great discoveries in physics. The measurement of the movements of the heavenly bodies has revealed to us the laws which govern the harmony of the heavens. Over the last centuries it has served as the starting point for the rise of the scientific spirit.

Le Verrier next undertook the critical examination of the instruments of the Paris Observatory, which furnished him with the opportunity to offer a veritable course in the history of astronomy for his audience of advisers. This examination was very clear: one may suppose that Le Verrier, not an observer himself, had discussed the matter at length with Airy and Struve during his visit to England. He made a general remark whose conclusion remains valid to this day:

¹⁴ These were the topics explored by William Herschel, whose influence on all nineteenth century astronomy was incalculable.

For a long time the construction of instruments was not the object of any particular art or profession, and astronomers themselves constructed the instruments they needed. A century and a half has scarcely passed since the building of instruments has passed to the hands of artisans. These craftsmen could acquire a very high degree of manual dexterity to which the astronomers themselves were rarely capable, being tasked with making the observations themselves. The accuracy of the instruments could only gain by this revolution. Perhaps the only regret is that the astronomers in their turn have become too unfamiliar with the art of instrument-making. One can believe that many of the fine points in the construction of instruments, notably the fabrication of large lenses, has hardly benefited by this overly specialized division of labor, and taking the example of Pulkova, the Paris Observatory ought to establish closer connections between astronomers and artisans.

Le Verrier examined in detail the equipment he found on his arrival, and made detailed proposals for its improvement and refurbishment. We will concern ourselves with this later. He regretted – and he was in a good position to know – that contrary to what occurred at Greenwich, nothing had yet been done in France for processing the observations: up until this time, indeed, “they were published in their raw form, without reduction, leaving to others the care of deducing from them the right ascensions and declinations.” He therefore insisted on the creation of a bureau of calculations.

He also wanted to establish closer connections with the public services which, up to this time, had been very limited. These public services were concerned with verifying marine chronometers, determining longitudes using the electric telegraph, and above all the physics of the terrestrial globe, such as the phenomena of meteorology.

Le Verrier also described the conditions of the observers, who carried out back breaking labors with very little remuneration (3,000 francs per year for the adjunct astronomers, 5,000 francs for the titular astronomers). Not only were they poorly paid: their lodgings at the Observatory were “barely habitable.” Even so, he did not want them to dissipate their efforts by participating in other activities, such as teaching. He himself was so little motivated by teaching and the popularization of science¹⁵ that almost the first thing he did was tear down the amphitheater that Arago had constructed in 1841 for the purpose of giving his celebrated course in “Popular Astronomy,” and at first he envisaged setting up in its place the new meridian circle. In the end, the amphitheater was replaced with his apartments, which would occupy 400 square meters! The demolition of the amphitheater cost some 30,000 francs, and the construction of the apartments some 80,000 francs.

Apart from his comments on teaching and the effort to popularize science, which were debatable, Le Verrier's report appears to us otherwise moderate and objective. The Observatory was obviously not in a state of good health. His proposals were in fact very reasonable. It was an innovation in particular to recommend forcefully the practical application of the electric telegraph to measure longitudes, to provide a

¹⁵ Le Verrier writes pp. 23–24 of his report: “The amphitheater is and will remain purposeless. The Observatory should not compete with the organizations of public instruction located in the very center of Paris, which suffice for their task. An institution which is requested to work at the progress of science, and which has to be organized in all its parts in support of the work of its scientists, must look for the most absolute tranquility, and the first condition is that no movement exists aside those necessary for its scientific activity extraneous activities shall be allowed that will distract from its purpose.” ... Something, as we shall see, which did not prevent Le Verrier from organizing gathering of his own societies in the Observatory.

time-keeping service, and, above all, to create a true weather service. In the end, the astronomers of the Observatory would acknowledge, despite the innumerable disagreements they had with him, that “in general his reasoning was basically sound; the fault was in his manners.”¹⁶

Lovely Promises That Were Not Kept

To carry out Le Verrier’s program, which was duly accepted by the Ministry of Public Instruction and by Imperial decree published in the *Moniteur* on 23 February 1855, money was needed. And here was the rub. The Government’s promises were not fulfilled in a timely fashion. With the money not arriving fast enough to satisfy the hot-headed Director, he addressed himself on several occasions to the Ministry, with detailed budgets and demands for more financing. One of these demands, dated 28 April 1856, detailed the amounts he had asked for in various posts, and included a request for supplementary credits of 280,000 francs in four years¹⁷; this was to be augmented soon after to 305,000 francs, and then to 330,000 francs. Nothing came of this, however, and Le Verrier was soon sending off more letters in protest.¹⁸

These letters produced no effect other than to elicit tart responses from Vaillant. Subsequently Le Verrier became even more insistent, apparently always without success, until 1856, when the National Assembly granted him a sum of 395,000 francs to construct a large refractor and a 120 cm reflecting telescope, of which we shall speak later. Before that time, he had to content himself with ordinary sources of funds to make improvements in existing instruments and to improvise new ones.

Instruments for Astrometry at the Paris Observatory

On his arrival at the Paris Observatory, Le Verrier found three instruments in operation devoted to the measurement of the positions of the stars and planets. To understand the use of these instruments, refer to the discussion in Box 4.2.

The oldest of the instruments still in use at the Observatory was an old meridian circle of 1.85 m in diameter constructed by Fortin with an objective lens by Lerebours, which had been installed in 1822 in the observing rooms, to the east of the old Perrault building. It did not have any serious deficiencies and continued to be used until 1860 without any appreciable change.

Next was a meridian telescope installed by Gambey in 1834, which at the time was the largest in the world, with a 15 cm diameter objective by Cauchoix. This instrument (Fig. 4.6) had a flaw in its design: the level, intended to insure the horizontal position

¹⁶ Interview of Gaillot by Bigourdan in 1888, BOP, *Documents divers sur l’Observatoire de Paris, 1854–1872*, cote 3567(3), folder AP.

¹⁷ BOP, Ms 1036.

¹⁸ Some of these letters and answers are the BOP, Ms 1047 D,6 and D,7.

Box 4.2 Astronomical Instruments During the Mid-nineteenth Century

The majority of the astronomical instruments at this epoch served the purpose of measuring the positions of stars, planets, and comets.¹⁹

Meridian circles are fixed vertically in the plane of the meridian along a wall, and are used exclusively for measuring the zenith distance of stars (i.e., the angle between the zenith and the direction of the star at its passage across the meridian). From this can be deduced the location of one's latitude if the declination of the star is known, or alternatively the declination provided the latitude has been previously determined. The direction of the vertical is often determined by viewing a basin of mercury at its base. With these circles the declination of the Sun, Moon, and planets, etc., can also be determined.

Meridian telescopes, also called *transit instruments*, move about a horizontal east–west axis, and must be situated in a meridian plane. These instruments are used to determine the times of passage (transits) of stars through the meridian, as referenced to a clock. They thus give the time and indirectly the longitude, or alternatively the right ascension of the stars or other celestial objects. In order to insure the consistency of the measurements, these telescopes are calibrated on targets set a great distance to the south or north of the observatory.

It is possible to combine these two types of telescopes in a *universal meridian circle*, which is no longer fixed along a wall, but rests on two pillars. A large calibrated vertical circle allows the measurement of zenith distances.

The *zenith telescope* is a fixed instrument pointed straight at the zenith. Freed of the problem of atmospheric refraction, it allows precise measurements of star positions.

All of these instruments are equipped with wire micrometers, in which the wires are either visible against a weakly illuminated background provided by a side light, or are laterally illuminated against a black background.

The ordinary *refractor* is useful for observing planets and satellites, the Moon, the Sun (through an absorbing filter), comets, and of course stars. The refractor is likewise used to measure small angular distances between stars (e.g., the separation of double stars), and to measure the position of the planets and comets with respect to neighboring stars: for this purpose one makes use of an ocular micrometer or other accessories like the heliometer. It is advantageous to equip the refractor with a *parallatic* (or incorrectly *parallactic*²⁰) mounting

(continued)

¹⁹ Many pictures of astronomical instruments from the origin to 1900, with very interesting information on their builders and on their use, can be found in Repsold (1908, 1914).

²⁰ From Arago (*Œuvres Complètes* t. 12, p. 32) “the parallactique or parallatique machine of the modern observers is called as such because its purpose is to follow the celestial objects along their diurnal parallels.” The term “parallactique” is an unjustified cognate of the word “parallaxe,” and in fact is unrelated.

Box 4.2 (continued)

(referred to nowadays as *equatorial*), one of whose two axes is parallel to the axis of the Earth's rotation, so that it suffices to move the telescope around only this axis in order to maintain the object fixed in the field of view; graduated circles allow one to easily find an object from its position, something that is not easily done with the more simple mounting (alt-azimuth), in which the axes are vertical and horizontal. Some equatorial mounts are equipped with a motor (clock drive) permitting one to continually to track on a star.

There are also telescopes with mirrors, invented by Newton (in English, *reflectors*). Until the end of the 1850s, the mirrors were made of an alloy of bronze and rapidly tarnished by oxidation. They therefore had to be often repolished, which was a time-consuming operation. It wasn't until Leon Foucault's realization, at the end of the 1850s, that mirrors made with coated glass could be easily resilvered that reflecting telescopes progressively began to supplant refractors.

Fig. 4.6 The meridian telescope of Gambey (1834). It was located in the observing rooms to the East of the main building of the Observatory. Note the two levers with counterweights, used to relieve the pressure of the pivots on the bearings

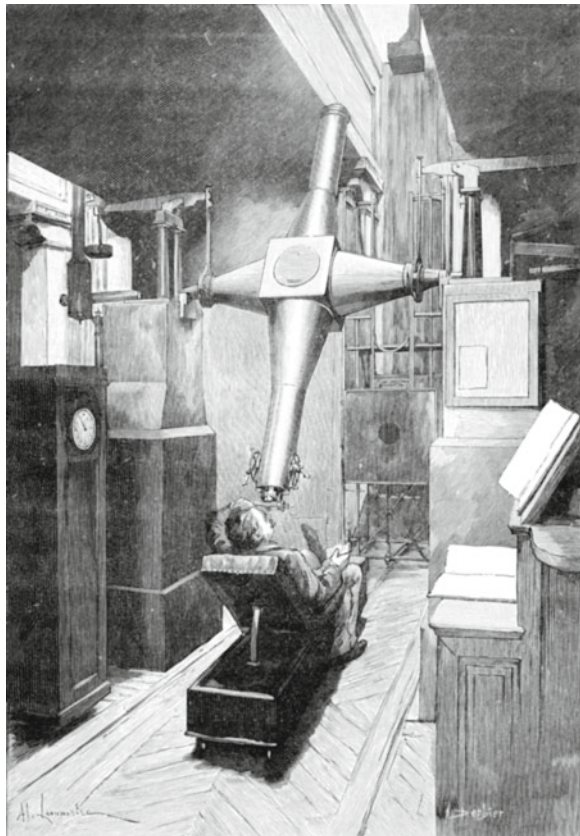
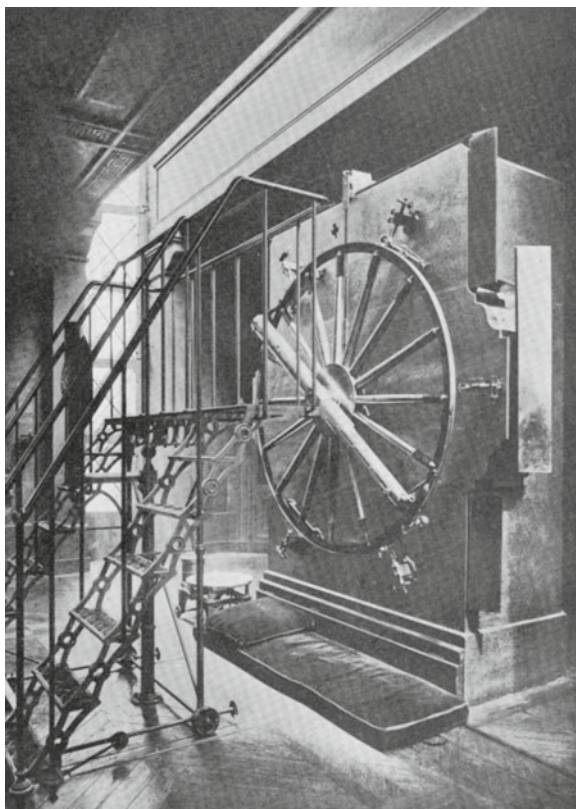


Fig. 4.7 The meridian circle of Gambey (1843)



of the axis, was not firmly attached to the axis but rather to the posts on which it rested, so that uneven wearing of the axle introduced certain errors. To alleviate this defect Le Verrier, upon his arrival, had a new level constructed; this time it was solidly fixed to the axis. The meridian telescope, equipped with a wire micrometer laterally illuminated by a lamp against a black background, would later be improved by the addition of another micrometer and an electric counter, installed in 1862; the counter was simply a relay activated by a pendulum, which produced a rather loud clicking sound on each oscillation and allowed an observer to better ascertain the instant of a star's transit across the wire. This meridian telescope and the meridian circle next to be described were to be used until 1908.

The final instrument for astrometry that Le Verrier found upon his arrival at the Observatory was a meridian circle by Gambey (Fig. 4.7), set up in 1843 in the observing rooms. Its graduated circle was more than 2 m in diameter and "its divisions seemed to be extremely precise."²¹ However, its objective lens, at 12.5 cm in diameter, was even smaller than that in the meridian telescope: the stars were difficult to

²¹ Interesting details on Gambey's method to build this circle can be found in *CRAS 68 (1869), pp. 207–220.

observe to the point where, according to Le Verrier, “a profound discouragement affected conscientious observers; and what is worse, after the observations were published, they were found, vis-à-vis foreign observations, to be inferior. Of course, this was quite intolerable.” While waiting for the replacement of this telescope – and in the end it never was replaced – Le Verrier improved its performance by isolating the wall that supported the instrument from the rest of the building, in order to dampen the vibrations due to the passage of vehicles in the street; he also put a pool of mercury beneath the instrument to help keep it from vibrating, but the mercury in the pool was also found to be affected by the vibrations. In the end the Director had to demand that a greater length of the road surrounding the observatory be macadamized. Once this was done, the problem was resolved, at least for the time being. The meridian circle of Gambey was to be used regularly until 1864, the date when a new instrument was commissioned, but was used irregularly afterwards.²²

Le Verrier also dreamed of a universal meridian circle larger than the one in Greenwich, which would permit both measurements of the zenith distance and the transits of stars across the meridian. The Paris circle, so-called, was due to Wilhelm Eichens, a Prussian instrument-maker who settled in Paris and became the head of the workshop of the Secrétan company. Marc Secrétan, the successor of Lerebours, had managed the company since 1855. The Paris circle was the prototype of an entire series of instruments with which the principal observatories in the world were equipping themselves. It was installed in June 1863 in the observing rooms, in place of the Fortin circle (Fig. 4.8). Initially it was to have been set up east of the central building as a replacement for the amphitheater, but Le Verrier’s apartments were there now. The telescope objective, manufactured by Secrétan himself, was a 10 pouces (24 cm) lens as compared to the 8 in. at Greenwich.²³ This instrument, christened the Grand Meridian Circle, would be for some time the largest in the world. It cost 50,000 francs, plus another 6,000 francs for accessories and a new clock.

Figures 4.9 and 4.10 give an interior view and an exterior view of the observing rooms, partially remodeled where the meridian instruments were deployed. The building still exists today and nowadays contains offices, the trapdoors having been filled in.

Le Verrier insisted that when observing the meridian transits of stars that the observer read the time off several clocks, rather than just one, since the correct running of only one was not easy to verify. He thought that it would be useful to place in the basement of the Observatory a master clock whose beats were electrically replicated throughout the Observatory, and would eventually be replicated also in different locations throughout the city of Paris in order to disseminate Observatory time there; moreover, electrical recording of star transits by the meridian instruments would permit refinement of the precision of the measures. All of this would take a long time to realize.

²² *CRAS 68 (1869), pp. 157–161

²³ For a detailed description see °*Ann. OP, Observations* 19 (1865), pp. 43–63

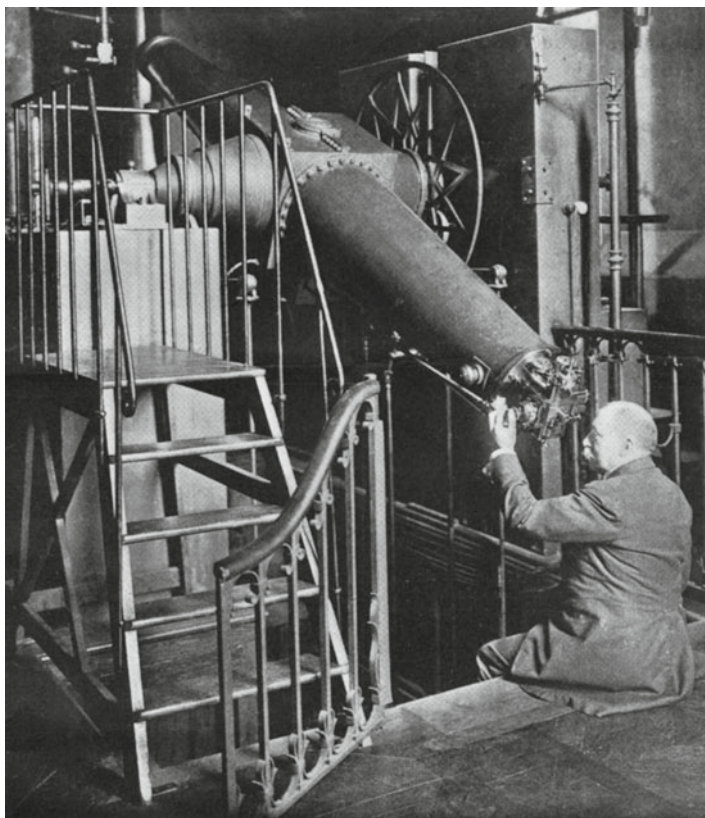


Fig. 4.8 The universal Grand Cercle Méridien of Secrétan-Eichens (1863). In this late photograph, some changes have been made to the original instrument represented in Fig. 4.9

The last instrument that Le Verrier would see installed before his death in 1877 was another meridian circle, bequeathed by the amateur astronomer and astronomical patron, the banker Raphaël Bishoffsheim; we will say more about it in Chap. 7.

Paris did not have any zenith telescopes (telescopes fixed in position toward the zenith), despite Hervé Faye's efforts to have one built.

The Astronomical Telescopes of the Observatory

The Old Refractors

Instruments for observing objects outside the meridian, i.e., for those not suitable for meridian instruments, telescopes of different design were needed. Their purpose, according to Le Verrier, was "to discover the existence new celestial bodies

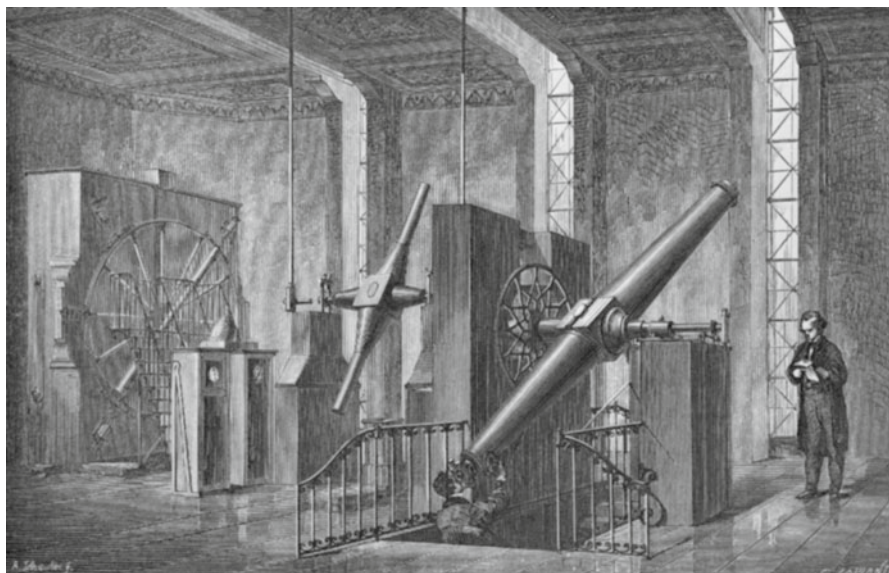


Fig. 4.9 The observing rooms at the end of the 1860s. From left to right, Gambey's meridian circle, the meridian telescope also by Gambey, and the grand meridian circle of Secrétan-Eichens

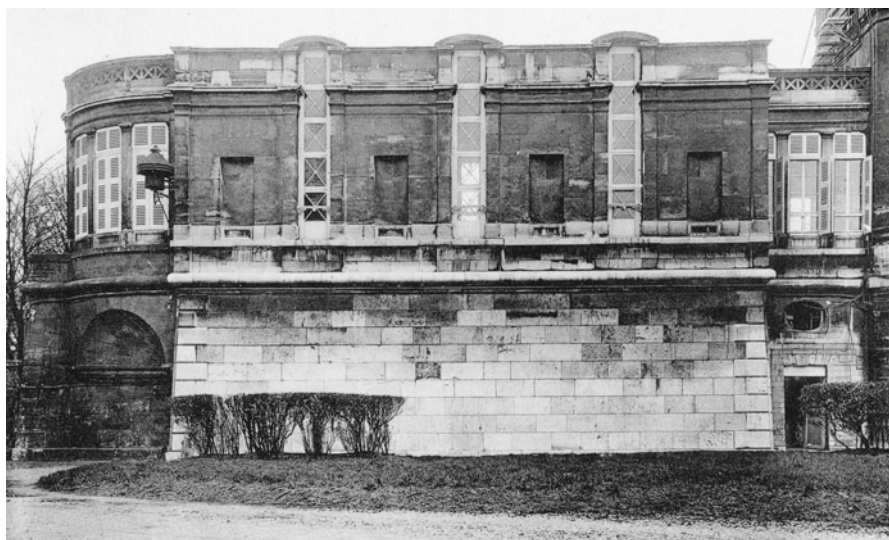


Fig. 4.10 Exterior view of the observing rooms seen from the North (old undated photograph). Three slits corresponding to the three instruments are covered by mobile trapdoors

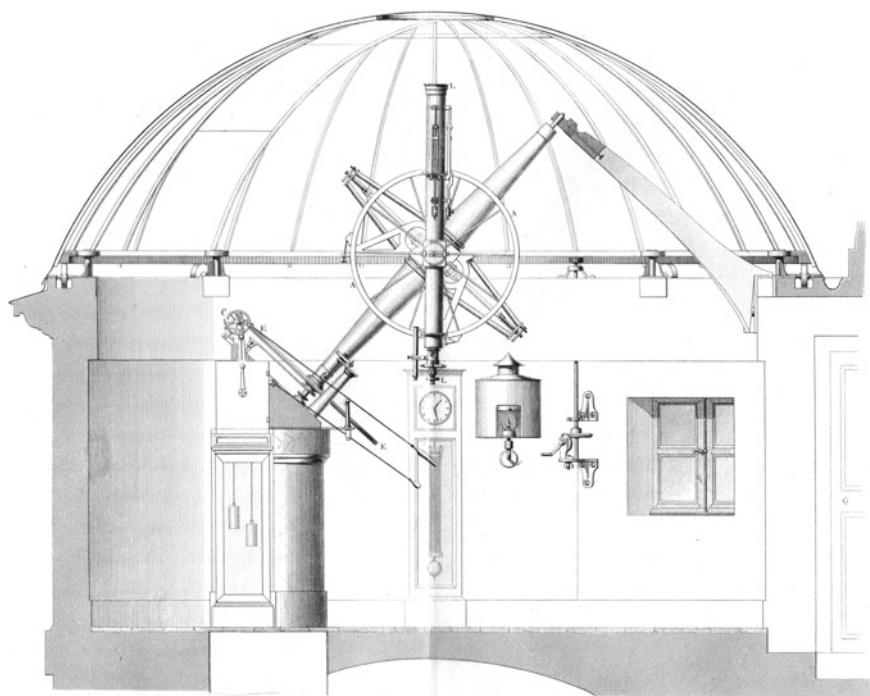


Fig. 4.11 Gambey's equatorial (1826) inside its small rotating dome on the *upper* terrace (engraving in Arago's *Astronomie populaire*). It has a large graduated circle on each of the two axes, in order to measure the position of a body with respect to reference stars

and systems.” However, for this important purpose the Observatory in 1854 had only a few telescopes which were stored inside the building and had to be taken outside to make observations, while the only telescope with a fixed mounting was the equatorial by Gambey, mounted in 1826 in the little observatory in the upper terrace (Fig. 4.11). This instrument, which earned its maker admiration at the exposition of 1823 and was lauded by Arago as “one of the most beautiful instruments ever fashioned by the hand of man,” followed the diurnal movements of the stars by means of a clock drive. However, having only an objective of 10 cm in diameter by Lerebours, it could not compete with the great telescopes in Germany, England, and Russia. Le Verrier bemoaned the unfortunate situation:

Except during the epoch of Dominique Cassini, the Observatory of Paris has always been a half century behind in the observing power of its instruments. Our telescopes were of a ridiculously small aperture compared with those of [William and John] Herschel. The Russian observatory at Dorpat [nowadays Tartu in Estonia] has had for the past thirty years an equatorial telescope of nine inches; for the past twenty years the observatories of Pulkova, London, Cambridge, and Markree, and more recently Boston, have been equipped with telescopes of twelve to fourteen inches with equatorial mounts.

Fig. 4.13 The equatorial of the East tower of the Paris Observatory, present state. There are some modifications with respect to the original instrument: in particular, the wooden tube has been replaced by a metallic one

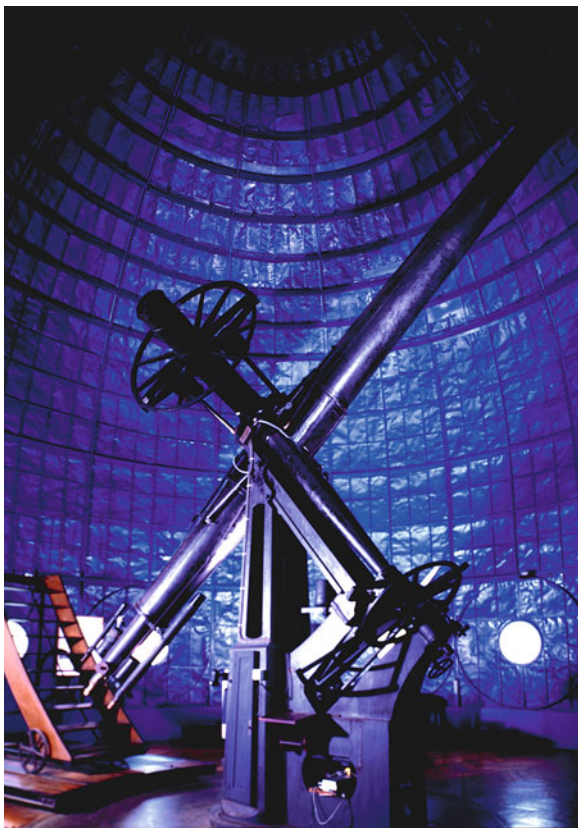


Fig. 4.14 Otto Struve (1819–1905)



1. The platform of cast iron which must serve as the base of the equatorial rests upon iron arches. This base is not very firm....
2. The said platform supported by iron arches will induce rapid changes in position simply due to the effects of temperature.... The diameter of the dome is 12 meters, and the height 10 meters. In relation to these dimensions the opening slit is only a meter wide; this is much too narrow. The equalization of temperatures between inside and outside is a vital condition for obtaining good images....
3. The movement of the dome is not rapid enough.
4. It is utterly useless that the platform where the astronomer stands turns with the dome.
5. Also it seems to be a great inconvenience that the dome rises so far above the other observing rooms and habitations.

Le Verrier took up the description of these flaws in his report. However, “despite these inconveniences, we believe that given the advanced state of the undertaking, it is necessary, above all, to complete the project, and to spare no effort to assure its success.”

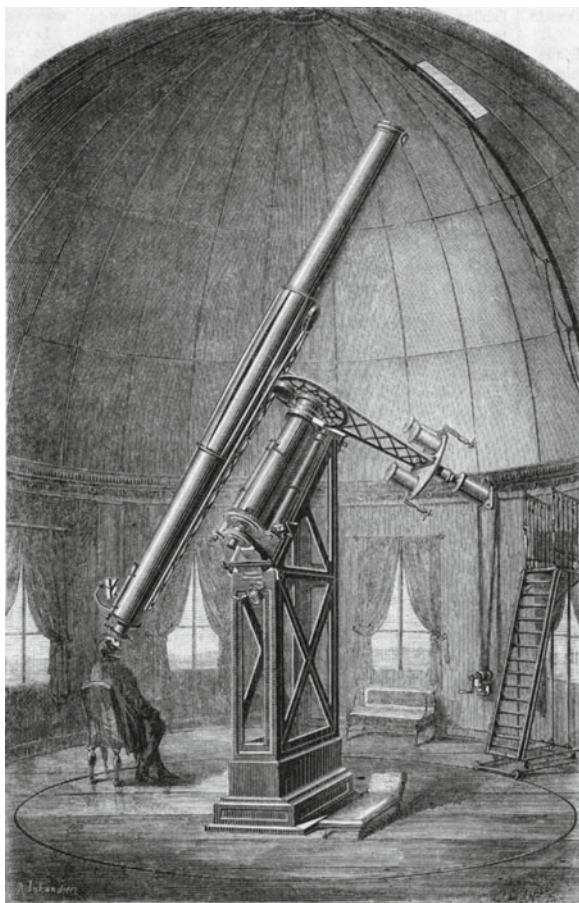
But the worst was yet to come. In November 1855 the objective lens of Lerebours was tested. It had been found to be good at its completion in 1844, and was purchased in 1844 for the hefty sum of 40,000 francs. Since then, the surface had been crazed by the appearance of a multitude of small cracks.²⁶ As to the equatorial mount, it was only delivered in 1859. The unusable objective would never be installed, and after partial repolishing, was only used by Cornu in 1874 for a new measurement of the velocity of light (see Figs. 5.17 and 5.18), and also occasionally for photographic work, as Cornu found out that by separating the two lenses the objective could be used in blue light, as long as images of the best quality were not required. A new objective was finally installed in 1883 by the brothers Paul and Prosper Henry, and the tube and the dome were modified. But by then it was too late. The equatorial would be used occasionally to observe double stars and minor planets, but on only a few nights every year, owing to the progressive deterioration of the observing conditions in the city.

More Telescopes

While waiting for the large 38 cm telescope, Le Verrier undertook, starting in May 1854, to install on an equatorial mount of Secrétan, a 9 pouces (24.4 cm) diameter objective constructed by Lerebours the father in 1823 (not 1830 as Le Verrier mistakenly wrote). This was the same lens mentioned above that had won Lerebours a gold medal at the exposition of that year. In fact, however, its figure was not very good, but it had a large light-grasp compared to the little equatorial of Gambey. With its special pavilion constructed in the garden, and a rotating dome, the instrument cost only 6,000 francs, and Le Verrier wrote: “It is our desire to show that with

²⁶ This defect of the crown glass would be corrected by Georges Bontemps at the Choisy-le-Roi workshop by modifying the chemical composition of the glass.

Fig. 4.15 The equatorial of the west tower of the Paris Observatory (1858). The dome was transported to the astronomical station of Saint-Véran (Hautes-Alpes), which is now maintained by amateurs, but the telescope no longer exists



a very modest expense individuals will be able, in France also, following the example of the English, to found small observatories that will render a very great service to science.” To this end the note describing the instrument contained a detailed description and operating instructions to put it into operation.²⁷ Clearly, Le Verrier expected it to serve as a model for amateur instruments. However, as the objective had cost 14,500 francs in 1823, there were few amateurs who could afford such instruments; it seems that it was never copied or emulated.

Beyond this, an equatorial entirely of metal (the first of its kind), made by the house of Secrétan, was installed in 1858 for 20,000 francs (plus 5,000 francs more for accessories and a clock) in the west tower of the Observatory, under a dome – this one was satisfactory – due to Joseph Jean, which cost 30,000 francs (Fig. 4.15). It had an objective with a focal length of 5.25 m and an aperture of 31.6 cm

²⁷ Le Verrier and YvonVillarceau, *CRAS 39 (1854), pp. 949–961.



Fig. 4.16 Interior view of the twin domes in 1862. Given the dates, the drawing can only correspond to a project. The two 24-cm refractors have an English-type equatorial mount but it is not certain that it corresponds to reality. The twin domes still exist, the one to the East containing the equatorial of the *Carte du Ciel*, indeed with an English mount

diameter, which may have been the one constructed in 1834 by Lerebours the father.²⁸ This objective was almost as large as the one in east tower, whose abandonment was therefore less lamented. Unfortunately, it was not very good, and would be replaced in 1884 by another telescope built by Adolphe Martin.

Finally, a new refractor with a diameter of 24 cm (9 in.) with a lens by Foucault was installed in 1868–1869 in one of the twin domes built by A. Deschars in the Observatory garden for the sum total of 12,600 francs (one of the two domes was to be constructed by Jean, but because of a law suit, he did not complete it).²⁹ The masonry supporting the second dome, entrusted to Lortias, cost 8,000 francs³⁰ (Fig. 4.16). The older of the two domes contained the 24.4 cm Secrétan-Lerebours telescope discussed at the beginning of this section. It had been moved because its original shelter was insufficient.³¹ The lens with which it was furnished was silvered for a period of time for observing the Sun without causing blindness, and later was restored to its original state.

In 1860, Le Verrier wrote to the ministry to obtain financing for a comet seeker telescope. This was a special telescope for which the observer did not have to move around in order to examine different regions of the sky (Fig. 4.17).³² The objective

²⁸ However, Le Verrier had foreseen the sum of 10,000 francs for the objective of this equatorial. Was this for repairs or improvements? The sum was insufficient for a new lens.

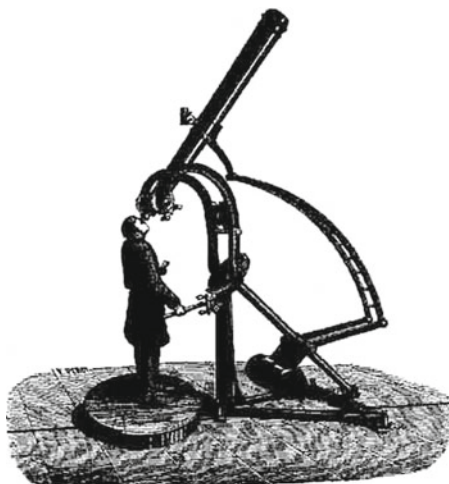
²⁹ Jean also realized in the 1860s the paneling and book shelves of the great gallery at the same level as the garden, another object for law suits.

³⁰ BOP, Ms 1060-1, file travaux.

³¹ *Rapport présenté à la commission d'inspection par le directeur de l'Observatoire [Delaunay] le 31 mai 1872*, BOP, Ms 1060-1. A smaller dome called “coupole Chacornac” existed in 1856, as Le Verrier asks 4,000 francs for repairing it.

³² BOP, *Documents divers sur l'Observatoire de Paris, 1854–1872*, cote 3567(4), folder B. This comet finder is described by YvonVillarceau in *Ann. OP, Mémoires*, 9 (1868), pp. A.131–A.135; according to him, it was invented independently by Brunner and by himself.

Fig. 4.17 Scheme of a comet finder. With this very special equatorial mount, the head of the observer is on the prolongation of the hour axis and close to the perpendicular axis which allows the motion in declination. Note the articulated lever and the large counterweight which balances the telescope around the declination axis. The observer can direct the telescope towards any point of the sky without moving

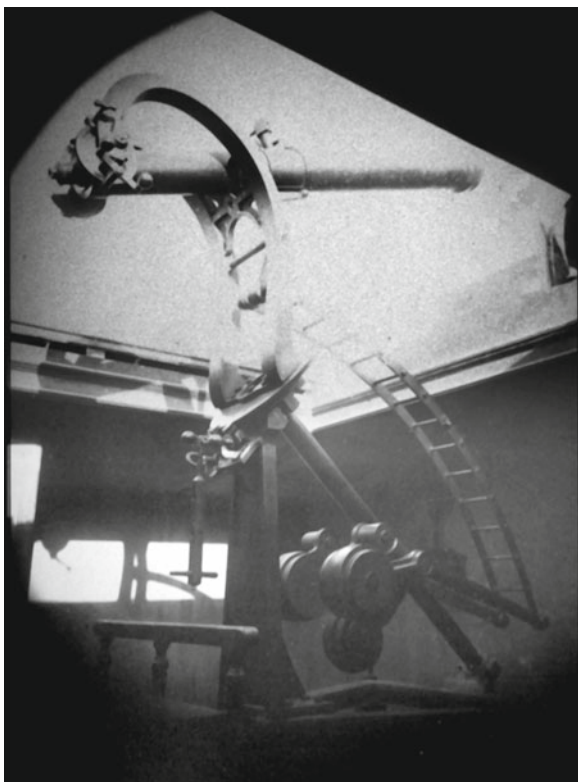


lens was to be 7 pouces (19 cm) and would cost 4,000 francs; another 2,000 francs would be required for the mounting and another 2,000 to modify the rotating dome of the building selected for it (no doubt the little observatory in the terrace). These funds were not granted, but Le Verrier would succeed in endowing the “branch” Observatory of Marseille with such an instrument (Fig. 4.18).

A Grandiose Project

Le Verrier had yet another inspiration: a giant 75 cm refractor. This project was born in 1855, on the occasion of the Universal Exhibition in Paris. The Chance brothers, from Oldbury near the English city of Birmingham, presented an optical scheme for lighthouses that won them a medal, “in consideration of their efforts to import to England the construction of lenticular lighthouses” (these had been invented in France by Fresnel). This citation would seem surprising if one did not know that the master glassmaker in the factory was a Frenchman, Georges Bontemps, polytechnician and former director of the Choisy-le-Roi factory, who had exiled himself to England following the Revolution of 1848 and was no doubt counted upon to take advantage of the opportunities offered by Fresnel’s invention. The Chance brothers also presented at the Exhibition two glass disks, one of flint and the other of crown, which attained the impressive diameter of 75 cm, and could be used to construct an achromatic objective for a giant astronomical refracting telescope. Bontemps hoped to sell them to the Paris Observatory and made an arrangement with Le Verrier which had the consent of the Ministry of Public Instruction: if the Observatory

Fig. 4.18 The comet finder of the Marseille observatory, old photograph. This instrument does not exist anymore, but a similar one can be seen at the Strasbourg observatory. Compare to Fig. 4.17



decided to acquire the disks, it would pay 25,000 francs, and if they were used to construct a quality objective, an equal sum would be paid out later.³³

Bontemps, therefore, left the disks in France. The Emperor saw them during a visit to the Observatory in May 1856, and gave his assent to the project. So the disks were duly acquired by Le Verrier. It was now necessary to determine whether their quality was good enough. This is where Léon Foucault came in (Box 4.3 and Fig. 4.19). A brilliant experimentalist, Foucault had attracted the attention of the Emperor thanks to his celebrated experiment with the pendulum, and it appears that it was the Emperor, Napoléon III himself, who had suggested that Le Verrier employ him at the Observatory. Le Verrier asked Foucault to provide him with a prospectus for work he wished to undertake. Foucault put forward two different ideas, keeping them deliberately vague because he wished to maintain his independence: one was the application of experimental physics to astronomy, the other the solution of problems in physics requiring the use of astronomical instruments (chief among the latter was the determination of the speed of light). At this stage he was not interested in constructing observational instruments. After long delays, Foucault was named

³³ Actually, on the insistence of the Chance brothers, the Minister paid them an extra 12,500 francs in spite of opposition of Le Verrier. Because the flint glass had some defect and that they did not intend to replace it, they had to be content with 37,500 francs in total.

Box 4.3 Leon Foucault

Leon Foucault, a self-taught scientist whose manual dexterity and technical inventiveness were exceptional, was born into a bourgeois family.³⁴ He would be especially renowned for his pendulum, which in 1851 allowed the visualization for the first time of the rotation of the Earth. But he would also be credited with many other magnificent achievements: the first experiments on infrared interference with Hippolyte Fizeau (1846), the experiment which directly demonstrated that light traveled less rapidly in water than in air and in this way definitively established the undulatory (or wave) theory of light (1850), the invention of the gyroscope (1852), the demonstration of the equivalence of work and heat, incorrectly called “the eddy current experiment,” and the first precise measurement of the speed of light (1862). He contributed in important ways to the development of the daguerreotype. Among astronomers, he is best known for the invention of the modern reflecting telescope, using a mirror of silvered glass, as well as pioneering the entire process for polishing and testing a mirror of this type (1857–1858). The end of his life was troubled by his run-ins with Le Verrier, which his rather dark, somber character was hardly able to cope with; the failure of several of his regulator mechanisms; and above all by the progression of the incurable sickness (multiple sclerosis) which eventually claimed his life.

Fig. 4.19 Léon Foucault
(1819–1868)



³⁴ See Tobin (2003) where many details on the work of Foucault at the Observatory will be found; Le Verrier, *CRAS* 66 (1868), pp. 380–389, describes in a very incomplete manner the contributions of Foucault to the Paris Observatory.

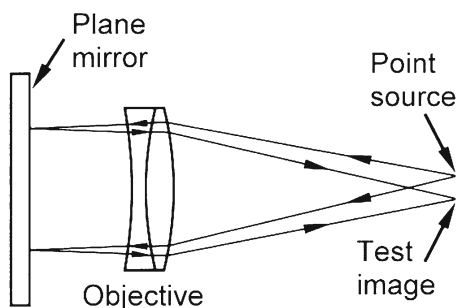


Fig. 4.20 Foucault's method to control the optical quality of an objective. The lens (or rather the two lenses) of the objective are placed in front of a plane mirror of good optical quality. This arrangement is used to image a point source located at the focus of the objective (self-collimation). One performs on this image various tests invented by Foucault, in particular the celebrated "knife-edge test", which allows defects to be seen which can then be eliminated by *retouches locales* (local retouching)

by the Emperor, on 20 February 1855, physicist of the Observatory, with a salary of 5,000 francs per year. His initial activities were few, because he had many other interests. This did not please Le Verrier.

The acquisition of the disks of the Chance brothers would force Foucault's hand. He would now have to work full time for the Observatory, because Le Verrier gave him the job of assessing the disks. He set to work early in 1856. It was no small undertaking, and it took him almost a full year to arrive at the conclusion that despite some imperfections in the flint disk, the two glass disks were acceptable. Foucault then planned to turn the disks over to the firm of Secrétan to have them figured and polished.

It is at this point that it was found that the 38 cm objective earmarked for the large telescope in the East Tower was unusable. Foucault proposed to replace it with a larger objective of 48 cm diameter that could be cut from the Chance disks. The dome was large enough to accommodate a telescope of this diameter. Le Verrier did not want to sacrifice for such a purpose these particular disks, which were unique in size. Lacking the money needed to build the 75 cm telescope, which would necessitate the construction of a new dome, all work on the Chance disks was suspended in 1862. Besides, Foucault would have needed a quality plane mirror 75 cm in diameter to carry out on this lens the testing technique which he had perfected (Fig. 4.20). He asked for 30,000 francs just to make preliminary studies. There was no question of his receiving it.

The effort was renewed in 1865. The preceding year, Le Verrier had founded the *Association pour l'avancement de l'Astronomie et de la Météorologie* (the Association for the Advancement of Astronomy and Meteorology), also known as the *Association scientifique* (Fig. 4.21), which merged later with the *Association française pour le développement des sciences* (French Association for the Advancement of Sciences); the latter had been founded in 1872. The goal was to create a lobby promoting Le Verrier's projects and fundraising. The lobbying was very effective, for the Association promised, starting in 1864, 50,000 francs for work on the Chance disks, and persuaded the Deputies to approve and underwrite a subvention of

ASSOCIATION

POUR

L'AVANCEMENT DE L'ASTRONOMIE

ET

DE LA MÉTÉOROLOGIE.

L'Astronomie et la Météorologie font de rapides progrès, dans l'ancien et dans le nouveau monde, grâce au concours des Gouvernements, de l'action individuelle et d'associations puissantes. De nombreux établissements sont fondés, de grands travaux sont accomplis sous cette triple impulsion.

Plus qu'aucun autre, le Gouvernement français donne à la Science un appui libéral et fécond. Les villes de Toulouse, Marseille, Montpellier érigent de leur côté des Observatoires. La Chambre de Commerce de Bordeaux fonde un prix annuel pour les observations météorologiques à la mer.

L'Association pour l'avancement de l'Astronomie et de la Météorologie a pour but de compléter les moyens d'action de la France.

1. Sont inscrits parmi les Membres de l'Association (sauf opposition spéciale du Comité), toutes les personnes qui en font la demande.

2. Les Membres de l'Association sont convoqués plusieurs fois par an, pour entendre le compte rendu des progrès de l'Astronomie et de la Météorologie, les propositions du Comité concernant l'Association et le meilleur emploi *scientifique* des fonds. — Les réunions se tiennent dans les galeries de l'Observatoire, ou dans tel autre lieu où les observations et expériences puissent être répétées.

3. L'Association comprend des *Membres associés* et des *Membres libres*.

4. Les *Membres associés* versent une somme annuelle de *dix* francs.

5. Les *Membres libres* versent simplement une somme de *deux* francs pour chaque séance ordinaire à laquelle ils assistent.

6. L'administration est confiée à un Comité pris parmi les *Membres associés*.

Les souscriptions sont reçues :

Au Secrétariat de l'Association, à l'Observatoire, tous les jours non fériés, de *neuf* heures du matin à *quatre* heures du soir ;

Au Secrétariat de la Société Météorologique, rue de Fleurus, n° 39, les lundis, mercredis et vendredis, de *onze* heures à *cinq* heures ;

Et par tous les Membres de l'Association.

On peut s'inscrire par lettre affranchie, adressée au Secrétariat de l'Association, à l'Observatoire, et retirer la carte de *Membre associé* ou de *Membre libre*, en versant la cotisation réglementaire, à l'ouverture de la plus prochaine séance.



Paris. — Imprimerie de GAUTHIER-VILLARS,
rue de Seine-Saint-Germain, 10.

Fig. 4.21 The announcement of the creation of the Association for the Advancement of Astronomy and Meteorology (*Association scientifique*)

395,000 francs for construction of the giant refracting telescope and also for the completion of a 1.2-m reflecting telescope to be discussed later. Foucault, with the aid of a student he was training, Adolphe Martin, was at this time in the process of finishing for the Observatory a 24 cm achromatic lens as well as a 19 cm objective for the Observatory of Lima, Peru, for which he had developed new methods for testing mentioned in the caption of Fig. 4.15. It certainly remained his intention to complete work on the 75 cm objective: the Chance disks by this time had been worked to a good figure, and preliminary polishing had commenced. But then it was realized that the disks were too thin (only 4 cm thick), a limitation Foucault had probably recognized when he had proposed to reduce them to 48 cm in diameter to replace the telescope of the East tower. Their thinness meant making an objective of excessively long focal length, but this would in turn require the construction of a truly immense (and expensive) dome. Foucault again proposed to reduce the disks to 48 cm diameter, a reduction which would have given them a suitable diameter to thickness ratio. Again Le Verrier would not consent. He had obtained the funding for a 75 cm telescope, and it was out of the question to make a smaller one. To this, add the fact that Foucault fell gravely ill, and by 1868 was dead. All work on the disks ground to a standstill³⁵ and would not resume until 1875; more will be said about the resulting instrument in Chap. 7, which is concerned with this later period.

Fixed Telescopes with Siderostats

A solution which had sometimes been used to avoid the mechanical problems associated with large movable telescopes and their immense domes was to construct a horizontal fixed telescope, into which the light is directed by the flat orientable mirror of a siderostat. Foucault placed great hopes on such a solution, which however would not have much of a future because siderostats are complicated and require a flat mirror of high quality, which is very difficult to polish and constitutes, moreover, an additional optical surface as compared to the mobile telescope. A siderostat conceived by Foucault, which he did not live to realize, was constructed at the end of 1869 by Eichens and Martin, with special financing by Napoléon III.³⁶ At first it fed a fixed horizontal reflector with a 20 cm diameter

³⁵ In the meantime, the Italian instrument builder Ignazio Porro, who lived in Paris, had proposed in 1858 to Le Verrier to buy for the enormous amount of 160,000 francs a refractor with a 52-cm diameter objective, which was installed in his premises on a rudimentary mounting. This proposal was not accepted: see Fuentès P. (1997) *L'affaire Porro*, *L'Astronomie* 111, pp. 270–272, and Tobin (2003), p. 221. Porro had previously problems with Le Verrier who refused to pay entirely another refractor in 1853. He went bankrupt in 1861 and returned to Italy.

³⁶ A paper by Anna Stoyko in *L'Astronomie* 92 (1978), pp. 94–99 alludes to this instrument in relation to an order from the Observatory to the clock-maker Winnerl relative to the distribution of time in Paris, but not to the siderostat itself. The exchanges between Wolf and Le Verrier reproduced in fac-simile in this paper are amusing.

Fig. 4.22 Foucault's siderostat (1869)



mirror, and subsequently, on a number of occasions, a refractor, but it would only be used rarely. This beautiful instrument (Fig. 4.22), which was equipped with a governor also conceived by Foucault, is preserved in the Observatory, and has recently been restored. One can also see several working functioning siderostats elsewhere, notably one designed by Foucault, which feeds a solar spectrograph at the Meudon Observatory.

A giant siderostat with a 2 m mirror feeding a 1.25 m diameter horizontal refractor, the largest ever made, was commissioned for the Universal Exposition of 1900 at Paris, but it scarcely interested the professional astronomers, all the more so because this project was purely for prestige, and went ahead without any input from them. The mirror and the two elements, flint and crown, of the photographic objective, are still at the Paris Observatory; the visual element was never made.³⁷

Reflecting Telescopes

Let us now proceed to telescopes with mirrors. Though there had been considerable activity in this direction in the eighteenth century, by the beginning of the nineteenth construction of these telescopes had been abandoned in France. This circumstance arose, no doubt, from the failure of the “grand telescope of Passy,” an instrument

³⁷ See for this instrument Launay: *J.Hist. Astron.* **38**, 459–475 (2007).

22 ft long and 18 in. (49 cm) in diameter, constructed in 1759–1761 by a Bernardin monk, Dom Nicolas Noël, for the physics and optics room of King Louis XV at la Muette, then a suburb of Paris. In 1800, Lalande estimated it had cost 500,000 francs. During the Revolution, the telescope was stored in the gallery of the Observatory, which is on the same level as the south garden. It was taken out for observing, under very unfavorable conditions. The optician Carroché in 1805 repolished the bronze mirror, which had tarnished, but the new surface soon became tarnished again and was highly degraded by 1807: it was decided, then, to leave the mirror as it was. The telescope was kept only to have something to show the public, who did not generally have access to the other instruments. It was finally taken down in 1841. Despite the success of the large telescopes of William Herschel and his son John, and those of Lord Rosse, reflecting telescopes were not constructed outside of England and its overseas possessions during the first half of the nineteenth century.

The Reflecting Telescopes of Foucault

The situation changed completely thanks once more to the genius of Foucault.³⁸ In order to verify the quality of the lenses that were to be constructed from the Chance disks, it was necessary for him, as we have seen, to obtain a good flat mirror at least 75 cm in diameter, which he was not able to finance. It was also, however, possible to employ for the purpose a concave mirror, which Foucault planned to polish himself. Suffice it to say, such a mirror is technically much easier to make than a flat mirror of high quality. Meanwhile, Foucault had seen, during a visit to England, a bronze concave mirror of 61 cm diameter constructed by William Lassell (Fig. 2.12) and installed in 1861 in his telescope at Malta. He also knew that William Herschel and Lord Rosse had successfully polished even larger mirrors. Several small bronze mirrors could be found at the Paris Observatory, and Foucault could see that their surfaces, viewed under the microscope, did not have a good finish. He turned his attention, therefore, to glass, which Newton already knew was easier to work than bronze. But glass, even when highly polished, does not reflect much light. For Foucault, the remedy was evident, because he had already experimented on small mirrors: one simply had to silver the surface by means of a process invented in 1835 by the German chemist Justus von Liebig, and later improved and patented in 1843 by the Englishman Thomas Drayton. This process involves reducing a solution of silver nitrate by means of an aldehyde or other reducing agent: the silver is deposited on the surface of a piece of glass placed in the basin. If the mirror became tarnished, one dissolved the coating by placing the mirror in a solution of nitric acid, and then deposited a new layer of silver.

³⁸ For more details on the matters treated in this section, see Tobin (2003), Chap. 12, and Aillaud et al. (2000).

Fig. 4.23 The Foucault-Eichens
20 cm reflecting telescope
(1858)



In 1857, Foucault completed his first concave silvered mirror. It was only 10 cm in diameter, but the celestial images it gave were of superior quality. He went on to make a usable mirror of 18 cm, then another of 20 cm. The latter was placed on an Eichens mount. This small telescope can still be seen in the Paris Observatory (Fig. 4.23). He fabricated additional mirrors, 32 and 36 cm in diameter, the quality of which was equally excellent. Having done all this, he next developed techniques for polishing and testing which were to be used for more than a century by opticians, and are still used today by amateur telescope makers. He went on to produce parabolic mirrors 25, 33, and 40 cm in diameter (his earlier mirrors were spherical). The 40 cm mirror was placed in June 1859 in a wooden equatorial by Eichens similar to the 20 cm telescope (Fig. 4.24). This mirror was supported by a cushion of rubber, which the observer could inflate at will by means of a tube in order to compensate for gravitational and thermal distortions. When the instrument was demonstrated, it was hailed with enthusiasm: “In the judgment of messieurs Le Verrier and Chacornac, never have such beautiful images of the Moon been admired over Paris skies.” The directors of the Cape Town Observatory in South Africa and of the Armagh Observatory in Ireland were present at the same demonstration; they also expressed their admiration. So did George Biddell Airy. Henceforth telescopes would never again be made using bronze mirrors, with the exception of that of Melbourne, Australia, which was a dismal failure.

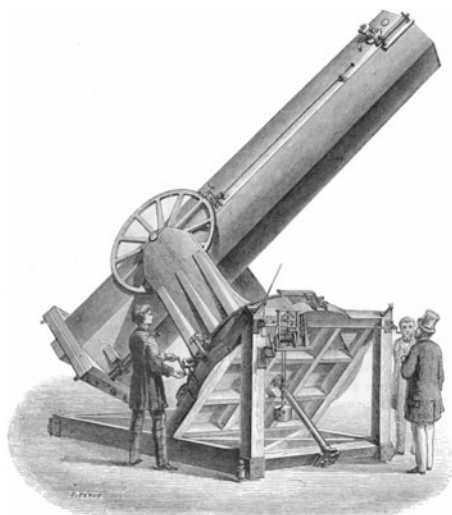
Thanks to Foucault’s genius, the Observatory henceforth had at its disposal an instrument with a larger diameter than existed anywhere else in the world, and which was also more convenient. Easily transported, the 20 and 40 cm telescopes were carried to Spain for observations of the total eclipse of the Sun of 18 July 1860, and later traveled on other expeditions (see Fig. 5.22). They were for a long time workhorses at the Observatory (Fig. 4.25).

Fig. 4.24 The Foucault-Eichens 40 cm reflecting telescope (1859)



Fig. 4.25 Many transportable telescopes are placed on the terrace of the Paris Observatory for observing the passage of Mercury in front of the Sun on 14 November 1907. From left to right, the 40 cm Foucault-Eichens reflector, the 40 cm Martin-Eichens reflector, two small refractors, the 20 cm Foucault-Eichens reflector and another refractor

Fig. 4.26 The 80 cm Foucault-Eichens reflecting telescope (1862) installed provisionally at the Paris Observatory



Le Verrier, who up to that point had complained of Foucault's lack of urgency in following his instructions, now regarded him with the highest esteem. Foucault was asked to build an even larger telescope, 80 cm in diameter: it was completed, without difficulty, by 1862. Again, it was mounted on an Eichens mount. It would win for Foucault promotion to Officer of the Legion of Honor. This telescope was nearly perfect, and might be considered the first modern telescope (Box 4.1). It was used with huge success (Fig. 4.26, Box 4.4). Chacornac, in particular, made with it a magnificent drawing of the Galaxy of the Hunting Dogs (M51, the "Whirlpool Galaxy") superior to the celebrated drawing Lord Rosse had made in 1845 with his 1.8 m Leviathan at Birr Castle.³⁹

This 80 cm telescope was installed in September 1864 at the Marseille Observatory, which was by then a branch of the Paris Observatory. It saw service there for a century without notable modification, residing in an original and practical cylindrical shelter designed by Foucault, which has unfortunately disappeared (Fig. 4.27). The telescope itself is preserved, after undergoing restoration. The mirror also survives intact. Foucault would go on to polish other telescope mirrors: a 33 cm mirror and a 50 cm mirror for the Observatory of Algiers, and a 33 cm for the Toulouse Observatory. And there were probably others. He tested and approved likewise a rather large number of small reflecting telescopes with silvered mirrors, which were manufactured and sold to individuals by Secrétan.

The Paris Observatory had another telescope with a 40 cm aperture, completed in March 1870 by Eichens, with a mirror by Martin (see Fig. 4.25); it is very similar to the 40 cm Eichens-Foucault reflector, except that the tube is made of metal rather than wood. The telescope moved on rollers, and in this way could be deployed from anywhere on the terrace.

³⁹ See Tobin, W., Holberg, J.B.: *J. Astron. Hist. Herit.* **11**, 107–115 (2008).

Box 4.4 The 80 cm Foucault-Eichens Telescope

This instrument can be regarded as the first modern reflecting telescope. Its parabolic mirror, corrected by methods invented by Foucault himself, is of excellent quality. To lessen or compensate for deformations in the mirror as the inclination of the telescope changes, Foucault made the rear face of the mirror highly convex – the mirror is 8 cm thick in the center but only 4 cm at the rim – and placed it on a cushion of air, the face of which was supported on three fixed points. The observer could vary the air pressure of the cushion in order to obtain the best images; this was the first example of adaptive optics, which has been applied since 1987 in all the large telescopes, although using a different technique. The Eichens mounting is a model of simplicity and sturdiness, but being made of wood, it was regarded as being provisional at the time, and presented some inconveniences. The tracking was done by a clockwork mechanism with a governor designed by Foucault. The telescope was not designed for photography, which wasn't sufficiently advanced at the time; in its Newtonian arrangement the light concentrated by the mirror was sent to the side of the tube near its opening, where the observer was situated. Its design did not lend itself well to anything except visual observation.

Until it was finally retired in 1962, the telescope was used for some remarkable original investigations. When it was first put in service, Édouard Stephan, director of the Marseille Observatory, used it to search for small bright galaxies, of which he discovered about 800 between 1869 and 1885. He noticed that these galaxies often appear in small groups (as in the celebrated group of five galaxies which bears his name), as well as in larger clusters.⁴⁰ Stephan also attempted to measure by interferometry the apparent diameters of stars, and the physicists Charles Fabry and Henri Buisson observed the Orion Nebula with the interferometer developed by Fabry and Alfred Pérot, marking the first use of interferometry in astronomy.⁴¹ From 1906 to 1962 Robert Jonckheere used the telescope to discover 3,350 double stars, proving in this way that reflecting telescopes are just as good as refracting telescopes for this type of observations.

A Giant Reflector for the Observatory

With the success of the 80 cm telescope Le Verrier began to envisage an even grander one for the Paris Observatory, with a 1.2 m diameter. The Saint-Gobain company, eager for the prestige of helping build the largest telescope in the world,

⁴⁰ See °*Bulletin astronomique* (1884) 1, pp. 286–290.

⁴¹ Because French astronomers were not very interested in this kind of astrophysical observations, the work of Fabry and Buisson and of their collaborator Henry Bourget was published in the *American Astrophysical Journal*, and in a French journal devoted not to astronomy but to physics, the *Journal de Physique*.

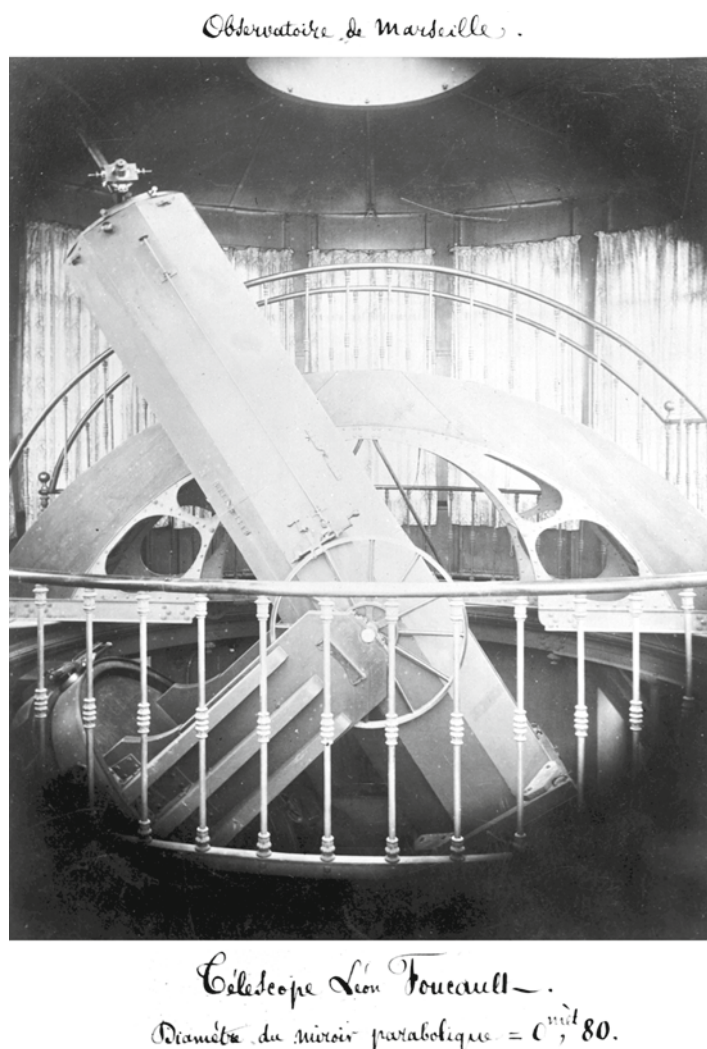


Fig. 4.27 The 80 cm Foucault-Eichens reflecting telescope at Marseille, in a shelter designed by Foucault. The observer stands on the Venetian-bridge staircase with access to the eyepiece (not visible, on the back of the tube). This bridge can be moved forward or backward according to the orientation of the telescope; it can also be placed to the east of it, or to the west of it (as in this photograph)

agreed to pour a glass disk at cost, namely, for 6,000 francs.⁴² Le Verrier accepted the proposal without consulting the Ministry, and in 1864, the Observatory found itself in possession of a beautiful 1.23 m disk. Foucault was reluctant to undertake the polishing of it. Obviously, it couldn't be done manually; a machine was necessary,

⁴² See Tobin (2003), pp. 263–266.

like the ones used by Lord Rosse and Lassell to polish their huge bronze mirrors, but Foucault first wanted to test the approach with a smaller mirror. In a letter sent on 31 August 1866 from Villeurbanne, where he had retired after he was forced to leave the Paris Observatory, Charcornac, who had used and greatly admired the 80 cm telescope, urged his friend to make the 1.2 m mirror his priority, and even tried to persuade him that he could build an even bigger one, for example, a 3 m diameter mirror.⁴³ But now the reflector was upstaged by the resumption of work on the large lenses for the giant refractor. Foucault was only able to test the mechanical polishing technique on some small mirrors before his untimely death on 11 February 1868.

So the giant telescope would have to be built without the genius of Foucault. At least the finances were favorable. Le Verrier had obtained in 1865 government financing of 395,000 francs for the large refractor, on which nothing needed to be spent yet, and for the reflector. The mounting was ordered from Eichens in 1869 but it would not be finished until 1875.⁴⁴ The optics were entrusted to Martin, but he did not have Foucault's genius and never succeeded in making a good mirror. We will speak further in Chap. 7 about the vicissitudes of this instrument. The 1.2 m telescope would be another failure and is reminiscent of the failure of the 38 cm refractor. Had Foucault lived, it might have been otherwise.

Refractor or Reflector?

It might seem surprising that after Foucault's success in constructing reflectors, refracting telescopes continued to be made, which for the same diameter are much longer and so must be housed in enormous domes at much greater cost. Moreover, the advent of photography necessitated the construction of double refractors, because it was not possible with two lenses to realize an achromatic objective both for visible light and for the ultraviolet light to which photographic plates are most sensitive. Another solution was the insertion of a movable glass in the objective, which transformed a visual into a photographic lens, or vice versa. These difficulties did not affect reflectors, which are achromatic by nature.

On the other hand, it is more difficult to polish a good mirror for a reflector than an objective lens for a refractor: the surface of a mirror must be about six times more precise than that of an achromatic lens, to achieve a comparable result. Moreover, reflectors could be more difficult to use than refractors, as Mouchez wrote in 1879⁴⁵:

One shouldn't pretend that using these large silvered glass mirrors is not a very delicate business requiring great skill, and much experience on the part of the observer if one wishes

⁴³ BOP, Ms 1037. In fact, it was not possible at this time to cast successfully a disk with a diameter larger than 1.20 m.

⁴⁴ See for a detailed description *La Nature* 4^e année, 1^{er} trimestre (1876), pp. 39–43.

⁴⁵ *Rapport annuel sur l'état de l'Observatoire de Paris pour 1878*, janvier 1879, p. 10.

to get the utmost from it. The least inequality of temperature of the glass of the mirror or of the tube, the least bit of water condensation on its surface, the slightest defect in the symmetry or equality of the suspension or in the mounting of the mirror or its support, suffices in disturbing the image in a manner which renders observation impossible. Moreover, in a climate as variable as that of Paris, in which the humidity of the air is ever changing, the metallic coating is often altered, and it probably would be necessary to resilver the mirror each year after the bad season. There would probably be very few evenings during which the instrument [he is referring to the 1.2 m reflector, which at the time he wrote had not yet been abandoned] will be able to give its full resolution.

The English popularizer Agnes Clerke wrote from her point of view in 1887⁴⁶: “Refractors have always been found better suited than reflectors to the ordinary work of the observatories. They are, so to speak, of a more robust, as well as of a more plastic nature. They suffer less from vicissitudes of temperature and climate. They retain their efficiency with fewer precautions and under more trying circumstances. Above all, they co-operate more readily with mechanical appliances, and lend themselves with far greater facility to purposes of exact measurements.”

It was claimed also that “a reflecting telescope was an instrument less enclosed than a refractor, so that the turbulence of the air had a greater effect on images,”⁴⁷ an assertion that might be true at a time when thermal problems within a dome were poorly managed. Finally, for brilliant extended objects like the planets, a refractor appears superior because the residue of chromatic aberration of the objective translates into color variations that are easier to appreciate than variations in luminance. “Viewed through even a mediocre refractor, the bands of Jupiter appear more strongly defined than in the best reflecting telescope,” wrote Foucault.⁴⁸

One would, then, see refractors and reflectors coexist side by side until, during the first half of the twentieth century, the latter would increasingly win out, as the technical problems accompanying the use of these telescopes and the conservatism of astronomers were finally surmounted.

Provincial Observatories

If the Paris Observatory, from the time of its foundation, had received all the attentions of the successive governments in France, still other observatories, usually private, nevertheless did exist in the provinces. There was a particularly strong astronomical tradition in Marseille, where several permanent observatories had operated since the beginning of the seventeenth century,⁴⁹ and a similar tradition

⁴⁶ Cited by King (1979), p. 246.

⁴⁷ BOP, Documents divers sur l’Observatoire de Paris, 1854–1872, cote 3567 (4), file AE.

⁴⁸ Foucault L. (1878) *Recueil des travaux scientifiques*, Paris, Gauthier-Villars, reprinting (2001) Paris, Albert Blanchard, p. 275.

⁴⁹ See Aillaud et al. (2000), t. 1, pp. 181–237 and 295–317; also Caplan and Prévôt (2002).

existed in Toulouse, Montpellier, etc. During the Revolution, all the technical resources in the field of astronomy were entrusted to the Bureau of longitudes. The Bureau itself would succeed in averting some of the destruction wrought by the Revolution, in particular the dispersal of the instruments of the Toulouse Observatory. The Bureau would send to the province Parisian instruments that were second-rate or outmoded, for example, in 1798 a refractor of Carroché was sent to Honoré Flauguergues for his personal observatory in Viviers. In 1838, the large mural (i.e., fixed to a wall) quadrant from the observatory of the École militaire in Paris, which Lalande and his “nephew” had used in constructing their star catalog, was assigned to the Toulouse Observatory where Arago had just named his student Frédéric Petit director; a meridian telescope, which Benjamin Valz “hardly used” at Nîmes, was likewise sent to Toulouse despite protestations of its owner who, in fact, had been nominated 2 years earlier to the post of director of the observatory of Marseille.⁵⁰ The latter had already received the meridian telescope of Jesse Ramsden acquired in 1803 by the Paris Observatory, as well as a small “equatorial machine” of Bellet, which came also from Paris.⁵¹

With such antique instrumentation, the provincial observatories had no choice but to atrophy and decline, as Wilhelm Struve noted in his 1847 letter to Le Verrier reproduced at the end of the preceding chapter. Indeed, the observatory of Marseille closed down in 1860, when its director, Valz, retired. The renovation of this observatory would occur because of what was actually a rather imaginary belief in the deterioration of the observing conditions at Paris, though to be sure, the climate was more favorable in the south of France than in the metropolis. A journal in Montpellier, evidently well informed by one of the local astronomers, described the situation, citing Le Verrier⁵²:

The oscillatory motion of Sirius, this brilliant star that shines during our winter nights, first led the famous Bessel to suspect a neighbor star. Convinced of the reality of its existence ... M. Le Verrier ordered that it be looked for with the new telescope [the 80 cm Foucault-Eichens telescope, which at that time had just been installed at the Paris Observatory]. It seemed that Sirius's companion could not escape investigations made using such a powerful apparatus.

Unfortunately, however, one had to wait three months for a favorable night to observe Sirius.

In the meantime, in America, the same search was being undertaken.... Favored by [good] atmospheric conditions, the Americans announced the discovery of the companion of Sirius. The astronomer located in Paris, to whom the Foucault telescope was entrusted, confirmed several days later, on a propitious night such as had been so long awaited, the correctness of this observation.

From that time, M. Le Verrier added, we have told ourselves that it has become necessary to search for a sky more favorable to astronomical investigations, and to transport our instruments to the south.

⁵⁰ Lamy (2007).

⁵¹ Lequeux (2008), Chap. 7.

⁵² *Messenger du Midi* (Montpellier), Saturday 21 June 1862.

The Marseille Observatory, Branch of the Paris Observatory

Le Verrier thus began to preoccupy himself with searching for a better site for the 80 cm reflecting telescope of Foucault: he contacted the Ministry, which authorized him to install it in the South. The cities of Marseille, Montpellier, and Toulon were put on the list of places vying for it. Marseille seemed to be best situated, because of its astronomical traditions. But there was concern that the observations might be disturbed by the mistral, a wind from the north that can blow very strongly in Provence. Le Verrier, assisted by Foucault, decided therefore to begin his visit of the three candidate cities with Montpellier. The *Journal de Montpellier*, edition of Saturday 14 June 1862, feted the arrival of these two men the preceding Thursday, who came “to evaluate the suitability of establishing in our city an important observatory, an annex, of sorts, to that of Paris, where the novel instruments of M. Foucault are to be installed.” They were evidently received with great hospitality, and entertained at a banquet at which Le Verrier recounted how Foucault, having constructed a mirror with which to test the two lenses of Chance, created his mirrors out of glass; that of 80 cm had performed so well that the lenses themselves became redundant.⁵³ But the mistral also blew at Montpellier and Toulon, and did so as strongly as at Marseille. There was, therefore, no particular reason to install the telescope at Montpellier. In the end, Marseille was chosen. Le Verrier was to take advantage of the situation by realizing his old dream of taking over the observatory of Marseilles as a branch of the Paris Observatory.

The mayor of Marseille was enthusiastic. The old observatory of the Accoules had been inoperative for the past 2 years. It was “enclosed in a quarter of small narrow streets; whereas Le Verrier wished for a rather imposing situation, well away from all important buildings where the various instruments could be set up in isolation on the grounds, and in such a manner so that they would not interfere with each other. After having at first set his sights on a point at the *Château Borely*, which later he judged to be too close to the sea, he opted definitively, in accord with the mayor who at the time was M. Rouvière, for part of Longchamp plateau, situated east of the city at an altitude of 75 m, approximately, and almost entirely surrounded by public gardens.”⁵⁴ In fact, already, there was a great deal of construction going on in the area, but very little industry, and public lighting was not yet the annoyance to astronomers it was to become. A provisional agreement was struck on 19 June between the Ministry and the mayor of Marseille, with a definitive agreement reached on 16 May 1865. A decree of 1863 established the Marseille Observatory as “a branch of the Paris Observatory”; Le Verrier was its director, and Édouard Stephan, the astronomer, directed on-site operations with the title of adjunct-director; he was assisted by two adjunct-astronomers, messieurs Borrelly and Coggia. The

⁵³ This contradicts Le Verrier’s insistence on building the 75 cm refractor; but perhaps this was a slip of the tongue, or the journalist failed to understand.

⁵⁴ Stephan E. (1914) L’Observatoire de Marseille, *Encyclopédie départementale des Bouches du Rhône*, Marseille, t. 6; reproduced in Caplan and Prévôt (2002).

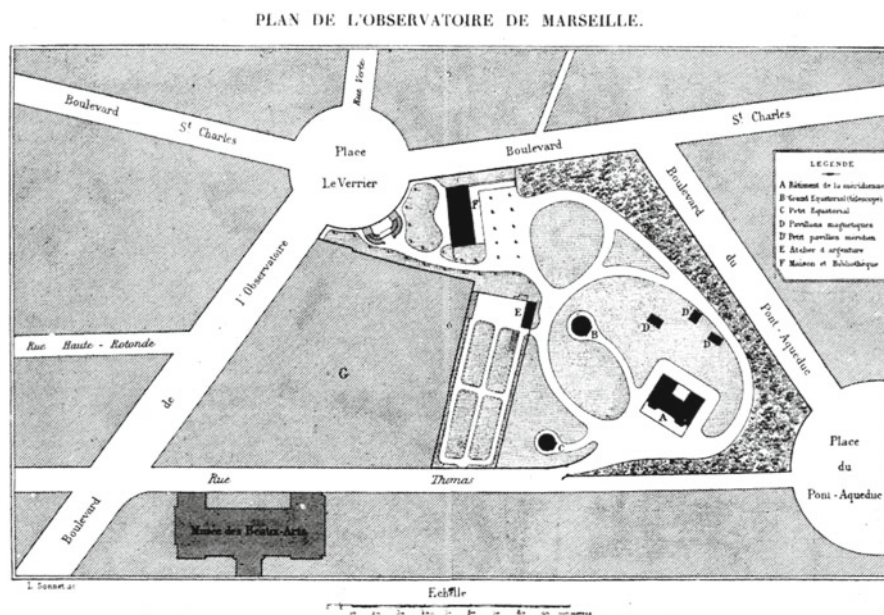


Fig. 4.28 Plan of the Marseille observatory at the end of the nineteenth century. *F* offices, and on the upper floors apartments of the director and astronomers; *A* and *D* meridian instruments; *B* 80 cm reflecting telescope; *C* 26 cm equatorial; *E* workshop and kitchen-garden; *D* magnetic pavilions

land and equipment of the new observatory belonged to the city of Marseille, which provided a grant of 15,000 francs annually toward the costs. The observatory was inaugurated at the end of 1864; the building was designed by the architect Espérandieu, who also designed the Cathedral “La Major” and Notre-Dame de la Garde. Work lasted for 14 years, during which instruments of the highest quality were installed⁵⁵ (Fig. 4.28 gives a plan of the ensemble). Sited there were:

- The 80 cm reflector installed in 1864;
- A comet-seeker of Eichens, with an 18.2 cm objective by Martin (1866) (Fig. 4.18);
- An equatorial of Eichens, with an objective of 25.8 cm by Merz, of Munich (1872). It included a governor by Foucault, as did the 80 cm reflector;
- A meridian circle by Eichens, with an 18.8 cm objective by Martin (1876);
- And finally, apparatus for measuring magnetic declination and the variation and intensity of the terrestrial magnetic field.

One may note that the comet-seeker equatorial and the meridian circle corresponded exactly to the instruments specified in 1847 by Wilhelm Struve in his letter

⁵⁵ Stéphan, E.: L’Observatoire de Marseille, °*Bull. Astronomique*, **1**, 122–132 (1885).

to Le Verrier. It is probable that Le Verrier, who no doubt would have very much have liked to have had such a comet-seeker in Paris, sought to follow Struve's recommendations to the letter. The reflecting telescope came as well, along with the magnetic apparatus which were involved in a project to draw up a magnetic map of France. The Observatory of Marseille would remain under the tutelage of the Paris Observatory until 1878. Thereafter it became independent.

The Other Provincial Observatories

A government decree of 1856 called for another branch of the Paris Observatory to be set up in Algeria. Initially this was limited to a meteorological station at the Algiers *Lycée* (high school).⁵⁶ But in 1858 another government decree, probably inspired by the Bureau of longitudes following an imbroglio involving Le Verrier and Faye, established an "astronomical station," which gathered together astronomical, meteorological, and magnetic observations. The observatory had use of a 33 cm diameter Foucault reflector, and another of 50 cm delivered in 1861, but the place was to languish nevertheless until 1873 when it was granted independent means and status, and especially after 1880, when it was placed under the leadership of a new director, Charles Trépied, who would make it one of the best observatories in France. It still exists, and all the original instruments are preserved.

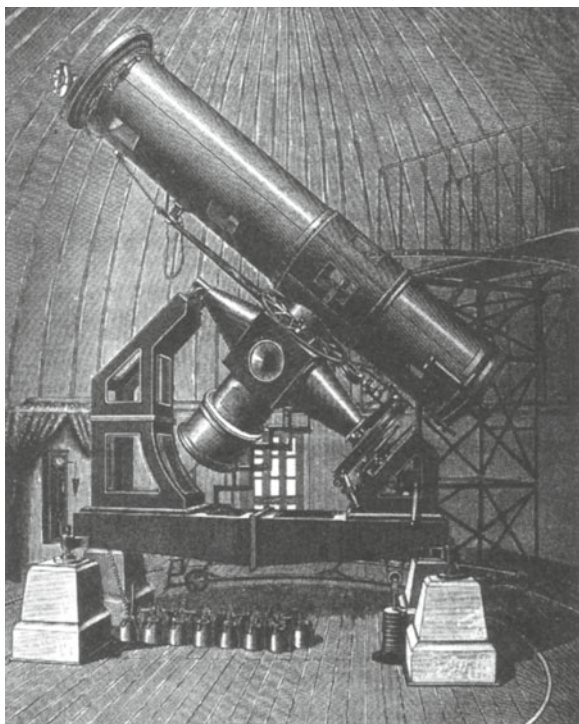
The even older Toulouse Observatory remained independent of Le Verrier.⁵⁷ It was poorly equipped and rather inactive. A decree of 15 July 1872 put it under the control of the state, and it was reorganized. Henceforth it would be comfortably funded. Félix Tisserand became the director in 1873, but for only a short time as he was soon appointed to Paris. His replacement, Benjamin Baillaud, would find this observatory in a rather bad state, as the two most remarkable assistants, Henri Perrotin and Guillaume Bigourdan, had followed Tisserand to the Paris Observatory. Also, the Foucault 33 cm diameter telescope lacked an equatorial mounting; the mirror of the large 83 cm reflector installed in 1875, a copy of the one in Marseille by the brothers Henry (Fig. 4.29), was badly in need of resilvering. It was not until the year 1880 that the Toulouse Observatory would finally achieve maturity with sufficient personnel and equipment.

The government decrees concerning creation of the Besançon, Bordeaux and Lyon observatories date from 11 March 1878: the tendency by then was toward decentralization. The Nice Observatory, privately founded through the patronage of the wealthy banker Raphaël Bischoffsheim, would be constructed starting in 1879. Bischoffsheim bequeathed it in 1899 to the University of Paris, but it was to be

⁵⁶ For an history of this observatory and of other provincial ones, see Le Guet Tully et al. (2008), and Trépied, C. (1884) °*Bulletin Astronomique*, Serie I, vol. 1, pp. 214–216.

⁵⁷ For an history of the Toulouse observatory, see <http://www.imcce.fr/fr/grandpublic/systeme/promenade/pages5/549.html>.

Fig. 4.29 The 83 cm reflecting telescope of the Toulouse observatory (1875). The original wooden mount was replaced in 1889 by the metallic one visible on this photograph



more appropriately affiliated with the University of Nice during the second half of the twentieth century.⁵⁸

For completeness, some mention should also be made of the Strasbourg Observatory, constructed between 1876 and 1880 by Germany in the framework of a grandiose program of modernizing the University, which was intended to be a showcase for German science. The considerable equipment allocated to it attests to the efforts made by the German government to build up scientific facilities in Alsace and Lorraine⁵⁹: an equatorial refractor by Repsold with a 49 cm diameter lens by Merz, the largest in the German empire, and a comet seeker also by Repsold and Merz and placed with the refractor in an immense dome. A large meridian circle with a 16 cm lens, again by the same craftsmen, was added to an old meridian telescope built by Cauchoix in 1828. The observatory was ceded to France in 1919 along with Alsace and Lorraine (according to the terms of the Versailles Treaty agreed after World War I).

⁵⁸ For the equipment of these observatories, see the preceding reference and Grillot (1986).

⁵⁹ For an history see http://fr.wikipedia.org/wiki/Observatoire_astronomique_de_Strasbourg#Histoire.

Over the course of time, fewer and fewer visual observations would be made in all of these establishments, which were generally situated in the middle of cities: soon, the time would come for observing stations to be set up in geographical situations more favorable for astronomical observations. An example would be the observatory of astronomical physics at Meudon (a suburb of Paris) occupying a position that was intermediate between metropolitan observatories and those situated in better climate and far from the light pollution of the cities.

Chapter 5

The Dictator (1854–1870)



Le Verrier, an engraving by Maurin after
Daverdoing

Le Verrier succeeded in obtaining excellent instruments for the Paris Observatory and its branch in Marseille, in particular several Foucault reflectors with silverized mirrors, a complete novelty. But astronomers are needed to operate these instruments, and where the problem arose. There would be a veritable train of successive astronomers and calculators at the Observatory. Why so many? In an obituary for Maurice Lœwy, an astronomer hired by Le Verrier who died in 1907, appears the following statement¹:

Le Verrier knew how to attract and discover young talent; but perhaps, once he had attracted it, once he had discovered it, he was not persistent enough in developing it.

For his part, the adjunct-astronomer Ernest Périgaud declared in 1888²:

Never did Le Verrier say to his subordinates, ‘Well done.’ Instead the first words out of his mouth were always: ‘That’s wrong, it doesn’t go like that.’

We will now look more closely at what was going on.

Life at the Observatory

The Character of Le Verrier

According to Camille Flammarion (Fig. 5.1),³ Le Verrier was, when he received Flammarion to the Observatory in 1858, “tall, pale, light blonde, dressed in white, with yellow leather slippers. Born in 1811, he was at the time 47 years old, but looked younger than that.” Flammarion adds later in his memoirs: “M. Le Verrier had the most appalling character imaginable. He was haughty, disdainful, inflexible. This autocrat considered all the employees at the Observatory as his slaves. He was detested very much.... The completely intransigent, brutal character of the director did not detract in any way from his mathematical genius, but it exerted an extremely depressing influence on the Observatory. The underlying cause of his unpleasantness may have been his hypochondriacal character – something, as I was to learn only much later, due to an indisposition of the stomach.”⁴

A more nuanced characterization was penned by Joseph Bertrand, who knew Le Verrier very well. In his obituary of Felix Tisserand,⁵ Le Verrier’s successor in celestial mechanics and director of the Observatory from 1892:

¹ *Bulletin astronomique* (1907) 24, pp. 385–395, see p. 385.

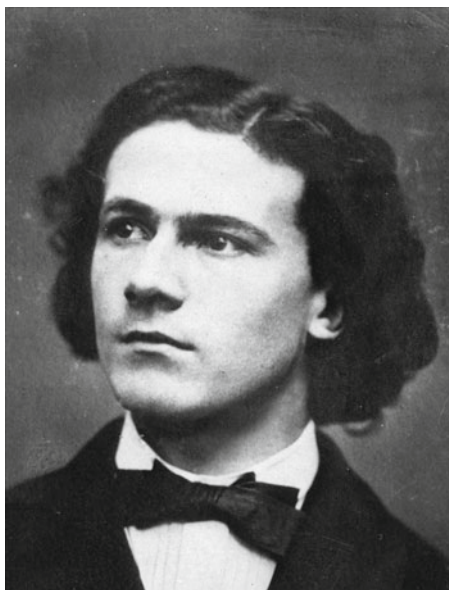
² Périgaud, interview by Bigourdan, BOP, *Documents divers sur l’Observatoire de Paris, 1854–1872*, cote 3567(3), folder AP.

³ *Flammarion (1911) pp. 139, 210–211.

⁴ This is a disputable assertion: Le Verrier could be pleasant and relaxed in private, and was equally disagreeable before and after his illness, which was due not to a disease of the stomach but the liver.

⁵ *Bertrand, J.: Vie et travaux de Félix Tisserand, *Revue Scientifique*, 4^e série. 13, pp. 65–72, see p. 66 (1900).

Fig. 5.1 Camille Flammarion (1842–1925)



Despite his outstanding qualities, Le Verrier, according to the common rumor, inspired great dislike, and the general opinion regarded him as having a difficult character, much complained of by his collaborators; he was hostile to some, despotic to others. He treated his colleagues with defiance and hostility. He was vigilant, also, and attentive to details, singularly skillful in reigning over the Observatory. Though he turned it into an excellent school, it had a reputation for being an intolerable one. The protests against him were passionate, and often against all reason. Though they must have contained an element of truth, they exaggerated, no doubt, and painted Le Verrier's colors darker than he deserved. Marshal Vaillant, a friend of authority, but with a conciliatory temperament, said and liked to repeat: 'The Observatory is impossible without him, and with him, even more impossible.'

The most lively and no doubt the most objective account on the subject of Le Verrier's character comes from certain manuscripts found in the Paris Observatory library that were collected by a historian, Victor Advielle d'Arras, perhaps with the intention of eventually publishing them.⁶ Advielle had been tasked with selling Le Verrier's

⁶ Advielle, V. (s.d.) *Notes sur l'Astronome Leverrier et sur plusieurs autres Astronomes français*. BOP, Ms 1027. Delaunay's testimony, in his 1869 letter to the minister of public instruction, reproduced in Bigourdan (1933) pp. A.30–A.32, is much more severe to Le Verrier, but it cannot be objective knowing the hatred of Delaunay for Le Verrier: "The main features of his character are a boldness and charlatanism that I have never seen elsewhere. Add to this that he lies as easily as he tells the truth, and with an incredible cheek. From a high-handed despotism, unbearable to his inferiors, he passes to an obsequious docility toward those which are above him and from whom he might obtain some favor. He is incredibly cowardly, and when he feels that he is caught he lies prostrate as a dog: but this is only to buy time until the opportunity presents to resume his usual behaviour. At the same time he succeeds through his boldness to intimidate most of those from whom he wants something and over which he has the slightest hold.

The interest of science means nothing to him. Everything gives way to his immense vanity, to his desire to raise in front of the eyes of the public the pedestal to his person. Nothing can stop him: decrees, regulations, the strict expression of the Emperor's will, all this he casts aside. He has relied on the extreme kindness of Her Majesty with continuous success during the last 16 years."

library after his death (an event which took place between 20 and 25 May 1878). He said, though without elaboration, that most of the texts he cited came from Charles Aimé Joseph Daverdoing, a painter familiar to Le Verrier who had painted his portrait in 1846. Advielle's account of how they first met is amusing:

In 1833 ... [Le Verrier] arrived often at his residence in the middle of the night, and to unwind from his astronomical calculations played the violin with frenzy, something which, it can be imagined, was not at all to the taste of his neighbors. Next to him there lodged a young law student, Daverdoing, later a notary and councilor of the prefecture of Arras, who was as passionate about the violin as Le Verrier. One night, therefore, as Le Verrier, according to his custom, kept his neighbor from sleeping, the latter got up, grabbed his violin and bow, and began in turn to play with such fervor as to bring the walls down. The following day the two neighbors introduced themselves, and became friends. Thereafter the house often resounded with the harmony of their two melodies. It is in this way that later the artist Daverdoing, brother of the lawyer, became acquainted with Le Verrier.

And here is how Le Verrier's character is described:

The astronomer was *à tous crins* [all out] even with his family and the members of the Institute: but in private settings, he was a good-natured fellow, very cheerful and good company. Le Verrier was excessively demanding to the calculators at the Observatory, and did not make allowance for the age or the stamina of the workers: he exacted from them a quantity of work that was often excessive, which gave occasion to incessant and bitter complaints. He was never one to bite his tongue, and once or twice laid hands on someone. Despite this, he had moments of great generosity, for example when [Eichens or Foucault?] completed his great telescope, Le Verrier, who eight days before had accused him of sloth and had dressed him down, ran to embrace him and tell him he had judged him wrongly... "Le Verrier was so demanding that he did not allow his office-assistant to be absent ever, no matter how pressing the emergency. When, occasionally, he found him missing from his post, he inflicted on him a 2 franc fine. The astronomers at the Observatory had a fixed salary but were eligible for a number of additional emoluments; the latter, however, Le Verrier allowed to be granted only to those who carried out a truly excessive amount of extra work.⁷ He was unstinting and did not accept any excuse. One day he sent Madame Le Verrier to summon an astronomer to his office. Madame Le Verrier found the astronomer dining. "Take your time," she said, "finish eating, it's not urgent." But Le Verrier had not authorized this. Rather what he had told her to say was that he wanted to see the man—at once!

A friend came to Le Verrier when he was occupied feverishly with his calculations on the planet Uranus. Le Verrier refused to receive him. The friend left. A moment later he heard Le Verrier playing the violin. This is too much, he said. But Madame Le Verrier was there, and explained to the annoyed friend that this was among the peculiar habits of the astronomer. As soon as boredom set in, or he encountered a difficulty in the calculations, he resorted to his violin, which had a soothing effect on his mind.

Usually, he was the Master. He insisted on obedience, as his wife explained to her friends in accounting for some of his eccentricities.

Le Verrier was very vain about honors. One day, when at Cherbourg, he learned that a Russian fleet had just arrived. He quickly telegraphed Madame Le Verrier, asking her to send his Russian decorations haste-posthaste. The following day he and the prefect went to visit the admiral on his vessel. Le Verrier was wearing a large Russian ribbon. As soon as Le Verrier came into view – to the prefect's considerable astonishment – a cannon sounded. The admiral was saluting the Russian orders worn by Le Verrier.

⁷ *Flammarion (1911) writes p. 144: "He imagined, in order to accelerate the work, to pay three sous [0.15 franc] per star observed at the meridian telescope! But then one was sometimes too fast and the observations lacked the necessary perfection."

In Le Verrier, we have many examples of human infirmities. He was often gauche in private: he farted, belched, etc., without noticing it or paying any attention to how those around him were affected. The more he advanced in age, the more he lived in a rarefied atmosphere that had nothing in it that was human.

A contemporary quipped⁸: “Le Verrier produces sharp words as naturally as the apple tree bears apples.”

The memoirs of Lucile Le Verrier contain a fair amount of information about her father.⁹ In reading them, one has the impression of a family without any conflict. Lucile was able to realize her dream of becoming a talented amateur musician thanks to lessons from great masters including César Franck. But one scarcely finds in her memoirs any descriptions of her father’s character, just a few testimonials of admiration: “My father is a celebrated astronomer, a great man” (p. 19); “the meeting of the Scientific Association [was] presided [over] by my daddy [January 1867]. He gave a lecture on the comets and the falling stars; he was superb, clear, eloquent, magnificent” (p. 27). But though Lucile clearly admired him, she does not seem to have felt much affection for him; she reserved that for her mother.

Concerning Wealth

Here again, Daverdoing is an attentive and uncompromising witness:

The household of Le Verrier was managed with considerable expense; and yet neither he nor his wife had inherited great personal wealth. His salary, at the time of his greatest income, included 30,000 francs as Senator, and an equal sum as Director of the Observatory. Probably he had 20,000 francs of additional income from various other sources. He lived, therefore, in Paris, on a total income of 80,000 francs, a considerable amount,¹⁰ of which, one imagines, Le Verrier could have set some aside. But he was little concerned about the future of his family.—As long as I live, he said to his friends, I can get a lucrative situation anywhere in Europe if I lose this one; when I am dead, my name alone will be a sufficient resource for my children; besides, they will work for their living, as I did.—He had, at his home, frequent receptions and banquets that often cost 1000 or 1500 francs. With this lifestyle one imagines that the adage of the thrifty ant [a reference to the fable “The Cicada and the Ant” by Jean de la Fontaine] was quite forgotten under his roof. Further, when the time came for his young daughter [Lucile] to marry, he could only scrape together and promise 20,000 francs for dowry; it was all he had. Luckily, the groom [Lucien Magne] was a wealthy architect, and did not require a lot. As to Le Verrier the son [Urbain Louis Paul], he could, thanks to his name, marry the daughter of a rich silk manufacturer from Lyon, who would bring him 200,000 francs for a dowry, it is said.

⁸ Cited by Levert et al. (1977) p. 122.

⁹ Le Verrier, L. (1994).

¹⁰ The franc at this time was worth more than 5 present dollars, and Le Verrier did not pay any rent for his very large apartment. The salary of servants was very low, and one could live comfortably with 7 or 8,000 francs per year. This was the salary of titular astronomers around 1870.

The journals of the time refer to these family events. Organized by Madame Le Verrier, whose beauty and amiability contrasted sharply with her husband's rebarbative character, they were attended by many persons of quality.

Le Verrier had excellent relations with Napoléon III and his court. The astronomer, a perfect courtier, was on familiar terms with the imperial family, as witnessed by the following amusing passage from a book¹¹ about the Empress:

One evening, after dinner, the conversation turned to astronomy. The illustrious Le Verrier had just discovered, as a result of his marvelous calculations, a star such that the electric spark ... would need an extraordinary number of years to reach the celestial body [sic: here is a good example of journalistic confusion!]. Le Verrier was there, to explain these astonishing things: the simple clarity of his words rendered the subject clear to the understanding of the infant prince, as well as to maids of honor and chamberlains. The young Louis-Napoléon, keenly interested, posed naïve questions to the savant, who very sweetly and eagerly attempted to satisfy him. At this point the royal couple approached; the circle opened respectfully. "What are you discussing?" asked the Empress.

'Madame,' replied the great man, bowing, your Highness's son deigns to explain to me his thoughts on astronomy. They are very remarkable.

The young Louis-Napoléon did not doubt the perfect justice of this compliment, for he appeared delighted. But his mother did not allow him to harbor for long such a favorable opinion of his scientific insight.

'O, monsieur,' she said to Le Verrier, "don't flatter this child who, unfortunately, likes to stretch the truth. His ideas about astronomy? Well, I can only imagine what they might be."

The Emperor sometimes directly addressed Le Verrier to obtain answers to some of his questions, which shows that the two men were on familiar terms. Le Verrier accompanied the Emperor to England in 1854, during the Crimean war; Madame Le Verrier played the role of maid of honor and was singled out "for her personality and the perfect taste of her toilette."¹² It is also said that Le Verrier accompanied the Empress to Egypt for the inauguration of the Suez Canal in 1869. As Lucile makes no mention of this in her memoirs, however, this claim must be regarded as suspect.

The Personnel "Turn Over Like Travelers in an Inn"

For the personnel of the Observatory, life was much less agreeable because of the constant demands of the Director. His lack of interest in anything outside his own domain and in work being done elsewhere provided little incentive for his subordinates. Here is what Advielle says about this:

Le Verrier did not have, properly speaking, a library. He read, moreover, almost nothing. He did well in his studies at the college de Coutances (in fact Saint-Lô), where he was outstanding in rhetoric; he knew Latin well, and a smidgeon of English. He was ignorant of other languages. Besides, he only considered the work of a few great astronomers worthy of his attention.

¹¹ Loliée, F. (1907) pp. 361–363.

¹² Levert et al. (1977) p. 122.

Just this month I have completed a review of the card index of his paltry library; I was able to verify, in effect, that almost all that he had received from France, England, and other countries in the last few years had hardly been read. Many of the periodicals were still in their wrapping. In some cases, he had torn the corner to see in what language the work was written, then threw it in a pile. Part went to the kitchen [presumably to be burned], part was to go on sale [a sale in which Advielle was in charge].

The Observatory staff had therefore to carry out, for a measly salary,¹³ their routine labor as the Director defined it, without asking any questions and without deviating one iota from their assigned tasks. Any deviation, even if it improved one's knowledge of astronomy, was penalized by a decrement of wages. Changes in salary, owing to the split between fixed income and incentives, occurred so frequently, as did the changes in personnel, that they are practically impossible to document from the archives.¹⁴ It was only in 1868, and then under external constraint, that Le Verrier finally managed to stabilize the situation.

Le Verrier also had the bad habit of appropriating the discoveries of his underlings. This was hardly calculated to motivate them in their work. And, according to Flammarion,¹⁵

The particular conduct of this personage in the [Paris] Observatory is only one symptom of his behavior in the broader scientific arena, where he will take up, without scruple, novel questions without noticing other astronomers who have treated them before, in an attempt to draw to himself all the glory: an exquisitely delicate process which this year again [1867] he put into practice in presenting his cometary theory of meteors, the original idea for which belongs to [Giovanni] Schiaparelli, Director of the Observatory in Milan.

What Flammarion alleges here is correct. There are swarms of meteoroids spread along the orbits of comets, that produce meteor showers on entering the Earth's atmosphere whenever the Earth crosses their orbits (Fig. 5.2). This was discovered in 1866 by Schiaparelli for two comets which, he showed, produce the shooting stars of November and August – the very ones that Le Verrier would consider in his article in 1867.¹⁶ Schiaparelli had published his idea in journals that Le Verrier could hardly have missed unless he had thrown them out before opening them. Delaunay was familiar with them, and added that the German physicist Ernst Chladni had already suspected such a connection as early as 1819, in his work *der Feuermeteoren*.¹⁷ Admittedly Le Verrier, beginning in 1856, had

¹³ *Flammarion (1911) writes p. 148: "My salary was modest: 50 francs per month the first year, raised to 80 the following January, to 100 at the end of the second year, to 150 after the third one and to 200 after the fourth. Extra hours were added during the winter months, when M. Le Verrier wanted to speed up as much as possible the publication of the *Annales de l'Observatoire*."

¹⁴ BOP, Documents divers sur l'Observatoire de Paris, 1854–1872, cote 3567(4), folder V.

¹⁵ *Flammarion (1911) p. 515.

¹⁶ *CRAS 64 (1867) pp. 94–99.

¹⁷ See °*Monthly Notices of the Royal Astronomical Society* 71 (1910) pp. 282–287.

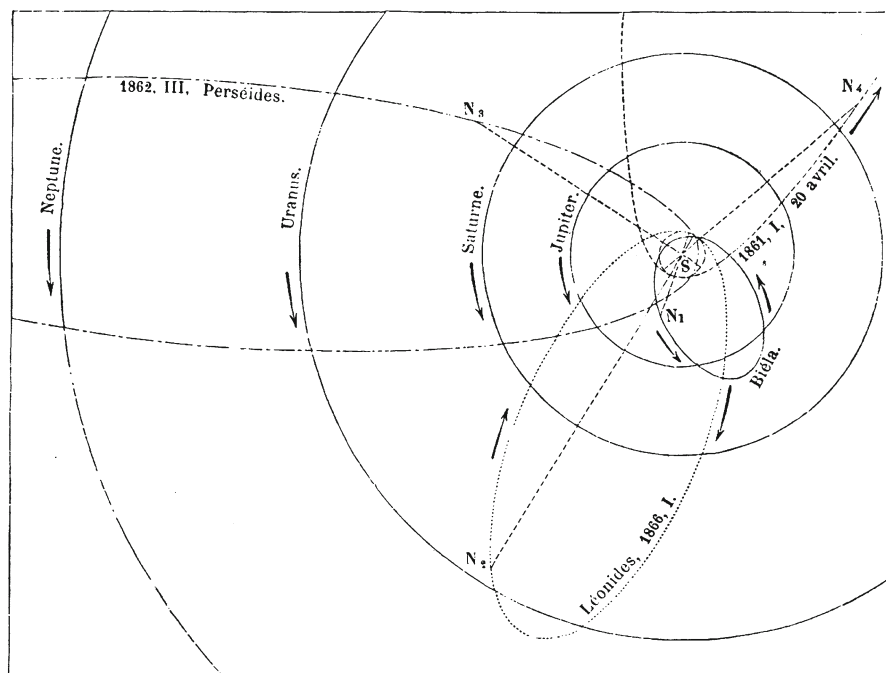


Fig. 5.2 Orbits of comets accompanied by shooting stars. These orbits cross that of the Earth (*small inner circle*). The periodic comets 1862 III and 1866 I are respectively associated with the Perseid meteor shower (in August) and the Leonid shower (in November). The names refer to the constellations from which the showers seem to emanate. Comet 1861 I and Biela's comet, which was observed breaking into fragments, produce similar phenomena, as was discovered somewhat later

had several simultaneous observations of meteors made from Paris and Orléans, which demonstrated that they were found at a very great altitude, sometimes more than 400 km above the Earth's surface¹⁸; but he was not the one who demonstrated their association with comets.

Sometimes Le Verrier took the places of other astronomers in giving presentations they had prepared¹⁹:

[The Geneva physicist Auguste] de la Rive was to perform at his Observatory an experiment reproducing the Northern lights, and was to be accompanied at the conference by Marié-Davy. As part of the general preparation, Le Verrier had Marié-Davy give his presentation.

¹⁸ See *Flammarion (1867–1880) *Études et lectures sur l'Astronomie*, t. 1, Paris, Gauthier-Villars, p. 132.

¹⁹ Wolf's testimony in 1888: see BOP, Documents divers sur l'Observatoire de Paris, 1854–1872, cote 3567(3), folder AP.

The next day Le Verrier said he was going to give it in his place. Delarive [sic] was very surprised, and refused to perform his experiment.

Dismissals and Resignations

The litany of atrocities allegedly committed by Le Verrier against the staff of the Observatory is virtually endless. One cannot, therefore, be surprised at the incredible number of people quitting or being forced out, which made the personnel, according to Flammarion, a nomadic tribe. Sixty-three astronomers and calculators quit the Observatory between 1854 and 1867 (Flammarion counted even more, claiming there were 104 up to 1870). It would be tedious to examine every case in detail, but the following is a partial list in the approximate order of their departure:²⁰

List of astronomers departed from the Paris Observatory from 1854 to 1867.

Six titular astronomers:

Faye, Desains, Liais, Simon, Puiseux, Chacornac;

11 ajunct astronomers:

Bess-Bergier, Barbier, Charrault, Lépissier, Leyssenne, Voigt, Serret, Lechartier, Vezy, Gernez, Babinet;

46 assistant-astronomers, calculators, and other assistants:

Butillon, Vinches, Bouchet, Garrit, Reboul, Gélén, Thuvien, Thirion, Delaire, Flammarion, Saffray, Harlant, Bourdette, Loiseau, Lartigur C., Oeltzen, Lartigur H., Dussolin, Durand, Barbelet, Boillot, Monin, Bulard, Tixier, Lafon, Lucas, Dumerthé, Leroy, Lecocq, Descroix, Boblin, Parault, Dien, Perrin, Hermitte, Thillay, Noël, Combres, Delépine (twice), Dubois, Rambosson, Massenot, Hirsch, Caniard, Maerleyn, Mme Neuville

Many notable scientists figure on the list. For instance, there was Victor-Alexandre Puiseux, a worthy mathematician who entered the Observatory in 1854, and whose son, Pierre-Henri, collaborated with Lœwy in taking magnificent photographs of the Moon. Another victim was Chacornac, an astronomer from Marseille, who was summoned to Paris by Le Verrier, also in 1854, after discovering several asteroids (see Box 5.1).

An excellent observer, Chacornac participated in tests of the 80 cm diameter reflector, and Le Verrier took great satisfaction in his observations.²¹ Chacornac was

²⁰ From Tobin (2003), p. 282. This list was prepared by the commission which examined the directorship of Le Verrier in 1867.

²¹ Chacornac saw with the reflecting telescope the companion of Sirius, just discovered in the USA: *CRAS 54 (1862) pp. 626–628; his observations of the Whirlpool nebula (M 51) are told by Le Verrier in *CRAS 54 (1862) pp. 888–889 and discussed by Tobin, W., Holberg, J.B.: *J. Astron. Hist. Heritage* 11, 107–115 (2008); finally, he saw the shadow of Titan on Saturn and sketched the Lyra planetary nebula (NGC 6720): *CRAS 54 (1862) p. 1012.

Box 5.1 Jean Chacornac (1823–1873)

Born in Lyon of a modest family, Chacornac entered business, and found himself in Marseille where he worked as a shop-assistant in a bazaar.²² He made the acquaintance of Valz, the director of the Marseille Observatory, who permitted him to make observations as an amateur. Shortly thereafter he discovered a comet, an event that would determine his choice of vocation. Valz entrusted to him the drawing up of maps of the ecliptic, to include all stars down to the tenth magnitude. While engaged in this work, Chacornac discovered two asteroids, Massalia and Phoebe. Le Verrier took note, and wanting to make changes in the personnel of the Paris Observatory, hired him as an adjunct-astronomer on 4 March 1854. Chacornac continued his work on the *Atlas éclipique*, and discovered six more asteroids. He also made remarkable drawings of celestial objects with the 80 cm diameter reflector. His rows with Le Verrier were perhaps partly due to his rather seamy personal life: “On visiting days, he introduced into his room a woman, and kept her there until the following visiting day. Even in his diary he wrote, in shorthand, of his rather immoral notions.” A deterioration in his health forced him to retire to Villeurbanne, where he built a refractor and a reflector and pursued observations of sunspots until his premature death.

one of the few astronomers authorized to use the equatorial telescopes “to studies more intrinsically interesting than meridian observations,” according to his friend Flammarion²³:

Chacornac sought to detect manifestations of solar activity by changes in sunspots, and was occupied also with the planets. We became fast friends. He always represented for me the personification of the accomplished student of the heavens. He drew up stellar equatorial charts,²⁴ and discovered several asteroids between Mars and Jupiter. His way of looking at astronomical matters differed fundamentally from that of the mathematicians. Le Verrier, of course, threw obstacles and hindrances in Chacornac’s path, so that the latter was obliged to resign from the Paris Observatory. He withdrew to Villeurbanne, near Lyon, where he continued his work, and we kept up a cordial correspondence.

²² Obituaries by Fraissinet in *+La Nature* (1873) pp. 358–360 and by Rayet in **La Revue scientifique de France et de l’étranger* (1873) 2^e série, 12, pp. 334–335.

²³ **Flammarion* (1911) pp. 157–158.

²⁴ These maps, covering a zone around the ecliptic not the equator, were used by observers searching for minor planets, as they allowed them to be distinguished from stars.

However:²⁵

Despite unbelievable obstacles, [...] the titular astronomers, adjunct astronomers and their assistants, brought to their work a determination equal to that which M. Le Verrier tried to oppose them, and were able to achieve useful results. The staff remained united in fighting against these provocations, which only showed the gratuitous malignity of M. Le Verrier's peculiar conduct.

At a reception in the year 1867, the astronomers appeared by themselves before the ministers, M. Le Verrier having neglected to put himself at their head. After a short conversation about the regrettable situation at the Observatory and the situation of the astronomers, Foucault ended by saying, "Monsieur Minister, there are some of us here, the titular astronomers, who can defend ourselves, if necessary; there are, however, many others who are not strong enough to do so."

Flammarion had entered the Observatory as a student-astronomer in 1858 at the age of 16, and a year later was promoted to calculator. He regarded himself as belonging in the first rank of Le Verrier's victims. He wrote in his memoirs²⁶:

The chief of service, M. Serret, made it known to me that I was to devote myself entirely to the calculations needed to reduce the observations, while as for making observations, that was a different job, one which I might arrive at later. I understood by these first conversations that M. Serret had never put his eye to an instrument. M. Puiseux, the chief astronomer of the service, had never undertaken researches with the aid of the refracting or reflecting telescopes; nor had M. Desains, another service chief for physical astronomy, although both held professorships at the Sorbonne. I also knew that the illustrious Director, M. Le Verrier, never observed. I soon made the acquaintance of several young astronomers in the observational service, notably Thirion, born in Langres, and I learned that this department had for its goal the determination of the precise moment in which stars passed the meridian, i.e., crossed the micrometer wire of a telescope permanently fixed in the meridian plane, and the determination of their distance from the pole.... When I hazarded to ask these observers about the constitution of the Moon, Venus, Mars, Jupiter, Saturn and the comets, and the stars and nebulae, their response showed me they had never even considered the matter.... But to me physical astronomy was the living astronomy, it represented what was most admirable in the science of the heavens – the study of the conditions of life in the universe. And this was entirely outside of the work of the Paris Observatory. I was thunderstruck! The writings of Arago, the former director of the Observatory, who had only died five years before, had, by contrast, given a completely different impression.... And before him, in the same observatory, the drawings of Mars by Maraldi, those of Saturn by Cassini, and the other varied researches which had made illustrious our grand national institution since it was founded by Louis XIV, gave a completely different notion of what constituted this most marvelous of the sciences.

Flammarion himself was laid off by Le Verrier in 1862. He was reproached for having occupied himself with other things than correcting the proofs of the

²⁵ *Mémoire sur l'état actuel de l'Observatoire impérial* (1870), pp. 12–13.

²⁶ *Flammarion (1911) pp. 154–156.

Annales de l'Observatoire, the task assigned to him. It's true that he was a dreamer who had just published, *La pluralité des mondes habités* (The plurality of inhabited worlds),²⁷ a popular account of a subject that was bound to incense Le Verrier. He tried to get himself reinstated by writing a pathetic letter, but in vain. He later wrote²⁸:

There was no pleading with the irascible autocrat. He simply said to me, with an authoritative tone that did not admit rejoinder, "I see, sir that you do not need to keep your place here. Nothing could be simpler: all you have to do is resign."... In obliging me to leave the Observatory or else endure endless tribulations, M. Le Verrier shattered my career as casually as one breaks a glass, without the least scruple. However, in leaving, I made an oath, just as the Romans, our ancestors, used to do. It was an oath of vengeance: 'He forced *me* to depart; *he* will depart.'

This oath I remained true to, as we will see in what follows. We will speak in due course of Le Verrier's revocation. It was spectacular. 'He who sows the wind harvests the whirlwind.'

Léon Foucault was another victim of Le Verrier, but to a certain extent, it was Foucault's own fault. In fact, his attendance at the Observatory was very irregular, since he liked to remain at home to work in peace. But this, inevitably, aroused Le Verrier's wrath. He did not hesitate to withhold Foucault's salary, even when he was perfecting, in part with his own modest means, the method of testing telescope mirrors that was to become one of the glories of the Observatory. Flammarion recalls in 1911 that Foucault was, because of this, "driven to the verge of insanity." Finally, after difficult negotiations in which the General Ildephonse Favé, a secretary of the Emperor, intervened, the Minister decided that Foucault's salary should be deposited directly by the minister of Finances, but only after he had filled out an attendance sheet at the Observatory.²⁹ This allowed him to save face. Foucault tried until the end of his life to achieve financial independence, so as to liberate himself from the Observatory. Alas, he did not succeed.

There were only a few, like Foucault, who both resisted Le Verrier and remained there for any length of time. There were two survivors from the Arago period, Yvon Villarceau and Charles-Louis Largeteau, who died in early 1857, and a few more Le Verrier had recruited, notably, Lœwy (Box 5.2), Wolf (Box 5.3), and Rayet (Box 5.4). It was Wolf and Rayet who made the discovery in 1867, using a very simple spectroscope

²⁷ *Flammarion, C.: *La pluralité des mondes habités*. Mallet-Bachelier, Paris (1862).

²⁸ *Flammarion (1911) pp. 210–211.

²⁹ From Tobin (2003), pp. 211–212. The minister would pay when necessary the salaries of other employees that were suppressed by Le Verrier: *Flammarion (1911) p. 515.

Box 5.2 Maurice Lœwy(1833–1907)

Maurice Lœwy (Fig. 5.3) was born in Vienna, where he began his astronomical career calculating the orbits of comets and asteroids.³⁰ These researches attracted the attentions of Le Verrier, who invited him to Paris, an invitation which, as a Jew, he was only too eager to accept, given the intensifying anti-Semitism in Austria at the time. He arrived at the Paris Observatory in 1860, and was appointed an adjunct-astronomer the following year. He continued his work in celestial mechanics, but also was attracted to other researches, and carried out numerous observations. He became a naturalized citizen of France in 1869, and served his new country during the war with Prussia in 1870. In 1872, he was appointed a member of the Bureau of longitudes. Admiral Mouchez entrusted to him a number of important duties at the Bureau, notably the improvement of instruments for astrometry and the supervision of the calculations and reductions for the *Connaissance des temps*. He was also heavily involved in the preparation of the *Annuaire du Bureau des longitudes*. All this activity outside the Observatory caused some difficulties with Le Verrier. Meanwhile, in 1871, Lœwy came up with the idea for a new observing device, the Coudé equatorial, which enjoyed a great success from 1877 onward and allowed him and Victor Puiseux to create a remarkable photographic atlas of the Moon. He succeeded Tisserand as director of the Paris Observatory in 1896, and was appreciated for his humane qualities. He died 15 October 1907 during a meeting of the Council of Observatories.

Fig. 5.3 Maurice Lœwy
(1833–1907)



³⁰Obituary in °*Bulletin Astronomique* 24 (1907) pp. 385–395.

Box 5.3 Charles Wolf (1827–1918)

After a brilliant career as a student at the École normale supérieure, Charles Wolf (Fig. 5.4) taught physics, first at Nîmes in 1851 and then at Metz, where in 1856 he completed a dissertation in physics.³¹ He was next appointed to the Faculty of sciences at Montpellier. Meanwhile, he had begun to develop a taste for astronomy. Le Verrier noticed him during his visit to the city, and appointed him titular astronomer in 1862. At the Paris Observatory, Wolf devoted himself to perfecting meridian observations and to the synchronization of clocks, which he carried out first in the Observatory and then all over the city of Paris. Other work included observing with the equatorial refractors, in the course of which he and Georges Rayet discovered in 1867 the class of stars with numerous broad emission bands that now bear their names. In 1875, he took over from Le Verrier his teaching duties at the Sorbonne. All the while he continued to carry out with great success a wide variety of researches including, in 1902, an invaluable work on the history of the Paris Observatory from its foundation to the time of the Revolution. He died at age 91, preserving his intellectual faculties to the very end.

Fig. 5.4 Charles Wolf
(1827–1918)



³¹ Obituary by Bigourdan in °*Bulletin Astronomique*, 2^e série 1 (1918) pp. 5–13, with a publication list, and in °*Mon. Not. R. Astron. Soc.* **79**, 235–236 (1919).

Box 5.4 Georges Rayet (1839–1906)

Like Wolf and Stephan, Georges Rayet (Fig. 5.5) had been a student at the École normale supérieure.³² After passing the competitive exam to be a teacher in the *lycées* (high schools) in 1862, he was appointed professor of physics at Orléans, then the next year entered the Paris Observatory as an adjunct to Marié-Davy in the recently formed meteorological service where he remained until 1874, applying himself as well to studies of terrestrial magnetism. He devoted himself also to astronomical observations, discovering with Wolf the stars with emission bands known as Wolf-Rayet stars. In his obituary notice in the *Astrophysical Journal*, the editor wrote: “Present-day observers of stellar spectra, who use photography and powerful instruments, have a hard time imagining how difficult it was to make the early visual spectroscopic observations of faint stars; these discoveries certainly merit greater credit than the discoveries with modern spectrographs.” He participated in the expedition to Siam in 1868 for the total eclipse of the Sun, where he made the best observations of the era of the brilliant coronal rays and solar prominences. Rayet left the Paris Observatory in 1874 for the Faculty of sciences of Marseille, then in 1876 for that of Bordeaux. He was named director of the Bordeaux Observatory at its creation in 1877, an assignment he carried out with great distinction until the time of his death.

Fig. 5.5 Georges Rayet
(1839–1906)



³²Obituaries in °*Bulletin Astronomique* 23 (1906) pp. 273–285 and (1908), p. 25, in *Astronomische Nachrichten* (1908) p. 4111 and in °*Astrophys. J.* **25**, 53–54 (1907).



Fig. 5.6 Principle of the simple, direct-vision spectroscope used by Wolf and Rayet. The light coming from the left is deviated by a crown prism *C*, a flint prism *F*, and another crown prism *C*, placed head to foot. There is no deviation for light of the mean wavelength, but the rest of the spectrum is dispersed

(Fig. 5.6) fitted to the 40 cm Foucault reflector, of the stars “whose spectra showed brilliant lines.” These stars, which have long been enigmatic, are known as Wolf-Rayet stars, and even today are the object of numerous studies.³³

“Why were there so many difficulties at the Observatory?” Perigaud asks,³⁴ “Why did [Le Verrier] torment his personnel? It was sheer spitefulness, a kind of madness [*espèce de folie*] in him.”

Le Verrier and the Bureau of Longitudes

We saw in the preceding chapter that Le Verrier, before becoming director of the Observatory, had wanted to free it from the oversight of the Bureau of longitudes.³⁵ He succeeded. It should not be surprising, therefore, that relations between the two institutions became execrable during the entire period of his reign. Split off from the Observatory, the Bureau, where Delaunay had a great deal of influence even though he was not yet a member, became a permanent hot-bed of revolt against Le Verrier. Here again, Flammarion’s account is the most lively and detailed³⁶:

[Delaunay] lived at rue Notre-Dame-des-Champs, 76. In the same building lived MM. Mathieu and Laugier. M. Mathieu was the brother-in-law of Arago, and owned the building; M. Laugier was his son-in-law. The Bureau of longitudes was located here as well. These savants were all implacable enemies of Le Verrier, and to such a degree that when one of the titular astronomers of the Bureau, Largeteau, died in 1857, the Bureau did proceed to replace him, so as to avoid naming Le Verrier, next in line for the post having been an adjunct-member since 1846.

We will have more to say about this particular affair later. For now let us continue with Flammarion’s account.

Delaunay, who knew me as a student at the Sorbonne, presented me [after my dismissal from the Observatory] to M. Mathieu, acting president of the Bureau (marshal Vaillant being the nominal president). I was hired as a calculator at the Bureau, and charged with

³³ *CRAS 65 (1867) pp. 292–296

³⁴ Interview by Bigourdan in 1888: BOP, Documents divers sur l’Observatoire de Paris, 1854–1872, cote 3567(3), folder AP.

³⁵ For more details, see Bigourdan (1931) pp. A.89–A.117 et (1932) pp. A.1–A.35.

³⁶ *Flammarion (1911) pp. 212–213.

calculating the positions of the Moon for the *Connaissance du Temps* [sic], at the same salary of 200 francs per month as at the Observatory. This job involved converting the positions of the Moon given in longitudes and latitudes into right ascensions and declinations. It was simple matter of trigonometry. The pages of the *Connaissance des Temps* for the two years 1866 and 1867 were calculated by me during 1862 and 1863.³⁷

I was thus extricated from the abyss into which Le Verrier had hurled me.... Since this time, the Bureau of longitudes has taken up a place (from 1873) in a stark building occupying the rear of the courtyard of the Institut [de France].³⁸

Accordingly, the Bureau hunkered down to the task of preparing ephemerides for sailors and astronomers, which were published 3 years in advance in the *Connaissance des temps*, together with excerpts intended for a wider audience in the *Annuaire*, which appeared only a year in advance. This was a grinding, routine labor, which involved simply the computation of the coordinates of planets and satellites for the different dates based on the formulas of celestial mechanics, computation of the times of rising and setting of the Sun and Moon, and predictions of the eclipses of the satellites of Jupiter. These publications didn't cost the Bureau anything because the costs of the publication were borne by the editor, and the Bureau's budget was limited to the allowance granted to its members, the salary of those performing the computations, and a few additional expenses. Even so, Le Verrier couldn't abide even this much. He denounced the waste that, according to him, this budget represented. No doubt because of his criticisms, the Bureau had great difficulty in obtaining from the Ministry additional funds to engage more computers in order to improve the ephemerides and to reduce the sale price of the *Connaissance des temps* by providing a subsidy to the editor. Over Le Verrier's opposition, these funds were finally granted in 1859 and 1860.

It was at that very moment that Le Verrier chose to come out against the Bureau before the Academy of sciences. We read the following note for 6 February 1860 in the *Comptes rendus*³⁹:

M. Le Verrier has called the attention of the Academy to deficiencies and a lack of exactness in the *Connaissance des Temps* and the *Annuaire* of the Bureau of longitudes. He cites examples. The *Connaissance des Temps* is no longer of any use to astronomers. A thorough inquest, which would address why it falls so far short of the foreign ephemerides, is urgently required.⁴⁰

Mathieu responded to these accusations at the following meeting of the Academy. As far as the *Annuaire* was concerned, it was only a matter of printing errors. For the *Connaissance des temps*, Le Verrier's charges were more serious. Here are extracts from Mathieu's response:

Already in 1856 M. Le Verrier had written in the *Compte rendus*: 'the *Connaissance des Temps* has not been a scientific work for a long time.' This accusation has gone without a

³⁷ In truth Flammarion was so little suited to this drudge work, that he was asked to quit the Bureau after 2 years. He already must have been spending a great deal of his time writing his popular works. He would live thereafter by his fertile pen, and was to be by the end of the century the most popular astronomer in France, perhaps even in the world.

³⁸ Its offices are still there, but most of the personnel presently works at the Paris Observatory, in the *Institut de Mécanique Céleste et de Calcul des Éphémérides* (IMCCE).

³⁹ *CRAS 50 (1860) p. 273; Mathieu's reply is pp. 348–351.

⁴⁰ One finds in Le Verrier's papers for this epoch of the proposal of a decree which would have removed completely from the Bureau des longitudes the redaction of the *Connaissance des temps*.

response from us, and so have the many others that M. Le Verrier incessantly throws at us. We kept our peace, thinking the field of battle poorly chosen for a debate in which the science is only the pretext. But, since M. Le Verrier, who belongs to the Bureau himself, has seen fit for a second time to bring his attacks before the Academy, instead of addressing his complaints directly to the Bureau, permit me to respond this time, and in detail, to this Assembly. It is necessary that the scientific world should know what M. Le Verrier knows perfectly well: that the Bureau of longitudes up to now has done all that lies in its power for the *Connaissance des Temps*, given the meager resources at its disposal. ... The Minister of Public Instruction, understanding well the necessity of coming to the aid of the Bureau to ensure its fulfillment of an important public service, has granted to it in 1859 a sum of 8,000 francs. As a result it has been possible to provide in the *Connaissance des Temps* the lunar positions from hour to hour instead of at 12 hours intervals.⁴¹ The same funding has been provided to the Bureau this year.... The Bureau has therefore entered upon a course of improvements. M. Le Verrier knows these circumstances, but wanting to give rise to a scandal, has chosen precisely this moment to attack the Bureau of longitudes, of which, I repeat, he himself is a member.

I had the good fortune in my youth to work in the Bureau side by side with the most outstanding savants of our time: Lagrange, Laplace, Legendre, Delambre.... These illustrious men joined their efforts in the single-minded pursuit of advancing science. But I have never before seen anything like what I am seeing today.

Liouville, and then Delaunay, soon threw themselves into the controversy. Liouville and Delaunay on one side, and Le Verrier on the other, engaged in a flurry of mutual recriminations, each faulting the other in a series of articles. The Academy attempted to calm the storm, but without success. The most damning accusation came from Le Verrier, who asserted that Delaunay had made serious errors in his *Théorie de la Lune*: he had predicted a certain value for a secular inequality in the movement of the Moon although the observations gave a different value. Eventually Delaunay was vindicated: the disagreement was real, but the reason for it came to light much later. These disputes dragged on in the *Comptes rendus* until the end of 1860. And of course they did lasting damage.

Around this time, Delaunay developed closer relations with the imperial court, and enjoyed to a certain degree the favors of the Emperor. On 3 February 1861, he presented to the Emperor the first volume of his theory of the motions of the Moon. Napoléon III took advantage of their meeting to question Delaunay⁴²:

Aren't you a member of the Bureau of longitudes? Well, then, I would like to put to you a question on this subject. There seem to be two opinions: some tell me it is necessary to reunite the Bureau with the Observatory, others maintain that the two institutions should remain separate and that vacant positions in the Bureau should be filled. What do you think about this?

Delaunay replied that he was in favor of keeping them separate. But it weighed on his mind to have to replace the vacant places in the Bureau, since Le Verrier, having been an adjunct-member since 1846, was entitled to promotion to titular member. So the Bureau procrastinated, and held up all nominations just to avoid promoting Le Verrier. In 1861, the Minister enjoined the Bureau to draw up two lists of candidates to replace Largeteau (who died in 1857), Poinot (died in 1859), and

⁴¹ The position of the Moon with respect to neighboring stars was one of the ways used by sailors to determine time in case of failure of their chronometers, in order to obtain longitude.

⁴² Cited by Bigourdan (1931) p. A.116. Mathieu, Laugier and his wife Lucie encouraged, in spite of their republican opinions, Delaunay to keep seeing the Emperor, in order to oppose Le Verrier.

Pierre Daussy (died in 1860) as titular members. The marshal Vaillant, who almost never came to the Bureau, was called on to help with the presentation of the candidates. But, in face of the difficulties the members had in proceeding to a vote, he had to demand precise instructions from the Minister, who replied with an extremely detailed letter. Finally the Bureau decided to propose a list of names among which Le Verrier did not figure. "So, it's very clear that you reject M. Le Verrier," said the marshal. After a stormy session behind closed doors, the Academy ratified the list, and added that of Le Verrier for the position freed up by the death of Poincot. But he was not elected! Weary of battle, the Minister put out on the 26 March 1862 a decree that increased from 9 to 13 the number of titular members of the Bureau of longitudes. This was the exact number of candidates, and so all were appointed according to this decree. Le Verrier was thus promoted to titular member, and so was Delaunay, who was also a candidate. After these appointments, the following were the members of the Bureau:

- 3 members of the Academy of sciences: Liouville, Le Verrier, Delaunay;
- 5 astronomers: Mathieu, Laugier, Yvon Villarceau, Faye, Foucault;
- 3 members of the department of the Marine: Deloffre, Admiral Mathieu, N.;
- 1 member of the War department: Marshall Vaillant;
- 1 geographer: Peytié;
- 1 "artist" with the titular rank: Breguet;
- 2 "artists": Lerebours, Brunner.

That was the end of this particular episode. But it was only the beginning of the war.

Taking new vigor from other articles of the decree of 1862, the Bureau of longitudes undertook initiatives which clearly encroached on the prerogatives that Le Verrier had arrogantly claimed. For instance, the following month "M. Delaunay [proposed] to the Bureau to take up a plan of operations regarding the physics of the Earth, which could be carried out beginning in spring of the next year. These operations would include geodesic and astronomical measurements of a number of locations conveniently chosen in French territory, observations of the pendulums [for the determination of gravity], of magnetized needles, etc. Arrangements could also be made with foreign scientists to carry out joint studies of this nature." These proposals were spelled out and adopted unanimously by all present on 5 November 1862, Le Verrier being absent as he usually was from meetings of the Bureau. However, Le Verrier had already begun a part of this work, aided by Yvon Villarceau; therefore, a new dispute arose, short but vigorous, of which the prime participants were Le Verrier on one side and Delaunay and Faye on the other. Finally, the Bureau, lacking the means to implement them, renounced all the astronomical parts of these projects, and Le Verrier enjoyed an open field. However, the Bureau did not entirely give up its ambitions in these directions, and in 1866 embarked upon a vast geodesic project referred to as the *fundamental meridians*. (We shall say more of this in Chap. 8).

Among the complaints formulated by the Bureau against Le Verrier was the problem of a meeting room and access to the library for Bureau members. The place rented from Mathieu on the rue Notre-Dame-des-champs was too small to accommodate

meetings, and the Bureau did not have any options other than a room at the Observatory that was scarcely heated and poorly maintained (a condition not due to chance!); it was even without a locked cabinet for safekeeping the documents, which therefore the secretary had to keep in his home. Then there was the question of the use of the library, which had always been shared between the Observatory and the Bureau but was now off limits to the Bureau's members. After several years of requests, the Bureau asked its president, marshal Vaillant, to negotiate with Le Verrier to obtain access to the library or to allow its division, but the marshal would soon give up this thankless mission. The Paris Observatory preserves an abundant correspondence on this minor problem. It went on for a long time without resolution.

The Organization and Work of the Observatory

A Rigid Hierarchy

The Observatory, as we have seen, had a hierarchical and compartmentalized organization. At the top was the director, overseeing the activities of four services, each of which had its own chief or leader:

- The service for meridian observations: Yvon Villarceau, then Lœwy;
- The calculating service: Serret then Gaillot;
- The service of equatorials and reflectors: Chacornac then Wolf;
- The meteorological service (also called the service of the physics of the globe, or the physical service): Liais, then Desains after the dismissal and departure of Liais for Brazil in 1858, and then finally Marié-Davy after Desain's resignation in November 1861. The observers in the service for meridian observations initially made the meteorological observations. Terrestrial magnetism also fell within the province this service,⁴³ which grew increasingly important. Marié-Davy, suffering continual harassment from Le Verrier, was relieved temporarily of his functions in December 1864 and permanently in December 1865. It needed on his part a great deal of perseverance to carry out his work.

Finally, there was the “physicist of the Observatory,” who was Léon Foucault until the time of his death in 1868.

The duties of each service were strictly spelled out, even as regards the “research personnel” who were allowed to use the equatorials and the reflectors. Needless to say, this did nothing to encourage innovation. Let us now look at what each service was responsible for.

⁴³ Rayet, G.: Recherches sur les observations magnétiques à l'Observatoire de Paris de 1667 à 1872, °*Ann. OP, Mémoires* **13**, A*1–A*40 (1876).

The Meridian Observations and Their Reduction

The observations of the times of passage of objects across the meridian, with the meridian refractor, and simultaneously of their angular distance from the zenith with the meridian circle, constituted at this epoch the principal activity at most observatories.

For stars whose positions were already well known, their times of meridian passage allow the determination of the local sidereal time. The observatory clocks are set accordingly (Fig. 5.7). Then measurement of the meridian passage of a star or planet (either one of the major planets, such as Venus, Mars, Jupiter, Saturn, or a minor planet) allows the determination of one coordinate: the right ascension.

Measurement of the angular distance from the zenith of reference stars (stars whose positions are well known) permits the latitude of the observing station to be determined (Fig. 5.8). The same observation made on any star or planet allows the determination of the other coordinate, the object's declination, given the latitude. Of course, all these operations require extensive knowledge of the instruments being used, including knowledge of how the reticule is oriented relative to the local meridian and of the errors in the graduations of the vertical circle used to measure the zenithal distance, etc. It would be tedious to explain how all these corrections are determined. Suffice it to say, they occupied a large part of the activities of the astronomers in this service, and were part of the price to be paid for valid measurements.

During the directorship of Le Verrier, some improvements were made in techniques for observations of the meridian passage. The most important was adoption of a system of registering the times of the passages on a strip of paper.

The observations of this service were, as we have said, of a routine character, and included the stars, the planets, and the Sun and Moon. Despite the challenges due to the continual turnover in personnel, Le Verrier succeeded in assuring reasonable continuity in securing these measurements. It helped that an arrangement had been entered into with the Greenwich Observatory. The planets were observed between Full and New Moon at Paris and between New Moon and Full Moon at Greenwich. Also, a truly monumental work was begun: the remeasurement of the 50,000 stars in Lalande's catalog. This project would not, however, be completed during Le Verrier's lifetime.

The service of the meridian observations was also responsible for the determination and dissemination of the time. Observations of the passage of the Sun across the meridian gives the true solar time from which, after corrections, the mean solar time is derived, which was distributed in turn to the clocks in Paris and several other cities.

Finally, this service was responsible for testing the theodolites and portable meridian refractors used to determine latitudes and longitudes at different locations in France, and for making these measurements (the Bureau of longitudes would have liked to carry out these activities, but ultimately gave up all claims to them). There was actually very little of this being done. It was as nothing compared to what had been done up to 1850 during the construction of the map of France at a scale of 1/80,000. Renewed interest in the measure of longitude, which will be described in Chap. 8, came about when telegraphic signals began to be used to synchronize the clocks. This was an enormous step forward.

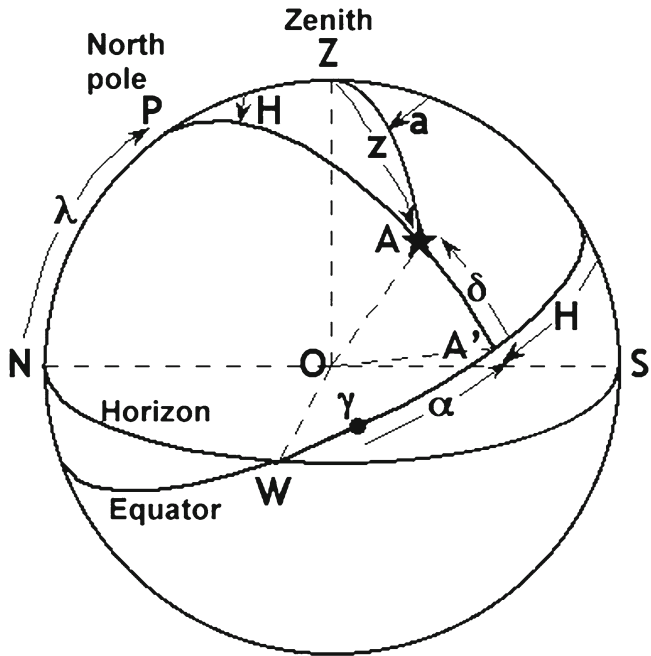


Fig. 5.7 The celestial sphere. The sky is represented by a sphere centered on the observer in O . The horizon is represented with three cardinal points N , W and S , and also the equator and its North pole P . The diurnal motion is such that the celestial objects rotate around the Earth's axis OP from East to West. The position of a star A can be defined with two angles: either the azimuth a and the zenith distance z , or in equatorial coordinates by the hour angle H and the declination δ . α is the right ascension, measured from the γ point, which is the intersection of the equator with the ecliptic (not represented), which is the annual apparent trajectory of the Sun. The origin of the sidereal time is the meridian passage of the γ point

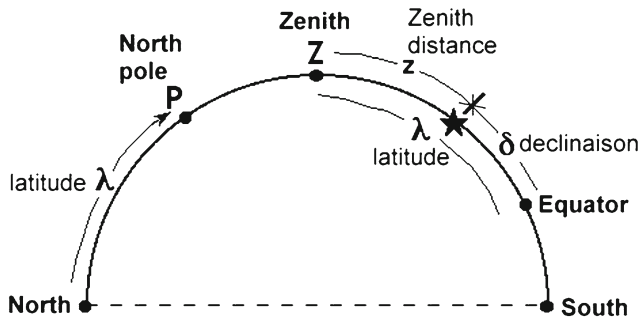


Fig. 5.8 Relation between the zenith distance z of a star at its meridian passage, its declination δ and latitude λ . The figure is drawn in the plane of the local meridian. The latitude l is the angular height above the horizon of the pole P of the Earth's rotation axis

Contrary to what had been done in Arago's time, the meridian observations were reduced as soon as they were made and were published by the service of calculations: they filled huge volumes of the *Annales de l'Observatoire impérial de Paris*. In all the activities described so far, Le Verrier's influence can be regarded as very beneficial.

The Observations with Refractors and Reflectors

It might have been expected that Le Verrier would not have regimented the observations with these instruments, which though without the same immediate application as the routine ones made with the meridian instruments were potentially much more versatile. But this was not the case. In a long report of December 1869, Le Verrier defined the class of observations to be made with these instruments as follows⁴⁴:

The equatorial service of the large building and of the reflectors comprehends regular observations and research personnel.

Regular observations are made with the large equatorial of the West tower, and if possible with the Gambey equatorial.⁴⁵ These include:

Observations of comets, of [minor] planets newly discovered and of those whose ephemerides are too poorly determined to allow direct observation with the large meridian circle.... [Here it was a matter of measuring the position of these objects with respect to neighboring stars, with the help of a micrometer.]

The construction of maps of the ecliptic zone... [This work, started by Charcornac, was not quite half completed at the time of his departure in 1869].

Personal researches included: The spectroscopic and photographic studies which have been underway for the past several years.

The investigation of one or several suitable double stars, and the determination of stellar parallaxes.

As with the routine services, initiative was snuffed out. Fortunately, the astronomers were able sometimes to discretely carry out various observations that were not strictly regulated. No doubt in this way Wolf and Rayet made their discovery of emission-band stars to which their names have been given. But the report of the service chief, who was none other than Wolf himself, inserted in the overall report of Le Verrier, would cause a modern astronomer cringe. He discussed the considerable efforts devoted to the construction of ecliptic maps, which made possible the introduction of new observational techniques using electrical apparatus and allowed more accurate measurements of stellar positions. However, nothing much had been done in this direction since the departure of Chacornac, who had constructed 36 charts out of an anticipated 80. Wolf added one more, Stephan at Marseille another.⁴⁶ The brothers Paul and Prosper Henry would complete 14 others after April 1872.⁴⁷ There was

⁴⁴ *Observatoire de Paris. Rapport fait en décembre 1869*, par M. Le Verrier, Paris, Gauthier-Villars.

⁴⁵ The 38 cm equatorial of the East tower was then abandoned because of the deficiencies in its objective.

⁴⁶ Mouchez, E: *Rapport annuel sur l'État de l'Observatoire de Paris pour 1879*, Gauthier-Villars, Paris, p. 10 (1880) .

⁴⁷ Mouchez (see preceding note) has therefore forgotten the maps obtained by the Henry brothers. 54 maps were eventually obtained, 36 were left unfinished. We thank William Tobin for this information.

also the observation of Mercury's transit across the Sun on 5 November 1868, which was only of interest for celestial mechanics. Wolf was also charged with synchronizing the clocks within the Observatory, by sending electric signals from a master clock, though he only succeed in this in June 1870, with the assistance of the clock-maker Winnerl. All of this took a great deal of his time. Therefore, we read:

Spectroscopy occupied us very little. The Winnecke comet of 1869 was too faint for its light to give a visible spectrum. A note from [Father Angelo] Secchi imposed on us the obligation of reviewing the three small stars discovered by us in 1867: the old observations showing the bright lines in their spectrum were confirmed. M. Rayet continued to carry out with the grand equatorial his excellent researches on the [solar] prominences.

All in all this is a paltry record of accomplishment at a time when Father Secchi (Fig. 5.9) was in the process of examining, at the small observatory of the Collegio Romano, the spectra of more than 4,000 stars and laying, with a few others, the foundations of stellar classification⁴⁸ (1863–1868; Figs. 5.10 and 5.11). Secchi made use of several spectroscopes constructed by Jules Janssen, of which one was a spectroscope with direct vision⁴⁹ (Fig. 5.12).

Already in 1860, the Germans Gustav Kirchhoff and Robert Wilhelm Bunsen had used spectroscopes to identify in the Sun several chemical elements that were also found on Earth.⁵⁰ In 1864, William Huggins (Fig. 5.13) in England obtained excellent spectra of several stars with a spectroscope of his own construction (Fig. 5.14), and identified the spectral lines of numerous elements.⁵¹ He discovered in 1864 and 1865, independently of Secchi, the presence of emission lines emitted by a gas in numerous planetary nebulae, as well as in the Great Nebula of Orion, answering in this way the question posed by the great William Herschel: do nebulae exist that are formed not of numerous stars but of a “brilliant fluid,” that is to say, a gas? He obtained also, just as Secchi did, fine spectra of comets. Early in the 1870s, he obtained the first photographic plates of stellar spectra,⁵² a feat that was accomplished at the same time as Henry Draper in the United States. In 1868, Huggins achieved another landmark result: he measured for the first time the radial velocity of a star, Sirius, using the shift of the spectral lines through the Doppler-Fizeau effect. Despite its various technical competencies, France was embarrassingly backward in these astrophysical researches: the impetus given by Arago had come to a sudden halt with his death and with Le Verrier's arrival, and it would be difficult to catch up.

⁴⁸ See *Secchi (1879) *Les étoiles*, Paris, G. Baillière, t. 1; his plates VII et VIII are reproduced here as Figs. 5.10 et 5.11.

⁴⁹ Secchi invited Janssen, who was visiting Italy, to install his spectroscopes at the focus of the 24-cm aperture refractor of his observatory. They observed together the spectra of many stars, but eventually Secchi alone published the results, but in a German journal, because he did not dare to publish them in Italy or in France. Janssen was furious to see himself dispossessed in this way: see Launay (2008) pp. 35–41.

⁵⁰ **Annalen der Physik und der Chemie* 110 (1860) p. 161.

⁵¹ Huggins's founding papers are in *Phil. Trans.* **154**, 413–435 et 437–444 (1864); **156**, 381–397 (1866), and in **Proc. R. Soc.* **14**, 39–42 (1865).

⁵² See °*The Observatory* (1877) 1, pp. 4–8 and **Phil. Trans.* **171**, 669–690 (1880) and plate 33.

Fig. 5.9 Angelo Secchi, S.J.
(1818–1878)

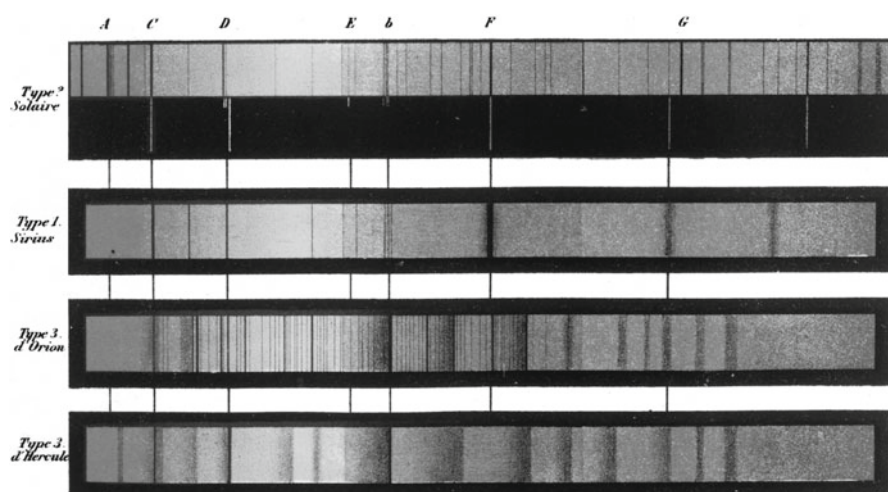


Fig. 5.10 Four typical stellar spectra drawn by Father Angelo Secchi from his visual observations. The wavelength increases from left to right. The principal spectral lines are given letters. Some were identified with lines of several elements observed in the laboratory. The two spectra at the bottom are from cold stars and show molecular bands. From top to bottom: solar-type star (spectral type G1); Sirius (type A1); Betelgeuse (type M1); α Herculis (type M5)

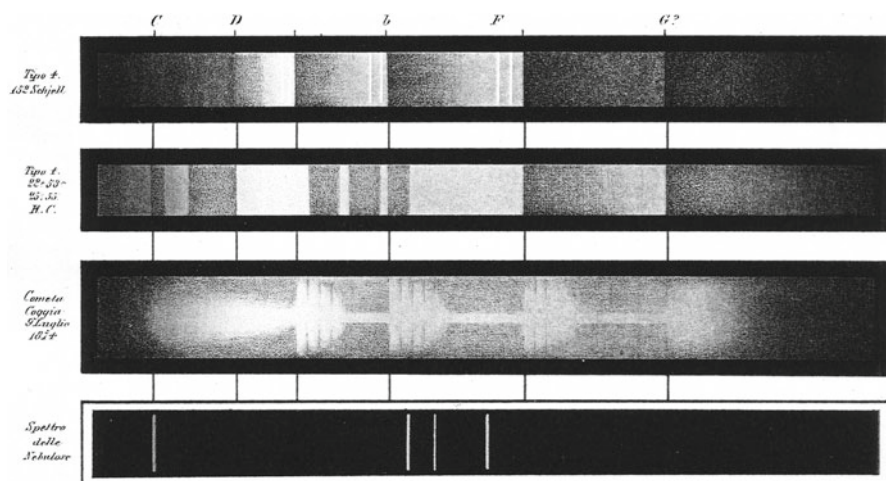


Fig. 5.11 Spectra of various objects obtained by Secchi. From *top to bottom*, two cold stars, a comet with its characteristic molecular bands, and an unspecified nebula, probably a planetary nebula

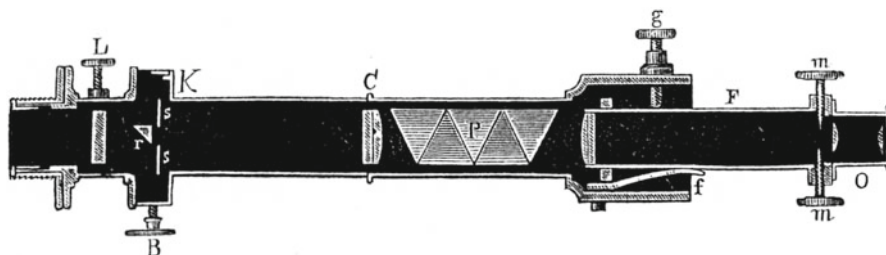


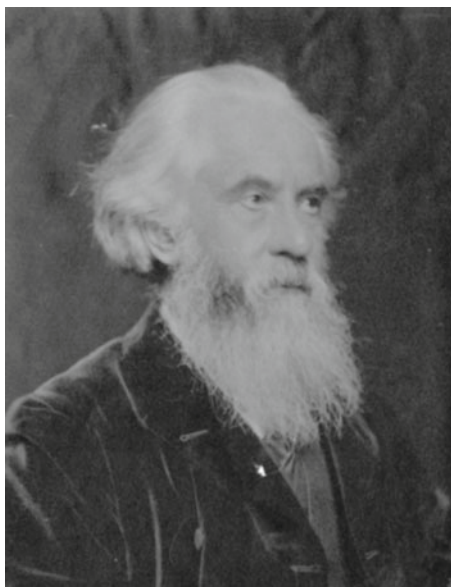
Fig. 5.12 The “pocket” direct-vision spectroscope of Janssen used by Secchi (**CRAS* 55 (1862) pp. 576–578.). The dispersive elements are a series of prisms *P*, alternatively of crown and flint, whose total deviation is null for a mean wavelength (see also Fig. 5.6). The light comes from the left. The eyepiece *O* to the right contains a micrometer mm. A small total-reflection prism *r* allows the light from a calibration lamp to enter the spectroscope

The Speed of Light

Nonetheless, there was one area in which the Observatory excelled, thanks to Foucault and later to Alfred Cornu: the measurement of the speed of light.⁵³ It may seem surprising to find the Observatory engaged in such a measurement, which might be thought to be more rightfully the province of physicists. The reason for

⁵³ For the subject of this section, see *Le Verrier U.J.J., 1853. *Mesure de la vitesse de la lumière dans le ciel et à la surface de la terre. – Conséquences pour le système du monde, L'année scientifique et industrielle*, 7^e année, pp. 37–54; Tobin, W.: *Vistas Astron.* **36**, 253–294 (1993); Tobin, Chap. 13 (2003); Lequeux Chap. 4 (2008). The methods used by Foucault, Fizeau et Cornu are described in detail in the three last references.

Fig. 5.13 William Huggins
(1824–1910)



this was that the numerical value of the speed of light was of interest above all to Le Verrier himself. One of the results of his immense personal work on the mechanics of the Solar System was the establishment of a relationship between the masses of the planets and the dimensions of their orbits. The orbital dimensions were well known as relative values, but their absolute values were poorly known. If one could arrive at a good value for the semi-major axis of one of these orbits, say the Earth's, one would then know all the others. Normally one expresses the semi-major axis of the Earth's orbit in terms of the solar parallax, defined as the angle that would be intercepted by the Earth's radius as viewed from the Sun, i.e., from a distance equal to the semi-major axis. The true value of this quantity is $8''.79415$. The best determination at the time of Le Verrier, deduced from an exhaustive analysis of the eighteenth century transits of Venus in front of the Sun by Encke, dated from 1824: $8''.571$, a value known even at the time to be not very precise.

However, the mass of one of the planets, the Earth, was very well known, thanks to measurements of its gravity with the pendulum of Borda or his successors.⁵⁴ By studying the perturbations of the planets on each other, Le Verrier was able to determine the masses of the different planets relative to the Earth, from which he could indirectly deduce the absolute values of the semi-major axes for each orbit. He obtained in this way a novel determination of the solar parallax: $8''.95$. Table 5.1 compares the values he published in 1858 with those which were generally accepted at the time and also modern values.

⁵⁴ More precisely, what is derived from the measurements of gravity and of the radius of the Earth is the product GM_E of the mass M_E of the Earth by the constant of gravitation G . But these are similar products which intervene everywhere in Le Verrier's calculations.

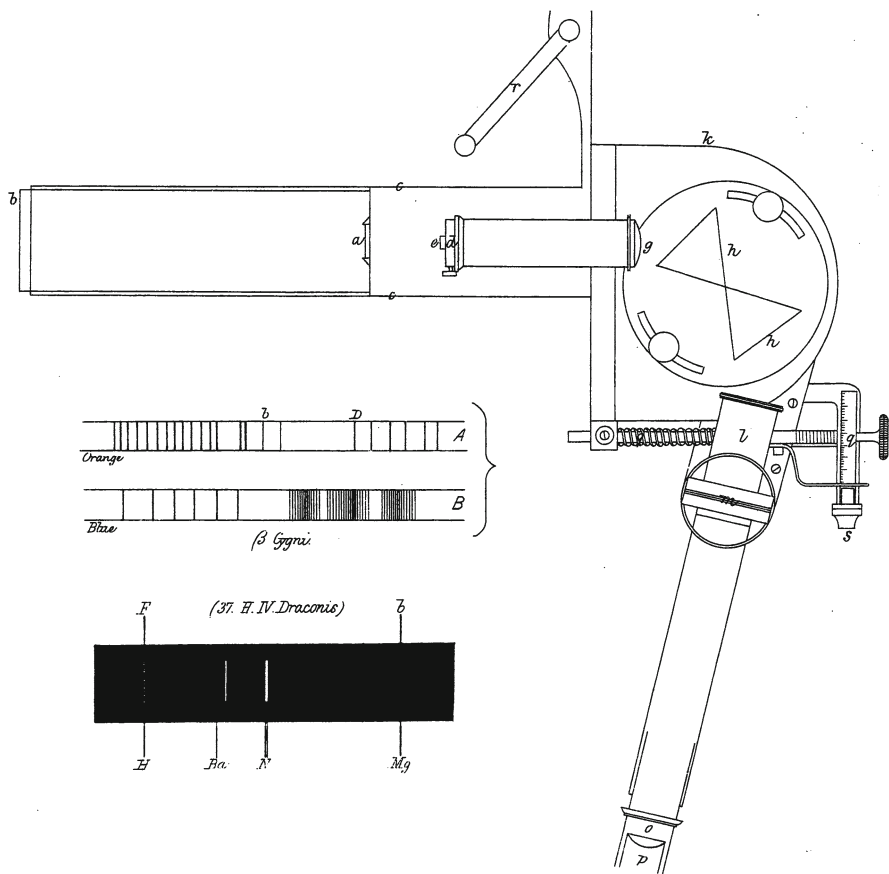


Fig. 5.14 Huggins's spectroscope. The entrance slit is at *d*, and a small prism *e* is placed in front of one half of the slit to send light from a comparison source, not represented; *g* is an achromatic lens, *hh* are two prisms in series and *l* is the telescope with its eyepiece *o* used to see the spectrum. *Fig. 4* shows two portions of the spectrum of the star β Cygni (spectral type K3II) and *Fig. 5* the spectrum of the planetary nebula NGC 6543 in the constellation Draco. One sees the emission lines that Huggins attempted in vain to identify: only that with a hydrogen line on the left is correct

Table 5.1 Masses of planets and solar parallax

Quantité	Accepted value in 1850	Le Verrier's prediction ^a	Modern value
$M_{\text{Venus}}/M_{\text{Sun}}$	$2.49 \cdot 10^{-6}$	$2.50 \cdot 10^{-6}$	$2.45 \cdot 10^{-6}$
$M_{\text{Terre}}/M_{\text{Sun}}$	$2.82 \cdot 10^{-6}$	$3.18 \cdot 10^{-6}$	$3.00 \cdot 10^{-6}$
$M_{\text{Mars}}/M_{\text{Sun}}$	$0.373 \cdot 10^{-6}$	$0.334 \cdot 10^{-6}$	$0.323 \cdot 10^{-6}$
Solar parallax	8".57	8".95	8".79

^a*Ann. OP, Mémoires 4* (1858) pp. 1–101

Since the value for solar parallax predicted by Le Verrier was quite a bit greater than that generally accepted at the time, he looked for a way to verify it. He thought that if one could measure precisely the speed of light in vacuum (or derive it from a measurement in air) one could, by combining this with the relative time it takes light to traverse the Earth's orbit, a value well known, obtain dimensions of this orbit, and

hence work out the solar parallax. Fizeau indeed, in 1849, had measured the speed of light between Suresnes and Montmartre, but he himself considered this measurement, the first of its kind, to be lacking in precision. Le Verrier therefore asked Foucault to measure the speed of light with greater exactness.

Foucault, therefore, went to work modifying an experiment he had already carried out in 1850, which involved a rapidly spinning mirror. He had Gustave Froment construct a new rotating mirror and a sort of timer whose purpose was to measure the speed of rotation of the mirror. At the same time, the celebrated organ-maker Aristide Cavaillé-Coll constructed a very steady bellows whose purpose was to propel the small compressed air turbine that spun the mirror.⁵⁵ Foucault set this up in the great meridian room of the Observatory, and had everything ready by the spring of 1861. But he needed more than a year to obtain his result, partly because he was kept very busy making the 80 cm diameter mirror for the Marseille Observatory, partly because the experiment was very difficult if the results were to be precise. Box 5.5 explains the principle of the measurement.

Measurements began in May 1862. After encountering some difficulties, Foucault modified the apparatus. Then it became necessary to wait for favorable weather, since the light used came from the Sun and was directed into the apparatus by a heliostat. Finally, on 22 September, Le Verrier presented to the Academy of sciences a brief note from Foucault announcing his result⁵⁶:

The speed of light is noticeably diminished... According to the value generally accepted, this speed would be 308 million meters per second, while the new experiment with the turning mirror gives, in round numbers, 298 million.

Box 5.5 Measuring the Speed of Light with Foucault's Rotating Mirror

Light from the Sun, sent into the apparatus by a heliostat, illuminates a finely graduated micrometer on glass, before arriving at a rotating mirror (Fig. 5.15). The latter sends the light to a fixed mirror that reflects it back to the rotating mirror. During the time it takes the light to make this round trip – the distance was 40 m in the 1862 experiment – the mirror will have rotated slightly, so that the image of the light on the micrometer is slightly displaced relative to the image formed when the mirror is immobile or turning slowly. Foucault measured the rotational speed of the mirror (400 revolutions/s) by comparing it stroboscopically to that of a toothed wheel with 400 teeth, geared to a clock that made exactly one revolution per second. From the rotational speed of the mirror and the displacement of the micrometer image, he deduced the speed of light from the length of its path.

⁵⁵ The original rotating mirror and “clock” are preserved in the Paris Observatory. Several replicas are in existence, in particular in the Musée des arts et métiers/CNAM where the presumably original bellows by Cavaillé-Coll can also be seen.

⁵⁶ *CRAS 55 (1862) pp. 501–503.

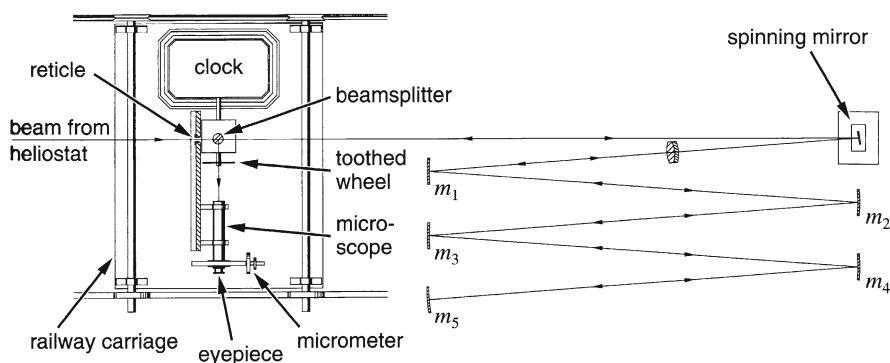


Fig. 5.15 Principle of the measurement of the speed of light by Foucault. Explanations in Box 5.5

One can, it seems to me, count on the exactness of this number. I believe that the corrections it might yet need to undergo cannot be more than 500,000 meters per second.⁵⁷

If one accepts this new figure ... and deduces from it the solar parallax ... one finds, instead of 8".57 [the value of Encke], a notably larger value, 8".86. Accordingly, the mean distance from the Earth to the Sun is diminished by about 1/30.

Le Verrier was jubilant: his predictions were borne out, for the new value of the solar parallax was closer to his than the old one had been. He had, all along, been continually urging Foucault to decrease the value of the speed of light. According to Moigno, he declared⁵⁸: "M. Foucault brought me each day his numbers, and I encouraged him to persevere until he arrived at a correct figure," that is to say, a number that was more to Le Verrier's liking. Not that there has ever been a question concerning Foucault's honesty in his measurements. Still, it was lucky for him that he arrived at a number pleasing to his superior."

Ten years later Alfred Cornu (Fig. 5.16), professor of physics at the École polytechnique but a regular at the Observatory, carried out new measurements, funded by the Observatory as had been those of Foucault. Cornu was a student of Fizeau, who had been the first to measure the speed of light with his dentate wheel and who had been on bad terms with Foucault. Cornu employed the method imagined by his master, a technique which he evidently regarded as superior to that of the rotating mirror, and which he improved. In 1872, a trial between the École polytechnique and Mont Valérien, 10 km away, gave him a value of 298,500 km/s, which was close to that published by Foucault and led him to retract his initially unfavorable judgment of Foucault's measurement. Next, in 1874, he made a second measurement between the Observatory and the tower of Montlhéry (Figs. 5.17 and 5.18), having first remeasured the distance separating these two sites by a new triangulation. In order

⁵⁷ This estimate of the precision of the measurement, one of the first in the history of science, is optimistic. The present value of the velocity of light, 299,792,458 m/s, differs by 1,800,000 m/s from that of Foucault.

⁵⁸ Cited by Tobin (2003) p. 233.

Fig. 5.16 Alfred Cornu
(1841–1902)



Fig. 5.17 Cornu's
measurement in 1874
(external view). The
provisional shelter on the
terrace of the Observatory
contains the principal station
for measuring the velocity of
light. The elongated part
contains the tube of 38 cm
equatorial, dismantled for the
occasion. The light beam (in
reality totally invisible) is
sent to the Montlhéry tower,
where a 15 cm aperture
refractor with a mirror sends
back the light to the
Observatory. This experiment
is the opportunity for a
society gathering



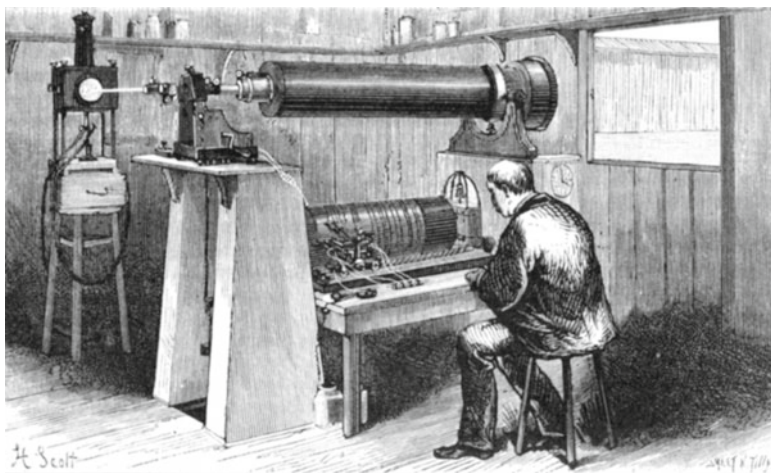


Fig. 5.18 Cornu's measurement in 1874 (internal view). The light source is a limelight (or calcium light), of the kind formerly used in music halls. The toothed wheel and its motor as well as the motor driving the registration cylinder are preserved in the collections of the Paris Observatory

to send the beam of light to Montlhéry and receive the reflected beam again, he used the refractor on the East tower of the Observatory, which had been dismantled and whose 38 cm objective had been reworked. This time he obtained a value of 300,400 km/s, with an uncertainty that he set at 300 km/s. The corresponding solar parallax deduced from this value is near the definitive value of $8''.79$, but was a bit less satisfying for Le Verrier than Foucault's value. Nevertheless, he seems to have reconciled himself to it.

The Total Eclipse of the Sun of 18 July 1860

This eclipse, for which the zone of totality passed through Spain and Algeria, excited great interest in Europe and particularly in England.

Already Arago had published detailed instructions⁵⁹ for the total eclipse of 8 July 1842, visible from the south of France and all the way to Russia, and had himself participated in observations at Perpignan, while Airy had observed it from Italy. Several observers had been "strangely surprised" to see a number of "red prominences 2 or 3 arc minutes in height, throwing themselves, so to speak, from the contour of our satellite."⁶⁰ Of course this was not the first time that these prominences

⁵⁹ *CRAS 14 (1842) pp. 843–861.

⁶⁰ Arago F. (1851) in **Œuvres complètes de François Arago*, ed. par J.-A. Barral, 13 vol., Gide et Leipzig, Weigel, Paris, t. 7, pp. 112–135.

Box 5.6 Hervé Faye (1814–1902)

Having been noticed by Arago, Faye entered the Observatory in 1836 as a student – astronomer. He became an astronomer in 1843, a member of the Academy of sciences in 1847 and of the Bureau of longitudes in 1862. He taught at the École polytechnique with great success from 1852 to 1855, and again from 1873 to 1893. For a time, he was rector of the Academy of Nancy and even for a while Minister of Public Instruction. An able observer, he discovered a comet in 1843 and imagined, with good reason, that a repulsive force issuing from the Sun gave to the tails of comets their characteristic forms, a force which James Clerk Maxwell later identified with radiation pressure on the cometary dust. Faye was very imaginative, and was pioneer in a number of areas. He pushed with Arago the idea of using electric telegraphy to measure longitudes. Before Janssen did so, he introduced in France with Porro the use of photography in observational astronomy and made remarkable photographs of the Sun. His contemporaries appreciated very much his theoretical ideas on the composition of the Sun, the formation of the Solar System and the origin of comets, though only a few French astronomers were interested in these problems at the time.

were seen during total eclipses, but they had not before been the object of so much attention. Arago thought that they involved “solar clouds floating in a gaseous atmosphere.” His idea was confirmed by observations made during the eclipses of 8 August 1850 and 28 July 1851. The prominences excited so much interest that they were the principal object of the observations that R. C. Carrington, the great English specialist on the Sun, proposed for his instructions for the total eclipse of 7 September 1858, visible from South America.⁶¹

The eclipse of 1860 was visible from more accessible locations, and two French expeditions were planned to observe it. One was organized by the École polytechnique, to Batna in Algeria, the other by the Paris Observatory to Spain. It was Hervé Faye (Box 5.6 and Fig. 5.19) who was charged with preparations for the latter. He was to lead the expedition, but when everything was ready, he quit, or perhaps was ousted by Le Verrier, who demanded to take charge of the expedition himself, with the assistance of Yvon Villarceau.⁶² Fleeing the latest tumult at the Observatory, Faye returned to the polytechnique, where he had been a teacher.

As France’s relations with Spain were still not very good, because of the memory of the war with Napoléon I, Le Verrier had to obtain a safe-conduct (Fig. 5.20). He forwarded toward the end of June no less than 33 crates of material, two refractors and

⁶¹ Carrington R.C.: *Information and Suggestions Addressed to Persons Who May Be Able to Place Themselves Within the Shadow of the Total Eclipse of the Sun, September 7, 1858*. Her Majesty’s Stationery Office, London (1858).

⁶² For details on the expedition, see Tobin (2003), pp. 218–221.

Fig. 5.19 Hervé Faye (1814–1902)

the two reflecting telescopes of Foucault of 20 and 40 cm diameter. Foucault also took part in the expedition, as did Yvon Villarceau and Chacornac. The goals included not only the observations of the prominences and the corona, but also the precise determination of the starting and ending times of the eclipse and the search for a possible planet near the Sun that would explain the anomaly of the motions of Mercury, a topic we shall discuss presently. The latter two subjects were the ones that interested Le Verrier, while Chacornac and Yvon Villarceau were charged with the observation of the prominences (they confirmed they were solar in origin) and Foucault with photographing the Sun with collodion wet plates. Everything except the planet search succeeded. Foucault's plates – together with others by Father Secchi and the Englishman Warren de la Rue – were the first ever taken of a total eclipse. Unfortunately, only de la Rue's plates are still remembered; those of Foucault have been lost.

An Eclipse in Siam

Another total eclipse of the Sun was to occur on 18 August 1868.⁶³ The zone of totality swept Asia from Arabia to Borneo, and the eclipse was long, with more than 6 min of totality. It was therefore of particular interest to the astronomers who set out

⁶³ For details and pictures of the Siam expedition, see Yvon Georgelin et Simone Arzano (1999) *L'Astronomie*, n°113, pp. 7–12, Aillaud et al. (2000) t. 3, pp. 47–59, and Stephan (1870) *Annales scientifiques de l'École normale supérieure* 7, pp. 99–162, accessible by http://www.numdam.org/numdam-bin/item?id=ASENS_1870_1_7_99_0.



Fig. 5.20 The safe-conduct delivered to Le Verrier and his "suite" for entering Spain

on expeditions to various locations to observe it: the Germans to Aden, the English to India, and the Dutch to the Celebes. In France, Janssen was well prepared, and had convinced both the Academy of sciences and the Bureau of longitudes to finance his expedition: on 19 March 1868, he obtained 15,000 francs from the Minister of Public Instruction, Victor Duruy. He installed himself at Madras. Le Verrier could not resist the chance of competing with the Bureau of longitudes, but he wasted time, and very late opted – in March 1868 – for an expedition to Siam, on the peninsula of Malacca. He also addressed Victor Duruy to obtain financing for the expedition, which was accorded to him on 19 May with a grant of 50,000 francs despite the fact relations between the two men were already deteriorating:

I accept the astronomical expedition that you propose. Call on M. Stephan, get everything underway. But let's try for some initiative to make up for the terrible delays.

The station chosen will gain us back some time. Couldn't you find some way to combine spectroscopy and photography with pure astronomical observation? Lacking astronomers at the Observatory, couldn't you find any at the École polytechnique or elsewhere? I would be greatly disappointed if France's expedition falls short, something that would be unworthy of her...

Finally, a bit of advice – speak less badly of me [to the Emperor]. This serves no useful purpose.

It is odd to see the Minister, who was not a scientist, recommending to Le Verrier work in spectroscopy and photography. This suggests the hand of another astronomer behind the scenes, perhaps Faye. The letter also alludes to the difficulties Le Verrier had in finding participants for the expedition. In truth, he invited astronomers of the Observatory but everyone refused him. Instead it was Stephan, enjoying at Marseille partial immunity to Le Verrier's despotism, who would direct the expedition. Stephan succeeded in persuading Rayet and Tisserand to come with him, and these three men embarked from Marseille. They carried a vast quantity of supplies (17 crates of instruments), including the same 20 and 40 cm diameter Foucault reflectors from the Paris Observatory used in Spain. The voyage began with a crossing by train from Alexandria to Suez (the Canal was not completed until 1869), continued by means of other overland crossings assisted by the imperial Marine, and finally arrived on the coast of the peninsula of Malacca, at the bay of Wha-Tonne. Fortunately the king of Siam, Mongkut, was interested in astronomy and wanted to be present during the observations. He generously offered all his facilities to the expedition. Everything was set up in timely fashion in an enclosed shelter that provided protection from tigers (!), and the observations were carried out in perfect weather (Fig. 5.21).

Tisserand observed the moments of contact with a 15 cm refractor by Cauchoix. Stephan, at the 40 cm reflector, observed the prominences (Fig. 5.22), and confirmed that they were indeed “dependencies from the Sun and not the Moon,” contrary to what had often been believed previously. Rayet observed the spectra of these prominences, in which he discerned and drew (Fig. 5.23) “a series of nine bright lines... These lines, it seems to me, can be related to the main lines in the solar spectrum.... The prominences are jets of an incandescent gaseous material, they are flames produced from a chemical phenomenon [sic] of extreme intensity.”

These observations were the best of all those carried out during the eclipse. However, the eclipse is best remembered for another development. Janssen recognized that



Fig. 5.21 The setting up of the French astronomers for the observation of the total solar eclipse of 18 August 1868. At the rear one sees, in front of a shelter, the 40 cm Foucault telescope, in the foreground the 20 cm telescope, on a rudimentary equatorial

Fig. 5.22 Drawing of the eclipsed Sun on 18 August 1868, by Major Tennant in India. The diffuse corona which surrounds the Sun is visible, although poorly represented. Several prominences are also seen

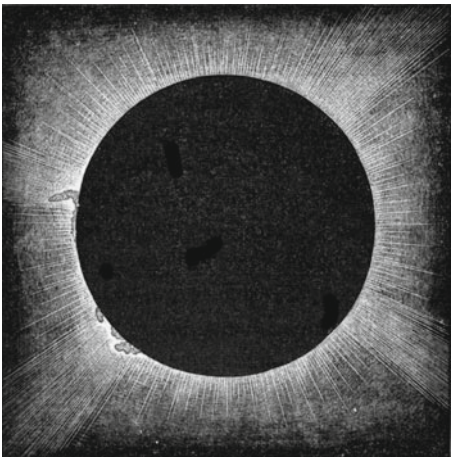
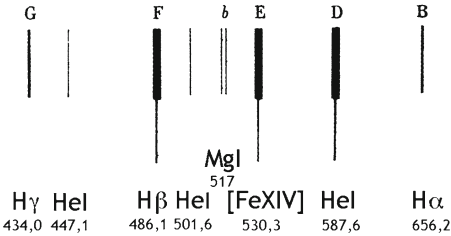


Fig. 5.23 The spectrum of a solar prominence observed by Rayet. The modern identification of the lines is indicated, with their wavelengths in nanometers. Line E is in fact a forbidden line of the solar corona



the spectral lines were so bright they ought to be visible even outside an eclipse, and demonstrated that this was so the very next day, by placing the slit of the spectroscope tangential to the Sun. England's Joseph Lockyer independently made the same observation.

The interpretation of the observed spectral lines took some time. Lockyer, Secchi, Janssen and Rayet were in agreement in identifying lines B, F and G with the three lines of hydrogen which were later called $H\alpha$, $H\beta$ and $H\gamma$. The line b, which is double, is due to magnesium. However, the double line D, which is very intense and is not at the same locations as the double line D1-D2 of sodium (later called D3 to avoid confusion) resisted all attempts at identification. Lockyer attributed it to a new element, which he called helium. Not until 27 years later would helium be detected on the Earth by William Ramsay and William Crookes. The line E, which Rayet had also observed and had mistakenly identified as a Fraunhofer absorption line, in fact had its source in the solar corona. Bengt Edlén demonstrated in 1942 that it was emitted by Fe XIV (an atom of iron that had lost 13 electrons).

The expedition ended rather badly, despite a great feast offered by the King of Siam; the sailors and several members of the expedition contracted malaria, as did King Mongkut himself, who died from it 2 months later. Stephan and his team departed as quickly as possible from the area, but not without having first precisely measured the coordinates of the observing station. They made a stop at Saigon, then again at Port-Said, visited the Pyramids, and returned to Marseille on 15 October 1868. Scientifically, the expedition was a resounding success.

The existence of a luminous gaseous layer around the Sun, later called the chromosphere and containing the prominences that are so clearly visible during total solar eclipses, was clearly established. So was the fact that they contained a great deal of hydrogen, whose emission lines were particularly intense. Rayet, and especially Janssen, would carry out a systematic study of them.⁶⁴ Le Verrier took advantage of the opportunity, while presenting the work of Rayet, whom he called "this skillful physicist," to recall that he had already suspected during the eclipse of 1860 that the Sun was completely surrounded by just such a gaseous envelope.⁶⁵

The Advance of the Perihelion of Mercury

A New Discovery by Le Verrier

In the midst of the vicissitudes of the Observatory, Le Verrier continued, his personal researches toward the realization of his grandiose dream: a complete theory of the movements of the planets.⁶⁶ In the course of this in 1859, he happened upon a seemingly

⁶⁴ See *CRAS 68 (1869) pp. 312–314 and 320–321.

⁶⁵ *CRAS 68 (1869) pp. 314–320.

⁶⁶ The book of Roseveare (1982), which we have relied on a great deal, is devoted to the advance of Mercury's perihelion.

Fig. 5.24 The advance of the perihelion of a planet. Perturbations by other planets are such that the orbit is not closed. Instead the ellipse rotates very slowly around the Sun by an angle $\delta\theta$ (here very exaggerated) at every revolution; the perihelion advances obviously by the same angle

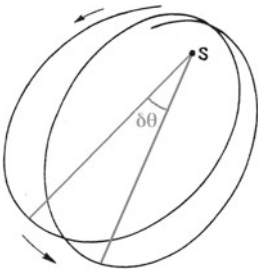


Table 5.2 Calculated advance of the perihelion of Mercury, according to Le Verrier^a

Perturbing planet	Advance of the perihelion (arc second per century)
Venus	280.6
Earth	83.6
Mars	2.6
Jupiter	152.6
Saturn	7.2
Uranus	0.1
Total	526.7

^aLe Verrier U.-J.: Théorie du mouvement de Mercure, *Ann. OP, Mémoires.* **5**, 1–196, 99 (1859)

inexplicable anomaly in the movement of Mercury. This apparently esoteric finding would excite considerable interest, leading to the premature announcement of the discovery of a new planet and culminating in the correct explanation of the anomaly with Albert Einstein’s publication of the General Theory of Relativity in 1915: it provided, in fact, the first observational verification of the General Theory.

What was this all about? The orbit of a planet around the Sun has the shape of an ellipse, as predicted by Kepler’s law. In fact, however, this simple law does not take into account the gravitational attraction exerted by the other planets. As seen from the north celestial pole, each of the planets revolves around the Sun in a counterclockwise direction. The combined perturbations of the other planets add an additional incremental force in the same direction. As a result, the orbit of this planet is not in fact a closed curve (Fig. 5.24); instead, the orbital ellipse turns around the Sun in the same sense (counterclockwise) as the planets revolve in their orbits. If one considers a characteristic point of the orbit, such as the perihelion (the point closest to the Sun), one finds that the perihelion advances regularly (winds around the Sun) with an angular velocity usually expressed in seconds of arc per century. In the case of Mercury, the effect of Venus’s perturbing force is greatest; but the other planets also contribute, as shown in Table 5.2.

The theory of Le Verrier allowed him to calculate the advance of the perihelion of each planet produced by the perturbations of the others (he had already done this in 1841, but his new determinations were more accurate). Only the orbit of Mercury

was sufficiently eccentric for the position of its perihelion to be precisely determined, allowing the calculated results to be compared with the observations. Le Verrier used for this purpose 397 observations of Mercury's meridian transits obtained at the Paris Observatory between 1801 and 1842. The Bureau of longitudes had wisely pointed out in 1834 to the astronomers of the Observatory the great importance of observing Mercury as often as possible, even though such observations were always difficult because the planet is never very far from the Sun and its meridian transits always happen during the day. Because of these difficulties, the measurements were not very accurate. Fortunately, Mercury occasionally passes in front of the Sun's disk, and during these transits it is possible to measure with great precision the instant at which it enters the disk, or leaves it. These transits therefore furnish an excellent set of measures of its position since the Sun's position is very well known. Le Verrier employed, therefore, these transit measurements, of which the oldest used went back to 1697, in order to check his theory. To his surprise, he found that the difference between the observations and his theoretical calculations were greater for the historical observations than for those of his epoch. He had sufficient confidence in the observations of his predecessors to be sure that the effect was real. This meant the difficulty had to be with the theory.

Le Verrier announced that the observed advance of the perihelion of Mercury was $38''.3$ per century more than the calculated value. This was no mere curiosity. In fact, it posed an enormous problem for astronomy: would the theory of Newton, so well vouched for until now by celestial mechanics, prove insufficient? Was there something wrong with the inverse square law itself? It was too soon to say that, and in any case, Le Verrier himself saw several less drastic solutions.

Was the mass of Venus too small? Increasing it by just more than 10% would explain Mercury's anomaly. But then the perturbations that Venus would cause in the movements of the Earth would be inadmissibly large. The masses of the other planets were sufficiently well known that changes in their values were also ruled out.

In due course, Le Verrier came to consider a much bolder hypothesis⁶⁷:

A planet, or if one prefers a group of smaller planets circling in the vicinity of Mercury's orbit, would be capable of producing the anomalous perturbation felt by the latter planet.... The disturbing mass, if it exists, would hardly have a sensible effect on the motion of the Earth. We don't know whether it would have any effect on Venus. While waiting for this point to be cleared up, we can at least allow that the effect [on Venus] would be either imperceptible or much weaker than that exerted on Mercury. According to this hypothesis, the mass sought should exist inside the orbit of Mercury [closer to the Sun]. Moreover, if one requires that the orbit never crosses that of Mercury, it is necessary for its aphelion distance [its furthest distance from the Sun] not to exceed 0.8 that of Mercury [whose orbit is very eccentric].

Le Verrier went on to estimate what the mass of the hypothetical planet had to be as a function of its distance from the Sun. Its mass would be of the same order as that of Mercury if it were halfway between Mercury and the Sun, and even more if

⁶⁷ Le Verrier U.J.-J. (1859), pp. 102–106.

it were closer to the Sun. The same relation would apply for the sum of the masses of the possible small planets. Le Verrier continued:

Wouldn't it be unlikely that a heavenly body, endowed with vivid brilliance and found so close to the Sun, could escape detection during a total solar eclipse? Wouldn't such an object sometimes pass between the Sun and the Earth, and wouldn't it then betray itself? Such are the objections that can be raised against the hypothesis of a single planet, comparable to Mercury, orbiting within the orbit of the latter planet. Those to whom these objections seem insuperable will be led to the idea of substituting for a single planet a group of asteroids, the combined influence of which would produce the same effect on the perihelion of Mercury....

It is extremely important that every regularly shaped spot, however small and insignificant it may seem, appearing on the disk of the Sun be followed for some time with great attention in order to be assured of its nature by knowledge of its movement.

These conclusions were published in the *Comptes rendus des séances de l'Académie des sciences* in the form of a letter to Faye, then secretary of the Academy.⁶⁸

Doubts and Confirmations

Le Verrier himself did not believe in the existence of a single intra-Mercurial planet, and he was not alone.⁶⁹ But in December 1859 he received an unsolicited communication from a certain Dr [Edmond] Lescarbault, physician and amateur astronomer, who said he had observed the passage of just such a planet in front of the Sun.⁷⁰ After reading a summary of Le Verrier's paper in the abbé Moigno's journal *Cosmos*, Lescarbault decided to write to Le Verrier. On receiving Lescarbault's letter, Le Verrier did not delay; he set out at once by train to the home of Lescarbault at Orgères-en-Beauce (Eure-et-Loire) to convince himself of Lescarbault's good faith and the quality of his telescope. He was convinced. Henceforth he accepted the discovery of this new planet, and even gave it a name, Vulcain (Vulcan).⁷¹

That, at any rate, is the official version. Flammarion, however, gives another, rather different account, which is amusing to read and may even be true⁷²:

⁶⁸ *CRAS, 59 (1959) pp. 379–383. This paper is easier to follow than that in *Ann. OP*.

⁶⁹ Le Verrier's doubts are confirmed by Simon Newcomb: °*Astron. J* 6, pp. 162–163 (1860). But Newcomb has also doubts about the asteroid belt which would produce the same effect.

⁷⁰ This letter, dated 22 December 1859, is reproduced by Le Verrier with history and commentaries in *CRAS, 50 (1860) pp. 40–46 and in °*Ann. OP, Mémoires* 5, pp. 394–399 (erroneously dated 1860 in the latter article). The history of Vulcan is the subject of the of Baum and Sheehan (1997).

⁷¹ There was already a Vulcan! Babinet had suggested that a big prominence seen on the edge of the Sun during the total eclipse of 8 July 1842 was a mass of gas orbiting around the Sun and called it Vulcan: *CRAS, 22 (1846) pp. 281–286. But this was soon forgotten.

⁷² *Flammarion (1911) pp. 188–190.

I witnessed the odyssey of this strange history, being at the time student-astronomer at the Paris Observatory, and finding myself in fact acquainted with the author of this supposed discovery, the doctor Lescarbault, of Orgères. On 26 March 1859, this excellent doctor, who passionately loved astronomy and appreciated its grandeur, had really seen a round spot on the Sun in the morning, before leaving for his medical rounds, and he saw it again when he returned for lunch; the spot had moved, but this displacement was due simply to the apparent diurnal motion of the Sun, the north–south meridian of which is vertical at noon, and oblique in the morning. The spot was not very far from the edge of the solar disk⁷³....

M. Lescarbault made his observation known to the journal *Cosmos*, and the director of the Observatory jumped, so to speak, with enthusiasm. He set out for Orgères, and suddenly arrived at the home of the worthy doctor requesting to see his observational journal. This journal did not exist. The doctor had the habit of writing his notes about his patients while in bed, and for this purpose used small wooden tablets on which he wrote in crayon. When a tablet was full and no longer useable, he shaved off the surface with a plane. This is what had happened to the solar observations, which M. Le Verrier had come to verify. As best he could, Lescarbault reconstructed from memory the drawing on a sheet of paper. The time of observation agreed with the requirements of the theory of Mercury. The illustrious astronomer declared himself satisfied and saw to it that Lescarbault was decorated with the Legion of Honor.

This little planet, between Mercury and the Sun, and christened with the name Vulcan, was supposed to revolve in 33 days around the radiant star. M. Le Verrier made calculation upon calculation and announced the dates on which the planet's transits before the Sun would be observed. Nothing was ever seen. I constantly declared myself against this illusion, which still persists. And let's be clear about this: it was never anything else.

Dr. Lescarbault died in 1894. His error isn't surprising for a lay person. Anyone could be deceived in such a way. Le Verrier, on the other hand, had no such excuse.

The stakes were huge. Had Le Verrier discovered yet another planet “with the tip of his pen”? Journalists and astronomers were mostly enthusiastic. A report in a professional English journal declared⁷⁴:

The singular merit of M. Lescarbault's observation will be recognized by all those who have examined the circumstances; and astronomers from all countries will unite in applauding this second triumph of M. Le Verrier's theoretical investigations.

However, Flammarion was not alone in having doubts. Emmanuel Liais, formerly of the Paris Observatory and now in Brazil, affirmed that he had observed the Sun a great deal and with a better telescope than Lescarbault's, but he hadn't seen anything unusual, even on the very day on which the doctor had made his alleged discovery. Liais insisted on the fact that no one had ever observed the passage of a planet other than Mercury and Venus in front of the Sun.⁷⁵ Lescarbault must surely have mistaken a small sunspot for a new planet. In any event, the object which he reported would

⁷³ This displacement was very probably due to the apparent rotation of the solar disk as seen in the alt-azimuthal mount of Lescarbault's telescope. However there are contradictions: in the drawing given by Baum and Sheehan (1997) p. 184, the displacement of the sunspot is inverted with respect to the expected one, unless Lescarbault had taken West for East because of the reversal of the image in the telescope. Flammarion writes that the observation took place in the morning, while the letter of Lescarbault to Le Verrier mentions the afternoon. Nothing is certain in this story.

⁷⁴ °*Monthly Notices of the Royal astronomical Society* 50 (1860) pp. 98–100.

⁷⁵ Liais E.: Sur la nouvelle planète annoncée par M. Lescarbault, °*Astronomische Nachrichten* 52, 369–378 (1860).

have been too small to account by itself for the anomalous advance of the perihelion of Mercury; it could only be one member of the conjectured asteroid belt.

Others went farther and raised questions that struck at the heart of Le Verrier's theoretical work. At the end of 1861, Delaunay, who never missed an opportunity to go on the attack, read before the Academy of sciences a note of which the following is an extract⁷⁶:

Since the discovery of universal gravitation, the tendency of astronomers has always been to establish tables for the movements of objects solely on the basis of this grand principle, and to take from observation only a few indispensable data.... But it has not been possible to succeed in one leap. Theory at first gives tables that are more or less imperfect, and one has no choice but to resort to empirical data in order to predict phenomena over a certain period of time. Gradually the efforts of astronomers have won territory for theory, and left diminished the role of empiricism. M. Le Verrier has marched along this path just as have all his predecessors. He has drawn up a table for Mercury, much more exact than we had before. But he has not been able to do this without recourse to empiricism.... It follows that one must not be too hasty in concluding in favor of the actual existence of the asteroid ring to which M. Le Verrier attributes the increase in the movement of Mercury's perihelion.

Delaunay, therefore, considered the possibility that a future improvement in the theory of the Solar System might cause the anomaly to disappear. He insisted in an article published sometime later⁷⁷:

M. Le Verrier ... continues to speak of this increase ... as if it were a fact derived directly from observation, and whose existence is not in any way contested; then he proceeds from this fact, and from another analogous one pertaining to the planet Mars, in order to establish the existence of various asteroid belts circulating around the Sun. The conclusions to which he arrives in this way seem to me to lack the character of certitude, or at least of high probability, with which M. Le Verrier presents them.

To this, Le Verrier reacted with indignation, and not without reason⁷⁸:

M. Le Verrier has already informed the Academy that he does not feel obliged henceforth to reply to attacks which have become systematic....

If one is pleased to call empirical any result based on observations, then one must logically proceed to a discussion of mere semantics, and classify the observational sciences, physics, chemistry and the rest, as empiricism.

One can understand Delaunay's doubts, but Le Verrier's theoretical work was very sound, even if his explanation was not. His startling result would be confirmed in 1882 by a similar and independent study⁷⁹ by the great American specialist in celestial mechanics, Simon Newcomb (Fig. 5.25). With new observations at his disposal, the latter would find the advance of Mercury's perihelion to be 43" per century

⁷⁶ *CRAS 53 (1861) pp. 950–955.

⁷⁷ *CRAS 54 (1862) pp. 77–82

⁷⁸ *CRAS 54 (1862) pp. 82–99.

⁷⁹ Newcomb S. (1882) *Discussion and results of observations on transits of Mercury from 1677 to 1881*. US Nautical Almanac Office, Astronomical papers, 1, Washington, U.S. Nautical Almanac Office, pp. 363–487.

Fig. 5.25 Simon Newcomb
(1835–1909)



rather than Le Verrier's 38". It was Newcomb's value that would be retained. Moreover, Le Verrier had noted in 1861 a similar effect, but weaker, acting on the orbit of Mars.⁸⁰ This, too, was confirmed by Newcomb.

The lack of observational confirmations of Vulcan did not stop Le Verrier from continuing to believe in its existence, or at least in the existence of an asteroid belt or ring. He would patiently await vindication until the very end of his life.⁸¹ In 1876, he came to the conclusion that the next transit of Vulcan in front of the disk of the Sun would not occur until 1881, and that it would be desirable to search for the new planet at other times than when it was on the disk. Janssen took advantage of the opportunity to advocate for solar photography, an area in which he was a great specialist and which seemed to offer greater objectivity than visual observations. He promoted especially the use of his "revolver photographique" (Fig. 5.26), which permitted successive images to be taken without having to load and unload the photographic plates⁸²:

A certain number of these instruments, systematically distributed across the surface of the globe, would provide complete surveillance, so that in a few years, the regions surrounding the Sun could be explored with a certitude and efficiency that would be impossible to expect by any other method.

⁸⁰ *CRAS 52 (1861) pp. 1106–1112.

⁸¹ See *CRAS 83 (1876) pp. 583–589, 621–624 et 719–723. Still in 1880, Tisserand publishes a "Notice sur les planètes intra-mercurielles" (*Annu. BdL pour* (1882), pp. 729–772) where he reminds the reader about the history of Vulcan and recommends to continue the observations.

⁸² *CRAS 83 (1876) pp. 647–655.



Fig. 5.26 Janssen's "revolver photographique" (For a detailed description, see Flammarion C. *Le passage de Vénus, résultats des expéditions françaises*, *La Nature*, 1^{er} semestre, pp. 356–358 (1875)). This apparatus contains a circular glass photographic plate that can advance in jerks in order to take successive images of the same object at regular intervals. It was used by Janssen in 1874 to observe the passage of Venus in front of the Sun. This is one of the ancestors of the Lumière brothers' cinematograph (and Janssen himself was one of the first subjects)

The Solution

Despite all efforts, no trace of a planet, large or small, within the orbit of Mercury was ever found.⁸³ How then was the anomalous advance of the perihelion of Mercury to be accounted for? Newcomb, having confirmed Le Verrier's conclusions, would imagine the unimaginable: a corrective term to Newton's law of universal gravitation. But he would not be successful with this. It is not our intention to discuss here the different possibilities that would be advanced in the attempt to resolve this problem; they are of interest to the historian of science, but are subsequent to the period we are considering.⁸⁴ The solution was to come from Albert Einstein, in November 1915, with his General Theory of Relativity.

A number of investigators, including the German Max Abraham and the Finnish Gunnar Nordström, had considered, like Newcomb, possible modifications to

⁸³ One can be certain at present that no asteroid with a diameter larger than 60 km circulates between the Sun and Mercury.

⁸⁴ See Roseveare (1982) for a good discussion. The original papers of Einstein are often difficult to understand, and the problem of Mercury's perihelion is treated more clearly there than in many textbooks about general relativity.

Newton's law, and applied them to the problem of Mercury's perihelion; but the numerical values they obtained were too small. Only Einstein was to arrive, with the mathematical collaboration initially of the Hungarian Marcel Grossmann, at a correct theory giving the right result. In simple terms, one can say that the deformation of space around the Sun due to its gravitation has the effect of adding a small corrective term to Newton's law. It is this relativistic term that is responsible for the anomaly. The values predicted by Einstein for the "abnormal" advance of the perihelion of Mercury and that of Mars are in exact agreement with what is observed. To conclude, we should render homage to Le Verrier for having made a remarkable and unexpected discovery, and for having sufficient confidence in his calculations to have published it. Thanks to him, General Relativity was to receive one of its strongest observational verifications.

Chapter 6

The Fall (1870–1872)

Paris, le 5 Février 1870.

Cabinet
DU MINISTRE
DE L'INSTRUCTION PUBLIQUE.

Monsieur le Sénateur,

*J'ai l'honneur de vous informer que, par
un décret en date de ce jour, l'Empereur
vous a relevé de vos fonctions de Directeur
de l'Observatoire Impérial.*

*Recevez, Monsieur le Sénateur, l'as-
surance de mes sentiments de haute
Considération.*



Le Ministre de l'Instruction publique,

Segris

Ms 1042 n°32

Dismissal letter of Le Verrier

Despite the good will of the Emperor, of whom Le Verrier was one of the most loyal supporters, the ravages inflicted on the Observatory by his difficult character could not be overlooked by the authorities. The situation became insupportable over the years, and the abscess would finally burst in 1868, leading, 2 years later, to the Dictator's dismissal – an extraordinary event occurring at a period when elites generally flourished with impunity. Here's how it all came about.

Mounting Hostilities

Ejected from the Observatory in 1862, Flammarion, who had become a celebrated journalist, relayed the recriminations of the astronomers to *Le Siècle* (The Century), at the time the most widely circulated daily newspaper in France. His first article dates from 10 February 1866, and there would follow many others during the next 2 years. Other journals, equally eager for scandal, emulated this example. Flammarion wrote, “M. Le Verrier at first responded to these public revelations, by sending to us the bailiff, and his responses were published along with my attacks, but he defended himself poorly, actually he had no real defense.”¹

Here is a sample from 1867 of what Flammarion wrote in *Le Siècle*²:

These past fourteen years this haughty scientist has placed himself above the Minister of Public Instruction, above the Emperor, above the law; these past fourteen years he has reigned as an autocrat, suppressing at his whim the salaries of his employees, opposing systematically all personal research...

Did he not exclude arbitrarily from the Observatory the most illustrious of our French physicists, M. Foucault, member of the Institute, and withhold his salary contrary to the wishes of the Ministry, which would continue to deposit to the treasury the amounts suspended by this arbitrary veto?

Did he not ignominiously treat the head of the meteorological division, the scientist M. Marié-Davy, to whom we owe the progress accomplished in meteorology these last years, because this astronomer published a work, (*The Movements of the Atmosphere and the Seas*)? He accused him of having stolen, more or less, the work of the Observatory, for the purpose of attributing it to himself personally, accusations as false as they are shameful, because M. Le Verrier himself had recommended the book and corrected the first proofs.

These are facts that M. Le Verrier cannot deny; nor that he padlocked the doors between M. Marié-Davy and his assistants, and forbade his domestic servant from making the fire in his room in the heart of winter; nor can he deny having stopped the salary of twenty-eight employees these past fifteen months, seven in the last month alone, salaries fully regulated by the Ministry. Can he deny wanting to trample underfoot his most eminent colleagues, Faye, Desains, Babinet, Puiseux, Liais, Chacornac, etc., as well as others...

It's enough, the measure is full, Napoléon III and M. Duruy have understood this.

¹ *Flammarion (1911), pp. 511–512.

² *Flammarion (1911), pp. 514–516.

An Oversight Commission and a Council for the Observatory

Indeed, the Minister of Public Instruction, now Victor Duruy, finally decided to convene the Commission provided for by the decree of 30 January 1854 (see Chap. 4). This Commission was supposed to meet every 2 years, but Le Verrier had avoided invoking it in order to have free rein. The composition of the new Commission strictly followed the decree: The members were:

- As president, Vice-admiral Fourichon, vice-president of the Council of the admiralty;
- Robiou de Lavrignais, Inspector general of the nautical engineers, member of the Council of admiralty;
- Serret, member of the Institute;
- Liouville, member of the Institute, member of the Bureau of longitudes;
- Delaunay, member of the Institute, member of the Bureau of longitudes;
- Antoine Jérôme Balard (a celebrated chemist), member of the Institute, Inspector general of higher education;
- Le Verrier, director of the Observatory;
- Bellaguet, division head of the Minister of Public Instruction, secretary.

The nomination of Liouville and especially Delaunay must have deeply incensed Le Verrier; but in any event, there was hardly anyone on the Commission who was not his enemy. Not being able to oppose its formation, Le Verrier tried in vain to become its president. Then he objected to the inclusion of certain members. Finally, he refused to attend its meetings. The Minister decided to overlook this. The first meeting took place on 9 November 1867.

The very detailed report which the Commission presented to the Minister appears to be, in fact, quite objective.³ Here is an extract from its conclusions:

1. Instruments already installed at the Observatory have undergone useful modifications, of which some were undertaken expressly for the purpose of assuring their stability; moreover, thanks to the liberality of the government, new instruments of great power have been installed. 2. The department of regular observations has been active on a grand scale, and the numerous observations that have resulted have been reduced and published with great regularity.

But along with these positive results, the Commission has noted with regret that leadership of the Observatory has not achieved one of the principal goals which an institution of this type ought to set for itself: the education and training of scientists. The Commission does not hesitate to attribute as the cause, completely or in part, the provisions of the decree of 30 January 1854 [which reorganized the Observatory].... The power invested in the director is excessive, and one would have difficulty finding another example of such omnipotence in a hierarchically organized administration. The director, invested with such power, can, in certain cases, be tempted to abuse it. The Commission, with unanimous voice, expresses the wish that the decree be revised in such a way as to protect the director from such temptation, so as--it must be said—to prevent the recurrence of such regrettable acts which, having been the occasion of numerous and legitimate complaints, are sufficiently known to Your Excellency not to need to be recounted here....

³ BOP, Documents divers sur l'Observatoire de Paris, 1854–1872, cote 3567 (4), folder G.

The Commission does not hesitate to think that, if it is true that exceptional work [i.e., work of a non-routine nature] carried out by the Observatory has not been more substantial,⁴ the principal cause must be the excessive turnover of personnel, amongst whom were some very eminent scientists... The Commission asks, foremost, that the decree of 30 January 1854 be revised, and that new revisions be introduced suitable for assuring the interests and the dignity of the personnel, so that the best possible work and the most fruitful astronomical studies can be achieved.

The Commission included with its report a proposal for reorganizing the Observatory, which was somewhat modified by the Minister and by the Council of State. It proposed the creation of a Council, composed of the director of the Observatory and the titular astronomers, which was to meet once weekly at a fixed time, and a Commission of seven members (the ones stipulated in the decree of 1854, but not the Observatory director), which was to meet annually. The Council of State added four foreign members to this commission, and decided that it should meet only once every 2 years. It also recommended that the meetings of the Council were to be held only monthly, not weekly. These arrangements, then, were the basis of the Decree of 3 April 1868, which was accompanied by rules for the meeting times of the Council, general guidelines concerning work at the Observatory, regulations for carrying out meteorological work, and finally a general plan for the astronomical projects of the year. Altogether, it was a very complete résumé.⁵ The salaries for the various ranks of personnel were finally fixed: auxiliaries earned 1,200–1,900 francs per year, assistant astronomers 2,000 francs, adjunct-astronomers 3,500–5,000 francs, under exceptional circumstances as much as 5,500 francs, and the titular astronomers 7,000–8,000 francs. In Marseille, the local director, Stephan, who was an adjunct-astronomer, earned 5,500 francs, and his two assistants, Poggia and Borelly, 2,000 francs, the same as the assistant astronomers in Paris.⁶ The official documents do not include any details about Le Verrier's remuneration, but he must have made around 30,000 francs annually, according to the information provided by Daverdoring (see the preceding chapter).

Taking advantage of the replacement, on 17 July 1869, of Victor Duruy by a rather obtuse person, Louis Olivier Bourbeau, Le Verrier was to try to obtain modifications of this decree and regulations. He also resumed his "oppressive habits." In a rather crafty way, he maintained in a report of December 1869 that it wasn't the decree itself that bothered him – he agreed that it had been amended in a beneficent way by the Council of State (a little flattery never did any harm). Instead it was the interpretation of it by the Council of the Observatory that bothered him⁷:

⁴The report says rightly elsewhere: "These exceptional and free works are those which lead most often to discoveries, and bring glory to the observatories where they have been performed."

⁵These texts, as well as all regulations concerning the Paris Observatory from 1854 to 1872 and other interesting texts were collected by G. Bigourdan at the BOP in Documents divers sur l'Observatoire de Paris, 1854–1872, cote 3567 (3) (4) et (6).

⁶BOP, Documents divers sur l'Observatoire de Paris, 1854–1872, cote 3567 (4), folder V.

⁷*Observatoire Impérial de Paris. Rapport fait en décembre 1869 par M. Le Verrier*, pp. 1–24, s.d., Paris, Gauthier-Villars.

In the month of January 1868, the Commission prepared a draft of a decree, the effect of which was to effectively transfer authority to the functionaries joined in the Council while suppressing that of the Director. This decree, presented by the Minister for the signature of the Emperor and his Council, was not accepted. It was sent back for examination by the Section of the Interior and Public Instruction of the Council of State.

The Section reworked the proposal in its entirety, and from its deliberations arose the text constituting the Decree of 3 April 1868. It is this which regulates us at the present time.

When the Decree of 3 April was returned, the formation of the Council, charged along with the Director with elaborating the rules, received special attention. It would have been important to assemble therein a majority of members that did not dispute the original proposal modified by the Council of State, but precisely the opposite took place. From the very first meeting, a large majority expressed its desire to interpret the Decree in a perverse sense, and to establish by regulations the very things in the original rules that were condemned by the Council of State.

Accordingly, the rules ended up being, in certain important points, contrary to the Decree. By false representation the authority was withdrawn from the Director which Decree of 3 April had meant him to retain, thereby depriving him of the power needed to conduct the scientific work of this institution.

Thinking it desirable to reestablish his authority by explicitly spelling it out in the text, Le Verrier proposed various modifications, of which the following is an example:

Text of the decree: each of the service heads will apportion the work to the [adjunct] astronomers and the assistants placed under his direction, and oversee their operations. He will be responsible for directing the training of the assistants from the point of view of observational practice.

Text proposed: the service heads will conform to the instructions that they receive from the Director for carrying out the work. They will propose to the Director the apportioning of work to the astronomers and assistants who are under their supervision; the nature and duration of work, both day and night; and the measures to be taken for the training of the assistants.

As was to be expected, these changes and modifications were rejected by the Minister. Le Verrier's attempt to rewind the matter had failed.

As for the Observatory Council, which Le Verrier pretended to believe was responsible for the perverse interpretations to which he objected, it had been set up by Duruy himself on 28 April 1868. This minister had stipulated on this occasion that the initiative for scientific propositions to be debated was as much the prerogative of the members as of the director. One can imagine that under these circumstances, Le Verrier, with his need for absolute power and control, would oppose, by any means at his disposal, the Council's meetings.⁸ The Council nonetheless did meet quite regularly at first, and was even presided over by Le Verrier himself, as the rules stipulated. But Le Verrier soon began to skip the meetings. He began to attend more regularly from February 1869 to see which way the wind was blowing, so to speak, though he frequently omitted calling it to order. Generally speaking, the Council was the scene of confrontations

⁸ The minutes of the Observatory council meetings are at the BOP in Documents divers sur l'Observatoire de Paris, 1854–1872, cote 3567 (3), folder Z.

that were more harmful than helpful, but it did accomplish a few things that were worthwhile. For instance, it published in December 1869 the first annual report of the Observatory since its beginnings, and it granted Marié-Davy, despite Le Verrier's opposition, 540 francs to buy an actinometer from Father Secchi (an actinometer, invented by John Herschel in 1825, is a device for measuring the heating power of radiation) and a thermoelectric pile made by Edmond Becquerel. With this equipment attached to the 20 cm diameter Foucault telescope, Marié-Davy succeeded in detecting the infrared radiation of the Moon,⁹ one of the rare observations of astrophysical interest that were made in France under Le Verrier.

The skirmishes between Le Verrier and his opponents continued, and were reported with relish in the journals, especially in *Le Siècle*, to which Flammarion contributed. In her diary,¹⁰ Lucile Le Verrier echoed the disturbing events:

25 November 1867. In September ... the minister attacked papa and wanted to have his administration investigated by two of his most notorious enemies. Naturally papa resisted.

7 December 1867. The Emperor and Empress, denying the rumors that were circulating, assured my father of their highest esteem. They graciously added: 'You are of course above M. Duruy, minister though he is.

12 August 1868. Much grief for my father, still persecuted by M. Duruy.

19 April 1869. My father's enemies have asked the Academy for the complete demolition of the Observatory, and the Academy, to the contrary, has decided that the Observatory is to remain as it is.

19 July 1869. The minister who has persecuted papa so much, and overturned the Observatory, is no longer minister. M. Duruy is replaced by M. Bourbot [Bourbeau], who is rather unknown.

Relocate the Observatory?

One episode in this battle, to which Lucile alludes in her diary, involved a proposal to move the Observatory of Paris and to demolish the seventeenth century edifice. Since its inception, the construction of Claude Perrault, however magnificent it might be, had been judged poorly suited for astronomy¹¹—and this is still true to this day. According to its first director, Jean-Dominique Cassini I, who had moreover required important modifications to the building,¹² shortly after his arrival in Paris in 1669:

⁹ Marié-Davy's paper is **CRAS* 69 (1869), pp. 1154–1158; curiously, it is under the section "meteorology," and was presented by Delaunay not Le Verrier; the latter did not appreciate such observations. Marié-Davy was probably intending to continue them but was not allowed to do so.

¹⁰ Le Verrier Lucile (1994), pp. 41, 45, 52, 61 et 74.

¹¹ However during the seventeenth century one observed from outside the building, because of the extreme focal lengths of the "aerial" refractors. The inadequacy of the building was less crippling than later, when it became necessary to roll the instruments onto the terrace for observing.

¹² On Claude Perrault and the construction of the Observatory, see Wolf (1902) and Picon (1988).



Fig. 6.1 The construction of Boulevard Arago, South of the Observatory. The Observatory is clearly visible on the left, the small building in the garden with its twin domes. It is easy to understand why Le Verrier was so disturbed by these developments and also by the smoke of the factories in the background

It was enough for Perrault to have imposed upon the façade and on the pile of the Observatory the grave and grandiose character fitting its purpose. This was the trademark of his genius, to create an impression. As for the rest, it little mattered to him whether an astronomer could observe with a bit more or bit less convenience.

In addition to the inconvenience of the building, the quality of the atmosphere at Paris had greatly deteriorated in the nineteenth century, because of urban development. The Observatory, which once had been in the countryside, now found itself surrounded on all sides by houses and factories. It was this, indeed, which had led Le Verrier to develop the “branch” observatory at Marseille. Some people thought, therefore, that it would be opportune to relocate the Paris Observatory itself to a less disturbed suburb. In 1854, on Le Verrier’s arrival, and before new work was undertaken, the possibility of relocating the Observatory was already being considered, but in the end it was decided to leave it where it was. However, in 1860, Baron Haussmann decided to build large, heavily trafficked, boulevards in the vicinity. This created a new crisis (Fig. 6.1). Le Verrier fought to limit their harmful effects. By the end of 1867 he thought he had been successful.¹³

¹³ *CRAS 65 (1867), pp. 776–781.

However, the relocation of the Observatory continued to be discussed. An astronomer of the Observatory, Yvon Villarceau, rose before the Academy of sciences on 23 December 1867, to present arguments for the Observatory to be moved.¹⁴ He recalled with malign satisfaction some passages in the report of the Commission, which in 1853 had recommended the improvements needed by the Observatory. This report had been inspired by Le Verrier himself:

The situation of the observatory at the heart of the capital, in a smoky atmosphere and on a shaky, agitated ground, is an inconvenience which the observatories of Greenwich and St. Petersburg (Pulkova) avoid by their location. They do so, at any rate, since the St. Petersburg Observatory was rebuilt 15 years ago at a distance of four leagues [16 kilometers] from the city center. The shaking of the ground is, course, incompatible with the use of sensitive instruments whose primary prerequisite is stability....

If the Commission does not demand the relocation of the Observatory, it is because it hopes that the inconveniences mentioned can be eliminated by various remedies, either in the interior of the building or in the area of its perimeter where it would be necessary, at the very least, to macadamize the roads. But as nothing will be able to remedy the lack of transparency of the atmosphere, the abandonment of the large central building, so inappropriately called the Observatory, would hardly cause a twinge of regret to the friends of astronomy....

As a further detail, Yvon Villarceau pointed out that astronomical observations were beginning to suffer from the effects of light pollution in the capital. He recommended that the Observatory ought to be transferred outside Paris, but not too far, so as to permit “the astronomers to frequent the Academies and Faculties, the libraries and the workshops.” He suggested a whole series of places in the southern suburbs, between 7 and 20 km from Paris, which seemed to him to be suitable. He was especially taken by the site of the windmills of Fontenay-aux-Roses. These communities were, he noted, generally well served by rail. Indeed, “for the past several years, most of our professors have taken up residence in neighboring localities to the train routes of the Orsay, Sceaux, and Versailles on the left bank of the river. The commutes for students [to attend university classes and those at the Collège de France] will not be any more painful than for their professors, and will be no greater than those that they are already obliged to undertake daily in Paris.” Finally, he pointed that by selling the lands belonging to the Observatory, valued at an estimated at 4–5 million francs, the cost of acquiring land now employed for growing cereals and comestibles would be covered, with a balance that would constitute a reserve for the future needs of the new Observatory. Drawing a contrast with Perrault’s adornments, he insisted that buildings of the new observatory “ought to be simple shelters, chosen for their sturdiness, rather than monuments of architecture that one would not hesitate to sacrifice to the needs of science, if further progress required their transfer or elimination.”

Le Verrier responded right away,¹⁵ with the backing of the chemist Jean-Baptiste Dumas, who was one of his loyal supporters, that the measures that he had already taken had had their effect, and that observations that required a clear sky had been transferred to Marseille, which was now equipped with the 80 cm diameter Foucault

¹⁴ *CRAS 65 (1867), pp. 1060–1073.

¹⁵ *CRAS 65 (1867), pp. 1073–1081.

reflector.¹⁶ Moreover, the observatory at Toulouse, created by Arago, which presented the same advantages as that at Marseille, was fully developed. He therefore opposed the proposal of Yvon Villarceau. “We hardly know how to understand,” he wrote,

what advantage there can be in mixing up the Paris Observatory in a pointless agitation, incited by incompetent, self-serving people, whose passionate polemics and motives have nothing to do with science. We can’t allow ourselves to be carried along with these agitations, and in writing these lines in the place made illustrious by those who have preceded us for the past two centuries, it seems to us that we speak in the name of all in defending one of our oldest and most glorious scientific establishments. At the moment when the needs of science called for it, this national institution was supplemented by the addition of a branch where the climate leaves nothing to be desired. One only has to give to this institution its natural preordained development in order to keep our country in the high exalted scientific role which is its due.

Yvon Villarceau did not admit to being vanquished, and the polemic became more heated. At the next meeting,¹⁷ he noted the variety of observations made at Pulkova, an observatory situated outside of a city, and affirmed:

Of all the work I have just enumerated, the only sort that could still be carried out at the Paris Observatory ... is limited to observing planets and comets of sufficient brightness, and cataloging stars. The Paris Observatory, restrained in this way, becomes a second-rate observatory, or even a third-rate one....

If the work no longer possible in Paris were to be done at a branch observatory, would not this branch take over the place of the principal establishment? In the attempt to avoid this eventuality, the astronomical instruments and observers are dispersed to Montpellier, and Bordeaux, and Toulouse, etc., where it is hoped to establish observatories which will eventually be successfully annexed to the Paris Observatory. This has already happened with regard to the one in Marseille. By this scheme, the Observatory would become a sort of Ministry of Astronomy, whose offices would occupy the central building and where the most useful astronomical labor would be performing calculations.¹⁸

A further response then came from Le Verrier: “One proposes to suppress not only the Director, but the Observatory itself, and to demolish it and sell the site at so much per meter. That would produce millions!” Then he remarked, not without reason, that the climate at Fontenay-aux-Roses would not be much different from that in Paris. Yvon Villarceau replied in turn, at a subsequent meeting,¹⁹ and his comments were followed by another response from Le Verrier, who invoked this

¹⁶ Ironically, the Marseille observatory was also soon surrounded by Haussmann- type buildings. Its only advantage over the one in Paris was the superior number of clear nights.

¹⁷ *CRAS 65 (1867), pp. 1099–1106.

¹⁸ This is to some extent the present situation. But observations are mostly performed in places located far from the cities, and especially overseas.

¹⁹ *CRAS 66 (1868), pp. 17–21. Le Verrier’s answer is pp. 21–29 and pp. 53–63, followed by a new intervention of Yvon Villarceau pp. 63–68, and by another answer of Le Verrier pp. 68–76. The following year, Le Verrier still felt obliged to show, using many results of observations with Gambey’s meridian circle, with a mercury bath defining the direction of the vertical, that “the vibrations of the ground don’t affect the observations performed at the Paris Observatory” (*CRAS 68 (1869), pp. 157–161).

time the view of Arago, according to whom “in every country permeated with an enlightened love of science, memories of the Observatory would amply suffice to save it from destruction.” But it’s clear that in contrast to Arago, Le Verrier wasn’t interested in the heritage, as such: “Whereas we would concede the removal of the upper floor in order to facilitate certain installations, Arago would not even agree to this operation. He knew too well people and things: he knew that removal of the first stone would pave the way for a second to be removed, and that the surest way to save it is to not permit the removal of anything at all.”

The noise of these disputes reached the Minister of Public Instruction, who asked the opinion of the Academy about tearing down and possibly moving the Observatory. The opinion was given in secret committee on 12 April 1869, and resulted in the following resolution, adopted unanimously, which managed to satisfy those on both sides of the question²⁰:

It is important that the imperial Observatory of Paris be maintained without the least diminishment, while adding to it quarters for the observers; but it is also necessary that another observatory of the first order, meeting all the requirements of science, with lodgings for all the personnel, be founded in a location suitably chosen outside, but near, the city of Paris.

The rooms or observation facilities of the new establishment will be placed in the center of an enclosed plot belonging to the state, of great enough area to insure their isolation at a sufficient distance from adjoining buildings and highways.

The old and new observatories will be absolutely independent of one another. Each of them will carry out its work freely under the control and supervision of the Ministry.

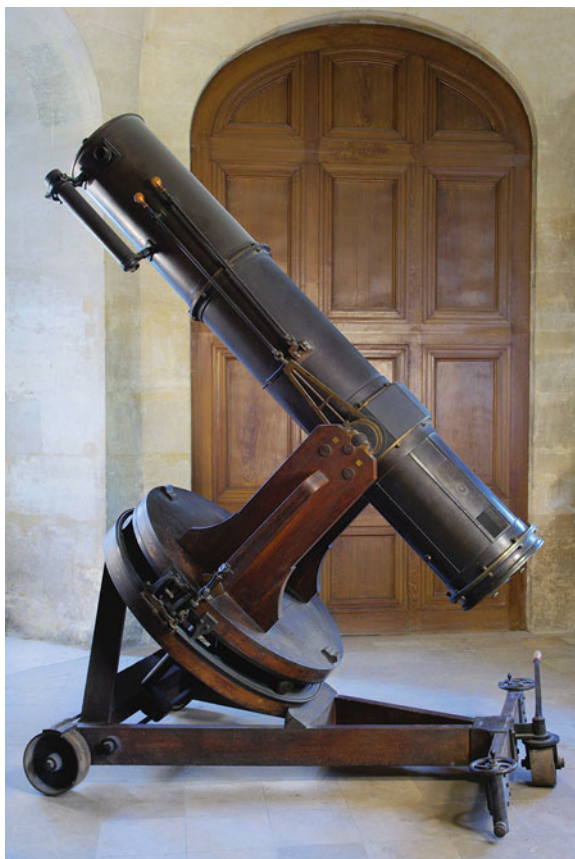
For want of funds, nothing happened for some time, but one finds in the proceedings of the meeting on 2 June 1870 of the Observatory Council the following remarks²¹:

[The ministry having contributed 7,000 francs in order to carry out] observations comparing the transparency of the air in Paris and outside of Paris, the question is to be taken up again, and a second telescope of 40 cm [Fig. 6.2] has been commissioned for use in these investigations, together with the telescope of the same aperture already in existence. While awaiting its construction, the observations are to be made in turn in Paris and Fontenay with the same instrument [the other telescope of 40 cm was that of Foucault-Eichens]. If the observations prove that the hill in Fontenay enjoys an incontestable superiority over Paris, the question ought to be discussed again, and the transfer of the Observatory might well be justified. If the advantages of the new location are not so clear-cut, perhaps it would be better, for the time being, to keep the advantages of residence in Paris and to mitigate as far as possible the main inconveniences of the present situation. It seems that if the Observatory building were completely razed, and the terrace transformed into an open hill, extended northward, the main obstacle to the observations would be eliminated.... [There follows a discussion; some present do not agree with this.] [M. d’Abbadie] argues that, since the start of this discussion, both sides have offered opinions rather than evidence. He believes that one could just as well observe in Paris than outside of it, even with regard to transparency, and cites to support his view the observations of Dawes, who saw the dark ring of Saturn through London fog, etc.

²⁰ *CRAS 68 (1869), pp. 207–220.

²¹ BOP, Documents divers sur l’Observatoire de Paris, 1854–1872, cote 3567 (3), folder Z. Delaunay was then the director, after the dismissal of Le Verrier.

Fig. 6.2 The 40-cm reflecting telescope of Eichens-Martin. It is an exact copy of the Eichens-Foucault one except with a metallic tube. It was built for a comparative site study between Paris and Fontenay-aux-Roses. See also Fig. 4.26



D'Abbadie put his finger, certainly, on the weak point of this affair: personal motivations do seem to have had the upper hand over the science. Interviewed by Bigourdan in 1889 and then in 1894, Gaillot, who had been present during all the discussions, summed up²²:

Concerning the transfer of the Observatory, proponents and adversaries both acted in bad faith.... [Yvon] Villarceau hoped that Le Verrier would not accept the directorship of an Observatory in the suburbs and that he would become Director himself.... Le Verrier was only the most shrewd of them, that's all.

The war of 1870 put an end to the affair. The plan to tear down and move the Observatory was tabled indefinitely, even though Le Verrier's successor, admiral Mouchez, attempted in 1883 to create, without success, a branch of the

²² BOP, Documents divers sur l'Observatoire de Paris, 1854–1872, cote 3567 (3), folder AP.

Observatory outside the city, resting his case on the “unanimous wish of the Academy in 1869, and the precedent given to us by all the great foreign observatories.”²³

An Observatory for Astrophysics

While the astronomers of the Paris Observatory were agonizing over the question of creating a new observatory at Fontenay-aux-Roses or some other location in a Parisian suburb, another astronomer, independent of any institution, submitted to the Ministry of Public Instruction the proposal for a new Parisian observatory devoted to “physical astronomy,” or what we would now call astrophysics. This astronomer was Jules Janssen (Fig. 6.3 and Box 6.1).

Janssen was already celebrated for instruments he had made, notably his spectroscopes (see Fig. 5.11), and had won acclaim for his work on the solar prominences, which he was able to observe very well during the total eclipse of 18 August 1868. Moreover, he had connections, and knew Victor Duruy quite well. In November 1869, he sent to the Emperor a request:

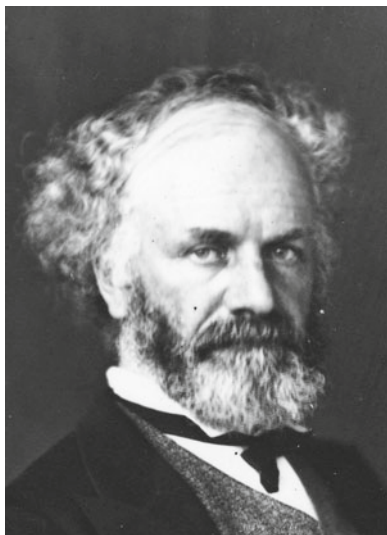
Over the past ten years, major discoveries have been made regarding light. These discoveries applied to astronomy have led to important and unexpected results. Astronomy has added one more branch [astrophysics], and this branch is already so considerable that it deserves to be endowed with special means of observation.

This request, with Faye’s support, was forwarded to the Minister. But it was no longer Victor Duruy. His replacements, Bourbeau, then Segris, did not agree to the creation of a new observatory, though they did set aside for Janssen in May 1870 at Sèvres, in the Pavillion of Breteuil (a building in the park of the Chateau of Saint-Cloud), a place for him to install his instruments. The war did not, however, give him the chance to take advantage of this bequest.

After the war, in 1871, Janssen contacted the new Minister of Public Instruction, Jules Simon, emphasizing “the great ardor and sacrifices that our neighbors [the English] are making toward the progress of science” and mentioning the considerable sums which had recently been made available to William Huggins and to the solar observatory at Kew. In 1873, Janssen was elected to the Academy of sciences and nominated to the newly created Council of the Paris Observatory. With his position thus reinforced, he got a deputy of Hautes-Alpes, Ernest Cézanne, to intervene to reactivate his project. The Ministry sought the Academy of sciences’ opinion, and the Academy appointed a Commission composed of Edmond Becquerel, Joseph Bertrand, Jean-Baptiste Dumas and the astronomers Maurice Lœwy and Hervé Faye. Faye was the secretary. The Commission report was a very interesting, well-documented and

²³ Mouchez, E.: *Rapport annuel sur l’état de l’Observatoire de Paris, année 1883*. Paris, Gauthier-Villars (BOP) (1884).

Fig. 6.3 Jules Janssen
(1824–1907)



Box 6.1 Jules Janssen (1824–1907)

Born into an artistic family, Janssen pursued scientific studies and obtained a science degree in 1855.²⁴ In 1857, although he was still unknown, he managed to obtain from the Ministry a directive to carry out observations of the terrestrial magnetic field in Peru. While there, he became gravely ill. Thus began a maverick scientific career, which was carried out entirely outside of official institutions. He constructed spectrosopes with which he observed the solar spectrum, and showed that certain absorption bands were telluric, that is caused by the Earth's own atmosphere. In Rome, he and Father Secchi obtained the spectra of many stars. He also organized expeditions for the purpose of observing total eclipses of the Sun and the transits of Venus across the face of the Sun, in which he achieved spectacular successes. He founded in 1875 the astrophysical observatory in Meudon, which he personally adorned with magnificent photographs of the Sun. In order to better observe the solar spectrum, he set up an observatory at the summit of Mont-Blanc, which was to function intermittently until his death in 1907. A highly gifted communicator and skillful in interpersonal relations, Janssen succeeded in almost everything he undertook, and injected new life into French astrophysics, which had been moribund since Arago's death in 1853.

²⁴ For a biography, that we used extensively, see Launay (2008).

vibrant plea for astrophysics.²⁵ It begins by recalling the work of Arago, who discovered the gaseous nature of the Sun by studying the polarization of its light, the work of Gustav Kirchhoff, who had found in the solar spectrum the lines of certain terrestrial chemical elements (his collaborator Robert Bunsen was, oddly enough, not mentioned), and the more recent work of William Huggins, Norman Lockyer and, of course, Janssen himself. It went on to discuss the situation at the Paris Observatory, and the uses of a new observatory:

To cover everything today, it would be necessary to add still more to all the requirements and conditions which make so difficult and honorable the old astronomy ... Physics, in just that part which is most profound and delicate; chemistry with its modern methods, practice in experiments, dexterity in manipulations; and also the kind of spirit that is proper to sciences, which demands more of the imagination even as it keeps itself more aloof from the severe discipline of mathematics. To ask all this at once is to ask too much. Evidently there is a need to specialize.

As physical astronomy can no longer be tied to mechanical astronomy, let's give it its own identity. The two sciences can then be allowed to develop in parallel, each making use of the skills and techniques it finds most proper. It was in this way that the theory of terrestrial magnetism and meteorology, born in our Observatory, separated from one another. Now they each have their own special facilities.

It is not our intention to imply by this that the old observatories must refrain from these researches, which have all the attraction of a new world, even less do we maintain that astronomy properly speaking can do without physics... To be more precise, if we look at our national observatory, we see that a place has always been accorded to physics; but, with some notable exceptions, physics was always secondary, and in the role of a servant... Far from wishing, therefore, to establish an absolute separation between the two, we would like to see the old observatory continue as before; but alongside of it, and independent of it, we would also erect a veritable laboratory of physics, chemistry, and celestial photography.... Certainly this would be something new for us; but for quite some time it has not been a novelty in England or in America, and it will soon be a *fait accompli* in Germany.²⁶

In consequence, Messieurs, your Commission has the honor of proposing that you respond to the Ministry of Public Instruction and tell it that the Academy gives its full support to the idea of creating in Paris, or in its suburbs, an observatory especially devoted to astronomical physics.

Despite certain misgivings, the Academy adopted the conclusions of this report. Le Verrier, whose relations with Janssen were always good, did not oppose it, although then again astrophysics did not interest him. However, certain others, like Henri Édouard Tresca, an influential professor of mechanics at the Conservatory of Arts and Measures, felt, not without reason, that the new observatory would duplicate the old one. Tresca wrote to Fizeau on 26 August 1874²⁷:

The report concludes by warmly embracing the idea of creating an observatory of physical astronomy. This reminds me of the story of Ampère, who made a hole for the passage of his little cat in order to reserve the main passageway for the mother. In any event, there will be a debate, and despite the result of yesterday, I don't yet consider myself entirely defeated.

²⁵ *CRAS 79 (1874), pp. 1018–1024.

²⁶ Here, Faye alludes to Kirchhoff's creation of a solar observatory in Berlin.

²⁷ Archives de l'Académie des sciences, files Tresca and Hippolyte Fizeau.

It was necessary, however, to wait a year before the president of the Republic, marshal Mac-Mahon, on 6 September 1875, signed a decree creating a new observatory, for which a sum of 50,000 francs was reserved for the initial installation in the 1876 budget. After envisaging its installation at Montmartre or (why not?) at Fontenay-aux-Roses, the observatory was temporarily set up on the Boulevard d'Ornano in Paris. There was talk of placing it definitively at Versailles, at Vincennes, or on Mount Valérien. Finally, however, Meudon was chosen as the site. Janssen established himself there on 13 October 1876, in a field still occupied by the military, until the land was finally allocated for an observatory in 1879.

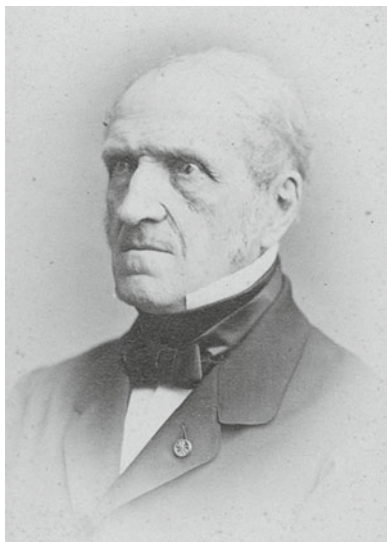
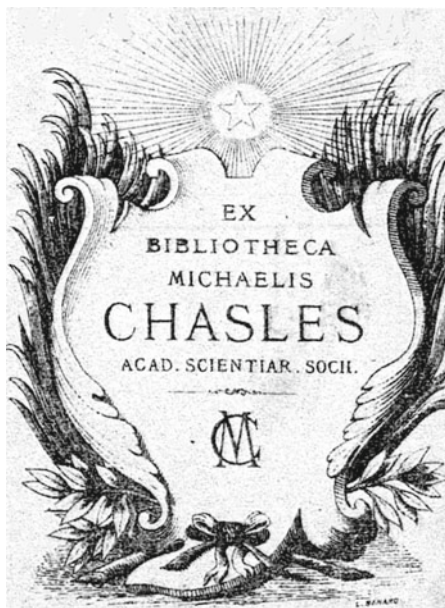
The creation of the Meudon Observatory of Physical Astronomy opened a new era in French astronomy. It would for a long time be completely independent of the Paris Observatory, and pursue very different scientific activity centering on visual, photographic and spectroscopic observations of the Sun, planets, and comets. It also boasted a laboratory for analyzing gases to permit the identification of the lines and molecular bands in the spectra of these objects. This category of observations progressively disappeared from the Paris Observatory. But, in 1926, the two institutions were reunited again, with each one continuing its own activity. Due to the First World War and a lack of interest on the part of the authorities, the number of personnel only increased by some 20 people. Astrophysics was endangered to the point that the government arising out of the Popular Front (a coalition of parties resisting the advance of fascism in France) created in 1936 the Astrophysical Institute of Paris, which had its observational site in Haute-Provence. At the present time, there appear to be only advantages to the reunion of fundamental astronomy and astrophysics, as the two disciplines are so mutually interdependent.

Interlude: The Chasles Manuscript Affair

On 8 July 1867, the mathematician Michel Chasles (Figs. 6.4 and 6.5) presented to the Academy of sciences two letters, concerning the foundation of the Academy, from the poet Jean de Rotrou to Cardinal Richelieu. He also mentioned that he possessed two others, from Rotrou to Pierre Corneille, in which Rotrou “predicts what the genius of the young Poquelin [Molière] will achieve.” There was nothing very extraordinary in that, except perhaps that this prediction was rather surprising. But the president of the Academy of sciences, the chemist Eugène Chevreul, asked Chasles if he would say something about a work of Pascal, which Chasles had mentioned some time ago, which indicated that Pascal had discovered the law of gravitation.²⁸ Now this was very interesting: if Chasles could bring effective proof that it was Pascal who discovered the law of universal attraction, Newton would be dethroned!

Indeed, Chasles presented to the Academy during the next meeting, on 15 July, several of Pascal's writings which, it was decided, ought to be inserted into the

²⁸ *CRAS 65 (1867), pp. 49–51.

Fig. 6.4 Michel Chasles (1793–1880)**Fig. 6.5** Ex-libris of Michel Chasles

Comptes rendus.²⁹ In particular, there was a letter of 2 September 1652 from Pascal to the English physicist and chemist Robert Boyle, followed by several notes. Here are extracts:

²⁹ *CRAS 65 (1867), pp. 89–93.

In celestial motion, the force acting in proportion to the masses and inversely proportional to the square of the distance is sufficient for everything and provides the basis for explaining all the grand revolutions which animate the universe....

A body, because of its tendency to move due to attraction impressed upon it, is capable of traversing a given space in a given time. Its initial velocity will therefore be proportional to the intensity of effort, or to the tendency impressed upon it by the attractive power; this intensity, in turn, will be proportional to the attracting mass at an equal distance, and, at different distances, to the attracting mass divided by the square of the distances....

We know the power of gravity on the earth, by the weight of falling bodies, and by evaluating the tendency of the moon toward the earth, or the departure from the tangent to its orbit, in any given time. This given, since the planets revolve around the sun and two of them (Jupiter and Saturn) have satellites, if we evaluate by their movements how much a planet tends toward the sun or departs from its tangent in a given time and how much various satellites depart from the tangents of their orbits in the same time, then we can determine the proportion of the gravity of a planet toward the sun and of a satellite toward its planet, comparing these to the gravity of the moon toward the earth and the distance between them....

One only needs for this, in conformity to the general law of variation of gravity, to calculate the forces that would act on these bodies at an equal distance from the sun, from Jupiter, from Saturn, and from the earth. Then these forces give the proportion between the masses contained in these different bodies. By this principle one finds that the quantities of material in the sun, Jupiter, Saturn and the earth are amongst themselves as the numbers 1, 1/1067, 1/3021, 1/169282.

Pascal.

According to this, Pascal had discovered in 1652 all that Newton was regarded as having discovered several decades later! Doubts soon arose, however. In the following meeting,³⁰ Constant Duhamel pointed out several inconsistencies in the writings presented by Chasles: in particular, he noted that the establishment of the inverse square law implies a comparison of the attraction of the Earth on the Moon and its attraction on a neighboring body, a comparison which implies that the Earth attracts bodies as if all its mass were concentrated in its center. But this fundamental property was first demonstrated only by Newton. For his part, Faye remarked that the first satellite of Saturn had not been discovered till 1655, 3 years after Pascal's notes³¹! Le Verrier reserved his own commentaries for the next meeting.

It is rather curious that nobody seemed to have noticed from the very outset a fatal flaw in the initial documents presented by Chasles: it is not velocity but acceleration that's proportional to the force exerted on a body, something Newton was the first to show. Without understanding this, it is impossible to go any further into the dynamics of planets and satellites.

There was a new dramatic turn of events at the next meeting: a specialist in Pascal, Armand Prosper Faugère, who had edited in 1844 the *Pensées* from the manuscript, announced that the specimens presented by Chasles were not even in Pascal's handwriting³²:

³⁰ *CRAS 65 (1867), pp. 121–135.

³¹ Also, the period of its revolution around the planet was only published in 1659.

³² *CRAS 65 (1867), p. 202.

My conviction in this regard is so complete, that I consider it a sacred duty to inform the Academy. As it is advisable to appoint commissioners who are clear on this essential point, I will do everything in my power to make available to them everything of value that I possess. They could also consult the autograph manuscripts of Pascal that are preserved in the imperial library.

Chasles was embarrassed by this declaration, but it didn't prevent him from suggesting that Newton had borrowed the ideas of gravitation from Pascal. Incredibly, he claimed to have proof of this in 10 years of correspondence between these two savants as well as in other letters he possessed.³³ But Faugère persisted, and the English physicist David Brewster contested the authenticity of the alleged letters of Pascal to Newton, finding no trace of them in the archives of the latter.

The subject was broached again in October 1867. In order to respond to certain objections, Chasles affirmed now, with proof in hand, that Pascal had already obtained his results in 1641... thanks to Galileo, whose letters he now exhibited. Finally, Le Verrier had something to say about all this³⁴:

To prop up the novel edifice he hopes to erect, various letters attributed to Galileo, Boullian, and Huygens were presented by him, in which appear the following results:

1. Galileo already had the idea that Kepler's ellipse could very well be due to an attraction varying as the inverse square of the distance, and he is supposed to have communicated this idea to Pascal;
2. Galileo is supposed to have sent to Pascal his own astronomical observations as well as various writings from Kepler, which served as the basis of Pascal's work;
3. Galileo is supposed to have discovered the satellites of Saturn;
4. Galileo is imagined to have constructed a powerful instrument: but his sight having become weak, he sent it to Pascal, who in turn forwarded it to Huygens. By means of these various resources, Pascal is supposed to have carried out his work, such as determining the masses of Jupiter, Saturn, and the Earth. For this, Galileo is supposed to have congratulated him in a letter of 7 June 1641; a letter in which the philosopher of Florence repeats, so that there can be no doubt, the values which Pascal was to have determined for the masses and densities of the planets.

But that's not all! According to a note about Galileo supposed to have been written by none other than Louis XIV himself, Galileo discovered the planet Uranus in 1639. Even the name Uranus was supposed to have been suggested by his grandson, Louis XVI, who was somehow made aware of the discovery. However, the position which Chasles gave for Uranus in 1639 was completely erroneous.³⁵

This extraordinary discussion was to continue until August 1869, and fills more than 400 pages of the *Comptes rendus*³⁶! Finally, it was Le Verrier himself who attempted to summarize the entire affair in 92 more pages of the *Comptes rendus*, by setting out all the objections against Chasles, especially those he himself had

³³ *CRAS 65 (1867), pp. 185–194. These other letters are from Pascal to Boyle, Hooke, Gassendi, Father Mersenne, Descartes, etc., and from Newton to Mariotte and to less known people.

³⁴ *CRAS 68 (1869), p. 1432.

³⁵ See *Flammarion (1911), p. 486.

³⁶ For a list of Chasles' publications in the *Comptes rendus*, see the index in *CRAS, table générale des t. 62 à 91 (1888), pp. 116–120.

discovered.³⁷ It was a considerable undertaking in which the scientist took on the role of the detective. Le Verrier discovered the source of the text attributed to Pascal: it had been copied from an article devoted to Newton in the *Histoire des Philosophes modernes* by Alexandre Savérien (vol. 4, 1764). In attempting to defend himself, Chasles declared that it was actually Savérien who had copied Pascal. There were also, curiously, extracts from the *Dissertation sur l'incompatibilité de l'attraction et de ses différentes lois avec les phénomènes* (Dissertation on the incompatibility of attraction and its different laws of the phenomena), an anti-Newtonian text by Father Giacinto Sigismondo Gerdil dating from 1754. Le Verrier summarized the exhaustive investigations of the paper and ink used in order to determine the authenticity of the pieces presented by Chasles. The chemist Balard, an enemy of Le Verrier, had participated. Here, Le Verrier got the better of him.

Finally, in no small part thanks to Le Verrier, the forgery was recognized. John Herschel sent his congratulations to Le Verrier in a letter written in French. Herschel found it hard to believe that a single individual could have produced the 27,320 items of supposed autographs, which were attributed to some 660 different personages, and which the naïve academician had acquired for the huge sum of 150,000 francs. But it was true. The originator was Denis Vrain-Lucas. Vrain-Lucas produced his forgeries in the office of the genealogist Letellier, who sold made-up family trees claiming descent from the nobility as fantasies for the bourgeoisie. He had accumulated in this way the prerequisite knowledge and talents of the forger. Sensing his prey, he introduced himself to Chasles, telling him he had just bought a considerable lot of old papers that had come over by way of America but were very badly damaged due to the sinking of the ship carrying them to France. He had just consulted an expert about what use he might make of this heap of useless papers. As a sample, he showed Chasles some very blemished letters eaten away at the edges. Chasles, recognizing Pascal's name on these letters, became very enthusiastic. This is how the affair began. Soon the forger was presenting himself almost every day at the home of the savant. The savant recounted to him the discussions taking place at the Academy. From these clues, the forger would furnish, in return for further payment, new specimens to confound the detractors. The repository from which Vrain-Lucas claimed to have drawn his documents was seemingly inexhaustible. He sold Chasles the most incredible documents, for example, a letter by Julius Caesar to Vercingetorix (Fig. 6.6). Among the most audacious pastiches, all of them written in old French, are letters from Alexander the Great to Aristotle, from Archimedes to Heron, a love letter from Pythagoras to Sappho, a petition from Lazarus, newly raised from the dead by St. Peter, etc. In the majority of cases, they were so conceived as to flatter the national ego, which could only please Chasles. Chasles was astonished to find all these letters written in French; but Vrain-Lucas, foreseeing this possible objection, confabulated that they had been assembled by Alcuin in an abbey in Tours, where Rabelais had then discovered them seven centuries later and translated and copied them. This caused their value to increase even more!

³⁷ *CRAS 68 (1869), pp. 1425–1433; 69 (1869), pp. 5–24, 72–95, 213–230.

Jules César au chef des Gaulois.
 J'envoie de ce jour un ami qui t'explique
 le but de mon voyage. Je veux couvrir de
 mes soldats la terre que tu ne dois pas. En
 vain que tu la voudras défendre tu
 es brave je le sçai car aussi le ferai
 moi tout au dix ans. rend moi tes
 armes ou prépare toi à combattre avec
 moi de plus Jules César

Fig. 6.6 Alleged letter from Jules Caesar to Vercingetorix. “Jules Caesar to the head of the Gauls. I send to you a friend who will tell you the purpose of my trip. I want to cover with my army the ground where you were born. It is in vain that you will try to defend it. I know that you are brave, but I will also be so, if it pleases the gods. So give me your arms or prepare to fight. VI of calends of July. July César” (*Flammarion (1911), p. 487)

The fraud was finally revealed by Chasles himself who, becoming increasingly doubtful, had the activities of the forger kept under surveillance by the police: a search of his house led to the discovery of the cache of materials he was making use of. His trial, taking place in February 1870 before the sixth correctional chamber of Paris, was the occasion of great amusement. Vrain-Lucas got 2 years in prison, and a 500 franc fine. Clearly, the one who was punished most by the whole affair was the hapless Chasles, whose reputation never recovered.

It might seem strange, given that the fraud was so evident from the very beginning, that Le Verrier took so many pains, and that the Academy wasted so much paper and expended so much ink, on the affair. It's true that Chasles was a well-known and highly respected savant, a very amiable person whose prestige had to be considered. Other academicians, moreover, had been almost as credulous as he, especially those who were not astronomers. The whole affair created a sensation. It was not unlike the case of the false Vermeers produced during the second World War by Han van Meegeren, a forger who was much more skillful than Vrain-Lucas: the specialists were fooled by him, even though the quality of the canvases by van Meegeren appeared at first sight far inferior to those of the authentic Vermeers. Here the swindling was on an entirely different scale, since an estimated 25–30 million

dollars were extracted by van Meegeren from the overly credulous buyers, most of them Nazis and including Hermann Goering.

The scientific fraud of Vrain-Lucas was not the only one to be unmasked with difficulty: recently we have seen those of cold fusion and water memory, and it is possible that a small part of the scientific community even now is persuaded that those two cases were not fraudulent.

Resignation and Recall

We return now to events at the Observatory. The situation of the staff had become unbearable. The crisis with Le Verrier finally came to a head in January 1870, with the mass resignation of the 13 titular and adjunct astronomers. These took advantage of the circumstance that on 2 January a new “liberal” ministry had come to power under Emile Ollivier, who nominated Emile Alexis Segris as Minister of Public Instruction. The latter received on 1 February from the astronomers of the Observatory a text printed in quarto, 16 pages long, summarizing their grievances and ending by announcing their resignations.³⁸ Here are a number of extracts:

During the last fourteen years, M. Le Verrier has been absolute master in the Observatory.... M. Le Verrier did not want to surround himself except with Daubentons.³⁹ Not being able to achieve this by normal means, he employed stratagems, and where necessary force. Before coming to the Observatory, he had hardly practiced observational astronomy: he was ignorant of the qualities and defects of the instruments at his disposal, and of the numerous and delicate adjustments needed to make good use of them. He only kept close to a small band of Arago’s students whom he regarded as needful to his plans. He received instruction from them, and had them write down what had to be done to make observations. As soon as he had taken what he needed from them, his only goal was to expel them by visiting them with outrages and insults.

It was thus that M. Yvon Villarceau, his first instructor, saw himself removed from all the services-- meridian room, equatorial, geodesy--into which he had initiated M. Le Verrier; and he was finally put out, in fact, from the Observatory by the following means. M. Y. Villarceau locked his office with great care whenever he left it. One day he found it wide open. He locked it back up, but hardly had he left than the doors opened again, as if by themselves. These goings-on lasted eight days, and as M. Villarceau did not give up the place, one morning he found his office had been invaded by numerous functionaries each at his own table. All his papers had been carted away⁴⁰....

M. Le Verrier acted in more or less the same manner with all the savants that he flattered with false promises.... All initiative was strictly forbidden at the Observatory, and the

³⁸ *Mémoire sur l'état actuel de l'Observatoire impérial, présenté par les astronomes à son exc. le Ministre de l'Instruction publique*. Paris, Lahure, 1870. Accessible via <http://www.e-rara.ch/doi/10.3931/e-rara-3100>.

³⁹ Daubenton had an uncritical attachment for Buffon, borne of admiration and respect, and lived in his shadow until the latter’s death in 1788.

⁴⁰ Yvon Villarceau had already addressed a printed complaint to the Emperor, which was probably distributed more extensively, where he details these exactions: BOP, Documents divers sur l’Observatoire de Paris, 1854–1872, cote 3567 (3), folder T.

Chief, reserving it himself alone, when convenient claimed the right to take up an idea and to impose it on everyone except, of course, the very person who was the author of it. Personal work, if it couldn't be completely eliminated, was usually persecuted with special severity.

One understands that when, during fifteen years, so many distinguished men have gone through such a great institution, and have been intellectually pressured by the director, to his exclusive benefit, to attribute a share of their work to him, the amount of it would, in the end, prove to be quite considerable. M. Le Verrier, it is true, has carried out great work at the Observatory; his administration has been productive for science. But one must make distinctions. The renown acquired by M. Le Verrier in this way might have been useful to him if it could be maintained indefinitely. But has science itself, and what it has won through so much collective effort, been so well-served? Everywhere else, one finds around men of science a seed-bed of young disciples, who count it a high honor to perpetuate the traditions of the master. But at the imperial Observatory the observers continue nothing; they come, and they go. Where can one find a single astronomer who calls himself a student of Le Verrier?

The document went on to analyze the obstacles to work occasioned by Le Verrier's attitude. It discusses the "disorganization" in the provision of supplies and in the administration of finances. As for the library, it was found to be in a state of the "most complete disarray," lacking in even the most indispensable works. "It does not even get the most important English astronomical journal – the *Monthly Notices* [of the Royal Astronomical Society], although it would take nothing more than for M. Le Verrier to request it from the Royal Astronomical Society." Finally,

It must be said that the Scientific Association of France, a private society founded in 1865 by M. Le Verrier, is quartered in the observatory itself. Moreover, expenses for gas, oil, candles, and carbon consumed during the meetings of the Association, and the meetings of its councils and commissions, are all supported by the Observatory. The administration of the Scientific Association absorbs, we won't say *all* of M. Le Verrier's time—this is his personal affair, and we do not concern ourselves with such things—but the efforts and labors of several Observatory employees.... The supplies of the Association and those of the Observatory are mixed and confounded.... The Association is the primary cause for the profound disorganization of the secretariat of the Observatory, and this an impediment to the progress of the other services.⁴¹

The document concludes with the resignation of the astronomers:

By remaining longer in the situation presented to them, the astronomers would share, despite their protests, responsibility for the ruin of French astronomy. The dictates of honor require of them to place the responsibility where it belongs. With profound regret, they carry out this ungrateful task, but with the calmness and assurance that comes from having fulfilled their duty.

Signed:

MM. Y. Villarceau, chief of the geodesy service.

Marié-Davy, chief of the astronomical physics service.

Wolf, chief of the equatorial service.

Læwy, chief of the meridian service.

André, adjunct astronomer, equatorials.

⁴¹ This was probably true, but the astronomers were ungrateful. They had forgotten that lobbying through this association had raised 395,000 francs for the large reflector and refractor. However, the main purpose of the association was to support meteorology, which was not to everyone's taste at the Observatory.

Folain, adjunct astronomer, meridian room.
 Fron, adjunct astronomer, meteorology.
 Leveau, adjunct astronomer, bureau of calculations.
 Lévy, adjunct astronomer, secretary of accounting.
 Périgaud, adjunct astronomer, meridian room.
 Rayet, adjunct astronomer, meteorology.
 Sonrel, adjunct astronomer, astronomical physics.
 Tisserand, adjunct astronomer, geodesy.

Even though everyone signed, they did not all do so with equal enthusiasm. Interviewed by Bigourdan in 1888, Gustave Leveau declared⁴²:

Concerning the memorandum of the astronomers of 1870, Villarceau signed, but without having pushed anything too hard. He was too restrained for that. Those who pushed hardest were M. Lœwy and Marié-Davy, who had the support also of Delaunay and Cie [that is to say, the Bureau of longitudes].

As for the facts presented in the memorandum, they were all true: Le Verrier did not refute a single one.

Even before receiving this pamphlet, the Minister of Public Instruction had already been made aware of the situation at the Observatory. The course of events is described with precision in the memoirs of Emile Ollivier, the then Prime Minister⁴³:

With profound conscientiousness, [Segris] informed himself on all points of view before coming down on either side. Above all he wanted to hear explanations from Le Verrier himself. The latter requested an inquiry [hoping in this way to gain time]. Meanwhile, all of the service chiefs of the Observatory and all the astronomers appeared at the secretariat of the Ministry and deposited their memorandum with their resignations (1 February [1870]).

'I cannot conceal the fact,' wrote Segris to Le Verrier in announcing to him this news, 'I felt great pain at seeing such a great institution as the Observatory descend into such a state of disarray; I am also much preoccupied with the urgent necessity of suggesting a prompt remedy. You have expressed the desire to be received by me next Saturday 5 February; you can be assured of finding me in my office at 8:30 in the morning.'

At the end of the meeting [of the Senate] on 2 February, the same day on which Le Verrier had been notified of the appointment given to him by Segris, Guyot-Montpayroux demanded that the Government be asked about the current situation at the Observatory. Segris replied in measured terms that that very morning he had appointed a commission composed of two superior officers of the department of the marine, members of the council of the admiralty, four savants and an inspector of finances, in order to investigate the causes for the resignation of the personnel and to carry out an examination of the accounts and inventories. Upon returning to the ministry, Segris learned that Le Verrier, without waiting for the appointment of the morning of 5 February, and abusing his position as senator, had just in that day's session deposited an interpellation [Fig. 6.7] in which, by virtue of the decree of the Senate of 8 September 1869, he demanded that the Government give explanations 'about the incidents related to the administration of the imperial Observatory.' In this way the minister had been called by his subordinate to the bar of the Senate, and was required to furnish explanations, when it was rather *he* who had the right to demand them. The minister resented the impertinence, and put down the revolt. He informed Le Verrier that very evening not to come to the scheduled appointment [Fig. 6.8] and had the Council of the Emperor approve an order for his removal [see the letter of revocation from Segris to Le Verrier at the beginning of the chapter].

⁴² BOP, Documents divers sur l'Observatoire de Paris, 1854–1872, cote 3567 (3), folder AP.

⁴³ *Ollivier (1895/1915): see t. 12 (1908), pp. 526–530.

Any terme du Sénat
 Consulté du 4 septembre 1869
 Le Sénat a demandé à
 interpellé le Gouvernement
~~sur les~~ ~~projets de~~ ~~lois~~ ~~relatifs à~~ ~~la~~ ~~gestion~~ ~~des~~ ~~affaires~~ ~~de~~ ~~l'Observatoire~~
~~impérial et sur ceux de~~
 Paris le 4 février 1870
 Le Verrier

Fig. 6.7 Draft of the request for interpellation of the minister at the Senate, written with an angry hand by Le Verrier

By striking in this way a highly placed personage, reputed to be in the imperial favor, it would be made clear that a lack of respect for the rules of order and hierarchy would not be tolerated from any quarter. Le Verrier tried in vain to justify his conduct to the Senate. The straightforward and fair explanations of Segris, heard with great sympathy, were confirmed by a fair and simple vote, and passed by a huge majority. The measure received practically universal approbation; Le Verrier's insupportable character had finally overshadowed his great scientific value.

The preceding paragraph alluded to the Senate's session of 8 February 1870. The interpellation of Le Verrier having been accorded by the president of the Senate, Le Verrier had the audacity to present himself before this assembly, knowing full well that he had already been removed from his post. His angry discourse is of little interest to us now, and was hardly listened to then.⁴⁴ When Segris concluded his reply with the words, "When a removable functionary in his capacity as senator wants to directly question the Government and the minister who are his superiors concerning the facts of his own service, I say that this is intolerable," numerous senators chimed in, "True, true!"

⁴⁴ This discourse is in Documents divers sur l'Observatoire de Paris, 1854–1872, 3567 (3), folder AL; extensive extracts in Levert et al. (1977).

Paris, le 14 février 1870.

Cabinet
DU MINISTRE
DE L'INSTRUCTION PUBLIQUE.

Monsieur le Directeur,

J'apprends à l'instant que vous avez eu devoir déposer aujourd'hui au Sénat une demande d'interpellation adressée par vous au Gouvernement sur les faits de l'Observatoire.

Cette circonstance m'oblige, Monsieur le Directeur, à ajourner la conférence qui devait avoir lieu demain, à 8 heures 1/2 du matin, dans mon cabinet, et que je m'étais empressé de fixer au 5 février, suivant le désir que vous m'aviez exprimé par votre lettre du 29 janvier dernier.



Recevez,

Monsieur Le Verrier, Sénateur,
Directeur de l'Observatoire.

M^e 1042 n° 31

Recevez, Monsieur le
Directeur, l'assurance de mes
sentiments de haute considération,
Le Ministre
de l'Instruction publique.

Segris

Fig. 6.8 Letter of the Minister of Public Instruction to Le Verrier, canceling the appointment for the next day

Owing to the actions of Le Verrier, who had effectively shot himself in the foot, events did not transpire as he anticipated. His removal from his position had taken place even before the commission created by the Minister of Public Instruction to give an opinion on the matter had time to meet. It would meet nonetheless, though its opinion would serve no purpose since the problem had already, in a sense, resolved itself.⁴⁵ But it is interesting to give some extracts from the commission's report,⁴⁶ because it clarifies the situation at the Observatory:

Since the decree of 3 April 1868 [which had attempted to limit Le Verrier's power], the regrettable acts against personnel ceased; no new complaint was raised on this head, but work slowed and progressively became more disorganized, despite the complaints and protests of the astronomers, and finally the latter were brought to the point of offering Your Excellency their collective resignation, 'to fulfill,' they said, 'their duty at the risk of ending their careers, by foregoing the fruit of long labor which for several of them approaching retirement age they could not begin over again.'

The failure of the effort to reorganize undertaken by M. Duruy could not be imputed to the decree of 3 April and to the regulations which followed: this decree had not been seriously applied; the regulations had not been carried out.... Once the decree had been signed by the Emperor and the rules approved by the Minister, M. Le Verrier still had the right to protest by resigning. But, by remaining as director of the Observatory, he accepted these enactments, and his duty was to conform to them.

The commission, after careful examination, does not feel in a position to propose any changes to the arrangements of 1868....

In sum, Mr. Minister, the decree which relieves M. Le Verrier of his functions as director of the Observatory anticipates the proposal which we ourselves would have made, being convinced of the necessity of this measure to restore order to this Observatory which is so profoundly troubled, so as to reconcile the personnel, and to restore its productive activity in scientific work.

We ask you, as a corollary of this decision, not to accept the resignations of the astronomers, who have taken up again their work with an ardor which we believe augurs well for the future.

Taking as our example the commission of 1867, we feel we have to insist particularly on the complete separation of the meteorological service from the Observatory, to take effect as soon as possible.

Hearing of Le Verrier's dismissal, Flammarion was jubilant, and congratulated Segris⁴⁷:

Mr. Minister, your ministry has just rendered the most eminent service to science. In relieving M. Le Verrier of his duties as director of the Observatory, French astronomy is at last permitted to establish itself on the solid basis it deserves, and to raise itself in an atmosphere that is serene and peaceful. Permit me to address to you sincere congratulations and profound thanks from a friend of science for this act of courageous justice. Having myself passed four years under the ominous shadow I could feel full well its fatal influence. For

⁴⁵ This commission has almost the same composition as that of 1867: admiral Fourichon, rear-admiral Baron Didelot, Liouville, Serret, Briot (professor at the Faculty of sciences), Breguet (the famous instrument builder) and Maisonneuve.

⁴⁶ Documents divers sur l'Observatoire de Paris, 1854–1872, 3567 (3), folder AM.

⁴⁷ Version of *Ollivier, (1895/1915), t. 12 (1908), pp. 526–530. That given by *Flammarion (1911), p. 522 is slightly different.

several years now, I have declared war against the dictatorial egoism. Now that which was not yet completely achieved by the sage and hardworking Duruy you have freely given to France. Rest assured, Mr. Minister, that the deed which you have accomplished will resound gloriously throughout all of Europe, and will merit a page of gratitude in the annals of astronomy. And remember that I am cordially devoted to the high Minister who knows how to put the general interests of the 'Republic' above all the petty quarrels of parties. I have the honor to be, etc.

Camille Flammarion.

Not everyone took that side, of course. In particular, several foreign astronomers, who knew Le Verrier well and did not know all the details of what had taken place in Paris, wrote to him confirming their admiration and support. Among these in particular was Otto Struve. It was necessary to provide for the transaction of day to day business. The rear-admiral Penhoat, president of the imperial Commission of the Observatory established by the decree of 1868, was named director for the interim. Apparently things did not go very well, as testified by numerous letters exchanged in February 1870 between the admiral, Le Verrier, and certain staff members.⁴⁸ A seal was placed on all the scientific documents. Le Verrier's files, however, were found to be empty. He claimed that the contents belonged to him personally, but in the end returned several documents to Penhoat. The latter had less difficulty obtaining documents in possession of the various service heads. When he saw that this was the case, Le Verrier also began to return more documents. Penhoat also faced some problems with the observatory in Marseilles. These vicissitudes remind one of what often happens when a ministry changes hands. But the interim director stood in for only a short while. On 3 March 1870, the minister appointed a new head of the Observatory, who was none other than Verrier's long-time enemy, Delaunay.

The Brief Reign of Delaunay

Delaunay wrote to his mother 2 days after his nomination: "I am received as a savior." But it was not long before he was singing a different tune. He confided on 19 March: "My tranquility is lost, and I don't know when I will be able to recover it.... The ministry must come to my aid.... I have found here a veritable chaos, and in addition, a staff that is overexcited and even a little too demanding."⁴⁹

Those in high places remained very circumspect, hoping especially that there would not be new problems at the Observatory. Moreover, a new Commission was created 11 April 1870 to keep an eye on the institution.⁵⁰ It was presided over by vice-admiral Touchard, and included also rear-admiral Didelot, Laugier, Faye, Serret, Briot, Delaunay, who participated as director, and Du Mesnil, chief of the division of the Ministry, secretary.

⁴⁸ These letters and drafts are preserved at the BOP, MS 1060-1.

⁴⁹ Cited by Danjon (1946c), p. 380.

⁵⁰ BOP, MS 1060-1.

The War and the Commune

All this occurred during a very troubled period. There were worker revolts of Marxist inspiration that the Ollivier government repressed with firmness. However, distressed by the growing independence of this government, an independence which could lead to a constitutional empire he did not want, Napoléon III decided to solidify his authority by calling a plebiscite on 8 May 1870. It was a personal triumph, and in announcing the results, he offered the view that “the Empire finds itself strengthened at its foundations,” and “more than ever it is possible to envisage the future without fear.” On 30 June, Emile Ollivier declared that “at no other time had the maintenance of peace in Europe been more assured.” Two weeks later, war with Prussia broke out. Only a couple of months sufficed for the collapse of a regime that had hitherto appeared unshakeable.

The French forces were no match for the Prussian forces. They soon fell into disarray, and what became an utter rout was consummated at Sedan, where the Emperor himself was taken prisoner and capitulated on 2 September 1870. On hearing of the capitulation, Léon Gambetta, Jules Ferry and a number of others proclaimed the Third Republic on 4 September, and formed a government for the national defense. The men who came to power were moderate republicans. The new government was presided over by general Louis Trochu, governor of Paris; among the ministers, Gambetta and Henri Rochefort were the only partisans favoring a socialist republic. Paris lay under siege by the Prussians beginning on 19 September, and the population, isolated, suffered from famine. The government was pessimistic but Gambetta wished to continue to fight: he left Paris by balloon on 7 October in order to organize the resistance, first at Tours and then at Bordeaux. However, defeats accumulated, and the government had to ask for an armistice on 28 January 1871, the date that marks the end of the siege of Paris. The treaty of Frankfurt, signed on 10 May 1871, established the constitution of the German empire, which had been proclaimed at Versailles on 28 January. By terms of the treaty, France ceded Alsace and Lorraine.

The national Assembly, elected on 8 February 1871, was dominated by monarchists, and Adolphe Thiers was named chief executive. But the revolutionary forces hostile to the capitulation were not in agreement with these arrangements. They rose up on 18 March, and formed an insurrectionist government, the *Commune*. This was overthrown following a new siege of Paris by the army of Thiers’s government, which had been based in Versailles since 10 March. The burning of numerous public buildings, including the palace of the Tuileries and the Hôtel de Ville, and other depredations of the *Communards*, led to an extremely bloody suppression during the week that followed the entry of the Versailles troops on 21 May. Some 25,000 people were killed. The revolt came to an end on 27 May. Although the Commune had been disavowed by all the bourgeoisie, even the most liberal, France remained profoundly divided over these events.

Naturally, no reforms could be undertaken at the Observatory while things were so unsettled in France. The Observatory itself was relatively spared by the Commune,

in part, no doubt, because, the instruments, and even the tube of the 38 cm refractor, had been dismantled on 19 September 1870 due to the siege of Paris and placed in storage. Only a portable meridian circle by Rigaud and some documents were destroyed by a fire that was quickly put out. It did not cause, as Yvon Villarceau asserted, “frightful ravages.”⁵¹

The Observatory Tries to Reorganize Itself

In the words of Bigourdan⁵²:

France, devastated by the defeat, dismembered and finished off by the civil war, turned inward upon itself and meditated upon the causes of its reverses; then, in all directions of its activity, it applied the reforms capable of restoring to it its place among the nations.

Affairs having simmered down, Delaunay busied himself with trying to resume observations. For this, a budget was needed, and he requested one through Jules Barthelemy Saint-Hilaire, the secretary of Thiers: no doubt the Minister of Public Instruction, his usual counterpart, had done nothing, or lacked the necessary powers. The request⁵³ was for an assortment of funds: astronomy and general expenses 128,060 francs; meteorology 25,000 francs; annual salary of the director 15,000 francs (in England, the analogous position paid 25,000 francs, while Le Verrier had approached 30,000 francs); other salaries 12,000 francs⁵⁴ (according to Delaunay, “the Observatory has been given the luxury of a general staff, to the detriment of an army of workers; but these are positions that have been acquired and must be respected”); station [meteorological] of Montsouris (“if it is to be maintained”⁵⁵) 5,000 francs; inspection of meteorological stations throughout France, including the cost of making the rounds, 10,000 francs (“this inspection will be very useful”); total 220,000 francs in round number. But nothing came of this, and Delaunay had to write several letters of petition, to which Barthelemy Saint-Hilaire responded only evasively. Here is an example⁵⁶ (dated 27 December 1871) in which Delaunay suggested that Le Verrier had intervened underhandedly, a suspicion we shall soon confirm:

Concerning astronomy, everything has been studied and elaborated, by various diverse commissions comprised of the most competent men. It is a question of emerging from the anarchy which I found upon entering the Observatory, almost two years ago. It is a question

⁵¹ *CRAS 72 (1871), pp. 611–612.

⁵² Bigourdan (1933), p. A.65.

⁵³ BOP, MS 1060-1.

⁵⁴ This figure is much too small. The requested total of 220,000 francs is inexact; but if one considers it as correct, the total sum for the salaries apart from that for the director is 37,000 francs, which looks more reasonable.

⁵⁵ There was indeed a meteorological station at Montsouris, distinct from the meteorological service of the Observatory: see later and Chap. 9.

⁵⁶ This text and the following ones are at the BOP, MS 1060-1.

especially of reconstituting the personnel of the Observatory. Out of six adjunct astronomers that we need, we have only four; out of six assistant astronomers, we have only one. There's no astronomer at the Toulouse Observatory; and we need some for the Bordeaux Observatory, where the city has just agreed on a budget, etc.

As to meteorology, the question isn't any simpler. It all comes down to this: the financial condition of France does not permit supporting in Paris two distinct meteorological establishments. Like all sovereigns,⁵⁷ M. Thiers is enveloped by intriguers, who are skilled at flattering the person in power, whoever it is, and knowing how to worm their way in and take advantage of all opportunities, to mask their demeanor under the most deceptive appearances, using nice words, protestations of devotion, disinterest, etc., whereas at root their self-interest is the only motive of their conduct.

For me, I can only hold firmly to my post. I have truth on my side, reason and justice; I am not afraid to speak it out loud, and I feel fortified in that. Am I asking anything for myself? I have been appointed to a post I did not seek, and in which I have lost tranquility, freedom, the possibility of devoting myself to my preferred labors. But it is a position of honor that I shall not abandon.... The intriguers who are fighting against me, I name without fear:

M. Le Verrier, a person of great merit without doubt, but at the same time the most perverse person in the whole world. His hatred of the living obstacles that he has encountered on his path, of the Bureau of longitudes, and of me in particular, will only end with his death. In the meantime, he will never cease to pursue his grudges. He has done much to kill astronomy in France, and he will do all that he can to prevent it from lifting itself up under our efforts, and above all, under mine.

Barthelemy Saint-Hilaire replied from Versailles 31 December, on letterhead stationary from the president of the Republic:

My dear sir and associate, I placed before the eyes of Mr. President of the Republic all the letters you have addressed concerning the Observatory. Mr. Thiers has the most lively interest, as you know, in science and in the Observatory; but so far the imperative occupations of his position have prevented him from giving the attention to this important question it deserves. Rest assured, etc.

Delaunay protested again on 12 February 1872; Saint-Hilaire replied the next day that "the question of the Observatory would be exhaustively discussed." Finally on 29 March, Jules Simon, the Minister of Public Instruction, summoned Delaunay to the Observatory. Finally Delaunay had a budget.

It was also necessary to draw up new regulations for the Observatory, as a replacement of those of 1854 and 1868. On 9 August 1871, the Minister invited the Bureau of longitudes to offer an opinion based on the proposal for reorganizing the observatories of Paris and Marseille by the Commission named on 11 April 1870, shortly after Delaunay's nomination. The decree authorizing the reorganization would take some time to work through, however, owing to disagreements between the Bureau and the astronomers.⁵⁸ The "overexcited" astronomers at the Observatory were not even in agreement among themselves, and several of them remarked on the proposal, as did marshal Vaillant, who contributed his two cents' worth even though he was no longer part of the Bureau. Wolf thought it would be a good idea if the Observatory were regarded as a "school for astronomy," but Faye disagreed. As the

⁵⁷ Thiers was then the President of the Republic.

⁵⁸ See Bigourdan (1933), pp. A.73–A.81.

ministry had not consulted the astronomers at the Observatory itself, the Bureau of longitudes asked Delaunay “to invite the titular astronomers to make known their views in writing,” which Wolf, Lœwy, and Marié-Davy did. After give and take between the Bureau and the Commission, they finally came to an agreement. On 27 September 1871, the Bureau adopted the draft of the decree.⁵⁹

However, it was then found that “astronomers [Le Verrier, no doubt, who was perhaps accompanied by others] had approached M. Thiers, and the Bureau [of longitudes] regrets that the astronomers of the Observatory did not appreciate how much the Bureau’s proposal was favorable to their interests.” Finally, the decree appeared on 5 March 1872, after 5 months delay: it separated the observatory Marseille from that of Paris, and gave the latter four titular astronomers including the director instead of the three the Bureau had supplied. The role of the Bureau of longitudes relative to the Observatory was further diminished to only giving advice about the nomination of the director and the titular and adjunct astronomers. The meteorological work was kept at the Paris Observatory, but Delaunay, already weighed down by all that he had to do to continue the astronomical work of Le Verrier, whose importance he never doubted, obtained from the ministry a new decree signed 15 June 1872, which separated the meteorological observatory of Montsouris from the Paris Observatory. It remained, however, an annex. This had the merit, at least, of separating functions if it did not quite manage to soothe hard feelings.

The last article of the decree stipulated:

Article 11:--Each year, the Observatory will be inspected by a Commission composed of the members of the Bureau of longitudes who are not part of the staff of the establishment, two members of the Institute designated by the Academy of sciences, and five people chosen by the Minister from the great bodies of the State. This Commission will convene, at the Observatory, the first Wednesday of the month of May; it will nominate its president and secretary. It will visit the establishment, hear the director’s explanations, and present to the Ministry a detailed report concerning the personnel, the materials, and the state of the work and its publications.

It was during the first meeting of this Commission of inspection that Delaunay presented a report on the situation at the Observatory, a report that was published 31 May 1872.⁶⁰ One sees therein that the program of observations had not changed since Le Verrier, but that the Observatory was to take up again the work of “geodesic astronomy,” i.e., measurements of longitude and latitude across the French countryside, which had for a while been dormant. Meridian observations, measurement of the stars of the Lalande catalogue, continued. The reasons for this work were reemphasized, as they had, no doubt, been somewhat forgotten:

⁵⁹ BOP, MS 1060-1. The Bureau proposes the following annual salaries for the personnel: For Paris, director 15,000 francs, two astronomes titulaires 8,000, six astronomesadjoints 3,500–5,500, head of the bureau of calculations 5,500, aides-astronomes and calculators 1,500–2,500; For Marseille, director 8,000, one astronome adjoint 4,500 or 5,500, two aides-astronomes 1,500–2,500.

⁶⁰ *Observatoire de Paris. Rapport présenté à la Commission d’inspection par le directeur de l’Observatoire*, 31 May 1872, 12 p., Paris, Gauthier-Villars.

The comparison of new observations with the positions given by Lalande, ... the definitive goal of the work, has as its object to discover any singularities that the starry sky presents (proper motions, changes in brightness); it also alerts us to accidental errors that have been committed by the observers, and allows them to be corrected in light of new observations.

There were some changes in the instruments of the Observatory. The alt-azimuthal circle constructed by Reichenbach for Laplace in 1811, which was situated in the small observatory of the terrace, had been taken down at the beginning of the siege of Paris. It was not replaced but instead installed in a glass showcase as a “historical instrument currently out of use”: it was the first element of a museum which would be developed later by admiral Mouchez. There was also the matter of taking down for good the large equatorial of Brunner, whose objective had deteriorated and was no longer of any use, and even of taking down its large dome which had been damaged by projectiles during the battles of the Commune, “in order to make way for some instruments of smaller dimensions; the idea of installing such a powerful instrument at the top of the tower had been given up.” This was a judicious remark, given the defects in conception of the whole plan. By contrast, two new instruments were put into service in March 1870: the siderostat of Foucault and the 40 cm reflector of Eichens-Martin.

Meteorological and magnetic measurements were not forgotten. The former continued without interruption during and after the war, and a monthly *Bulletin of the [meteorological] observatory* was created on 1 January 1872 to summarize each month “the progress that had been made and that was important to carry out: it contained also a monthly summary of the meteorological observations carried out by the French stations.” As to the measurements of the Earth’s magnetic field, which had been more or less abandoned, these were taken up again, using not only the old instruments of Arago⁶¹ but also new recording instruments.

Finally, if Le Verrier had on his arrival seemed concerned with the poor condition of the lodgings of the astronomers at the Observatory, in practice he did little to remedy the situation. Delaunay announced that he had begun an initiative to improve the situation. Two of the new rooms were to be occupied by Lœwy and Folain, an adjunct astronomer who was the oldest of the observers, and it was planned to give to other people six smaller lodgings. Moreover, work offices were reserved for the astronomers that did not lodge at the Observatory. This was new. The workshops, offices and laboratories were improved. Finally, “during the long months of the Paris siege, the Director of the Observatory occupied himself by putting the library of the establishment in order and sorting through the significant collection of manuscripts that it housed”: we have seen that this had hardly been a priority for Le Verrier. Also, “there was a reading room for the members of the Bureau of longitudes and for the personnel of the Observatory, as well as outside persons who had received authorization. The library was open each day, apart from holidays, from 10 o’clock till 4 o’clock,” something that was entirely new.

⁶¹ See Lequeux (2008), pp. 331–336.

The Death of Delaunay

Delaunay had accomplished this much, but would not have time to do more. On 5 August 1872, less than 3 months after the Commission of inspection of the Observatory met, he drowned in the harbor of Cherbourg during a boat outing in bad weather. The *Phare de la Manche*, a journal that had favored Le Verrier's candidacy in 1849 for the legislative elections but had later withdrawn its support, wrote on 8 August⁶²:

A telegram this evening informs us that four people capsized in the harbor of Cherbourg, and that one of these four persons was M. Delaunay, director of the Paris Observatory.

His death is a great misfortune: with one blow of fate, France has lost a good citizen, society has lost an honest man, and science has lost a researcher that cannot be replaced. M. Delaunay was not yet 56 years old....

We owe an eternal debt of gratitude to M. Delaunay for the firmness with which he always fought against the dictatorial excesses of M. Le Verrier: he knocked down with all the force of reason and right this autocratic senator, and his merit is that much greater as his natural disposition was modest and retiring.

Delaunay's sudden disappearance from the scene shocked the scientific world, and even the political one, and occasioned a new crisis in the affairs of the Observatory.

Delaunay has often been reproached for not having restored the Observatory. But these criticisms fail to recognize the difficult situation in which he found himself. It was not his intention to accomplish vast transformations, but simply to improve the conditions of work for the staff, and he was unstinting in recognizing the credit that Le Verrier deserved for improving the instruments and methods of work. Yvon Villarceau addressed this matter in his draft for a funeral eulogy,⁶³ which he never gave because Delaunay was not buried in Paris:

Full of respect for Arago's memory and with sympathy for his family, who had thrown such a vivid splendor on French science, a stranger moreover to the work being carried out in the observatories, M. Delaunay would never have supposed that his predecessor had improved the state of the Paris Observatory; but, in becoming director in turn, he saw more and more evidence of the reality of the progress carried out since the directorship of Arago. We have heard him many times attest to his support for the various innovations introduced by his prudent predecessor. One will not be surprised therefore to learn that the two supposed rivals have recently agreed to ask the government for the means of continuing the astronomical and geodesic work necessary for the progress of science.⁶⁴ These undertakings must be carried out through cooperation between the Bureau of longitudes and the Observatory. Alas! Pitiless destiny has snatched away from Delaunay the collaborative role he had accepted!

⁶² Cited by Levert et al. (1977), p. 164.

⁶³ *Annales des Mines*, 7^e série, vol. 2 (1872), accessible by <http://www.annales.org/archives/x/delaunay.html>.

⁶⁴ This is a surprising assertion, knowing that Le Verrier had intervened underhandedly against Delaunay. There is no other trace of this alleged agreement between the two men. Yvon Villarceau was perhaps trying to insure his future by marrying the goat with the cabbage, so to speak.

Fig. 6.9 Paulin Talabot
(1799–1885)



Meanwhile, what had Le Verrier been up to during this period? We know what he was doing almost on a day to day basis thanks to the journal of his daughter Lucile⁶⁵:

10 February 1870 [5 days after Le Verrier's dismissal]. Tomorrow, we will leave. We will go to rue des Saints-Pères, 1, in a lovely apartment overlooking the quai Voltaire.

8 May. My father is not with us, he has gone home to [la Manche] to work on this terrible plebiscite [the vote took place on the same day].

14 August. [War had been declared on 19 July]. My father has joined the national guard.

1 September. We are in Limousin, several leagues from Limoges, with Mme Gavarni, wife of the illustrious caricaturist and the niece of Mme Talabot.

9 September. [The Republic had been proclaimed on the 4th.] Papa has been here since Monday evening.

21 September. [The siege of Paris had commenced on the 19th.] We leave post-haste for Marseille, the chateau du Roucas Blanc.⁶⁶

31 October. There arrived from my father [who is probably still in Limousin] menacing letters from Marseille, saying that we were Croesuses...

6 November. We found out at the chateau that there was a good deal of anger at Le Verrier in the town, we were called greedy imperialists, and we were to be apprehended... As we were magnets of danger, it was necessary to follow my parents, who were very frightened. And for good reason. The train took us as far as Toulon. But there, beside the fact that the town was full of sailors, a circumstance that rendered the stay difficult for me, there had been many arrests. Thus, there was no security for my father. [Therefore they again departed, this time for Aix-en-Provence.]

12 November. Mama is troubled by a letter that my father sent from Tours [where the government of the national defense was situated, of which Gambetta was the minister of War], and where he demonstrated a means of communicating with the Parisians by sunlight during the day and by electric light at night, if our army managed to advance further after

⁶⁵ Le Verrier Lucile (1994).

⁶⁶ This was a superb chateau that still exists on the Roucas Blanc hill, in the heart of Marseille, property of Paulin Talabot (Fig. 6.9), chief engineer of the Bridges and Roads, an inspired businessman who had created the Paris-Lyon-Mediterranean (PLM) company of railroads, of which at the time he was the general director.

this victory [Orléans, taken back from the Prussians]. I would be very happy if this proposal were considered, and if my father were tasked with its execution. He would make thereby his contribution to the national defense, and resume a place worthy of him.

4 December. I am at Montpellier, discontented ... at having left Aix.

8 December. My father has plunged himself into work on his instrument [for communications as described above]. Alas! It will hardly be to any purpose, since we have news of another retreat across the entire line.... Orléans has been taken back by the Prussians.

16 December. We had come to Montpellier for a couple of days. But have established ourselves here now.

25 December. [The instrument] has to do with a means of communicating across great distances. It's a mirror which reflects sunlight during the day and electric light at night. By covering and uncovering this mirror, one produces alternations of light and shadow, and by making these alternations more or less long, one imitates the Morse telegraphic signals. Each letter is represented by a certain number of alternating dashes and dots.⁶⁷ This method is so powerful that there are hopes of communicating from the Alps with Lyon, when it will be under siege, alas. It is my father who is constructing the apparatus and is testing it at Nîmes, because the prefect of Montpellier was too foolhardy to authorize it there. Everything was ready, it was accepted by the committee for the defense of Lyons, and all that was needed was to wait for the results. What a pity!

4 January 1871. I am again at Roucas. My father is still at Nîmes; I am quite afraid that he will decide not to return to Roucas.

11 March. My friend, we will return to Paris in a fortnight; my father has already left last evening. [Le Verrier had, therefore, returned to Roucas blanc].

3 May. Papa is at Versailles [where the government of Thiers had been set up; Le Verrier had been there since 27 March], working hard on his astronomical tables.

29 May. [The end of the Commune had taken place on the 27th]. Happily my father and M. Maurey have not budged from Versailles. My father is thinking of settling in Marseille.

13 June. It is decided, that we shall return to Paris next week, mama and me [They arrived on 21 June.]

20 July. Ah, how we feel the 30,000 francs of the senator cruelly lacking in our budget.

24 July. The salon of Mme Talabot is closing, alas.... I had the amusing opportunity to take part in a meeting watched over by the police. Here are a few names of the invited: Bournat, Le Verrier, Vitry and Rouher... Poor reactionaries! We must console one another.

3 September 1871. M. Thiers [who had just been elected president of the Republic] requested an interview with my father.⁶⁸ It is very gracious of him, but what does he want from my father? We're not republicans, no indeed.

There is nothing more of much interest to us. The death of Delaunay on 5 August 1872 was not even mentioned by Lucile. The Le Verriers probably moved back into their Parisian apartments, where Le Verrier once more devoted himself to celestial mechanics. They moved on 15 October 1872 to 11 quai Voltaire, near the location of their previous domicile. Despite maintaining a low profile at the moment, Le Verrier was not forgotten by the newspaper satirists of a republican bent, who eagerly caricatured him. And he was about to mount a comeback.

⁶⁷ More details in *CRAS 72 (1871), pp. 269–270.

⁶⁸ This is probably the visit of “astronomers” to Thiers in Septembre 1871, to which we alluded previously; this visit was therefore solicited by Thiers.

Chapter 7

The Second Reign (1873–1877)



Posthumous portrait of
Le Verrier by Giacomotti
(1878) at the Paris
Observatory. A similar one is
in Versailles, at the National
museum of the Chateau and
the Trianons

The Return of Le Verrier

The death of Delaunay plunged the Minister of Public Instruction into confusion. Who should replace him? None of the astronomers at the Observatory were eager to take on the responsibility. Finally, it was the president of the Republic, Adolphe Thiers, who decided the matter. Thiers had maintained contact with Le Verrier, and decided to bring him back. The latter was therefore nominated director on 13 February 1873, 6 months after the drowning of Delaunay. His family didn't seem to be in a hurry to move back to the Observatory, fearing, no doubt, a cold reception there. Though several astronomers, including Wolf, had hoped for his return, Le Verrier never forgave them for having signed the infamous manifesto of 1870 which ended in his dismissal, and in the end Wolf and Le Verrier would no longer be friends.¹

Let's take up again the journal of Lucile Le Verrier:

Saturday 16 August 1873. Tuesday, the furniture will be at the Observatory and the clothes in boxes. Wednesday morning we will depart.

They departed – but it wasn't for the Observatory! The family went on vacation with the Talabots, in the domain of Maury, near Limoges. Le Verrier was with them, or came to see them from time to time, for on 11 October Lucile wrote: “We have taken up the game of bowls, wherein my father helps us with his astronomy and knowledge of measures to estimate the distances of the balls.”

26 October. My father just left for good [for the Observatory], and Mme Talabot is actively pushing the preparations for departure.

A New Organization

Mindful of the prior experience with Le Verrier, the Minister of Public Instruction took his precautions: he reactivated the Commission of inspections created in 1872, at the time of Delaunay, by asking it to draw up a set of regulations. The latter, published 15 February 1873, that is immediately after Le Verrier's nomination,² created at the Observatory a scientific Council (Figs. 7.1 and 7.2), “required to meet once a month, on a specified day.” It had not been forgotten that the preceding Council, created in 1854, did not meet very often, and was often a scene of violent confrontations. The new Council was comprised of

- The Astronomer-director, president (Le Verrier)
- The chiefs of services (Lœwy, Wolf, Yvon Villarceau, Rayet);
- The six councilors named by the minister to the Council, of whom four were to belong to the Academy of sciences or to the Bureau of longitudes: these

¹ Interview of Gaillot by Bigourdan in 1888: see BOP, Documents divers sur l'Observatoire de Paris, 1854–1872, cote 3567(3), folder AP.

² *Journal officiel de la République française*, 15 February 1873.

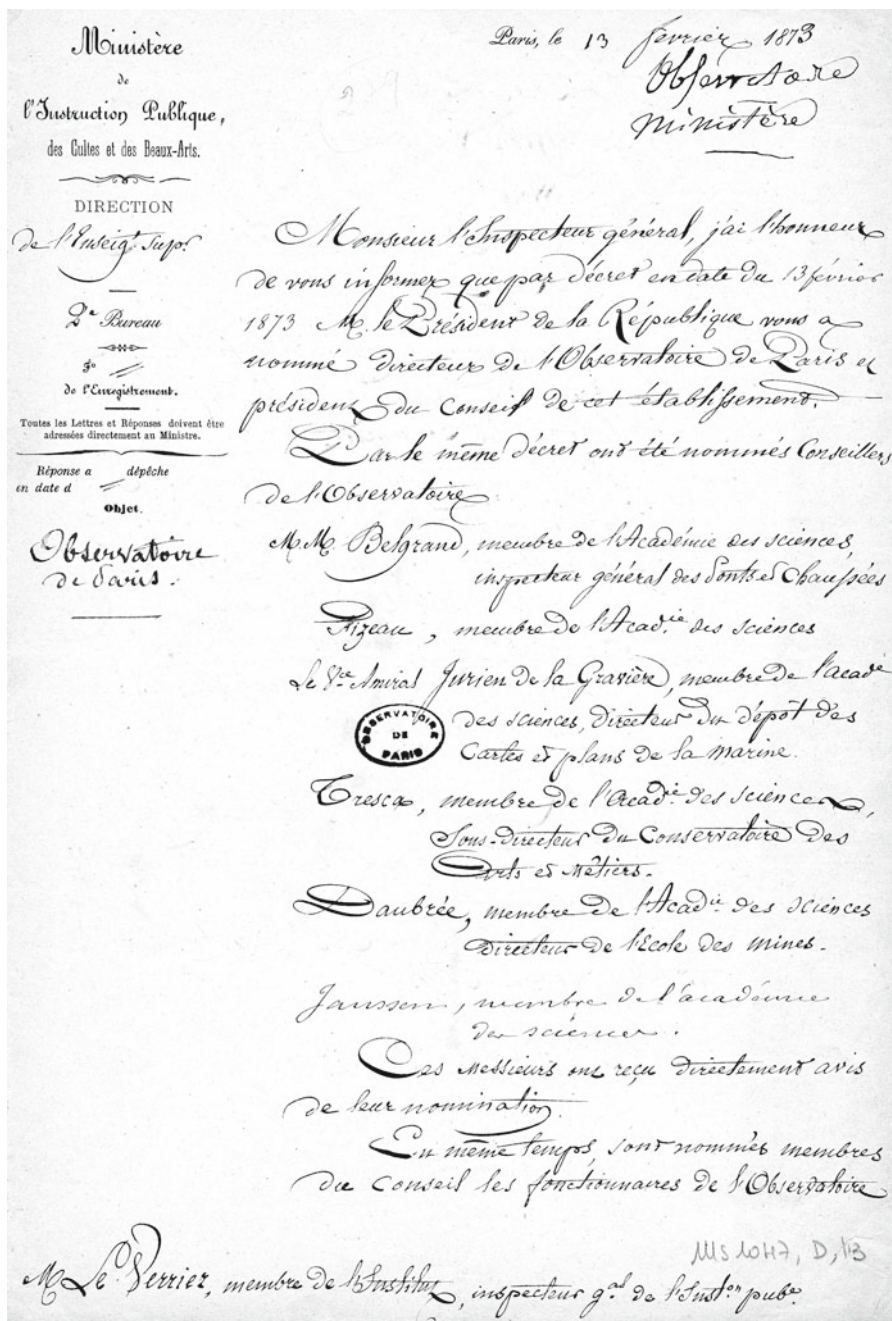


Fig. 7.1 Text of the creation of the Council of the Observatory, recto. Note that the only title of Le Verrier is: General inspector of public instruction

Pour les noms suivants:
 M. M. Yvon Villarceau, astronome
 Wolf _____ as
 Gaillet _____ astronome adjoint
 Rayet _____ ne

Je vous prie de vouloir bien porter à la
 connaissance de ces derniers les dispositions du
 décret qui les concerne.

J'aurai l'honneur de vous adresser
 prochainement les amputations des trois décrets:

- 1° portant organisation des Observatoires,
- 2° relatif à l'institution du Conseil,
- 3° réglant les attributions de l'Observatoire et
 de Montsouris en ce qui concerne les services
 météorologiques.

Recevez, Monsieur l'Inspecteur général,
 l'assurance de ma considération très distinguée,

Le Ministre de l'instruction publique
 et des Cultes,

Jules Simon

Fig. 7.2 Text of the creation of the Council of the Observatory, verso. Only the signature of the Minister, Jules Simon, is autograph

included Eugene Belgrand, Inspector general of Bridges and Roads, Auguste Daubrée, director of the School of Mines, the vice-admiral Edmond Jurien de la Gravière, Fizeau, Janssen and Henri Tresca. These were all good choices, for the councilors needed diverse competences and interests. Fizeau (Fig. 7.3) and

Fig. 7.3 Hippolyte Fizeau
(1819–1896)

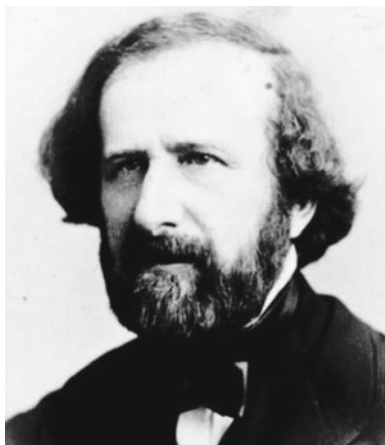


Fig. 7.4 Henri Tresca
(1814–1885)



Tresca (Fig. 7.4), mechanical engineer and professor of the Conservatory of arts and measures and promoter of the new metric standard which would in 1889 replace the old metres of the Revolution, played a particularly important role in the Council. Besides the chiefs of the services, Gaillot, adjunct-astronomer, represented the Observatory.

Every service chief would “bring directly before the Council scientific questions concerning their area.” One could consider that these democratic measures reflected a new spirit, that of the Republic. But not so: they were simply measures to counter-act Le Verrier’s dictatorial tendencies.

On 17 March 1873, the Minister announced that the first meeting of the Council would take place on the 19th and the occasion would be used to install Le Verrier, nominated 1 month before (Fig. 7.5). Certain foreign astronomers who had been shocked by the firing of Le Verrier congratulated him on his reappointment. Here, for example, is an extract of a letter from Airy, dated 17 March³:

Even if there were any truth in the complaints made by some persons, this was totally insignificant in comparison with the great service that you had rendered to your country and the world.[...]The astronomical essays which you have given as a private person, and the extraordinary scientific excitement which you have produced by your organisation of the Association Scientifique,⁴ have placed you in a position which any official man of science might envy.

I had however a very high respect for M. Delaunay too; and, without adverting specially to his connexion [sic] with the Observatory, I could have wished that he had been spared to finish his Lunar Theory.

Airy added a postscript, in thinking of his own all-powerful situation:

The non-personal system of the Paris Observatory is not such as we should have adopted in England.

A Relative Calm

Naturally, feelings were not completely assuaged at the Observatory: in 1946, one of Le Verrier's successors as director, André Danjon, affirmed: "The survivors of this epoch—I have heard them—put into circulation a heap of more or less biting anecdotes concerning their former chief. They have left in the archives of the place little hateful notes, anonymous of course."⁵ Tensions remained high between the director and the staff, as one can see from reading the minutes of the meetings of the Council.⁶ Nonetheless, this did not prevent the Observatory from functioning better than it had during the first reign of Le Verrier, and decisions were taken by a vote that was not necessarily favorable to the director's positions: a true democracy had taken hold, often to the great irritation of the director. This democratic spirit would continue up to the present day.

The Observatory became again the site of social events; they were frequent before 1870 (see Fig. 5.17) but had more or less disappeared in Delaunay's time.

³ BOP, Ms 1072 (33).

⁴ Remember that the Association scientifique, an abbreviation for the Association for the advancement of Astronomy and Meteorology, had been created by Le Verrier in 1864 for lobbying in favor of his projects, and was very successful in this regard.

⁵ This affirmation perplexes us. We could not find such notes in the Observatory archives, only interviews by Bigourdan of diverse survivors of the Observatory and of its councils in 1888–1889 (BOP, Documents divers sur l'Observatoire de Paris, 1854–1872, cote 3567(3), folder AP). These interviews are not anonymous and the opinions they relate look generally objective. They have never been published before the extracts given in this book.

⁶ These minutes are preserved integrally in BOP, Documents divers sur l'Observatoire de Paris, 1854–1872, cote 3567(4), folder AA.

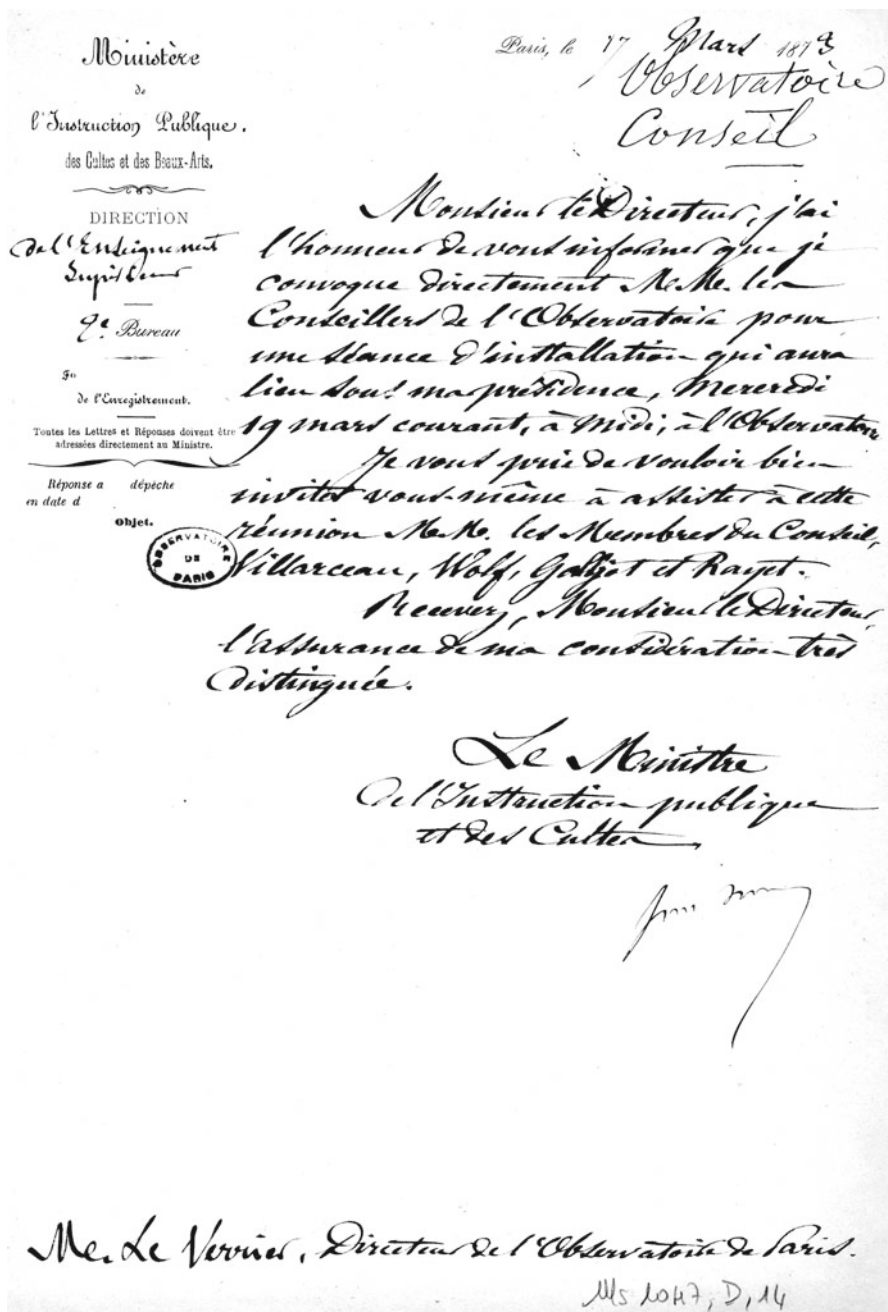


Fig. 7.5 The minister Jules Simon invites Le Verrier to the installation of the Observatory council. Only the signature is autograph

Fig. 7.6 Social event on 10 April 1874 in the meridian room of the Observatory. Various instruments and scientific demonstrations can be seen

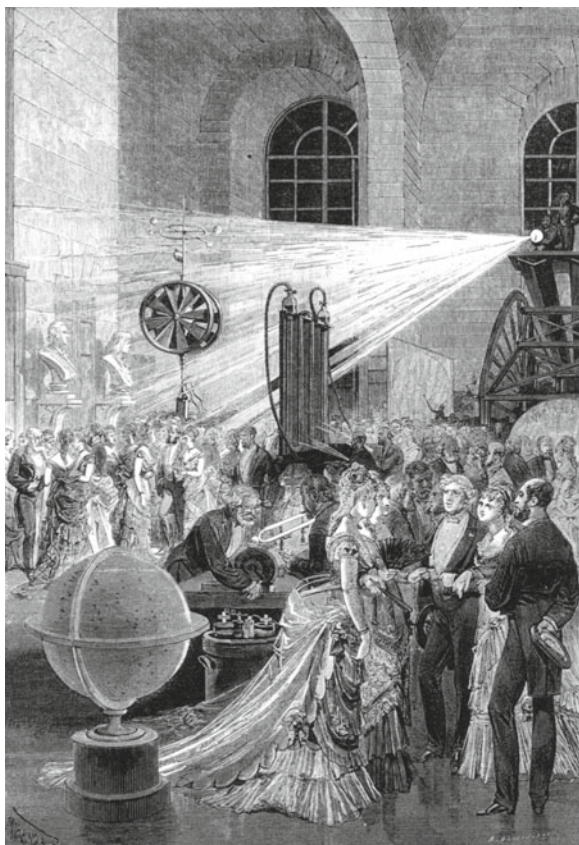


Figure 7.6 shows by way of example the picture of a soirée including the exhibition of scientific equipment and experiments, organized in the largest hall of the building (the *salle Cassini*).

Problems Again with the Bureau of Longitudes!

Recall again the veritable war that had taken place during Le Verrier's first reign between himself and Delaunay, therefore between the Observatory and the Bureau of longitudes, of which the two were the respective leaders. That war had left many deep scars. Moreover, the very survival of the Bureau was now in question. During a discussion of the budget for public instruction by the Chamber, on 9 December 1872, a deputy who would become famous, Paul Bert, attacked the Bureau thus⁷:

⁷ *Journal officiel* of 10 December 1872, cited by Bigourdan (1933), pp. A.65–A.68.

“Section 14 concerning astronomical establishments is written into the budget with a sum of 463,660 francs.... In this section ... one finds a subsection in the amount of 109,000 francs with the title ‘Bureau of longitudes.’ Now, eminent individuals [a reference evidently to Le Verrier] maintain ... that the Bureau of longitudes will hardly render to the astronomical sciences the services expected of this institution.... If we look at the manner in which the sum of 109,000 francs is used, we see that 30,000 francs approximately are allocated for the publication of the *Connaissance des Temps*, and that the 70 odd thousand francs that remain are allocated as allowances to the members of the Bureau of longitudes.

Permit me to tell you that, by assurances of all the astronomers [that is to say in fact Le Verrier] it would be preferable to create a bureau especially charged, as in England, with the publication of the *Connaissance des Temps*, and to give it a budgetary allocation which ought not to surpass 40,000 francs. As for the allowances of the various astronomers, geometers, sailors, soldiers, etc., or current members of the Bureau of longitudes ... they could be continued as honorific pensions or stipends; but competent people think that the institution itself should be suppressed.

The president of the Chamber of deputies, Jules Grévy, decided that the problem should be examined by a commission of the Chamber. Highly distraught, Faye, who at the time was president of the Bureau of longitudes, undertook a vigorous defense of the institution,⁸ pointing out that the Bureau was actively occupied with geodesy and terrestrial magnetism, and that it had organized Janssen’s expedition to the South of India to observe the total eclipse of the Sun of 18 August 1868 – a great success! Convinced, the Academy took up the cause of the Bureau, and contacted Barthélémy Saint-Hilaire, the secretary of the president of the Republic, who gave the commission the best assurances of the support of the latter; then the commission sent a delegation to the president of the Chamber. Everything was arranged: Paul Bert said nothing and wrote to the Academy to minimize his role in the affair. But there was some fallout. A new decree reorganizing the Bureau was signed 15 March 1874, which would for many decades govern the constitution and operation of this organization.⁹

The New Life of the Observatory

Upon his arrival, Le Verrier had to concern himself with reorganizing once again the institution. In a long proposal presented to the Minister of Public Instruction in the middle of 1873, he wrote¹⁰:

The progress of Astronomy has suffered some setbacks during these past few years. The enterprises underway have hardly made any progress. The last volumes of our *Annals* and of our *Atlas* were sent out in 1869.

⁸ *CRAS 75 (1872), pp. 1721–1729.

⁹ The Bureau of longitudes still exists, but the ephemerides are now prepared by a common laboratory with the Paris Observatory, the Institute of celestial mechanics and calculation of the ephemerides (IMCCE). Its internet server provides for free the positions of planets and satellites for any epoch.

¹⁰ BOP, Documents divers sur l’Observatoire de Paris, 1854–1872, cote 3567(3), folder X.

Fig. 7.7 Paul Henry (1848–1905)

It's a matter now of resuming these great undertakings, of filling in the deficiencies, of providing for the present, so as to achieve again a rank that honors France. The astronomers of the Paris Observatory are firmly resolved not to neglect anything needed to reach this goal. We ask the State for the means for indispensable actions....

We ask also for the strong support of the eminent colleagues who constitute the Council of the Observatory.

On 9 April 1873, the services were reorganized, and the head responsible for each was named. Then, on 15 May, the composition of each was precisely spelled out:

Division of theory and the instruments of Gambey: chief of service Le Verrier, personnel Perigaud, Folain, Leveau, Ludinard and Renan;

Division of celestial physics and the equatorials: Wolf, assisted by [Charles] André and by Paul and Prosper Henry (Figs. 7.7 and 7.8);

Division of the great meridian instrument: Lœwy, assisted by Bossert;

Division of geodesy—as soon as it is possible to institute it: Yvon Villarceau, assisted by Chevallier;

Bureau of calculations: Gaillot, assisted by Vincent;

Division of meteorology and the physics of the globe: Rayet, assisted by Fron, Mouveau and Boinot.

One notices that Le Verrier assumes, among his direct responsibilities, a large part of the meridian observations. Meteorology occupied an important part of the work of the Observatory. Geodesy posed some problems: Chevallier was unable to manage the affair and exchanged places with Renan, but this worked no better, and he would be scapegoated for this. Relations between Yvon Villarceau and Le Verrier experienced highs and lows (especially lows!); his service moreover would be transferred to Lœwy in 1874.

Fig. 7.8 Prosper Henry (1849–1903)

The New Instruments

Le Verrier was as active as ever in obtaining new instruments for the Observatory. Two of them, the great refractor and the giant reflector, had been decided upon for some time, but another was also undertaken and brought to completion: a new large meridian circle. And then, in 1873, Lœwy came up with the idea for a new instrument, which he called a “refractor with two mirrors,” but which would be better known by the term equatorial coudé.

The Equatorial Coudé of Lœwy

In the account rendered of the meeting of the Council on 7 January 1874, one reads the following sentence:

M. Bichofsheim [sic] offers to give 20,000 francs for the construction of a refractor with two mirrors by M. Loëwy [sic].

A keen amateur astronomer, Raphaël Bischoffsheim was a banker whose businesses were extremely lucrative. He was to finance other astronomical projects as well, notably, in 1879, the great observatory at Nice.

The Council must have been surprised at the recipient of this offer: it must not have been aware of Lœwy’s project, as Lœwy had probably taken the initiative of approaching Bischoffsheim directly and in person. In effect, during the next meeting on January 15: “M. Lœwy presents to the Council and describes a model of his equatorial instrument with two mirrors whose purpose is to lessen the fatigue of the observer by permitting him to remain in one place.” Council members raised various objections to the principle: for example, Fizeau feared, not without reason, that thermal deformations of the two plane mirrors of the instrument would degrade the images. This did not prevent his being disposed to vote in favor of the instrument, if only because it was difficult to resist such a generous offer as Bischoffsheim’s. Besides, in order to complete the observatory’s

large star catalogue, a work that had been in progress for many years, a special instrument was needed “which would allow zones to the right or left of the meridian to be studied,” that is, without waiting for each star to cross the meridian, as is necessary with meridian instruments.¹¹ The coudé telescope of Lœwy seemed to answer this need, and the Council gave its assent in principle for the construction of the instrument. Its diameter would be 8 French *pouces*, or 22 cm.¹² On 12 February, Eichens estimated the cost of the equatorial without its objective, which it was hoped perhaps could be obtained elsewhere, would be 20,000 francs, to which had to be added another 18,000 francs for the shelter and installation. This seemed expensive, and since there was not universal agreement regarding its construction, the Council decided on 14 May, this time unanimously, to postpone it. It was meanwhile determined that M. Bischoffsheim’s gift, whose value had in the meantime increased to 26,000 francs (happy epoch!) did not include restrictive conditions. The money was now destined for a meridian circle.

Lœwy’s innovative project was, therefore, abandoned. It would resurface, however, after Le Verrier’s death: a first coudé equatorial of 27 cm diameter would be put into service at Paris in 1882, followed, in 1890, by an even larger one (Fig. 7.9), 60 cm in diameter and with a focal length of 18 m. The latter was equipped with two objectives: one for visual observation and the other for photography. The observatories at Besançon, Lyon, Nice and Algiers were provided with similar instruments whose diameters were, respectively, 33, 35, 40, and 34 cm. A seventh coudé equatorial would be given to the Observatory of Vienna by baron Salomon Albert de Rothschild, as a way of rendering homage to Lœwy, who had started his career in that city. All of these instruments would be constructed by Paul Gautier, with optics by the Henry brothers.¹³ The large coudé equatorial in Paris was used chiefly by Lœwy and Pierre-Henri Puiseux (Fig. 7.10) to construct their magnificent *Atlas photographique de la Lune*, published in sections from 1896 to 1910; its photographs illustrated well the lunar features, and were still being consulted in preparation for the Apollo missions. The instrument would then be used in spectrographic work, and its 60 cm visual objective was later transferred to the Pic du Midi Observatory. The instrument, dismounted, is currently warehoused at the Meudon Observatory waiting for better times. The condition of building which housed it is now, alas, much decayed.

The Great Reflector of the Observatory

Le Verrier had obtained from the Chamber of deputies in 1865 a renewable budget of 395,000 francs for the construction of a giant refractor 16 m long and 75 cm in diameter, and a large reflector. Already, in 1863, a glass disk 1.215 m in diameter,

¹¹ The instrument will often be presented as able to “measure large angular distances.” However, position measurements cannot be as precise with it as with meridian instruments.

¹² Note the permanence of the ancient units even amongst scientists, while their use was forbidden in principle since 20 years for the benefit of the metric system.

¹³ For a complete history of the coudé equatorials, see Lequeux, J.: *°J. Astron. Hist. Herit.* **14**, 191–202 (2011).

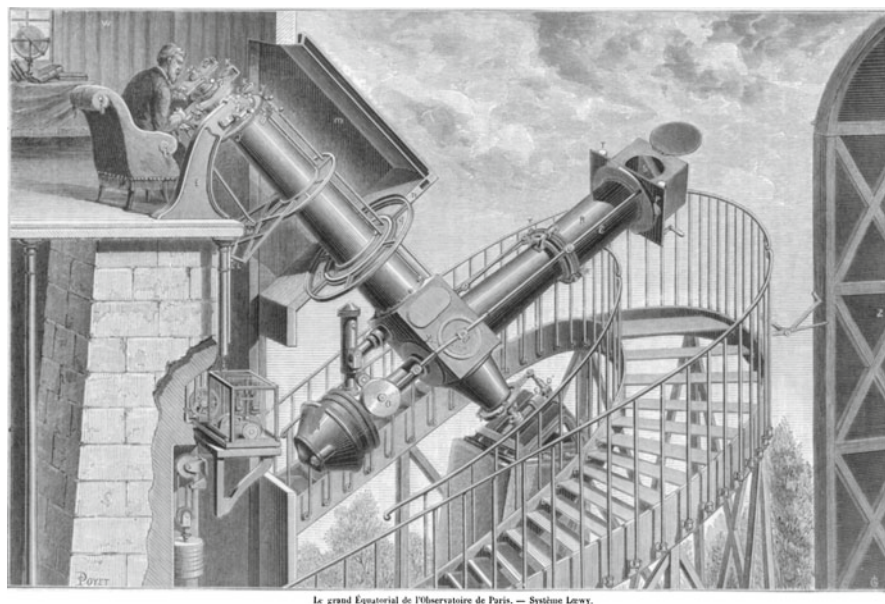


Fig. 7.9 The small equatorial coudé (elbow equatorial) put into service at the Paris Observatory in 1882. A plane mirror at 45° located in the cube at the end of one of the arms and followed by the objective sent the light down the arm where it fell onto another plane mirror which directed the light toward the observer. The observer sat in a fixed position along the extension of the hour axis, parallel to the rotational axis of the Earth. By turning the cube about its own axis, it was possible to change the declination being viewed, whereas the hour angle was determined by the hour axis. This instrument insured great comfort for the observer at the price of using two more plane mirrors than an ordinary equatorial

Fig. 7.10 Pierre-Henri Puiseux (1855–1928)



weighing 700 kg, and destined for the telescope, had been cast at the Saint-Gobain factory under the direction of Théophile Pelouze. The mirror was ground to a figure that made it ready for polishing in the workshops of Sautter and Lemonnier, while the polishing itself was to have been the *chef-d'oeuvre* of Foucault. However, his premature death in 1868 prevented him from realizing this task. The project was not abandoned because of this: the polishing of the mirror was entrusted on 25 May 1869 to Adolphe Martin, the only student of Foucault, whereas the mechanical part of the undertaking was the subject of an 8 June contract with Eichens. Wolf was charged with overall supervision of the work.

Le Verrier's dismissal, and then the war with Prussia, caused the affair to languish. Eichens took his time, and didn't finish the mounting and tube of the telescope until October 1875. A rolling shed was also delivered at this time by the Lyon-Mediterranean Company; the ladder or steps which permitted the observer to access the Newtonian focus was made by the Hemery and Gautier forges at Persan. The instrument (Fig. 7.11) was driven by a clock movement with a Foucault-type governor, constructed, like several others at the Observatory, by Yvon Villarceau.¹⁴ All this mechanical equipment worked satisfactorily. The total price of the instrument came to 190,000 francs. On 8 October 1875, Le Verrier announced to journalists¹⁵ "the completion of construction of the telescope, which will be presented to the press on Saturday 9 October." At this stage, only the mechanical work was finished. But the telescope was of no use without the mirror.

It was only on 3 May 1876 that Martin announced that the mirror was finished and had been mounted in the telescope, so that testing could commence. On the very next day, a Committee of inspection, consisting of Fizeau, Cornu, Le Verrier, Tresca and Wolf began its work. The report has been preserved at the Observatory.¹⁶ We present a few extracts. Here Le Verrier is speaking:

15 May.... I looked at the Pole Star (M. Martin being present) with a magnification of 500 [x]. It was very bad. It was nothing but rays of light through which one detected the companion. I looked at gamma Virginis [a double star whose components with the same brightness are separated by 7 seconds of arc] under the same conditions. There were two intense nebulae which joined and intermixed their luminous centers.

M. Martin agreed that this was worthless: but assured me that it was the fault of the eyepiece?, something which was impossible to judge.

19 May 1876.... At my request (given that I was suspicious of internal diaphragms in the eyepieces) MM. [brothers] Henry proceeded to measure the exposed part of the mirror ... At half past midnight, M. P[rosp]er Henry announced his conclusion: 24 centimeters of the circumference of the mirror was blocked out; only 72 centimeters of the center remained exposed. There was nothing to be said about this. It could only be addressed when the Commission [of inspection] came to inspect the mirror.

¹⁴ There is a detailed description of this instrument in *La Nature*, 4^e année, 1^{er} semestre (1876), pp. 39–43. But this paper, written before the reception of the reflector, was overoptimistic about the quality of the mirror!

¹⁵ BOP, Documents divers sur l'Observatoire de Paris, 1854–1872, cote 3567(4), folder AR.

¹⁶ BOP, Documents divers sur l'Observatoire de Paris, 1854–1872, cote 3567(4), folder AE.

Fig. 7.11 The telescope of 120 cm diameter at the Paris Observatory. The mounting is of the Newtonian type, the observer places himself at the side of the upper end of the tube, the light is concentrated by the main mirror and deflected by a small flat mirror set at a 45 degree angle into the eyepiece. The observer uses a mobile staircase that can be adjusted to different heights. When not in use, the telescope is protected by means of a rolling shelter



What had happened? Rather imprudently, Le Verrier had authorized the construction of diaphragms that could be inserted in the eyepieces in such a way as to limit the useable part of the mirror. Martin did not hesitate to use these diaphragms in the optical system during the testing, as Le Verrier found out. Besides, “two members of the Commission [of inspection] had expressed on several occasions their astonishment at the small amount of light given by such a large mirror, without being able to assign any cause. It was this surprising feebleness of light, which led me, on the night of 19 May, to look into the cause.”¹⁷

On 26 May, the report of the Commission declared:

The images measured at least 5" rather than the theoretical value of 0.1" or the reasonable limit of 0.5" that one might expect from a good mirror. One can say, from our present experience, that this reflector has no more power than a good 15 to 20 cm diameter reflector.

¹⁷ Report by Le Verrier to the Observatory council on 13 April 1877: BOP, Documents divers sur l'Observatoire de Paris, 1854–1872, cote 3567(4), folder AC.

It was a matter, no doubt, of the lamentable performance of the fully illuminated mirror, the diaphragms having been removed from the eyepieces. The testing was taken up again, with different diaphragms, whose existence Le Verrier finally revealed to the Commission. He must have felt little personal responsibility in that regard, because he threw the blame on Wolf:

12 June (Monday)... [The director] tells the Commission that about two months ago he had authorized the construction of diaphragms which could be inserted into the eyepiece. As is usual with this service [that of Wolf], the director has not been shown these diaphragms; he was not even told of their existence, so that he could not tell the Commission to look into their use....

It was time to look at gamma Virginis, and while preparing for that, M. Tresca went to the equatorials where M. Paul Henry shows him in both refractors gamma Virginis. He would then be more prepared to judge the view in the great reflector....

M. Tresca declares that he first saw gamma Virginis (in the presence of M. Martin) with a diaphragm reducing the aperture to 60 centimeters. The image was similar to that observed by himself with the 9 *pouces* equatorial [24 cm, with an objective by Foucault], but not as good. Next a diaphragm was used that increased the aperture to 90 cm. The images of the two stars then looked like flames...

When the useful diameter of the mirror is extended beyond 60 or 70 centimeters, the image is worse; at 90 cm, it is intolerable. What would it be with 120?

Martin pretended that the optics could be corrected by compensating for the defects of the mirror by modifications of the eyepiece, a method which had been sometimes used by Foucault. But the defects of the 1.20 m mirror were too great: “to affirm that by a modification of the eyepiece one would turn a bad mirror into a good one, is a delusion,” said the Commission. In spite of the opinion of Wolf, who had already observed various nebulae with the telescope and recommended that the mirror be accepted in its present form, because he so badly wanted to continue to use it, Martin was asked to resume polishing. Martin, who had showed so much dishonesty in this affair, was forced to oblige. But he did not want to hurry, the more so because he had already been paid. He promised that the retouching of the mirror would be finished by the end of October of the same year 1876, but “as he is settled in the Observatory, everyone can see that he barely turns to the work. Roughly, he uses less than one working day of a worker every day, and he himself is present less than 20 min.” In fact, Martin did not possess the skill of Foucault, who had had no difficulty in realizing the 80 cm mirror for the Marseille Observatory. In the end, Martin was unable to finish the work. Also, the mirror he had made was too thin and too easily lost its figure.

It would have been very good publicity to present the telescope, which would have been the largest in the world, at the 1878 Exposition universelle. In order to do so, however, Le Verrier, feeling ill at ease, envisaged in early 1876 the construction of a second mirror, which would take the place of Martin’s if the latter finally had to be rejected. He therefore contacted the Saint-Gobain company, whose board of directors decided on 1 September 1876 to offer a new disk to the Paris Observatory. Le Verrier, therefore, went on 25 November to the Saint-Gobain factory (Aisne) and spent 3 days there discussing the matter of annealing of the glass, which had caused problems for the first mirror: it would be necessary to extend the process over at

least 6 weeks. Indeed, there would be several castings in which the disk cracked because the temperature variations occurred too rapidly. Success finally came on 1 March 1877: two disks were made, and Paul Henry went to Saint-Gobain and ascertained that one of the disks was superb. The Minister of Public Instruction, who was now William-Henri Waddington, authorized the creation of a new mirror from this disk, following a visit by Le Verrier on 17 February; the figuring and polishing were assigned to Sautter, in the rue de Suffren. The disk arrived at his workshop on 4 April 1877. Sautter obtained the funds to install the necessary equipment, but it was now quite late, and he would not have time to finish before the Exposition.

The notes from which we have extracted this information¹⁸ stop on 25 June 1877, because of Le Verrier's illness and subsequent death on 23 September. In January 1881, the mirror was returned to the Observatory from Sautter, along with the tools used to work it.¹⁹ Also, appended to the account of the Observatory Council, there is this remark in the hand of Bigourdan:

28 September 1888 the glass was in the east storage area at street level of the north court; here are its dimensions: Diameter, measured approximately with a cord: 1 m.198, thickness of the cell 0.195 or, of the glass 0.19[m].

What has become of the second large disk? It was not seen again. What is sure is that the telescope was not presented at the Exposition universelle of 1878.

The effort to improve the telescope was not yet abandoned, however. Le Verrier's successor, admiral Ernest Mouchez, ordered new tests on the mirror, which Martin had once again agreed to retouch, though as before he made very little progress. In 1880, the question of retouching the mirror came up again, and this time it was proposed that it be used for photographic studies. Wolf continued to use it to observe nebulae, though without much success. Finally, in 1883, the astronomer Henri Deslandres, of whom we will say more later, took it up, and used it to make more fruitful spectroscopic observations from then until 1897.

The telescope was being discussed again in 1912: the young astronomer Jules Baillaud reexamined the mirror using a new optical test devised by the German optician Johann Franz Hartmann.²⁰ He found that the optical surface was suitable, but "the surface of the mirror is subject to continual deformations... It will no doubt be impossible to obtain, in any permanent way, good images with this mirror." Moreover, "the mounting of this telescope has to be looked at again, and the tube completely redone, as well as the clock train and transmissions." That was where the matter was left for the time being, not least because the First World War put on hold all activity at the Observatory.

Another episode occurred in 1930. We read the following notice: "In carrying out the plan to completely rework the Eichens-Martin telescope, the [optical] laboratory has been occupied ... with working on a parabolic mirror of 1.20 m."

¹⁸ BOP, Documents divers sur l'Observatoire de Paris, 1854–1872, cote 3567(4), folder AE.

¹⁹ Mouchez, E.: *Rapport annuel sur l'état de l'Observatoire de Paris, année 1880*. Gauthier-Villars, Paris (1881).

²⁰ Baillaud, B.: *Rapport annuel sur l'état de l'Observatoire de Paris en 1912*, pp. 56–58. Gauthier-Villars, Paris (1913).

A young optician, André Couder, was charged with repolishing the mirror. This time it was a success.²¹ Then director of the Paris Observatory, Ernest Esclançon, envisaged rehabilitating the telescope, for a sum of 800,000 francs.²² But even though work on the dome started in the middle of 1936, the national department for scientific research, which would become in 1939 the CNRS (the National Center for scientific research), proposed transferring the instrument to the observatory of Haute-Provence, whose creation had just been decided. Esclançon was furious to be deprived of this instrument, but had no choice but to comply with the decision. But he immediately began to plan as its replacement the construction of a 1.7 m diameter. It was never constructed because of the Second World War.

Furnished with a new mechanical system, built by the Secrétan firm, the telescope was installed in 1941 at the St.-Michel Observatory (Fig. 7.12). It went into service in 1943. Even this was not the end of the instrument's vicissitudes.²³ The mirror was damaged in January 1945. A second mirror was then figured by Couder, and installed in 1953 (did he use the second disk of 1876?). The instrument thus modified is still in use today, alongside its larger brethren of 1.93 m and 1.52 m aperture.

The Giant Refractor

Like the large reflector, a giant refracting telescope had been in the works for quite some time. We have already discussed (in Chap. 4) how in 1856, the Observatory had acquired from the Chance brothers the two 75 cm diameter disks, one of crown glass and the other of flint glass, necessary for making an achromatic objective. Financing for it through the special allotment of 395,000 francs approved in 1865, Foucault was assigned to test the suitability of the disks, and found them more or less suitable, though too thin, which would require that any lens would have a very great focal length. This meant a telescope that would be costly to mount and house. Before Foucault could begin figuring the lens, he became ill, and died in 1868. Subsequent events at the Observatory and the war with Prussia kept the project in suspended animation for several more years.

Le Verrier never abandoned it, however, and decided to relaunch it in 1874. Since Martin seemed to have nothing on his hands and insisted that he could not work during the summer on the 1.20 m mirror for the large reflector, Wolf proposed on 9 July to the Council that he make a start on polishing the objective “for which the Observatory possessed an excellent crown disk; the disk of flint contains slight defects

²¹ Esclançon, E.: *Rapport annuel sur l'état de l'Observatoire de Paris en 1930*. Gauthier-Villars, Paris (1931).

²² Esclançon, E.: *Rapport annuel sur l'état de l'Observatoire de Paris en 1937*. Gauthier-Villars, Paris (1938).

²³ Véron, P.: *Préhistoire de l'Observatoire de Haute Provence, Colloque Observatoires et patrimoine astronomique français*, Nantes, accessible by http://www.obs-hp.fr/www/histoire/pre-histoire_ohp.pdf (2001).

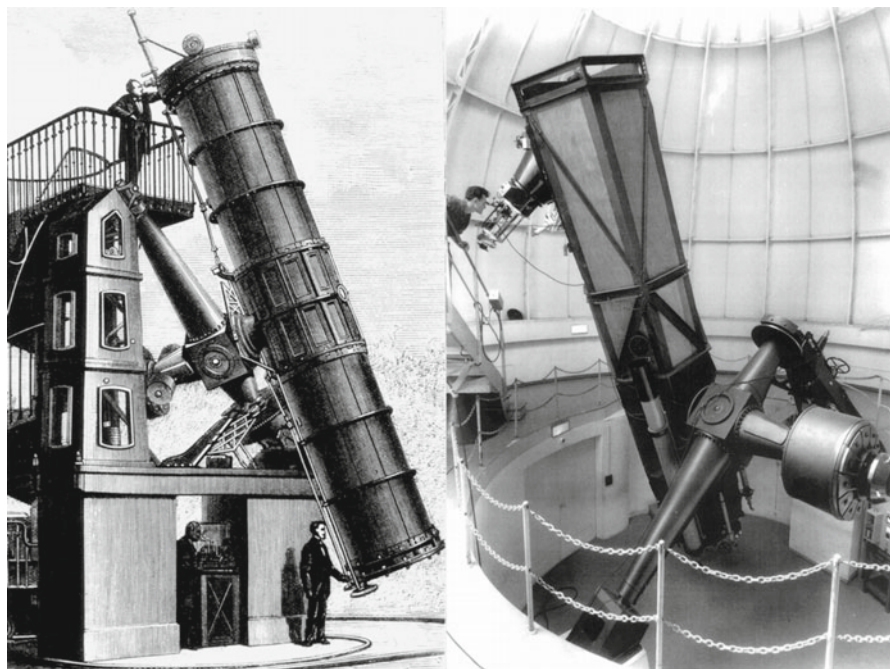


Fig. 7.12 The 1.20 m diameter reflecting telescope of the Observatoire de Haute-Provence (to the right) uses the mount of the large telescope of the Paris Observatory (to the left). This mount has been inverted in order to facilitate the access to the Newtonian focus

and perhaps can be replaced.”²⁴ It was necessary also to procure a plane mirror of 80 cm diameter to test the objective using Foucault’s method, the purchase of which was approved. But it was imprudent to entrust at the same time to Martin the construction, for the sum of 12,000 francs, of a smaller objective, 12 in. (32 cm) in diameter, destined to replace the one in the equatorial in the West tower which possessed irremediable defects. Fizeau had reservations about this undertaking. They proved to be justified, as Martin, who was to deliver the objective in March 1876, had not completed it by this date. It still remained unfinished even 6 months later.

In February 1875, Le Verrier asked several instrument-makers about the construction of the mechanical parts of this telescope, for which he had available a sum of 187,257 francs. Only two of them responded, and not until September. One was Eichens, the other Froment-Dumoulin, who was less expensive but lacked experience in this area. Eichens was awarded the job, after numerous discussions. By 8 March 1877, his work had advanced to the point where he felt justified in asking for a first payment of 18,750 francs. But then everything came to a halt. Though it is hard to say

²⁴ There was some talk of replacing this disk, using 12,500 francs remaining in the budget for the lens, by another one supplied by Charles Feil, who was the only one able to supply flint glass in France at the time. Feil in 1875 produced several flint disks, but the best of them broke during the annealing process.

exactly why, it seems that the Observatory had lapsed into lethargy with the illness of Le Verrier and his death on 23 September 1877: the Council meetings stopped on 8 May that year.

Le Verrier's successor, admiral Mouchez, would try to revive construction of the reflector "for the Paris Observatory is so outdistanced nowadays by other observatories that with its small refractors of 24 cm and 31 cm it is hardly even able to verify the discoveries and novel observations made in other countries with refractors of 0m,60 and 0m,65. The builder [Eichens] moreover has suffered noticeable losses owing to the suspension of work which has forced him to advance considerable sums, and he would be well within his rights to demand from the State the execution of the contract agreed to."²⁵

In 1879, Charles Feil furnished two new disks of flint to replace the existing one, but these also had defects. Martin thought that one of them was good enough, however, and set about working on it. Eichens meanwhile returned to work on the mechanical part of the telescope.²⁶ The following year, Feil had to provide another disk of flint, the previous one having been finally judged too defective for polishing. The construction of the dome was assigned to Gustave Eiffel in 1882, and after Eichens turned over his company to Paul Gautier, the latter took up the project again by a contract signed on 27 March of that same year. The refractor was to be placed in the "Arago site" south of the Observatory (Fig. 7.13) but soundings taken in 1883 revealed there were catacombs there. Consequently, "the construction and installation of this refractor was further delayed by the impossibility [which the Observatory now found itself contemplating] of setting up the telescope on the grounds of the Paris Observatory." At this time, Mouchez envisaged again the possibility of creating a branch of the Observatory outside the city, but the project was aborted, so that in 1884 "subsequent to the rejection of the proposal to build a branch of the Observatory ... we need not occupy ourselves further with [the refractor] until we have a solution that is at present not even foreseeable." Furthermore one reads in the annual report of the Observatory for 1886: "The coudé equatorial of 60 cm, which had been accorded to the Paris Observatory as a replacement for the large 74 cm refractor ceded to the Meudon Observatory, is under construction by M. Gautier."

This is the epoch when Janssen wished to equip the astrophysical Observatory of Meudon with a lens even larger than that of the abortive Paris instrument. Finished in 1896, the "grande lunette" of Meudon (Fig. 7.14) was a double instrument: the rectangular tube used an achromatic objective suitable for visual observations, 83 cm in diameter, and in parallel an objective for photography of 62 cm, corrected for the blue-violet part of the spectrum to which the photographic plates of the period were sensitive.²⁷

So what was salvaged, finally, of the great Parisian refractor in constructing that at Meudon? Gautier, who had contracted for the latter, probably utilized some of the mechanical elements. As for the glass disks for the Meudon objective, they were

²⁵ Mouchez, E.: *Rapport annuel sur l'état de l'Observatoire de Paris*. Gauthier-Villars (BOP), Paris (1879).

²⁶ The informations here are from the *Rapports annuels de l'Observatoire de Paris*.

²⁷ Dollfus, A.: *La grande lunette de Meudon*. CNRS Éditions, Paris (2006). See also Launay (2008), pp. 134–137.

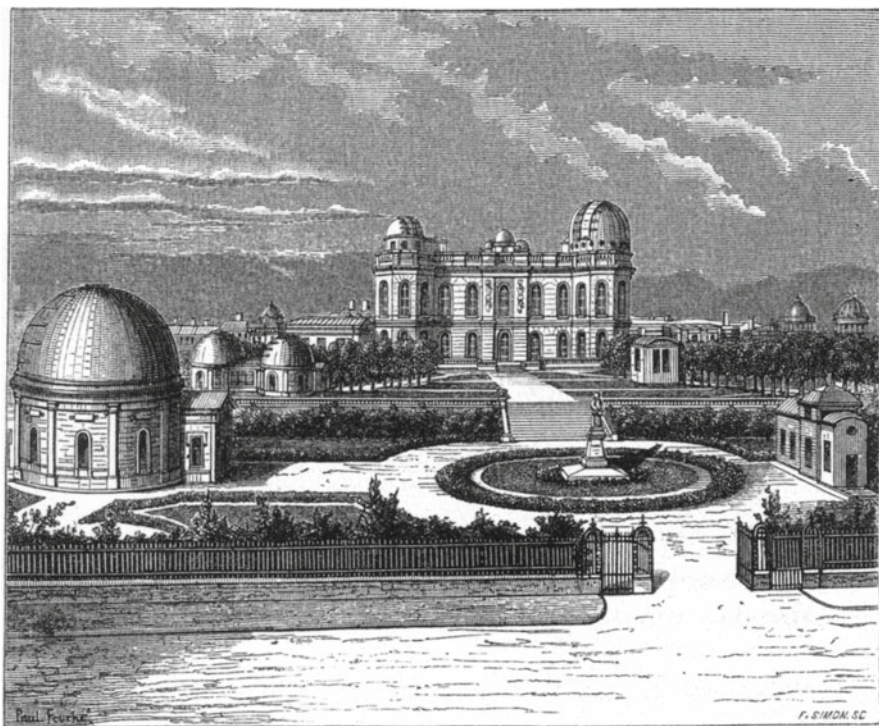


Fig. 7.13 Plan for an enlargement of the Paris Observatory (1881). The main building on the back is seen from the South. *Left*, the dome of the large refractor, in the back the twin domes. *Middle*, the statue of Le Verrier, which finally will be placed in the North courtyard. *Right*, the building of the small coudé equatorial, which will actually be constructed, then destroyed after World War II. In between, in front and to the right of the main building, the shelter of the Bischoffsheim meridian circle (replaced after World War II)

furnished by Mantois, Feil's successor. Did he perhaps utilize, in reducing the diameter, the disks that Martin had already begun to polish, in making the photographic objective of 0m.62 for the Meudon refractor? Whatever the case, only one unworked crown disk, of 0m.75 diameter, remains at the Observatory. There is a mystery here.

The Meridian Circle of Bischoffsheim

The only new instrument that Le Verrier saw through to completion during his second reign was the meridian circle financed by the banker Bischoffsheim, installed in 1877.²⁸ Bischoffsheim had in 1875 made a grant of 26,000 francs to the Observatory,

²⁸ See for a description Wolf, C.: Le cercle méridien de l'Observatoire de Paris, *La Nature*, 5^e année, 2^e trimestre, pp. 406–410, (1877).

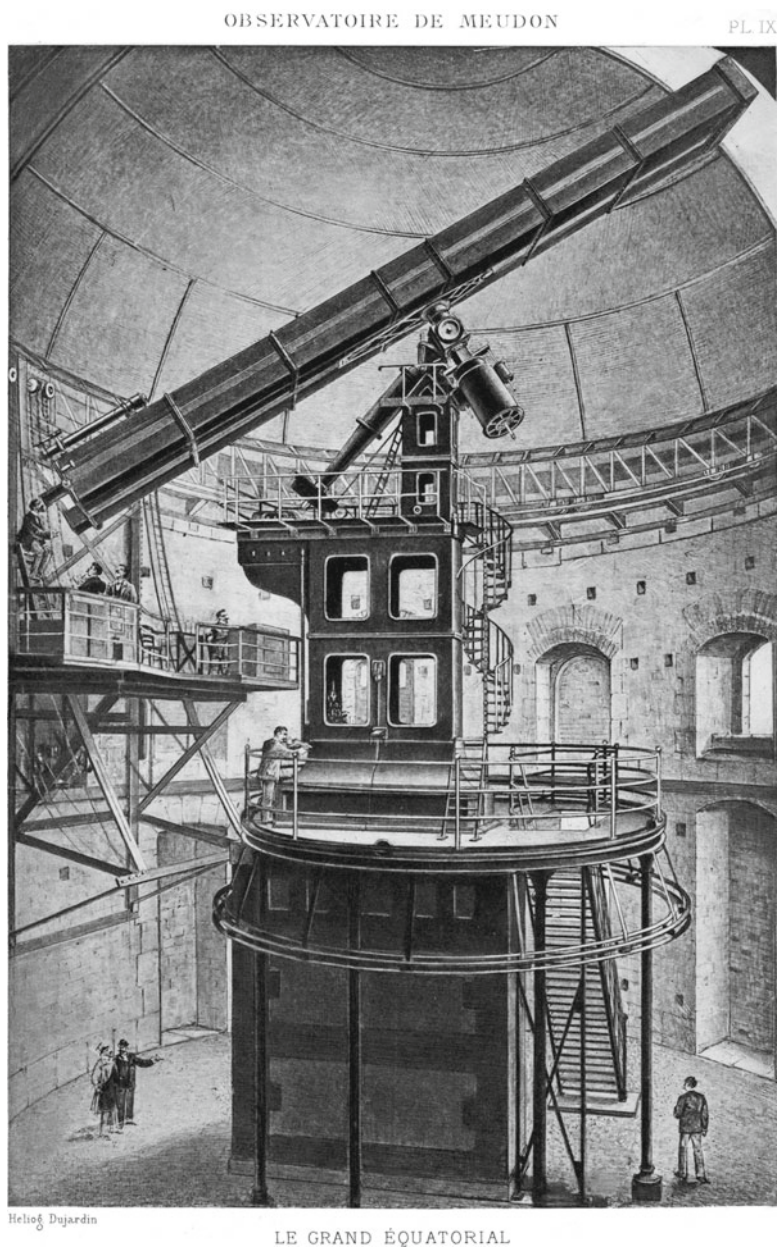


Fig. 7.14 The large equatorial of the Meudon Observatory (1896). It still exists, though it has recently been furnished with a new dome

which had initially been earmarked for the coudé equatorial devised by Lœwy. Instead, the grant was used to finance this new meridian circle. This large instrument (Fig. 7.15) was the third of a new generation built by Eichens.²⁹ The first was installed in 1868 at the observatory of Lima in Peru, the second in 1876 at that of Marseilles. All three had objectives of 20 cm in diameter. The Lima one was finished by Foucault, those at Marseille and Paris by Martin. Soon afterwards, a similar meridian circle, though smaller, was installed at the Lyon Observatory. It also was financed by Bischoffsheim. These instruments were reversible, that is to say, the telescope could be detached from its mounting and reversed so that the upper part became the lower. Then it could be repositioned on its supports. This allowed the collimation to be determined and corrections applied to the measurements. The principle had already been tested with the portable instruments constructed for geodesy by Brunner, Rigaud, and Eichens himself. The new meridian circle of Paris was not installed like the other astrometry instruments, in the observing rooms of the east wing of the Observatory, which were too narrow and whose thermal properties were, moreover, judged to be unsatisfactory. Instead the Minister of public works funded a hangar consisting of a sheet metal roof that could open widely. It was placed in the garden of the Observatory (see Fig. 7.13).

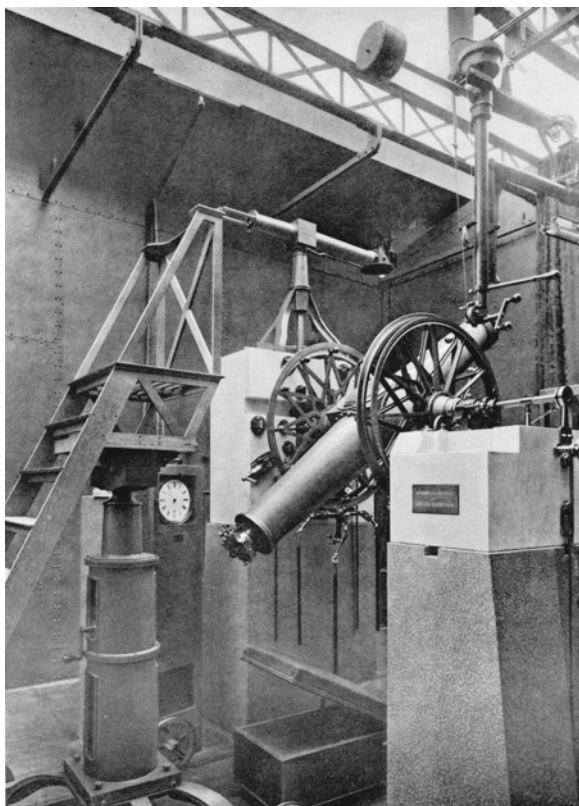
The large meridian circle of the Observatory was used during of the rest of the nineteenth and the first half of the twentieth century, with various modernizations. In 1961 the building was so dilapidated that it had to be replaced; however, funding was lacking to complete the renovation. This made observations difficult. It was then retired in favor of new instruments, more practical and better performing, such as the automated astrolabe of Danjon. The Bischoffsheim circle nevertheless is still there.

Star Catalogues

One of the great undertakings of the Paris Observatory, begun in 1837 at the instigation of the Bureau of longitudes, was the construction of a new catalogue of star positions, revising that drawn up by Jérôme de Lalande. The observations for the Lalande catalogue, which contained 48,000 stars, had been made in large part by his adopted nephew Michel Lefrançois de Lalande, with the Bird quadrant of the

²⁹ In the paper cited in the preceding note, Wolf writes: "A simple look at the great meridian circle of the Observatory, at the equatorial of the West tower, at the great reflecting telescope and at the new instrument given by M. Bischoffsheim, all constructed in the workshops of our celebrated artist, M. Eichens, shows the revolution that occurred in the construction processes. Instead of instruments made of pieces of laminated brass, assembled by simple screws or even by tin welding, these are refractor bodies made of cast iron bolted on cast iron and steel axes, whose aspect is strong and elegant; brass circles cast as a single piece and protected from distortion by many crossed beams. This is the art of the engineer applied to the construction of astronomical instruments, with the strength provided by the choice of metals and the thickness of the parts, and with the precision allowed by the machine-tools. This revolution started in England around 1847, by the illustrious director of the Greenwich Observatory M. Airy."

Fig. 7.15 The meridian circle of Bischoffsheim (1877)



Observatory of the École militaire. The instrument is currently preserved at the Paris Observatory. This enormous work was spread out over a period of 10 years, and led to the publication of the catalogue³⁰ in 1801.

The “revision” of the catalogue had as its goal to make more precise the value for the constant of precession, and would furnish, according to Le Verrier,³¹

An opportunity for the intelligent and zealous astronomer to make discoveries, such as of the proper motions of the stars, highlighting those suspected of having companions, which could therefore be objects of important research. The changes in brightness of certain stars would also lead to the recognition of variable stars; double stars will be detected, etc., etc.

The “fixed” stars are not in fact immobile, and their proper motions, so called, are nothing more than their apparent displacements across the sky relative to more distant objects, as expressed in seconds of arc per year (Fig. 7.16). Observations of the positions of stars at remote epochs allow the detection of their proper motions. One of the chief interests of the time was the recognition of nearby stars whose

³⁰ Lalande, J.: *Histoire céleste française*, contenant les observations faites par plusieurs astronomes français. Paris, Imprimerie de la République. Accessible by <http://books.google.fr/> (1801).

³¹ *CRAS 65 (1867), pp. 873–876.

Fig. 7.16 The proper motion of a star. The star moves with respect to distant stars, assumed to be fixed. The annual angular displacement is μ , expressed in arc seconds per year. The lateral velocity v of the star with respect to the Sun is related to μ and to the distance d by the relation $v(\text{km/s}) = 1.45 \mu("/\text{year}) d(\text{light years})$

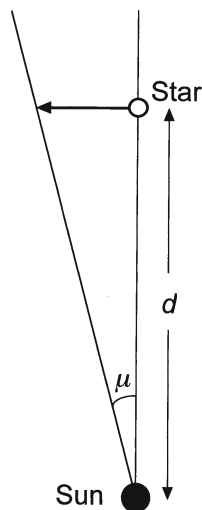
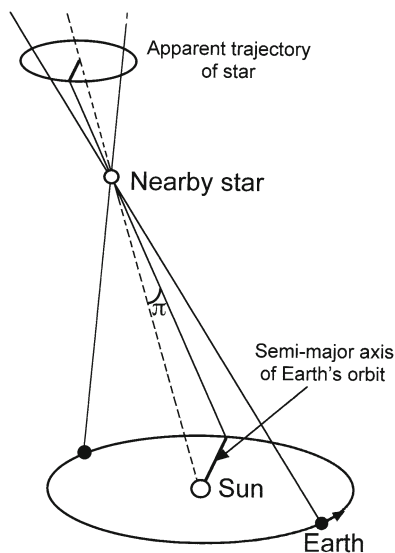


Fig. 7.17 The annual geometric parallax of a star. During the annual revolution of the Earth around the Sun, a nearby star seems to move with respect to the distant ones on an ellipse. The angle π of the semi major axis of this ellipse is the parallax, usually expressed in arc seconds. It is inversely proportional to the distance of the star. A star with a parallax of $1''$ is at a distance of 1 parsec, or 3.26 light years, or $3.08 \cdot 10^{16}$ m (but no star is as close as this)



distances might be directly measured: in effect, stars which have a large proper motion are likely to be close because, for any given linear velocity, the angular velocity will be that much greater the smaller its distance. The measure of the distance to a star would use the method of geometric parallax, which consists of measuring the star's displacement over the course of a year relative to more distant background stars produced by the orbital motion of the Earth (Fig. 7.17). It was in this way that the distance of the star 61 Cygni was determined by Friedrich Wilhelm

Bessel in 1838, the first star for which this measurement was successful.³² Another application for measurements of proper motions of stars, which Le Verrier might have had in mind but which he did not explicitly mention, was that of measuring the displacement of the Sun relative to neighboring stars. As early as 1783, William Herschel had discovered this displacement by studying the proper motions of only 17 stars. Argelander (Fig. 7.18) had confirmed and refined the result in 1830 by using 390 proper motions, and one could do even better by observing more stars.

The comparison of the new catalogue with that of Lalande would also allow the discovery of variations in brightness of certain stars. The existence of these variable stars had been known for quite some stars, and Lalande had distinguished the existence of 30 “changing stars,” which are mostly red giant stars.³³

The observations for the revisions of the Lalande catalogue had advanced little under Arago, and the same was true during the first of Le Verrier’s reigns. In November 1867, Le Verrier announced, not without optimism, that their completion would require only 7 years. The new observations took more time than Lalande’s, because the meridian circle and meridian telescope of Gambey, which were generally reserved expressly for this purpose, did not permit simultaneous measurement of both coordinates of the star, which moreover had to be remeasured at least three times in order to be sure of the result. Accordingly, the project had to be reassessed in 1873.

On the 4 December of this year, Le Verrier made before the Observatory Council a grand declaration concerning the catalogue³⁴:

M. Le Verrier explained that at the Greenwich Observatory, which has shone with such brilliance, and also at the Pulkova Observatory, work was always carried out according to a precise plan following a regimen that has been in place for a long time; at Paris unfortunately this has not been the practice [whose fault was this?] and the workplan had necessarily had to be changed. The result is that the Lalande catalogue, whose revision had begun in 1837 [by Arago] and 1854 [on Le Verrier’s arrival], has not yet been [completely] re-observed. To complete and finish the work would take a very long time. Given this, the Director, in accordance with M. Lœwy, has decided to undertake observations by zones³⁵ of all stars down to magnitude 9 ½. In this way a catalog of 300,000 stars could be obtained. By making use of the large meridian instrument [that of Secretan-Eichens], equipped with a micrometer with crosshairs, one could register the passage of 60 stars per hour, so that allowing for 120 nights of observation each year, the catalogue could be completed in 8 years. To achieve this, however, it would be necessary to give up the observation of minor planets, which has been carried out [with this instrument] for the past 10 years.

Here’s a good example of failing to come to terms with reality! There were those who had doubts about the new proposal:

³² The Paris Observatory, which already missed this measurement at the time of Arago (see Lequeux 2008, pp. 275–279), did not distinguish itself in this very important domain during the reign of Le Verrier. But then little was done elsewhere either. After 1854, there was essentially no parallax determination until the measurements of David Gill in Scotland, then at the Cape of Good Hope, who used a heliometer as Bessel did in 1838.

³³ See Dumont (2007), pp. 231–232.

³⁴ BOP, Documents divers sur l’Observatoire de Paris, 1854–1872, cote 3567(4), folder AA.

³⁵ Observations of stars for large catalogues are usually done by zones of declination.

Fig. 7.18 Friedrich Wilhelm Argelander (1799–1875)



M. Wolf finds the proposal very tempting, but he could not see without distress the abandonment of observations for the eminent French and Parisian catalogue of Lalande, in order to undertake a labor which, following a proposal of the *Astronomische Gesellschaft* [the German astronomical society], had begun more than six years earlier and involved a dozen other observatories.... [However] it is M. Læwy's conviction that this catalogue [of the *Astronomische Gesellschaft*] would never be completed.

Despite reticence,

the proposal to produce a catalogue of 300,000 stars was adopted, and the Director agreed to give satisfaction to the Council in interrupting meridian observations of the minor planets only on those days when they would be supplemented by other means.

Nevertheless, shaken by the scope of the competition which Wolf had mentioned, Le Verrier made inquiries of his various friends (including probably Otto Struve) and presented to the Council, during its 12 March 1874, the following information:

It was in 1867 that Argelander proposed to observe all stars down to the 9th magnitude, lying between -20 degrees and $+80$ degrees of declination.³⁶ In 1869 the heavens had been partitioned into zones of 5 degrees and each of these zones assigned to an observatory³⁷.... The undertaking

³⁶ This project was a follow-up of the *Bonner Durchmusterung*, a catalog containing about 300,000 stars observed with a meridian circle, begun under Friedrich Argelander. The catalog was extended after the death of Argelander by Eduard Schönfeld, and for the Southern hemisphere by the *Cordoba Durchmusterung* under the direction of J.M. Thome. The total number of observed stars is about a million, down to magnitude 9.5 or 10. Argelander and two assistants made all the observations in Bonn between 1852 and 1857 with a small meridian circle with an aperture of 7.6 cm. This catalog has been extensively used, but the positions are not very accurate so that Argelander wanted to have it superseded by a more precise one. For a biography of Argelander, see °*Monthly Notices of the Royal Astronomical Society* 36 (1876), pp. 151–155.

³⁷ Twenty observatories were supposed to participate in the project. Actually they were less: Poulkova, Kassar (Kazan), Dorpat (now Tartu), Nikolaïeff (Ukraine), Helsingfors (now Helsinki), Christiania (now Oslo), Leipzig, Leiden, Cambridge (UK), Cambridge (USA), Chicago, Berlin. No French observatory in the list.

has reached the following stage. At Pulkova all the fundamental stars have been determined.³⁸ At the other observatories, an average of 60 percent have been observed, and stars of two zones, those of Berlin and Leipzig, have been completed, so that the printing should begin shortly.

Given this situation, the director wonders whether the Observatory plans ought not to be modified.

However, Lœwy insists and demands a specialized instrument (his coudé equatorial, however poorly adapted for such a program), two or three new assistants, and 50,000 francs per year (1 franc per observation!). By way of compromise, Le Verrier proposes to observe for 1 year, as a sort of trial, 2100 stars between -5° and 0° of declination, each three times. But nothing would come of these attempts, and instead, for better or worse, work continued on observing the stars of the Lalande catalogue.

In 1878, at the time of Le Verrier's death, only 22,000 stars in the catalogue had been measured, out of 50,000 planned, of which 10,000 had been measured only once, and others had been measured too often.³⁹ Despite the efforts of Le Verrier's successor, Mouchez, the observations, whose progress can be followed in the annual reports of the observatory, were progressing slowly. The publication of the final catalogue, the *Catalogue of Paris*, would stretch out from 1887 to 1933.

For its part, the AGK (*Astronomische Gesellschaft Katalog*), which Argelander had established, would not be entirely realized either, so vindicating, at least in part, Lœwy's skepticism about the project; published in 20 volumes between 1890 and 1924, the AGK would contain nevertheless more than 188,000 stars, almost four times more than the Parisian catalogue.

The catalogue of Paris was only one of numerous star catalogues which flourished in the course of the nineteenth and the early twentieth centuries. Already facing competition from the AGK, the catalogue of Paris would be completely superseded in 1930 by the second version of the former catalogue: the AGK2, this time photographic, and encompassing close to 200,000 stars whose positions were measured to a precision of better than $1''$ of arc.

In any case, it seems that each observatory considered it its duty to make a star catalogue, without regard to its utility. But then why didn't the Paris Observatory, as well as those of Marseille and Toulouse, participate in the great international program, the AGK catalogue, to which Germany, England, Russia, and America were all committed? No doubt Le Verrier's scientific isolation entered in; for him, observations of this type held little interest, and he had them carried out to some extent out of a sense of duty and inertia.

The situation would change with Mouchez, who would be the promoter of a great international program of astrometry, this time using photography, called the *Carte du Ciel*, which undertook to photograph the heavens from a number of different

³⁸ These were bright stars whose position was measured with the highest possible precision; they serve as references in order to measure the zenith distance and the time of meridian passage of the other stars of the catalogue.

³⁹ Mouchez, E.: *Rapport annuel sur l'État de l'Observatoire de Paris* [for 1878], pp. 3–4. Gauthier-Villars, Paris (1879). He writes: "This considerable work began in 1854 [actually in 1857]. To be useful, it should have been organized following a methodic plan, well studied in advance and actively pursued.... Unfortunately this work, much too neglected, was conducted with an insufficient personnel and too often left to chance."

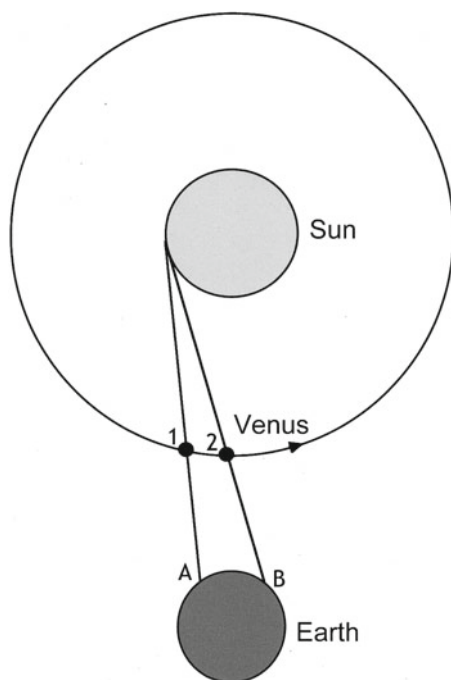


Fig. 7.19 The transit of Venus in front of the Sun. Observer *A* on the Earth sees Venus enter the disk of the Sun when it is at position 1, and Observer *B* when it is at position 2. Seen from the Sun, the angle between 1 and 2 is equal to that between *A* and *B*, whose projected distance is known from geodetic measurements. This allows calculation of the length of the arc 1–2, because the ratio of the distance of Venus to the Sun to that of the Earth to the Sun is known. As the period of revolution of Venus is known, the measurement of the time separating the two events allows calculation of the length of its orbit, hence the radius of this orbit which is the quantity being sought. One can just as well use the time of egress of Venus from the solar disk. Actually, the calculation is somewhat more complicated than described here, because it is also necessary to take into account the rotation of the Earth and the eccentricity of the orbit of Venus

observatories and to measure with precision the positions of stars visible in these photographs. But this immense undertaking, too, would never be finished.⁴⁰

The Transit of Venus

Venus passed in front of the Sun on 9 December 1874, and this phenomenon was visible over a large part of the terrestrial globe. This rare event was of great importance because it allowed the measurement of the distance of the planet and at the same time that of the Sun: in fact, the absolute values of the distances were poorly known for the Solar System, even though their relative values were well known (because of Kepler's third, or harmonic, law). In order to utilize the transit of Venus for the purpose, it is necessary to observe it simultaneously from several points on the Earth greatly separated from each other, as explained in Fig. 7.19.

⁴⁰ See Lamy (2008).

Fig. 7.20 Photographs taken by Mouchez at the Île Saint-Paul, showing Venus near the edge of the solar disk



The method, the principle of which had been laid out in 1716 by Edmond Halley,⁴¹ allowed Encke to obtain in 1824 a value of 8."58 for the solar parallax,⁴² based on observations made at the transits of Venus of 1761 and 1769. This value remained for a long time the best available, but there were hopes of improving it by observing the 1874 transit with better instruments and, especially, better synchronized clocks. The matter was deemed of such importance to astronomers that a number of expeditions were organized to different points of the globe where the transit was visible: 20 English, 8 American, 7 Australian, 5 German, etc. France, for its part, sent 9. Tisserand and Janssen went to Nagasaki in Japan, where Janssen employed for the first time his *revolver photographique* (see Fig. 5.27) allowing him to take some 30 successive images of the transit.⁴³ Mouchez led an expedition to Île Saint-Paul, a minuscule island belonging to the French Southern and Antarctic Lands in the Indian Ocean, situated 1,300 km NNE from the island of Kerguelen (Fig. 7.20).

⁴¹ **Philosophical Transactions* 29 (1716), pp. 454–465. For a very detailed study of the passages of Venus, see <http://www.imcce.fr/vt2004>, and Arlot (2004).

⁴² We recall that the parallax of the Sun is the angle under which the equatorial radius of the Earth would be seen from the center of the Sun. The present value 8."79415, corresponds to a mean distance of the Earth to the Sun (the astronomical unit) of 149.6 million kilometers.

⁴³ For the observations at Nagasaki, see Flammarion, C.: *Le passage de Vénus, résultats des expéditions françaises*, **La Nature*, 1^{er} semestre 1875, pp. 356–358, (1875).



Fig. 7.21 Medallion of the French expeditions of 1874 to observe the transit of Venus, here represented as the goddess in front of Apollon's chariot while Uranie, the Muse of astronomy, looks at the phenomenon

A medallion (Fig. 7.21) commemorates the French expeditions organized by the Academy of sciences.

As for Le Verrier, he did not interest himself in the enterprise. The Paris Observatory did not organize any expedition, and none of its members participated in the observations (Tisserand was no longer at Paris, but directed the Toulouse Observatory). Here's what the chemist August Wilhelm von Hofmann, biographer of the chemist Dumas, says about this⁴⁴:

While in 1872 and 1873, the Academy discussed measures to allow French astronomers to participate in observing the transit of 1874, a singular circumstance occurred: Le Verrier, who, by his scientific position should have had the privilege of taking the lead in directing this affair, withdrew from it. The celebrated astronomer expressed his repugnance to the large expenditure of effort and money necessarily involved in such great astronomical expeditions, since he expected the main object of these expeditions (that is to say, the determination of the ratio of the dimensions of the Earth and of the planetary system) to be achieved in the near future, more rapidly and with more accuracy, by another method: the detection of the perturbing influence the mass of Earth exerted on the neighboring planets.

In any event, the other French astronomers did not share the feeling of indifference which derived from Le Verrier's scientific convictions.... It was a pleasure to see, under the circumstances, that Dumas, who perhaps recalled his former relations with Laplace, did not hesitate to place himself at the head of a group [aimed at promoting observations of the transit] to which several of the most eminent French astronomers and physicists belonged.

In this way the French expedition to observe the transit of Venus [from Nagasaki] was organized and Dumas, on 9 October 1876, could announce to the Academy of sciences the publication of the first volume of observations.

Le Verrier, obviously, had more confidence in his own celestial mechanics methods for determining the dimensions of the Solar System than in those based on

⁴⁴ *Hofmann, A.W.: *Biographie de Jean-Baptiste Dumas*, pp. 68–69. Bureau du Moniteur scientifique, Paris (1880).

the transits of Venus (although Hoffmann, being a chemist, did not understand what these methods were). As we saw in Chap. 5, Le Verrier had deduced from celestial mechanics a number for the solar parallax of $8''.95$, and by combining the constant of aberration with the speed of light measured in the laboratory by Foucault, he obtained a value of $8''.86$ in 1862. These values are both larger than the value of $8''.58$ determined by Encke from the eighteenth century Venus transits. The latest measure of the speed of light made by Cornu between the Observatory and Montlhéry confirmed Foucault's value, furnishing a value for the solar parallax between $8''.88$ and $8''.90$.⁴⁵ These values being closer to Le Verrier's than the one deduced from the transits of Venus, Le Verrier had no confidence in what could be achieved from the transit, consequently he had little interest in it: he expressed his mistrust in a short note in the *Comptes rendus* in 1875, therefore after the observations of the transit of 1874 had taken place.⁴⁶

Actually, the observations of the transit of 1874 initially seemed not to have given results better than those of Encke. But another transit took place in 1882. This time the Observatory, which was now directed by Mouchez who had the experience of having observed the transit of 1874, sent to Martinique an expedition comprised of Tisserand, Bigourdan and Puiseux, as well as nine other expeditions. Examining the results of the two transits, Newcomb affirmed that the solar parallax was $8''.79$: this value accords well with the modern very precise one measured directly from radar determinations of the distance of the planets to the Earth, $8''.79415$. Le Verrier, for once, was mistaken: the determinations on which he had such great confidence turned out to be less reliable than those deduced from the transits of Venus. To celebrate the success of the French expeditions, Mouchez had a large oil painting placed on the ceiling of the Observatory Council's meeting room, which is shown in Fig. 7.22.

The Achievement of Le Verrier's Great Work and His Death

Le Verrier's great work, the fulfillment of the grandiose dream which he pursued his entire life, was the complete theory of the movements of the planets in the Solar System.⁴⁷ The parts related to the motions of Mercury, Venus, the Earth and Mars had appeared in the *Annales* of the Imperial Observatory of Paris, in 1859, 1861, 1858, and 1861, respectively, but Le Verrier's productivity slowed down in the 1860s. His removal from the directorship in 1870 gave him the necessary leisure to take up his work again; it was the most difficult that he had undertaken, and he attacked it tenaciously. According to Tisserand⁴⁸:

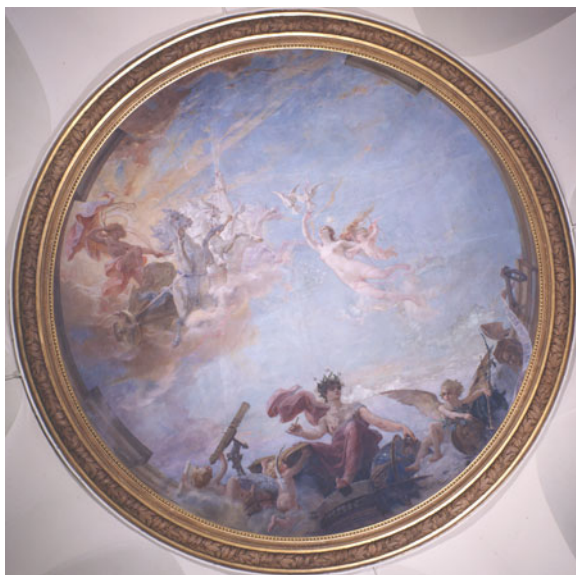
⁴⁵ *CRAS 79 (1874), pp. 1361–1365.

⁴⁶ *CRAS 80 (1875), pp. 290–291.

⁴⁷ A very clear summary of the chronology of Le Verrier's works on the dynamics of the Solar system is presented by him in *CRAS 79 (1874), pp. 1421–1427.

⁴⁸ Discours prononcés à l'occasion de la cérémonie d'inauguration de la statue de Le Verrier (1889), pp. 25–34.

Fig. 7.22 The transit of Venus in front of the Sun, painting by Dupain (1886). Venus is going to pass in front of the Sun, represented as Apollo in his chariot, while Uranie asks a little angel to observe the phenomenon with a telescope. Another angel, more to the right, holds a portrait of Halley and the trumpet of fame. Near the banderole to the right, there is a portrait of Le Verrier, and an anchor which symbolizes the ties between Astronomy and Navy (remember that Mouchez, who ordered the painting, was an admiral)



The final part of Le Verrier's career was devoted to studying the movements of the four giant planets, a study infinitely more complex than the previous ones. The intertwining of the formulas is extreme, and one can only admire the author who directs with clarity his thoughts over these masses of symbols and numbers.

In 1874, Le Verrier was on the point of publishing a long article entitled, "Determination of the mutual actions of Jupiter and Saturn, to serve as a basis for the theories of the two planets."⁴⁹ He worked now without let-up on the theory of the giant planets, assisted only by Gaillot (Fig. 7.23) for calculations, and did so despite declining health from a serious disorder of the liver (probably cancer). In his final years, he was never comfortable except when lying down, and sometimes suffered so much that his mind began to wander. On 18 October 1873, he wrote to his friend the abbé Barthélémy Aoust, professor of mathematics at the Faculty of sciences of Marseille, whom he must have met there in 1870 or 1871, a letter that is moving because of its human touch—it is this which makes it exceptional for Le Verrier. In it he related his health problems for the years 1870–1873. Here are some extracts⁵⁰:

The illness has mysteries, which have to be experienced, not in order to explain them, but in order to believe them.

Affected by a complete absence of strength, I was obliged to try four times to climb the three flights at quay Voltaire. As soon as I had gone up ten steps without resting, I felt very ill. I was well, if ever, only when lying down.

But, the curious thing was, I couldn't send a letter. The sick have their intuitions. One of the things that fatigues the most is to be constantly passing from one subject to another, and

⁴⁹ ° *Ann. OP, Mémoires*, 10 (1874), pp. 1–304 and additions pp. 1–67.

⁵⁰ Cited by Levert et al. (1977), p. 168.

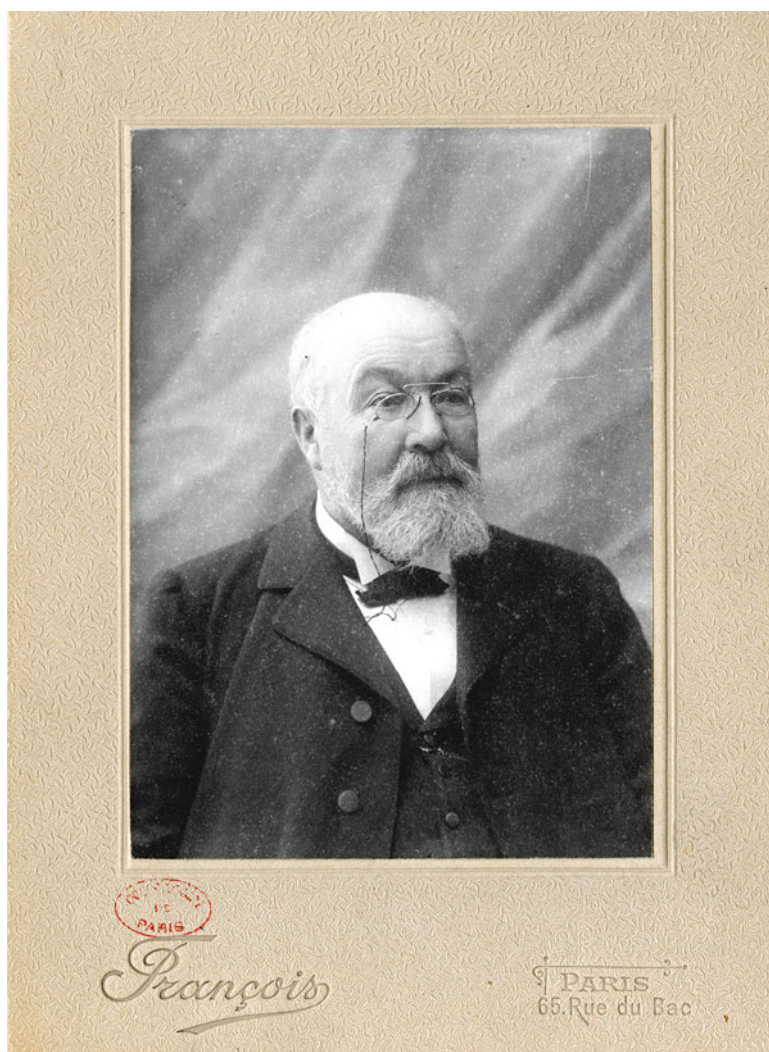


Fig. 7.23 Aimable Gaillot (1834–1921)

it is this which, in administration, is so wearing. And I imagine that this going and coming from one subject to another over so many years has contributed a great deal to my exhaustion.

Something else contributing to it is the carrying out of the very scientific work whose communication seemed to you a sign of health. People who passed at night on the Saints-Pères bridge have seen, for many nights, a lamp lit until 3 or 4 in the morning, which has left spots of oil on the manuscripts of Jupiter, Saturn, Uranus, and Neptune.

Indeed, three issues of the *Annales* of the Paris Observatory, dating from 1876, contain the theory of the movement of Jupiter, Saturn, Uranus and Neptune, with

Le Verrier.
Cable de Saturne
15 Septembre 1875.

M. le Ministre,
 J'ai eu l'honneur de vous adresser par le câble de Saturne
 les tables de Saturne que j'ai fait à l'Académie
 des Sciences. Je vous prie de vouloir bien en faire l'usage
 que vous jugerez convenable. Je vous informe que je viens de
 recevoir du Supérieur du Nautical Almanac
 anglais l'assurance qu'au plus tôt imprimées les nouvelles
 tables de Saturne, il en sera fait usage pour
 la rédaction du Nautical Almanac. En même
 temps mes collègues de St. Petersburg m'ont informé
 de leur désir de voir adopter l'impression de ces
 tables et ils m'ont assuré qu'au besoin, ils y
 pourvoieraient.

OBSERVATOIRE
DE
PARIS

J'ai adressé mes remerciements au représentant
 de l'Université anglaise à mon collègue
 d'Angleterre à qui nous devons déjà d'avoir
 vu introduire dans la science l'usage
 des tables du soleil, Mercure, Vénus, Mars,
 et Jupiter. En même temps j'ai remercié
 mon collègue de St. Petersburg. Les
 tables de Saturne seront imprimées par les
 soins de l'Observatoire, comme l'ont
 été les tables précédentes.

Fig. 7.24 Draft of a letter dated 15 September 1875, in which Le Verrier informs the Minister of Public Instruction that his tables of Uranus will be used by the English Nautical Almanac. This letter was probably dictated by Le Verrier, who could only affix his signature to it

tables corresponding to the first two planets, from which the ephemerides could be calculated. These tables, along with those that had already appeared, were very much appreciated overseas, and were utilized by the English *Nautical Almanac*, a circumstance of which Le Verrier did not fail to inform the Minister (Fig. 7.24).

By March 1877, Le Verrier's health had so deteriorated that he was unable to go to Lyon to attend his son's marriage. On 9 July 1877, Tresca visited him and wrote to Fizeau⁵¹:

I've just seen him lying on the same mattress in the rotunda—very feeble, very weary, and quite upset that his doctor tells him that he is doing better, whereas he feels worse and worse.

He continues to eat meat, enough meat, he says, but his digestion doesn't function well, and it is necessary to administer purgatives. At any moment he becomes somnolent, and speaks very softly.

In a word, my visit left on me a bad impression, and the few illusions which I harbored [of his recovering his health] have pretty much been dispelled.

It was at this time that the tables of Uranus and then those of Neptune were published.⁵² Le Verrier corrected the last proofs of the Neptune tables only a few days before his death. With this he had completed the work of an entire lifetime, an undertaking whose scope and quality were truly exceptional.⁵³ It is difficult for the non-specialist to appreciate, but it will suffice for us to say that the tables of Le Verrier (and of Gaillot, the importance of whose contribution ought not be forgotten) continued to be used until 1984, and thus for more than a century, in order to construct ephemerides for the *Connaissance des temps*.

Le Verrier had a rival in the United States in the person of Simon Newcomb. We have seen in Chap. 5 that Newcomb confirmed and calculated more precisely in 1882 the anomalous advance of the perihelion of Mercury discovered by Le Verrier. Like Le Verrier, he also undertook to construct a complete theory of the Solar System, and his results were sometimes ahead of those of the Parisian astronomer. In 1866, Newcomb published, in effect, 10 years before Le Verrier, a theory of Neptune's movements and corresponding tables, which allowed the construction of the first ephemerides for Neptune.⁵⁴ Then, recognizing that Uranus's ephemerides were still based upon a theory that did not take into account perturbations by Neptune, and which still used tables for Jupiter and Saturn that were more than half a century old, which is explained by the fact that no one wished to undertake the work necessary for revising them, Newcomb attacked the problem by himself, a toil which absorbed the major part of his leisure from 1867 until 1872. In 1873, that is 3 years before Le Verrier, he published a complete theory of Uranus, with tables incorporating for the first time its perturbations by Neptune.⁵⁵ Later, this

⁵¹ Archives de l'Académie des sciences, file Tresca.

⁵² These publications are in °*Ann. OP, Mémoires*, 11, 12 (1876; the entire volumes), 13 (1976), pp. 1–228 and 130, 14 (1877) 1st part pp. A.1–A.92 and A.1–A.163, and 2nd part pp. 1–70 and 1–96.

⁵³ There is an excellent account of the work of Le Verrier in °Gaillot (1880), in which very clear details are given on his work of the motions of the major planets.

⁵⁴ Newcomb, S.: *An Investigation of the Orbit of Neptune with General Tables of its Motion*. Smithsonian Institution, Washington (1866).

⁵⁵ Newcomb, S.: *An Investigation of the Orbit of Uranus with General Tables of its Motion*. Smithsonian Institution, Washington (1873). See also Newcomb, S.: Theory of the motion of planet Uranus. In: *Annual report of the Smithsonian Institution for 1872*. (1875).



Fig. 7.25 Le Verrier on his death bed, drawing by Giacomotti. This is, together with the painting of Daverdoing, the only direct likeness taken of Le Verrier. It was certainly used by Giacomotti to paint the portrait presented at the beginning of this chapter

time with more substantial assistance than with Uranus, whereby the Smithsonian Institution relieved him of the tedium of certain numerical calculations, he would realize, for his part, the same grand *oeuvre* as Le Verrier. These tables of Newcomb's would serve as the basis of American ephemerides until 1883, but his tables of Neptune and Uranus would be used by the *Connaissance des temps* only until 1883, when they were replaced by those of Le Verrier. There are minor differences between Newcomb's and Le Verrier's theories, but it is satisfying to recognize that they lead to results that are very similar, which shows the high quality of the work done by both men. Le Verrier, who did not read English and who discarded, often without opening, publications in this language, appears to have been ignorant of Newcomb's work. The converse is probably not true, especially since Newcomb had spent some time at the Paris Observatory in 1870–1871.

His work accomplished, Le Verrier could die content (Fig. 7.25). He had received the sacraments [of the Roman Catholic Church] on 29 June 1877, and he expired on 23 September, on the anniversary of Neptune's discovery. His funeral took place on 25 September at noon, at the Church Saint-Jacques-du-Haut-Pas, "with great solemnity." The dignitaries who marched with the coffin included Eugène Peligot, an economist who at the time was president of the Academy of sciences, the chemist Jean-Baptiste Dumas, vice-president of the upper Council of Public Instruction, Fizeau, vice-president of the Academy and member of the scientific Council of the Observatory, Faye, president of the Bureau of longitudes, the commandant (and future admiral) Mouchez, general Arthur Morin, vice-president of the Scientific

Association, of which Le Verrier was president, and an English astronomer, John Russell Hind, Superintendent of the Nautical Almanac office. The obligatory representation of the State and École polytechnique were reduced to a minimum. The burial took place at the Cemetery Montparnasse, division 11, line 1 east, no. 15. Discourses were given at the graveside by Dumas, Yvon Villarceau, Tresca, Faye and Janssen. Joseph Bertrand, who was a long way from Paris, sent to the Academy a short letter. Other funeral eulogies by Bertrand and Tisserand would be published in 1880 in the *Annales* of the Paris Observatory.

Also in failing health, Le Verrier's wife would not long survive him: she died on 1 November 1877.

Chapter 8

Telegraphic Longitudes



The world telegraphic network in 1874

The Problem of Longitudes and the Promise of Telegraphy

It is easy to determine the latitude of a location by observing the height above the horizon of the Sun or of a star whose position is known, as it passes the meridian, but the determination of longitude has for a long time given geographers and navigators headaches (see Box 8.1). It was the problem of longitudes which occasioned the foundation of the Greenwich Observatory in the seventeenth century, and in France that of the Bureau of longitudes during the Revolution.

The electric telegraph would revolutionize the determination of longitudes. It allowed the synchronization of clocks at different locations that were linked by the telegraph to a high degree of precision. The first French electric telegraph, due to the scientific instrument-maker Louis Breguet, made its appearance on 18 May 1845, the date on which the telegraph line following the railroad between Paris and Rouen was completed. For its part, the Morse system, which had been employed since 1844 in the United States, took hold also in France beginning in 1854, and would progressively supplant other systems because of its greater reliability. As for the optical system of Claude Chappe (1763–1805), which functioned since the Revolution, it disappeared in 1855. Arago figured greatly in this communications revolution. Starting in 1842, he promoted the electric telegraph before the Chamber of deputies, describing successful trials made in England, then presented before the Bureau of longitudes the first system of Breguet on 14 May 1845, just 4 days before it was put into operation. He understood that the electric telegraph should permit the synchronization of clocks, and therefore would lead readily to the determination of the difference in longitude between two places. The first determination of this sort was carried out in 1846 between Philadelphia and Washington. It would soon become possible to measure even the difference in longitude between continents and islands, thanks to submarine cables, the first of which was laid down toward the end of 1851 between Dover and Calais.

Arago wrote in 1853¹:

This idea was so natural that it was born almost as soon as the first telegraphs were installed, so that it is hard to say from whom, exactly, it took its birth. I can only assure you that the Bureau of longitudes has persevered with this since the beginning, and that, moreover, it has sought to establish direct communication between the Paris Observatory and that in Greenwich, as soon as it became possible to establish an undersea cable between Dover and Calais. This project has not yet been realized. The failure must not be imputed to any other cause than to the difficulty encountered by M. Airy in establishing a direct connection between the Observatory which he directs [Greenwich] and one of the electric lines terminating at Dover and the submarine cable. For our part, we have been ready for some time to send and receive signals. To this end, communication has been established by means of a subterranean cable along Faubourg-Saint-Jacques street, between one of the rooms of the Observatory and the central administration situated at the ministry of the Interior on Grenelle street.... The Bureau is waiting only for completion of arrangements at Greenwich before proceeding with a connection between Dunkirk, one of the points on the grand meridian of France, and the Greenwich Observatory.... Let me add, finally, that arrangements have been agreed on ... for daily transmission of the time

¹ *CRAS 36 (1853) pp. 29–31.

Box 8.1 The Determination of Longitudes

Longitude is the angle between the meridian plane passing through the point under consideration on the Earth, and a reference meridian plane. It is nowadays, by international convention, the Greenwich meridian (in the time of Arago and Le Verrier, the meridian of reference for the French was that of Paris, and for the English that of Greenwich). Longitudes are expressed in degrees, from 0° to 180° toward the east (longitudes East) or toward the west (longitudes West), or in hours, from 0 to 12 h, where each hour corresponds to 15° . The difference in local sidereal time for the passage of the same star across the meridian for each of two localities is equal, apart perhaps from the sign, to the difference in their longitudes. One can therefore obtain the longitude by observing these passages, provided one uses times that are synchronized between the two locations—but herein lies the difficulty. It is necessary, therefore, either to transport a reliable chronometer from one place to another, or to observe the same signal, for instance a celestial or astronomical event, from each of the two points to synchronize clocks, which one must hope are reliable. The first good clocks, constructed by Christiaan Huygens, allowed time-keeping fairly precisely (better than one second per day) but were not transportable while they were functioning. Fortunately, it was noted that eclipses of the Moon (passages of the Moon into the Earth's shadow) commence at the same moment all over the Earth, which allowed the synchronization of clocks at different locations. This idea was first applied by Nicolas Claude Fabri de Peiresc (1580–1637) during the eclipse of the Moon of 28 August 1635, which was observed from various locations around the coast of the Mediterranean. The measurements of longitude of these locations led to a reduction of one-third in the east–west length of the Mediterranean. Moreover, Galileo and others understood very quickly, after the discovery of Jupiter's four satellites, that the frequent eclipses of these satellites in the shadow cone of the planet presented the same characteristics as eclipses of the Moon, and could be used to synchronize clocks. In order to know when to observe these events, ephemerides were constructed to predict these eclipses. This method was quite effective from land, and was employed for almost a century by all voyagers equipped with good chronometers, but it was impossible to use at sea because of the swaying of the ship, which made it impossible to view Jupiter's satellites. Though it was possible to observe the position of the Moon relative to the stars near it in order to synchronize clocks at sea, this required difficult calculations, and moreover chronometers were severely affected by the rolling and pitching of the ship. The solution of the longitude problem came with the construction of marine chronometers that were precise and reliable by the British clockmaker John Harrison, in several stages between 1737 and 1773. Good marine chronometers would also be made in France by clockmakers such as Berthoud, Duroy, Lepaute, etc., and tested on land and sea by astronomers: by 1800, they allowed precision to a fraction of a degree, even after 1 or 2 months at sea. An improvement

(continued)

Box 8.1 (continued)

in measuring longitude on land would occur when clocks began to be synchronized by means of a chain of light signals visible from one remote point to another. Also, in Italy, shooting stars were utilized, which were visible from locations far apart.² Next was the application of the electric telegraph, which was much simpler and more precise than any of these. The hour signals conveyed by telegraph, and more recently by radio and finally by GPS, resolved definitively the problem of longitudes.

in Paris to various ports, such as Le Havre, Nantes, etc., and navigators can rely on these daily communications to set their chronometers to a high degree of accuracy.

The Synchronization by Telegraph of Greenwich and Paris

Arago died on 2 October 1853, and it fell to his successor to realize the plan he had prepared with the Bureau of longitudes. Before speaking of this in detail, it seems worthwhile for us to say a few words about what had been done previously.

The interest in astronomers in knowing with precision the difference in longitude between the two observatories of Greenwich and Paris was evidently to allow observations made at the two locations to be compared, for example, in order to verify the ephemerides of a planet. Furthermore, these two observatories, serving as origins of longitude for English and French navigators, respectively, required knowledge of the difference between the two longitudes in order to use the marine charts published in the other country. Accordingly, the numerous attempts to improve this determination will come as no surprise.³ As Le Verrier wrote in 1854⁴:

Successive efforts have been made to apply to the difficult measurement of the difference in longitudes of the two observatories all the resources available to the human mind: eclipses of the Sun [or rather of the Moon], stellar occultations by the Moon, eclipses of the satellites of Jupiter, variations in lunar coordinates, signals produced by flares exploded at high altitudes whose light could be seen at great distances, geodesic triangulation, and finally the simultaneous transport of a large number of marine chronometers carrying by turns Paris time to London or London time to Paris.

The best measurement no doubt was one carried out in 1825 by John Herschel and Captain Sabine, on the English side, and Colonel Bonne and Lieutenant Largeteau, on the French side, who synchronized a chain of clocks at various stations between the two observatories by “instantaneous firing of gunpowder” which was perceptible from any two consecutive stations. Once the clocks at the two observatories were

² *CRAS 12 (1841) pp. 426–430.

³ See Lequeux (2008) pp. 199–201 and 213–215.

⁴ *CRAS 39 (1854) p. 562.

synchronized, the meridian passages of the same stars were observed, and from this a difference in longitude of 9 m 21.46 s was deduced.⁵

Once telegraphic connections had been made between the two observatories, their directors, Airy and Le Verrier, agreed to establish a painstaking procedure for measurement. Astronomers who measured the passage of stars behind the cross hairs of a meridian instrument noted the time with a certain delay, dependent both on their appreciation of the moment the star's image intersected the cross hairs and by the speed of their reflexes: this is what is called the *personal equation*. In order to limit its effect, it was decided to exchange astronomers: Faye would make his observations at Paris, then go to Greenwich and carry out the same observations, while the English astronomer Edwin Dunkin would do the same, in the reverse sense. An analogous problem presents itself in measuring the instant of arrival of an electrical signal. Each station sent the signal from a battery pile, and it was necessary to ascertain at the other point the instant when a magnetized needle began to move as the signal arrived in a coil. Faye and Dunkin would make these measurements as before.

It was also necessary to take into account the time of propagation of the electric signal from one place to the other, a time already known to be not instantaneous: Charles Wheatstone believed he had found a velocity of propagation of 460,000 km/s, but in 1850, Fizeau and Gounelle had made the same measurement using the conductors in the telegraph linking Paris to Rouen, and obtained a velocity somewhere between 100,000 and 180,000 km/s, which is more reasonable.⁶ The uncertainty resulting from this delay could be avoided by having the signal leave first from one and then the other station, Airy and Le Verrier even decided to reverse the polarity of the piles, which (though they could not have known this at the time) was unnecessary.

From 26 May to 24 June 1854, measurements of this kind were made on about 1,700 signals, half from Greenwich and half from Paris. The observations were independently analyzed in the two observatories, and the resulting difference in longitude was 9m 20.63s, that is almost a second less than that which had been established with the gunpowder signals.⁷ The transmission delay was found to be about 0.08 s. But, as was the unfortunate habit of the period, the authors did not give any estimate of the errors of their measurements. It should also be noted that the astronomers who carried out the measurements, Faye and Dunkin, were not the ones who attached their names to the article. In this era, all the glory redounded to the observatory directors.

Triangulation and Astronomical Geodesy

In 1856, Le Verrier and General Blondel, who was in charge of triangulation efforts in France, decided to measure, by means of the electric telegraph, the difference in longitude between Paris and Bourges, or more exactly the distance from the Paris Observatory and a station situated on the plateau of Berri-Bouÿ (nowadays

⁵ *Philosophical Transactions of the Royal Society of London* 116 (1826) pp. 77–126.

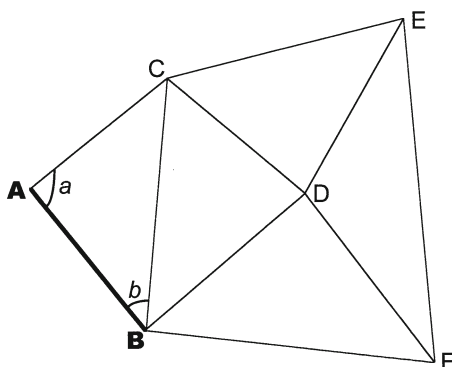
⁶ *CRAS 30 (1850) pp. 437–440.

⁷ *CRAS 39 (1854) pp. 553–566.

Box 8.2 The Method of Triangulation

One measures with rulers or chains the length of a base AB (Fig. 8.1). Then, considering another point C, for example a church steeple, one measures the angles a and b , which requires that each of the vertices of the triangle ABC is visible from the others. The triangle is therefore completely defined, and the length of BC can be calculated, which serves as the base of the triangle BCD, etc. Certain triangles are redundant, which allows for cross verifications. The errors in measurement accumulate, but the precision of the angular measurements is excellent. In principle one must take into account the relative altitude of the points, for example, by measuring with a bubble level at A the angular elevation of C relative to the horizontal; but this would not be done systematically until toward the end of the eighteenth century. Astronomical observations allow one to find the orientation relative to the north (azimuth) of the base or of one of the sides of the triangle, which is necessary if one wishes to measure a meridian arc.

Fig. 8.1 Principle of triangulation



Berry-Bouy), some dozen kilometers from Bourges. The motivation in this case was not astronomical but geodesic: it was a question of verifying and understanding certain anomalies that had been noted during the great triangulation of France carried out from the time of the Revolution to 1850.

Triangulation is a procedure that has been used since the beginning of the seventeenth century to measure great distances on the Earth. One begins with a base whose length is measured with the greatest possible exactitude, by means of rulers placed end to end. Next, one sights from the two extremities of the base a target, for example, a church steeple, and one measures from the two ends the angle between the base and the sighting direction: the triangle formed by the two extremities of the base and the target is completely determined, and one can calculate the length of its sides and construct from this other triangles (Box 8.2 and Fig. 8.1). Until the time of the Revolution, no attempt was made to take into account differences in altitude between the three extremities of the triangles, because this would have greatly complicated the calculations. In the course of measuring the distance between Dunkirk and Barcelona, along the Paris

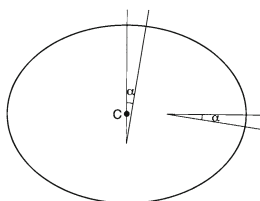


Fig. 8.2 The difference of latitude between two places on the same meridian is the angle between the verticals, which are perpendicular to the surface of the Earth but do not cross in general at the center C of the Earth because it is not spherical. The arc corresponding to the same angle is longer near the poles. The flattening of the Earth is much exaggerated on this figure

meridian, for the purpose of defining the meter, Jean-Baptiste Joseph Delambre and Pierre-André Méchain, however, measured the altitudes of all their targets and carried out the corrections needed to reduce them to the horizontal measures of length.

Triangulation in this way allows the measurement of the length of an arc on the surface of the terrestrial globe. If one wishes to know the orientation of this arc, it is necessary to determine the azimuth of one of the sides of the triangle, that is, the angle between this side and the direction north. This requires a precise knowledge of this direction. It can only be obtained by an astronomical observation, generally of the North Star. However, this is the only point at which astronomy enters the process.

In contrast to triangulation, there is also what is known as astronomical geodesy. This consists of measuring the latitude by observing the altitude of stars above the horizon at the moment of their meridian passage, and by the procedure that we have indicated earlier also measuring the differences in longitude.

Were the Earth perfectly spherical, there would be a simple relation between the arc length and the difference in longitude and latitude of its endpoints. For example, a North–South arc of 111 km would correspond to a difference in latitude of 1° . But the Earth is not spherical, and the relation between the angle that is the difference in longitude and latitude and the corresponding arc depends on the Earth's shape (Fig. 8.2). By measuring by triangulation the length of a meridian arc subtending 1° difference in latitude by astronomical means in Peru, in France, and in Lapland, it was decisively shown in the eighteenth century that the Earth is flattened (i.e., its shape is that of an oblate spheroid) as Newton had predicted. One further complication arises from the fact that, just as the longitude and the latitude involves the direction of the vertical, the relation of arc and angle is affected by possible local gravitational anomalies, due, for example, to the attraction of a nearby mountain. The existence of these deviations was known from the eighteenth century, thanks to the English astronomer Nevil Maskelyne.

In 1852, Faye had proposed a grand project of astronomical geodesy, encompassing systematic measurements of latitude and longitude. For the latter, the electric telegraph would be used systematically⁸:

In announcing to the Academy the grand project of joining, in the same electrical network, the chief locations of all our *departments*, the minister of the Interior opens to the sciences

⁸ *CRAS 35 (1852) pp. 820–821.

a new and irresistible path forward. I propose to determine, by new procedures now available to science, not only longitudes but also astronomical latitudes of all our principal locations, and to compare them to the geodesic coordinates already known, for the purpose of completing the earlier work and of characterizing local irregularities affecting the surface of our terrestrial spheroid.... These efforts would not need to be taken up again in any place on our globe except, perhaps, out of a purely theoretical interest.

The grand project of the minister of the Interior was actually carried out. However, the rest remained unfulfilled. This will not seem surprising, because these measurements are difficult and require considerable logistical means. It would be necessary to wait for systematic gravimetry and spatial geodesy in order to know precisely the irregularities of the geoid. For the time being, it was enough to have measurements of some well-chosen points. The motivation for the French project for astronomical geodesy was well explained by its promoters, Le Verrier and General Blondel, in their article describing the measurements at Bourges⁹:

It is of the greatest importance to determine definitively the local anomalies of all the principal geodesic locations, and to find out their magnitude and extent. Apart from the fact that this research is necessary in order to know the irregularities in the surface, and the composition of the exterior crust of the globe, these studies would allow the determination of the general shape with an exactitude that would otherwise be impossible. It is only after having removed local perturbation effects from each of the measurements that they could be applied to a definitive determination of the shape of the Earth's surface.

To perfect French geodesy therefore would require nowadays that a sufficiently large number of astronomical observations be undertaken, of sufficient precision, for the principal points in the mesh of triangles [used in the triangulation of France]....

The imperial Observatory is now on the point of perfecting astronomical methods, whereas the War Department, whose officers have successfully carried out the triangulation of France, should, for the benefit of science, remain in charge of questions of geodesy properly so called.

The Difference in Longitude Between Paris and Bourges

Despite the care that Delambre and Méchain applied to their measurements of the Paris meridian during the Revolution, admittedly under difficult circumstances, their results were not faultless. In 1826–1827, the geographical engineers of the Department of War measured again by triangulation a part of the meridian between Fontainebleau and Bourges, and obtained a distance of 25,613.21 m between Bourges and Dun-le-Roi (now Dun-sur-Auron, to the southeast of Bourges) instead of 25,609.23 m as Delambre had found.¹⁰ The difference here is not negligible. In 1836, Louis Puissant discovered a rather subtle error in Delambre's calculations. The very same Puissant, analyzing various triangulation and astronomical measurements, decided that the two sides of France on either side of the Paris meridian belonged to ellipsoids, one flat and the other elongated. For his part, Colonel

⁹ *Ann. OP, Mémoires* 8 pp. 1–36.

¹⁰ This information, and much of the rest in this section, comes from a manuscript text of Yvon Villarceau dated December 1866, *d'Exposé sommaire des travaux géodésiques exécutés par les savants français et proposition d'une nouvelle mesure de la méridienne de Dunkerque*, BOP, Documents divers sur l'Observatoire de Paris, cote 3567 (4), folder AS.

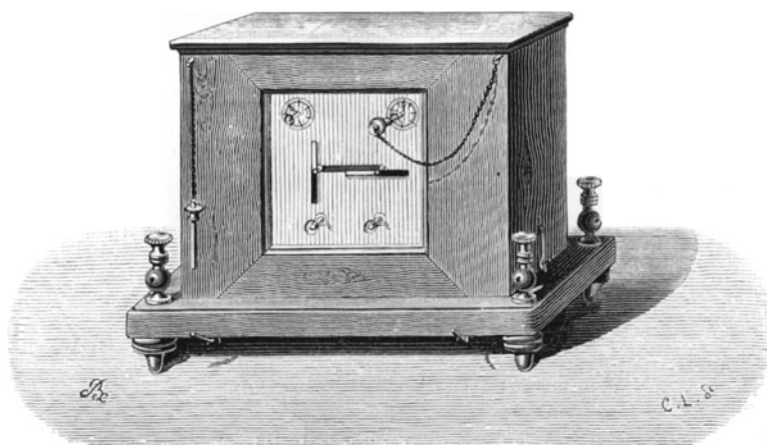


Fig. 8.3 The receiver of the electric telegraph of Foy and Breguet, with two needles. Each letter or sign corresponds to different orientations of these two needles. The code reminds one of that of the Chappe optical telegraph. This was adopted because it was thought that the operators would have less difficulty using a code with which they were already familiar rather than trying to retrain themselves on a new one

Broussard deduced, from existing observations of longitude, that the length of a degree presented considerable variations without satisfactory explanations. It was therefore necessary to carry out new observations.

As Bourges lay approximately in the center of France, it was logical to begin with this station and to relate it to the Paris Observatory. The difference in latitude posed no problem, and it was sufficient to measure the difference in longitude. Operations began in October 1856. Le Verrier himself observed on the one end and Commander Rozet on the other between Paris and Bourges, then they switched places and observed again. Thanks to “eagerness and extreme generosity of the Vicomte de Vougy, general Director of the Administration for telegraphic lines,” two cables connecting the Paris station to that of Bourges had been made available for the observers during the night: two cables, because the telegraphic system constructed by Alphonse Foy and Breguet, which was used at the time between Paris and Bourges, employed two orientable needles, the return for the current being through the Earth (Fig. 8.3). This time, the telegraphic signals were registered at Paris on a drum of rolling paper, “as with the Morse telegraph,” on which ticks of time, produced by a pendulum set to sidereal time, were also recorded. To avoid problems resulting from dried pen points of the Morse telegraph, electrochemical recording was employed using an electrolyte compound coating the rolling paper. By using the round-trip signal over the two wires, it could be shown that the time of propagation was negligible (effectively, it is on the order of a thousandth of a second) and because of this, there was no need to install a recorder at Bourges; instead, it was satisfactory to rely only on a synchronization of clocks between Bourges and Paris.

The astronomical observations of the meridian transits of stars were made at Paris with the meridian telescope of Gambey, at Bourges with a portable meridian refractor (Fig. 8.4), likewise of Gambey, belonging to the War Department.

Fig. 8.4 A portable meridian telescope



Reading the report on the observations, one gets an impression of great professionalism. Clearly Le Verrier, though maligned as incompetent in the practical business of observing, had learned perfectly the skills needed by an observer, and was up-to-date on all the problems to be encountered in observing. The most modern techniques of the period were employed. The result of all this work, in which the provisional observatory of Berri-Bouÿ was also related by triangulation to the other geodesic points in the triangulation of France, was to show a difference of 0.40s in time (or 6 arc sec corresponding to 125 m) between the determination by triangulation of the longitude of Bourges and its astronomical determination. However, no estimation of the error in this quantity was forthcoming. The article concluded, rightly:

We will not attempt, for the moment, to draw any conclusions from this datum. It will be necessary to make many more measurements before attempting a useful discussion of the small deviations that might exist between astronomical coordinates and geodesic ones. Only by comparing an ensemble of results could we say anything of true scientific value.

Other Measures and New Quarrels

In 1861, Le Verrier tackled the difference in longitude between Paris and Le Havre. It was a question this time of installing in the port an accurate timepiece, by which navigators could set their marine chronometers before setting out to sea. The procedures of measurement were similar to the ones used at Bourges; the two observers

were Le Verrier and Lepissier. The results of four measurements carried out between 17 and 20 November agreed within 0.4 s, which was quite satisfactory. The discrepancy among previous measures utilizing gunpowder signals had been greater.¹¹

From 1861 to 1865, the Paris Observatory determined the difference in telegraphic longitude between the principal points of the French trigonometric network and Paris, and measured again their latitudes and sometimes also the azimuths of one of the sides of the triangles used for the triangulation. After Bourges and Le Havre, the observations involved Brest, Dunkirk, St. Martin-du-Tertre near Paris, Strasbourg, Nantes, Saligny-le-Vif (another station near Bourges), Talmay (Côte-d'Or), Marennes, Lyon, Rodez, Carcassonne, Biarritz. To these were added Greenwich and Madrid. Almost all the measurements were made by Yvon Villarceau and his assistants, first with existing instruments and then in 1864 with two portable meridian circles by Rigaud equipped with an objective by Secrétan, which "furnished results that rival in accuracy those furnished by fixed instruments of permanent observatories."¹² The results were published in the *Annales de l'Observatoire impérial de Paris*.¹³

These efforts ended after the campaign of 1865, and Le Verrier charged Yvon Villarceau with comparing the astronomical determinations of longitudes and latitudes with the results of triangulation, in order to study, from within France, the shape of the Earth. Yvon Villarceau had to correct the triangulation in order to eliminate errors in the 1827 measures of the meridian of Fontainebleau. Questions were also posed as to the validity of the azimuth determined by Delambre in order to orient the meridian of Paris, but the new values confirmed it. Yvon Villarceau now obtained values for the oblateness and the equatorial radius of the Earth which seemed more deserving of confidence than the previous ones, but the problems remained: for instance, there was a discordance in the longitude of Lyon of 12". Yvon Villarceau, whose competence in geodesy was acknowledged by everyone (his observational techniques would be followed for almost a century), established a theorem which allowed him to correct the astronomical measurements for local deviations in gravity, the remaining problems being due to flaws in the geodesic network. He concluded: "Either the surface of France cannot be fitted to an ellipsoid of revolution, or there exist in the meridian of Dunkirk, in particular to the south of Rodez, imperfections other than those uncovered by the measure of the meridian at Fontainebleau."

¹¹ *CRAS 56 (1863) pp. 164–170.

¹² For a description of these instruments, see °Ann. OP, *Observations* 18 (1864) pp. 43–63 and °Ann. OP, *Mémoires*, 9 (1868) pp. 1–25.

¹³ See °Ann. OP, *Mémoires*, 8 (1866) pp. 1–36 (Bourges), 37–82 (Le Havre), 209–256 (Dunkirk), 257–308 (Brest, Biarritz, Madrid and Nantes), 309–390 (Strasbourg and Talmay), 392–398 (Marennes); 9 (1868) pp. 26–55 (Brest again), 56–88 (Rodez), 89–124 (Carcassonne), A.1–A.33 (Saligny-le-Vif), A.34–A.75 (Lyons), A.76–A.90 (Paris, station of the Observatory garden), A.91–A.130 (Saint-Martin-du-Tertre). The papers concerning Dunkirk, Strasbourg and Talmay, Brest (2^d), Rodez and Carcassonne, Saligny-le-Vif, Lyon, Paris and Saint-Martin-du-Tertre are signed by Yvon Villarceau.

However, not everyone looked kindly at the geodesic work of the Observatory. In permanent war with Le Verrier, the Bureau of longitudes would itself have liked to direct the whole operation. It was remembered that the decree of 1862 had given the Bureau the prerogative of offering advice on scientific questions related to its area of competence, i.e., geodesy. It would not refrain from using this prerogative in creating a commission composed of Delaunay, Faye and Laugier in making a report, which was adopted in 1862. The report proposed the following program¹⁴:

- To study the influence of local attractions on the vertical, and to calculate them;
- To verify the questionable parts of the meridian of France;
- To take up again determinations of the astronomical longitudes and of gravity along the parallel of Paris (from Brest to Strasbourg) and also the mean parallel (passing through Bourges);
- To correlate the geodesic standards of different countries;
- “To establish a permanent collaboration between the War Department and the Bureau of longitudes”;
- To set up a special service dedicated to the study of geodesy.

As this program duplicated Le Verrier’s projects, the Bureau’s motive was evidently, at least in part, revenge against Le Verrier. It would get its opportunity through an initiative coming from abroad when the retired director of geodesy in Prussia, Johann Jakob Baeyer, proposed to his war minister in 1861 a project of international cooperation in geodesy that had been adopted in Prussia and that arrived by diplomatic channels to various European countries. In 1862, 14 of these countries had made known their intention to participate in the organization, called *Europäische Gradmessung*, which later became the *Association géodésique internationale*. The protocol was transmitted to the Academy of sciences, which asked Faye to examine it. His report¹⁵ was very favorable. Besides, the Minister of Public Instruction sent the protocol to Le Verrier, who was equally enthusiastic, and emphasized his telegraphic measurements of longitude. Le Verrier promised the full assistance of the Observatory. But Faye and Delaunay objected. They maintained that this fell outside the Observatory’s scope, and that it was the Bureau of longitudes which ought, in collaboration with the War Department, to carry out all geodesic work according to the terms of an international framework that had previously been agreed. Indeed, after the measurement of the longitude of Bourges in October 1856, Le Verrier had himself renounced all further collaboration with the War Department.

The controversy between Le Verrier and Faye, and therefore between the Observatory and the Bureau of longitudes, raged on, and as it did filled numerous pages in the *Comptes rendus* of the Academy of sciences for 1863.¹⁶ Finally, due to

¹⁴ See Levallois (1988) pp. 129–131.

¹⁵ *CRAS 56 (1863) pp. 28–34.

¹⁶ See e.g. *CRAS 56 (1863), pp. 105–118; this text gives a good summary of the geodetic work of Le Verrier before 1863.

lack of means on the part of the Bureau, the dispute subsided, but each side clung tenaciously to its position. Le Verrier and Yvon Villarceau would be able to carry out the measurements of latitude, azimuth, and longitude telegraphically, as we have described.

According to Jean-Jacques Levallois,¹⁷ these disputes, despite their bitterness, were actually quite beneficial for French geodesy:

They had attracted the attention of the high authorities convincing them of the necessity of modernizing the geodesic network of the geographical engineers; in particular, the President of the Bureau of longitudes from 1860 to 1870, marshal Vaillant, was minister of War at the time¹⁸ and could therefore transmit his orders directly to the War Department.

The plan of the Bureau of longitudes traced the lines that were to be followed.

The excellent work of Le Verrier and [Yvon] Villarceau had highlighted the insufficiency of Delambre's meridian, and pointed out a way of astronomical validation of the geodesic network.

However, no decision had yet been taken in 1866 concerning modernization of the geodesic network, so that Yvon Villarceau, in writing the report mentioned in footnote 10, would conclude with a vigorous plea (full of nationalist overtones) favoring a new measurement of the meridian from Dunkirk to Perpignan.

The New Triangulation of France

It was Captain François Perrier who would be the key figure in the new geodesic work. An accomplished diplomat as well as an excellent observer and organizer, he worked out in 1869 a project for new measurements inspired by the Bureau of longitudes and the document of Yvon Villarceau. He submitted it to the Bureau's geodesic commission. This commission had just added to its membership Yvon Villarceau, who at the time was at odds with Le Verrier over the possible relocation of the Observatory. The Bureau's report, evidently very favorable because the principles of the project emanated from the Bureau itself, was transmitted through the Ministry of Public Instruction to the minister of War, who at the time was the marshal Adolphe Niel. The latter summoned Perrier and decided, after hearing him out, to act on this proposal. Here's an extract of his response to the Bureau of longitudes¹⁹:

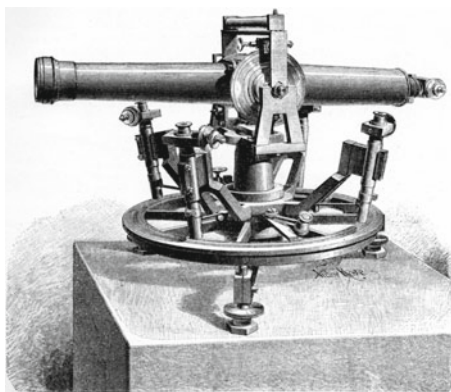
I have the honor of informing you that I gladly approve the proposal to carry out by officers of the War Department, first, revision of those parts of the geodesic meridian of France which are known to be defective, second, the prolongation of this meridian to Algeria.

¹⁷ Levallois (1988) p. 131.

¹⁸ Inexact: the marshal Vaillant was indeed the president of Bureau of longitudes, but he was no longer minister of War, but minister of the Emperor's house. However, he maintained a considerable influence in all domains.

¹⁹ Minutes of the meeting of 19 Mai 1869 of Bureau des longitudes, cited by Levallois (1988) p. 132.

Fig. 8.5 The reiterative azimuthal circle of Brunner



The plan, then, was not to simply revise the meridian, but to extend it to Algeria where a 900 km chain of triangles had already been measured in 1863. (The measurement ran from Tunisian frontier to that of Morocco.) For the second part, Captain Perrier, realizing how old the geodesic instruments were at the War Department, saw to the construction, at his own expense, of a new instrument by Brunner, the azimuthal circle (Fig. 8.5). It performed so well that it would be used with some improvements until 1945, when it was finally replaced by a theodolite with glass graduations. The old rulers of Borda, moreover, used until then for measuring the bases were replaced with new ones conceived by Porro and likewise made by Brunner (Fig. 8.6). The meridian operations began in 1870, but were interrupted by the war with Prussia. They resumed only in 1872, and were completed in 1888. This long delay was due to the small number of surveyors who had to work both in France and in Algeria where the triangulation was pursued in parallel. The azimuth of the new triangulation had been determined by Yvon Villarceau himself in 1866, between Saint-Martin-du-Tertre and the cross atop the Pantheon; the latter serving as the central geodesic station in the Parisian region.²⁰

The prolongation of the meridian into Algeria, completed in 1879, was a heroic achievement. Before it could start, the national geographic department in Spain had to complete its triangulation in the south of the peninsula and coordinate it with the French triangulation in the region of Perpignan. It would have been easy to link up southern Spain with Algeria by passing from Gibraltar to Morocco, but the political situation did not allow this. Instead it was decided to link them up directly using summits in the two countries, with lines of sight of 225–270 km! Carried out at night, these sightings required powerful projectors using an arc lamp powered by a dynamo of Gramme; the latter in turn was powered by a small steam engine. One can imagine the hardship in bringing all this equipment to the summits of the mountains, notably that of Mulhacen (3841 m) in the Sierra Nevada, where weather conditions were inclement. The measurements were excellent despite the difficulties. They brought to their participants, and

²⁰ *CRAS 63 (1866) pp. 776–785. The Paris Observatory, the origin of the longitudes at this time, was already lost in the thicket of other buildings, and not visible to long-range sights. It therefore had to be linked up geodetically with the Pantheon.

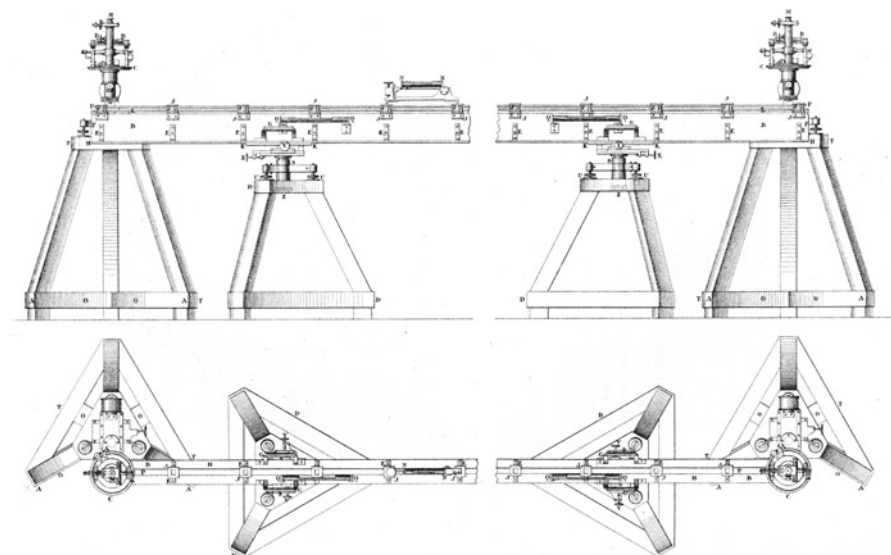


Fig. 8.6 The “base apparatus” of Brunner. The ruler used to measure lengths is placed horizontally thanks to a level (*upper figure*) on a complex support which limits the flexures (*lower figure*). The graduations at the ends of the ruler are observed with a microscope. After reading, one displaces the ruler by its own length while leaving one of the microscopes in place, so that it observes now the other end of the ruler. The base can be measured step by step in this way

especially to Captain Perrier, great renown in geodesic circles, as well as significant compensation. Perrier, for whom this would be the last observational campaign, was promoted to the rank of general. Sadly, he died prematurely in 1888.

These various geodesic operations would be the kernel of the new triangulation of France which would ultimately lead to maps at a scale of 1/25,000, which French hikers to this day know very well.

The Development of Telegraphic Longitudes

What did the Observatory do during this time? Not much. It seems that Yvon Villarceau, who had been a stalwart supporter of Le Verrier until 1866, fell out of favor. Nothing happened during Delaunay’s reign. He had other irons in the fire more important than organizing campaigns of astronomical geodesy. Nevertheless, after Delaunay’s death in 1872, the Bureau of longitudes again decided to take up work on longitudes. This time they were allied to Le Verrier, it seems. To this end, it obtained a budget of 12,000 francs.²¹ There was also the question of coordinating

²¹ Minutes of the installation meeting of the Observatory Council, 19 March 1873: BOP, Documents divers sur l’Observatoire de Paris, 1854–1872, cote 3567(4), folder AA. These minutes were very useful in writing this chapter.

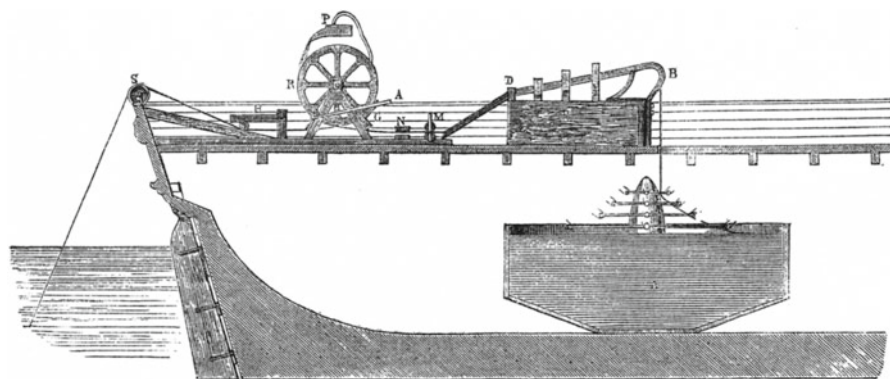


Fig. 8.7 The apparatus for laying the telegraphic cable from Marseille to Algeria in 1857. The cable is wound in a special container in the hold of the ship, and a rotating arm allows it to be directed vertically onto the deck, where it is guided to the pulley at the stern

between the Observatory and the Bureau of longitudes the measuring rods used in the different countries for measuring the bases in order to reconcile their triangulations. Evidently relations between the two institutions were off to a fresh start, and an *entente* was negotiated between them and the Minister of War, “to support the necessary relations between triangulations and astronomical geodesy.” The Observatory’s geodesy department was reorganized and placed under the direction of Yvon Villarceau. However, at the end of 1873, it was found that the Minister had not approved the 20,000 francs requested for the War Department. One of the two technicians who supported Yvon Villarceau performed inadequately, and was finally cashiered; the other simply retired.

However Yvon Villarceau had just measured with the military geographic engineers the telegraphic longitude of Vienna in Austria as well as a fundamental azimuth at Labastide-du-Haut-Mont (Lot). The plan was to measure between the winter of 1873 and the spring of 1874 the telegraphic longitudes of Marseille, Algeria, Toulouse, and Bregenz. A trans-Mediterranean cable was laid in 1857 between Marseille and Algeria by a ship especially equipped for the purpose (Fig. 8.7). This cable would permit measurements of the difference in their longitudes.

The determination of longitudes by means of telegraph followed logically from the development of the telegraphic network, which became world-wide (see the figure at the front of the chapter). The English, who dominated maritime traffic and possessed a far-flung colonial empire, were the first to interest themselves in this development. After unsuccessful attempts, the first trans-Atlantic cable was laid in 1866. It was the achievement of the Anglo-American Telegraph Company, and was laid between Valentia in Ireland and Saint John in Nova Scotia, by the ship *Great Eastern*, under escort of two warships. In 1870, the British Indian Telegraph Company completed the laying of a cable which secured the connection Gibraltar-Malta-Alexandria-Aden-Bombay. Submarine cables, which had a tendency to break, were perfected (Fig. 8.8); here again the English were pioneers, because the essential gutta-percha (the milky juice of the Malaysian sapotaceous tree *Palaquium Gutta*)

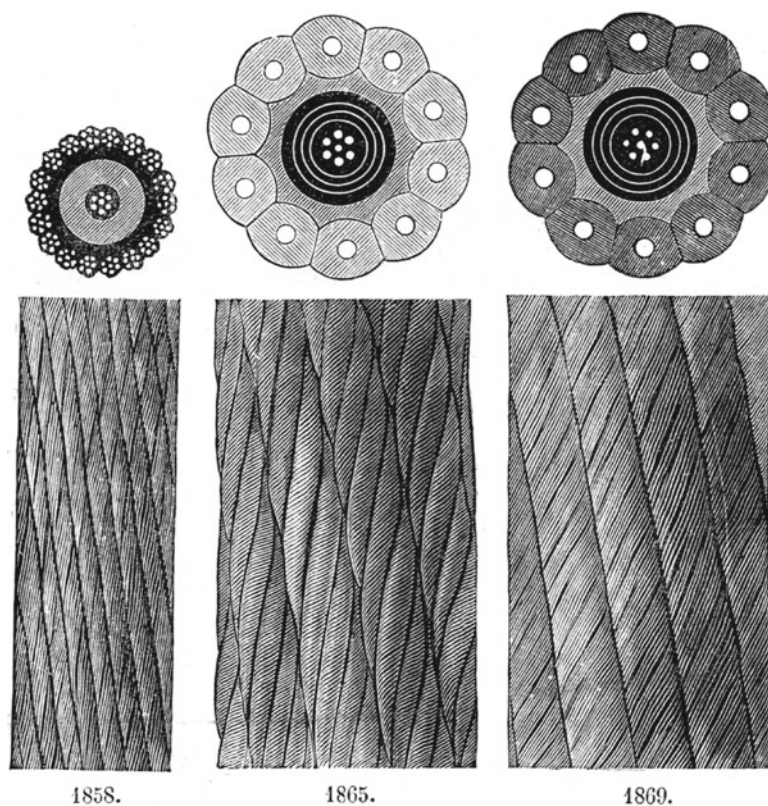


Fig. 8.8 Various undersea cables laid between Ireland and Newfoundland from 1858 to 1869

used as an insulator was under their exclusive control. Use of this means of communication remained, however, delicate and slow because of the weakness of the electric signal (there were no repeaters capable of amplifying the signal along its course). It was necessary instead to use extremely sensitive galvanometers, which were developed by William Thomson (Lord Kelvin) (Figs. 8.9 and 8.10).

Of course, France, which had its own colonial empire, could not remain indifferent to these developments. In 1867 it laid a cable from Brest to Saint-Pierre, in the archipelago of Saint-Pierre-et-Miquelon (Fig. 8.11). But England maintained its supremacy. On the eve of the First World War, the English had laid 250,000 km of submarine cables, as against 145,000 for the United States, and only 35,000 for France (while Germany had even less). England in this way had all the advantages not only in information acquisition but in propaganda dissemination. Fortunately, political problems did not affect the determination of telegraphic longitudes, as the whole world found it in their interest to cooperate. The International Telegraph Union, founded in 1865, did much to facilitate this cooperation. Figure 8.12 shows the locations in Europe whose longitudes had been determined by 1881 using the electric telegraph.

Fig. 8.9 A galvanometer of William Thomson (Lord Kelvin) used for reception of trans-Atlantic telegraphic signals. The signal feeds a coil, at the center of which a small movable mirror is suspended to torsion wires. A small magnet is glued to the back of this mirror, and the magnetic field created by the current in the coil rotates this magnet. A light beam falls on the mirror and is reflected on a scale where one can read the deviation of the reflected beam

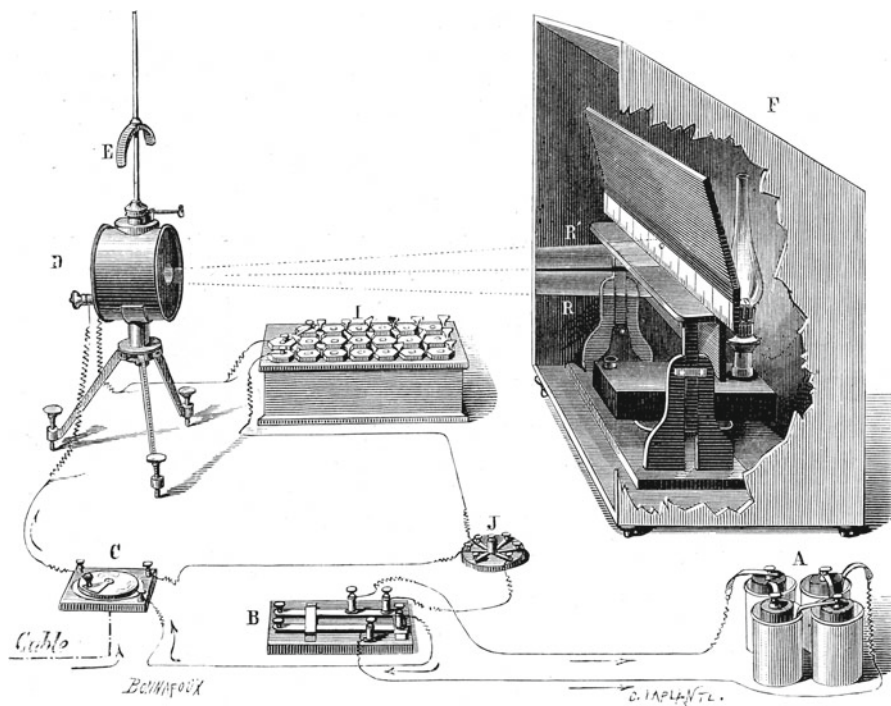
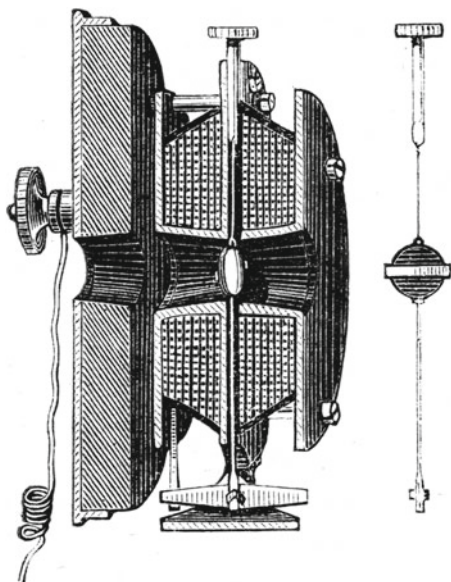


Fig. 8.10 The receiving station of the transatlantic telegraph at Brest in Saint-Pierre-et-Miquelon (France, south of Newfoundland), in 1867. A galvanometer similar to that of Fig. 8.9 sends a light beam to the graduated ruler on which its deviation due to the electric signal can be observed

Fig. 8.11 Section of different cables used for transmission from Brest to Miquelon in 1867

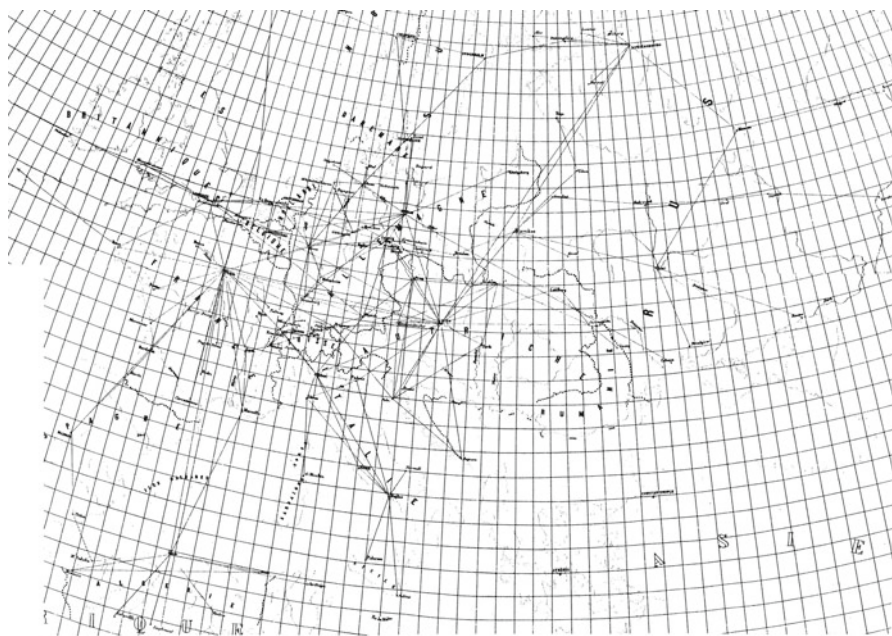
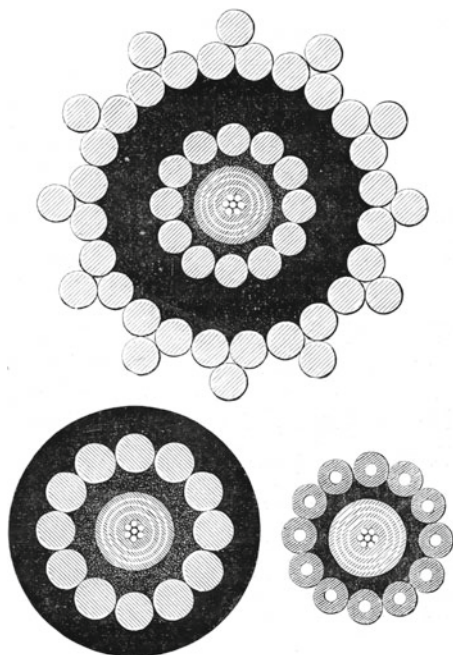


Fig. 8.12 A map of Europe indicating the places between which the difference in longitude had been measured with electric telegraph until 1881. It is certainly incomplete for the British Isles. The American network was at least as dense as the European one

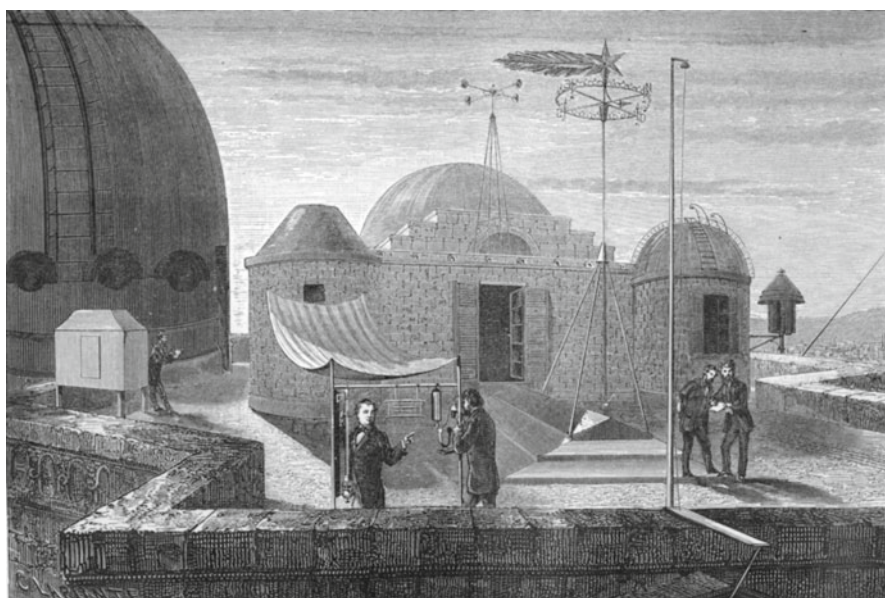
How could the Observatory participate in these campaigns with its financial means reduced to almost nothing? It was only in May 1874 that Le Verrier found some small financing that allowed him to send an astronomer to Bregenz, Marseille, and Algeria. It wasn't Yvon Villarceau, but Lœwy. In October 1874, the longitude of Bregenz was measured through an exchange between Lœwy and Theodor Egon von Oppolzer, the two observing alternately between Paris and Bregenz. Next, the longitude of Marseille was measured in the same way by Lœwy and Stephan, who also measured the difference in longitude between Marseille and Algeria. For verification, the difference in longitude between Algeria and Paris was measured by Lœwy and Perrier, through an accord brokered by the War Department. Lœwy was nominated toward the end of 1874 "director of the special department of terrestrial longitudes and latitudes." Yvon Villarceau was sidelined. Overburdened with work, because he still directed the astronomical observations and had to edit the *Connaissance des temps* for the Bureau of longitudes, Lœwy would not receive any recognition or gratitude except for the following words spoken by Le Verrier before the Council on 11 November 1875:

The [meridian] department has doubled in extent and gained in absolute accuracy ever since M. Lœwy has ceased to take part in it [in order to carry out his astronomical geodesic measurements].

However, the director of the Observatory—or the Council—had some remorse regarding Yvon Villarceau. In September 1875, Le Verrier hoped to see him again take an "active role in the work of the Observatory." He wanted to entrust to him a new measurement of the constant of aberration of starlight, which he would combine with that of the speed of light made by Cornu in order to obtain a more accurate value of the dimensions of the Earth's orbit. This proposal seemed not to please Yvon Villarceau very well, who above all would have liked to reorganize a geodesic department over which he would be in charge. Given the indefiniteness of the situation, he addressed himself to the Minister, as did Lœwy, to obtain instructions as to what he should do. Finally Le Verrier made available to him the equatorial of the West Tower for his personal work. As to the department of terrestrial longitudes and latitudes of Lœwy, as there was nothing more to be done, it disappeared of its own accord. This was the end of geodesy at the Observatory. It represented the final definitive separation between astronomy and geodesy in France. Though the Observatory would carry out still a number of measurements of telegraphic longitude, triangulation and geodesic astronomy would henceforth entirely become the domain of the Geographical Section of the Army as it was known until 1940 when it was renamed the *Institut géographique national* (IGN).

Chapter 9

Le Verrier and Meteorology



The meteorological instruments on the terrace of the Observatory

As strange as it may seem, Le Verrier was arguably as interested in meteorology as in celestial mechanics, and it was in this domain that he was able to fully utilize his organizational talents, despite difficulties of all sorts, including those inevitably produced by the rigidity of his character. Beginning with his arrival at the Observatory in 1854, he started to set up an organization that would plant the seed of the current *Météo-France*. The great innovation was that it had become possible, thanks to this service, to effectively predict the weather, something that had been envisioned well before Le Verrier but which had never been realized on a large scale. Let us first see what the situation had been at his arrival.

The Precursors

Meteorological Instruments

Mankind has always studied and aspired to predict the weather. However, meteorology only became a science after it had at its disposal measuring instruments that were reliable, reproducible, and convenient to use:

- The anemometer, invented by Leon Battista Alberti in 1450, was reinvented by Robert Hooke around 1687; the anemometer with two hemispheres used nowadays was invented in 1846 by the Irish astronomer Thomas Romney Robinson;
- The graduated thermometer was invented by Daniel Fahrenheit, 1717, and then by Antoine Ferchault de Réaumur, 1730; before that thermometers did not have graduations.
- The first graduated barometer of high quality was that of Nicolas Fortin in 1800; the metal aneroid barometer was invented in 1843 by Lucien Vidie;
- The hair hygrometer (not to be confused with a hygrometer, which is a different kind of instrument) was invented between 1776 and 1781 by Horace Bénédict de Saussure; for accurate measures it was replaced in 1825 by the psychrometer of the Prussian Ernst Ferdinand August, which in turn was perfected by Victor Regnault in 1845;
- The rain gauge is the simplest of instruments, but this did not preclude its undergoing numerous improvements.

Usually, meteorologists contented themselves until the middle of the nineteenth century, notably at the Paris Observatory, with making one or several instrument recordings per day, and establishing monthly and annual averages.

The First Attempts at Simultaneous Observations and a Prediction

However, certain savants realized very early on that it would be of interest in predicting the weather to compare meteorological observations made at the same time from different stations. The first coordinated observations of atmospheric pressure were effected

between 1649 and 1651 in Paris, Clermont-Ferrand and Stockholm at the instigation of René Descartes and Father Marin Mersenne: Mersenne observed in Paris, Blaise Pascal and his brother-in-law and assistant, Florin Périer, in Clermont-Ferrand and Descartes in Stockholm. Other attempts took place in England and Germany. The first international meteorological network was formed in Florence starting in 1653 by Ferdinand II, Grand Duke of Tuscany. It was taken over in 1657 by the *Accademia del Cimento*, which Ferdinand founded that year. The network included several cities in Italy, to which were added Innsbruck, Osnabruck, Paris and Warsaw. It did not outlive, however, the dissolution of the Academy in 1667. Subsequent German and English attempts were even more ephemeral, or led to no tangible results.

In France, the creation of a national network was instigated by physicians under the leadership of one of them, Paul-Jacques Malouin. The reason physicians (as opposed to physicists) were involved is that Malouin believed, as many of his colleagues did, that the weather affected the course of certain illnesses, notably epidemics. At his request, Louis Cotte, parish priest of Montmorency and friend of Jean-Jacques Rousseau (author of a *Treatise on Meteorology* published in 1774 and *Memoirs on Meteorology* in 1788¹), put in place such a network, beginning in 1765. This network was taken up again, in a more official capacity, by the Royal Society of medicine, created in 1776, of which Cotte was the secretary. Many physicians made meteorological recordings three times a day and sent them to him. The network, which included up to 206 stations at one point, extended to the Low Countries, where the Medical society of The Hague collaborated in the project, to various European countries, even to America. The society functioned from 1776 until 1792, but its work was interrupted by the Revolution. The data it acquired was finally exploited in 1965, when it was used to study climates in the past by Jean Meyer and Emmanuel Le Roy Ladurie. At the same time, the Royal Society for agriculture, founded in 1761, brought together observations by numerous correspondents and published works on weather and its effects on agriculture. In other countries, mention should be made of the Meteorological Society of the Palatinate formed in 1780 in Mannheim by Karl Theodor, Elector of the Palatinate. This society furnished measuring equipment to 57 institutions throughout Europe and North America, and collated their observations. It ended operations in 1792.

Of course, the results of these measurements, which only involved temperatures and, qualitatively, a few other parameters, took time to reach those who used them, and could only describe a meteorological situation after the fact. They could not, therefore, be used except for statistical purposes.

It seems that it was the chevalier Charles de Borda, a brilliant physicist well-known for his contributions to geodesy, who was the first in France to compare barometric observations carried out simultaneously in various places in order to arrive at general conclusions. With his friend Étienne-Nicolas Blondeau, he arranged to make observations, in 1777 or 1778, over a period of 2 weeks, on the same days and at the same hours, with barometers placed at the extreme points of France: at

¹ *Cotte, L.: *Traité de météorologie*. Imprimerie royale, Paris (1774); (1788) **Mémoires sur la météorologie*, Paris, Imprimerie royale.

Brest, Paris, and Strasbourg, and probably Lorient and Rochefort. His conclusions were reported by Antoine Lavoisier²:

1. The barometer does not vary at the same time at all points in an area of large extent, but in succession.
2. The variations take place successively in different locations following the direction of the wind, such that, for example, given a wind out of the west, the barometer varied first at Brest, the next day at Paris, then 2 days later at Strasbourg.
3. The variations don't always pass through the same places, before coming to equilibrium, but often follow more or less indirect routes.
4. There is a correspondence between the force and direction of the winds and barometric variations measured in a large number of locations over great distances. Thus, given two of these three quantities, it is often possible to deduce the third.
5. The columns of air which compose the atmosphere are in a state of continual oscillation; at a given point, these occur sometimes at a higher, sometimes at a lower elevation, and they arrive at a state of repose only through such oscillations.

And Lavoisier adds:

... Struck by the importance of the results that could be obtained by following the same plan, [Borda] attested to several members of the Academy a desire he had to undertake a collaborative work following the same object, and I offered to second him in this interesting enterprise, or rather follow the plan he had proposed.

Unfortunately, the Academy of sciences showed minimal interest in the program, and it was never carried out. Lavoisier, who before his execution by the infamous Committee for the Public Safety, collaborated with Borda in the definition of the meter and prepared with him the necessary materials for measuring the Paris meridian, was the only one who remained interested in the project. He had already written in 1765, "Rules for predicting changes in weather from variations in barometric pressure," which are still often observed by astronomers waiting to see what the weather may bring³:

The rise of mercury in the barometer generally heralds good weather; its fall on the other hand heralds bad weather, rain, snow, wind and storms. [etc.]

He concludes:

Predicting changes in the weather is an art which has its principles and rules, and requires a great deal of experience and the attention of a seasoned practitioner.... The data needed for this craft are: habitual and daily observation of changes of the height of mercury in the barometer, the force and direction of winds at different altitudes, and the hydrometric state of the air. With all of these data, it is almost always possible to predict one or two days in advance, with fairly high probability, the weather that will occur. One even thinks that it would not be impossible to publish every morning a journal of predictions, which would be of great use to society.

The turmoil of the Revolution, which saw intellectuals like Lavoisier swept from the scene, pushed meteorology into oblivion. The situation reverted to what it had

² *Lavoisier A. *Œuvres* t. 3 (1865), pp. 759–762.

³ *Lavoisier A. *Œuvres* t. 3 (1865), pp. 765–771; this text was published in *Literary Magazine*, October 1790, but was read on 8 May 1765 before the Academy.

been in the 1760s, and nothing more ambitious was undertaken than the collection of data. The sole immediate successor of Lavoisier in France was Jean-Baptiste Lamarck, who published between 1799 and 1820 a *Yearbook of Meteorology* in the face of general indifference. Poor Lamarck even reaped the scorn of Napoléon I, who would have liked to see the publication stopped and who said⁴: “Your meteorology is absurd.... This yearbook does no credit to you in your old age.” Even though it attributed to the Moon an influence on the weather (this was seriously questioned by Arago,⁵ though many people still believe in it today), Lamarck’s *Yearbook* contained much of genuine interest. For example, in the 1807 edition he offered a proposal for a central Bureau of meteorology, which would not come into existence until 1878. Apart from Lamarck, another successor of Lavoisier, though a more distant one, was the civil engineer Pierre Morin, who envisioned in 1829 the possibility of numerical meteorological predictions. He was, admittedly, unduly optimistic – what he foresaw would not actually be realized, and then incompletely, until the second half of the twentieth century⁶:

It should be possible to arrive at equations whose numerical coefficients will provide an approximation for predicting in detail atmospheric phenomena several days in advance, and for the seasons, perhaps several years in advance.

In order to make forecasts, an essential element was lacking: rapid transmission to one location of measurements effected throughout a larger area. The optical telegraph of Claude Chappe could have supplied this deficiency since, for example, a dispatch from Brest to Paris took only 8 min (when the weather was good). During a presentation on the telegraph in 1793 to the Constitutional Assembly by the deputy Gilbert Romme, Romme, taking up again an idea of Lavoisier, mentioned that this might open up “the possibility for predicting storms and for giving warnings to sailors and farmers.” Chappe himself proposed to Bonaparte, a little later, that the telegraph be made available to private interests and the world of business, and used for transmitting newspapers. However, the proposal was not accepted. Bonaparte insisted that only the government and the army should have the right of using it. Everything stagnated, therefore, until the middle of the nineteenth century; all the more so since Arago, who had reorganized observations at the Observatory and written many pages on meteorological phenomena in the *Annual of the Bureau of longitudes*, declared in a voice that was loud and clear⁷:

Never has a word left my mouth, neither in private conversation nor in the public discourses I have given for forty years, never has one line been published with my consent, that could authorize anyone to attribute to me the idea that it would be possible, given our current state of knowledge, to forecast the weather with any certainty as it will be in a year, a month, a week, I would say even a day in advance.

⁴From Fierro (1991), p. 93.

⁵*Arago F. *Œuvres Complètes* t. 8, pp. 25–82, 83–146 and 120–124.

⁶Cited by Fierro (1991), p. 96.

⁷*Arago F. *Œuvres Complètes* t. 8, pp. 1–24, see p. 1.

Fig. 9.1 Charles
Sainte-Claire Deville
(1814–1876)



The Meteorological Society of France

Only in 1852 did things begin to move again, with the creation of the Meteorological society of France by two mining engineers with a passion for meteorology, Émilien Renou and Charles Sainte-Claire Deville (Fig. 9.1); the latter was the brother of the celebrated chemist Henri Sainte-Claire Deville. Other founding members were Antoine d'Abbadie, a wealthy voyager who had explored Ethiopia, the physicist Auguste Bravais, and three members of the Society for natural sciences of the Département of Seine et Oise: Adolphe Bérigny, J. Haeghens and Martins. They started up an annual “intended to receive each year observations that savants full of zeal and impartiality have collected from various points in France.” Here are some extracts of the circulars they sent on 17 August 1852 announcing the foundation of the Society⁸:

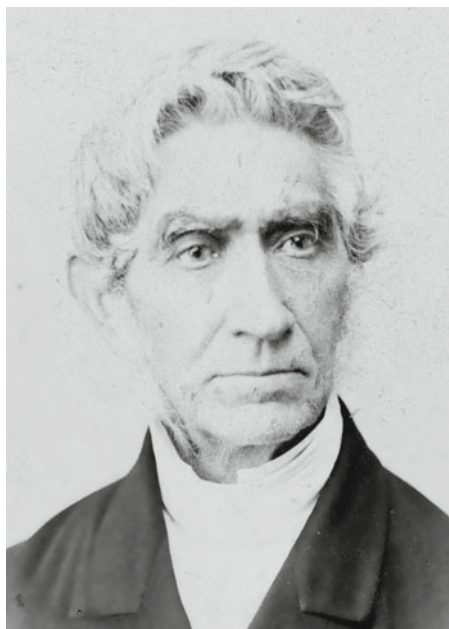
Agriculture, which is the basis of all wealth, is an essential beneficiary of Meteorology.... Attentive observation... has already, in many cases, led to the most important practical results: to cite but one example, it is known that the hydrometric commission of Lyon, after several years of study, was able not only to announce several days in advance the rise in the Saône river but to predict with remarkable accuracy the height actually attained by the waters of the river. If one reflects on the prodigious rapidity with which communications are advancing every day, it is easy to see how immensely useful such warnings will soon be to agriculture and industry.

But these benefits of science will no longer be limited to one region. Soon, all of Europe will be criss-crossed with metallic wires, which will abolish distances and permit reports of atmospheric phenomena to be sent out even as they are happening and their remote consequences to be anticipated.

... Several friends of the physical and natural sciences, to whom the editors of the Annual are keen to offer their immediate assistance, have dreamed of the creation of a Meteorological

⁸ Cited by Fierro (1991), pp. 104–105.

Fig. 9.2 Adolphe Quételet (1796–1874), director of the Brussels Observatory. He played an important role in the development of meteorology



society, a common center through which all the observations could be coordinated, and which would serve as a liaison to those devoted to patient meteorological research.

The first meeting of the new society, on 14 December 1852, was a great success: there were about 150 participants, of whom 20 were members of the Academy of sciences. Among them were many French and foreign scientists, the chief administrator of telegraphs, Alphonse Foy, and his counterparts in Austria and Italy, also the directors of the Brussels Observatory (Adolphe Quételet; Fig. 9.2), Geneva (Plantamour), Urbino (Serpieri), etc. But Le Verrier was not among them. Auguste Bravais was elected president, Sainte-Claire Deville and Haeghens secretaries. The publication of an Annual was resolved. In 1855, Renou edited and published an Instructional guide to meteorological observers in France, which would fill the role for a quarter of a century. But the Society was limited in its objectives: it contented itself with collating observations and calculating statistics, and did not attempt to make any actual predictions.

This, then, was the situation that Le Verrier faced in his meteorological undertakings. Given his personality, there was bound to be friction.

In Other Countries

Meanwhile, there was no lack of activity outside France. Meteorological institutes were created in 1847 in Prussia, at the instigation of Alexander von Humboldt; in 1850 in England, in 1851 in Austria, and in 1854 in the Netherlands under Christophorus Hendrik Buys-Ballot (Box 9.1 and Fig. 9.3). In England, the chief meteorologist was an admiral. Robert FitzRoy (commonly spelled Fitz-Roy; Box 9.2 and Fig. 9.4).

Box 9.1 Christophorus Henricus Diedericus Buys-Ballot (1817–1890)

The son of a pastor of the Dutch Reformed Church, Buys-Ballot was born in Kloetinge, the Netherlands. He received a doctorate in 1844, and taught mineralogy and geology, then theoretical chemistry, in Utrecht. In 1847 he was named professor of mathematics, and from 1867 until his retirement he was professor of physics (one of his students was the well-known astronomer Jacobus Kapteyn). His best-work, however, was in meteorology. In 1854 he founded the Royal Dutch Meteorological Institute, which by 1860 was providing a storm forecasting service based on Buys-Ballot's law (see below). He remained its director until his death, and seeing the need for international cooperation, became the first chairman of the International Meteorological Organization in 1873. He died in Utrecht.

Buys-Ballot's law (actually first discovered by the American William Ferrel) states that if an observer in the northern hemisphere stands with his back to the wind, the low pressure will be on the left. This follows from the fact that wind travels counterclockwise around low-pressure zones in the higher latitudes of the Northern Hemisphere, and is reversed in the Southern Hemisphere. (But the law does not hold at low latitudes, since there the angle between the pressure gradient force and wind is not a right angle.) His storm forecasting was based on his observation that if the pressure is at least 4 mm higher in Maastricht in the south of the Netherlands than in Groningen in the north, a storm could be expected to arrive from the west.

Despite his training in physics, Buys-Ballot concerned himself with establishing regularities in the weather, rather than in explaining them. In this respect, he contrasted with contemporaries such as Ferrel.

Fig. 9.3 Christophorus Buys-Ballot (1817–1890)



Box 9.2 Admiral Robert Fitzroy (1805–1865)

A British naval officer, Fitzroy was captain of the celebrated *HMS Beagle* at the time of its expeditions to Patagonia, the Magellan Strait, Chile, Peru, and the Galapagos Islands. Charles Darwin was the naturalist of the expedition from 1831 to 1836. FitzRoy was governor of New Zealand from 1843 to 1845, and became chief of the new department of meteorology of the Board of Trade in 1854. He was among the first to exchange weather data with foreigners, and from 1860 published these data on a daily basis. He organized a storm warning system for sailors, which was well received in France as well as in England. However, he was also opposed: many English scientists of the day thought it premature to publish weather forecasts, and as a result, relations between FitzRoy, who could be moody and difficult, and his colleagues were sometimes strained, and deteriorated even more toward the end of his life when he began to investigate rather whimsical theories concerning the effect of the Moon on the atmosphere. Depressed, he committed suicide on 30 April 1865, just after a visit from Matthew F. Maury, the celebrated American meteorologist and oceanographer.

Fig. 9.4 Admiral Robert Fitzroy (1805–1865)



Buys-Ballot and FitzRoy both played important roles in global meteorology. In contrast to Le Verrier, who was above all an organizer, they attempted like their American counterparts to understand the physics of atmospheric movements. Figure 9.5 shows a drawing by FitzRoy; it already begins to resemble what satellite images show us nowadays.

The United States also showed an active interest in meteorology. Already, at the time of its foundation, George Washington, the first president, kept a meteorological journal until the very day of his death. So did Thomas Jefferson, the third president. The government appointed an official meteorologist, James Pollard Espy, who explained in 1830 the formation of cumulus clouds by convection of humid air and established the first synoptic maps of the weather. Similar work was being done at the time by Elias Loomis. In 1831, William Charles Redfield discovered that tropical

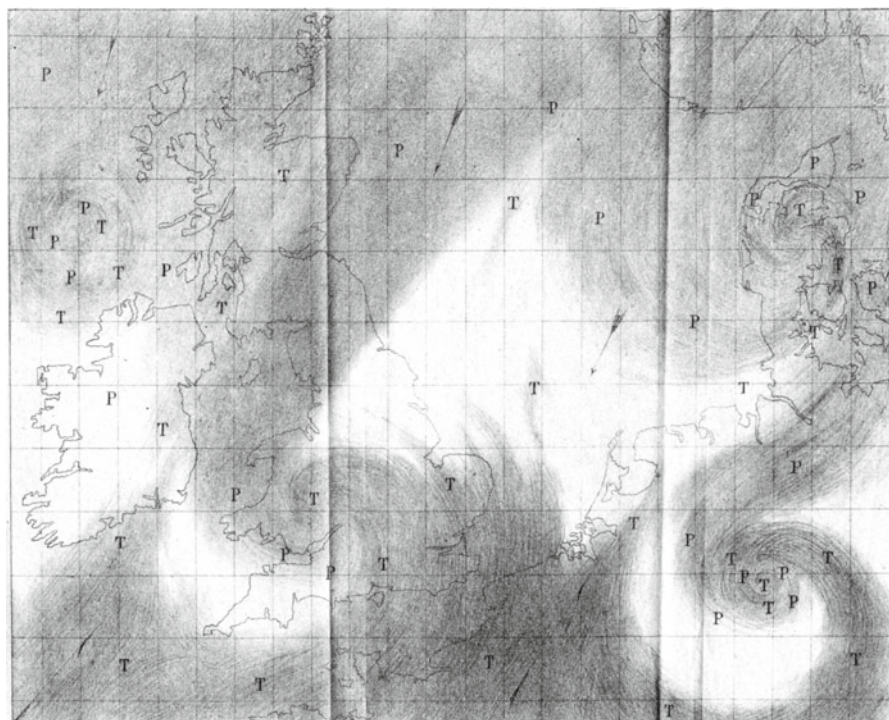


Fig. 9.5 Model for the interaction of air masses, by FitzRoy (1863)

cyclones were vortices circulating in a counterclockwise sense (in the Northern Hemisphere). Espy returned to the question in 1841. Then, in 1860, Buys-Ballot explained the relation in terms of the famous theorem formulated by Coriolis in 1835, finding that the acceleration named after Coriolis, due to the rotation of the Earth, effectively governs the circulation of the winds. It was also in the United States that the electric telegraph of Morse, put into service in 1845, was used for the first time to transmit meteorological information. The telegraphic operators soon noticed that they could predict changes in the weather by observing meteorological conditions to the west. The physicist Joseph Henry, author of the celebrated work on electromagnetic induction, proposed in 1847 the systematic use of the telegraph to “alert observers to the North and to the East of the approach of storms.” His wish was realized in 1849. By the end of that year, 150 posts were in service. When the Morse telegraph was inaugurated in the United States, England already had several hundred kilometers of telegraphic lines, which would soon be used to transmit information, and in 1849 the *Daily News* issued each day a weather bulletin from 30 stations. France, however, was very far behind; as yet, nothing of the sort existed there, even though the first telegraphic line had opened in 1845 between Paris and Rouen.

It was an American, too, Matthew Fontaine Maury (Fig. 9.6), director of the U.S. Naval Observatory in Washington, who became the true founder of maritime

Fig. 9.6 Matthew
Fontaine Maury
(1806–1873)



meteorology. It was he who, after studying the winds in the Atlantic Ocean and the Pacific Ocean, recommended to navigators routes which enabled them, in both cases, to gain some 20 days in the duration of their journeys. Maury also was the initiator of the first international meteorological conference, held in Brussels in 1853. Presided over by Quételet, it was a modest meeting, with 12 participants representing 10 countries. Most were naval officers. The one from France was a hydrological engineer, A. Delamarche. The conference asked sailors of all countries to make certain specified observations at sea, along the lines of those that Arago had recommended for observations by French vessels.⁹ Subsequently, the minister of the imperial Navy decided that all French warships would carry out meteorological observations, and he also encouraged captains of merchant ships to do so.

Le Verrier's Proposal

In his grand proposal of December 1854 to reorganize the Observatory, Le Verrier pointed out the inadequacy of meteorology in France (he failed to mention the Meteorological Society, even though the latter had been founded 2 years before)¹⁰:

Of all the scientific institutions that the state possesses, the observatories of Paris and Marseille are the only ones where meteorological observations have been made in a regular manner. In Marseille, six observations are recorded every day; in Paris, formerly, only four, at 9 a.m. at noon, at 3 p.m. and at 9 p.m. However, since last November, additional observations have been added at 6 p.m. and at midnight....

⁹*CRAS 1 (1835), pp. 380–410, or *Arago F. *Œuvres Complètes* t. 9, pp. 4–133, with an introduction.

¹⁰°*Annales de l'Observatoire de Paris* 1 (1855), p. 54 and following.

It should be emphasized that the French nation has an eminently scientific spirit [!], and praise-worthy efforts have been made in some of our *départements* by certain individuals, who have tried to supply with their zeal the deficiencies of the state institutions.... But most of the instruments used for their observations are unknown; while many observers were unable to procure suitable stations for their instruments, a circumstance which, unfortunately, renders some series of observations uncertain, even though they were made with such great devotion to science.

Le Verrier then turns to the question of forecasting the weather:

The barometer, with its variations, informs the navigator of approaching storms. Being thus forewarned, if he is near a dangerous coast, he can move away, or seek refuge in one of the ports available to him.... However, the indications deduced from isolated observations with the barometer have little value compared with those that can be obtained by examining simultaneous observations using all the meteorological instruments, some of which have been but little employed. Careful series of measurements will allow us to better know the prognostics that can be had by combining these observations with the appearance of the sky.

By joining, with telegraphic lines, various stations at which meteorological observations are made, it should be possible to know from moment to moment the direction and velocity of propagating storms, and to announce several hours in advance on the coast high winds, especially the most dangerous ones.... Already in the United States, high winds have been forecast on several occasions.

Next he presented his proposal:

1. Equip the Paris Observatory with standard instruments;
2. Organize, at this establishment, a complete and regular system of meteorological and magnetic observations;
3. Undertake a meteorological journal, as complete as possible, for accidental phenomena;
4. Establish relations with the various individual observatories in France and with some of the principal foreign institutions; promote the establishment of new meteorological stations; verify their instruments; publish instructions on the way to set up thermometers, hygrometers, etc.
5. Publish each day in the journal observations for Paris and the principal stations in France, observations that would be transmitted to the Observatory by telegraph;
6. Present at the end of the year, in a general publication, a summary of all the observations, and a discussion of results to which they give rise;
7. Discuss the observations previously made in Paris, and attempt to assemble documents on all that has been done in France.

This proposal, which evidently was the result of mature reflection, but still modest, would be fully realized. Le Verrier would go well beyond it, for the Observatory would become a European center for meteorological forecasting. This is how it all developed.

The Storm of November 1854 in the Black Sea

During the Crimean War, while the port of Sebastopol was under blockade by the allied fleets of France, England, Piedmont and Turkey, a powerful storm arose on 14 November 1854 and sank 38 coalition ships, including the French vessel *le Henri*

IV. Needless to say, this disaster made a great impression, and provided the opportunity Le Verrier was looking for to launch his telegraphic meteorological project. He presented a study by Liais of meteorological phenomena on 31 December 1855 to the Academy of sciences, in which he wrote¹¹:

We have not forgotten the hurricane which, on 14 November 1854, caused many victims in the Black Sea, and led to the loss of the *Henri IV*. On that same day, or within a day depending on the locality, strong winds tore across western Europe, and over Algeria and Austria. The phenomenon seemed, therefore, to extend over an immense surface area. This remarkable circumstance attracted the attention of our illustrious associate, the marshal Vaillant, minister of War, who wrote to me and invited me to undertake a study of the conditions that produced this phenomenon, while giving me assurances of his assistance.

In order to put ourselves into a position to respond to the marshal's wishes, I sent a circular to astronomers and meteorologists in many countries asking them to send me the information they had collected on the state of the atmosphere on the dates 12, 13, 14, 15 and 16 November 1854....

In response to this circular, the Observatory has received more than 250 documents coming from England, Belgium, Holland, Prussia, Austria, Sweden, etc., and even India and the French colonies. We have entrusted the analysis of all these documents to the head of the meteorological section at the Observatory, M. Liais, and it is his outstanding work which we are now going to discuss.

From this information, Liais concluded there had been two successive storms that moved from the West to the East. He noted that the direction of the wind had no actual relation to the movement of these perturbations, and that barometric variations followed waves, or atmospheric oscillations, which traversed Europe; the passage of a depression was followed by strong winds and precipitation. Commenting on this study before the Academy of sciences, Le Verrier concluded:

The presence of a phenomenon on such a large scale, in which the general conditions overwhelmed the minor local ones, leads to the hope that it will become possible to subject to analysis the primary circumstances giving rise to them, and I see with pleasure M. Cauchy¹² is motioning his agreement. But before mathematically attacking this problem, it is necessary first to understand well the conditions of the phenomenon, and for this the already numerous observations must be multiplied still more.

One wonders, finally, in seeing this regular movement of the storm of November, whether the presence of an electric telegraph between Vienna and the Crimea would not have been able to forewarn our army and the fleet. In learning at Vienna that the storm had struck at such and such an hour on the coast of France, at such an hour in Paris, at such an hour in Munich, and all the while was increasing in intensity, couldn't it have been foreseen that the Black Sea, too, should expect it?¹³ We are under no illusions; we know there will be great difficulties in arriving at such practical results, but believe that we will eventually be able to surmount them. The Observatory is working on it, and soon I will be able to submit to the Academy the first measures that will be taken to advance this question.

¹¹ *CRAS (1855) 41, pp. 1197–1204.

¹² Then aged 65, Augustin Cauchy, who filled with calculations many pages of the *Comptes rendus* of the Academy, was a very respected mathematician. One sees that Le Verrier, who probably did not know the visionary ideas of Pierre Morin, was already contemplating the possibility of numerical prediction of the weather.

¹³ In his *Historique des entreprises météorologiques* of 1868.

Indeed, Le Verrier had submitted to the Emperor on 16 February 1855 a proposal for a vast meteorological network “designed to warn sailors of the approach of storms”¹⁴:

This project, very complete, received the approbation of His Majesty, and starting tomorrow, 17 February, we, M. de Vougy, Director general of the telegraphic lines, and myself, are authorized to undertake and pursue the organization proposed.

It happened that just the evening before, *le Semillante*, a warship heading for the Crimea with 700 soldiers on board, had been wrecked by a storm on the isles of Lavezzi, near the strait of Bonifacio between Corsica and Sardinia: there were no survivors. Was this incident known to Napoléon III the next day? If so, it could hardly fail to hasten the creation of the French meteorological network.

The Formation of the Meteorological Network

The Beginnings

Le Verrier’s proposal was made possible through the opening of the telegraphic lines to the public. Indeed, Napoléon III, while he was still president of the Republic, had shown himself to be anxious to promote commerce and industry by making available to them this new tool of communication. He decided in November 1850 on the opening of the telegraph lines, effective on 1 April 1851. The dramatic events which we have mentioned, together with the good relations that Le Verrier had formed with the telegraphic administration, were such that the project got underway very quickly. On 19 February 1855, only 3 days after approval of his project by the Emperor, Le Verrier could show the Academy in the afternoon a map of the “state of the atmosphere” of France obtained that same day at 10 o’clock in the morning. The next day 20 February 1855, the Minister of Public Instruction, Hippolyte Fortoul, presented to the Emperor a plan for improving the resources of the Observatory, so that the project could be administered from there. Le Verrier designated Foucault, who had just been named physicist of the Observatory – the coincidence of the dates can hardly have been owing to chance – to direct the meteorological work. But Foucault had little appetite for this kind of work. Therefore it remained in the hands of Liais.

The following month, Le Verrier wrote to Alexander von Humboldt, who also thought that “a prompt knowledge of the simultaneous meteorological variations,

¹⁴ Le Verrier (1868) *Historique des entreprises météorologiques*, 1854–1867, Paris, Gauthier-Villars. This paper collects and organizes the content of notes published in *CRAS 40 (1855), pp. 620–626; 42 (1856), pp. 1939–1942; 60 (1865), pp. 1317–1327; 61 (1865), pp. 136–144; 62 (1866), pp. 1045–1052 and 1107–1108; 66 (1868), pp. 227–230. All citations in this section come from this document, with some exceptions indicated explicitly.

favorable by the rapidity of the telegraph, could in certain cases be very useful,” that his wish had been granted¹⁵:

The drawing up of atmospheric maps by means of information collected by the administration of telegraphic lines ... was only a first attempt. It has been decided that a large number of meteorological stations, equipped with precise instruments, the sites of which were already chosen, would be set up through the care of the Administration, and charged with transmitting each day their observations.

These stations, added to those already set up by the honorable scientists to which we render homage, will complete a meteorological network that is respectable and capable of rendering important service.

The organization of the colonies and of studies at sea will come soon thereafter.

The development of the meteorological network followed closely on the heels of the telegraph, which at the end of 1855 connected Paris to almost every prefecture of the *départements* (Fig. 9.7). The division of responsibilities was defined in June 1856¹⁶:

... it was agreed with the director general De Vougy that those responsible for administering the telegraphic lines would see to it that observations would be collected by its agents and transmitted to the imperial Observatory of Paris, some by telegraph, some by post; whereas for its part, the Observatory would furnish instruments and instructions, process all the observations and see to their publication.

In 1856, there were 24 meteorological stations, “distributed over the various basins of the Rhine, the Seine, the Loire, the Gironde and the Rhône, so as to make known as well as possible the overall atmospheric conditions in each of these great basins.” Thirteen of these stations transmitted by telegraph an observation on the opening of the office at the Observatory, the 11 others sent theirs by post in order to avoid overwhelming the lines with too much traffic. There were telegraphers therefore who three times a day, at the time the office in Paris opened, at 3 in the afternoon, and at 9 in the evening, read the instruments, calibrated at the Observatory by comparison with the standard instruments, and sent the results to the Observatory. These results were published daily by several newspapers. This didn’t involve as yet predictions, but only information about the meteorological conditions at the various stations. Le Verrier added: “It would be of great interest to extend the organization to other countries. The inquiries made in this sense have everywhere been greeted with great enthusiasm.”

The month of April 1857 saw the unveiling of the first weather prediction:

At the end of April of this year, the temperature was for several days very low, and only got up around noontime to 7 or 8 degrees. One became very uneasy, and on the 29 of the month the Emperor wanted to know if there were prognostications of a coming change in weather.

We installed ourselves, M. de Vougy and myself, in an office of his administration, and gathered together telegraph data on temperature and wind from various regions, letting ourselves be guided by the first documents obtained. We could conclude in the course of the

¹⁵ *CRAS 40 (1855), pp. 620–626.

¹⁶ *CRAS 42 (1856), pp. 1939–1942.

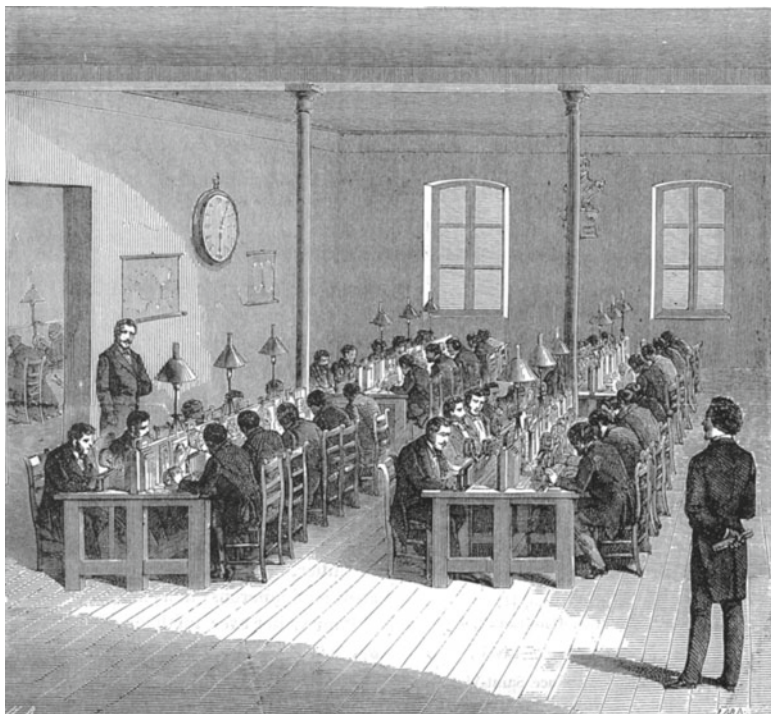


Fig. 9.7 A telegraphic operator room in Paris in the 1860–1870

day and make it known that in two or three days the situation would improve. Indeed, the temperature at noon had risen, on 2 May, to 11 degrees, and on 3 May to 15 degrees.

This same year saw the birth of a European collaboration on meteorology: “Towards the first months of 1857 all was in readiness, and the delays encountered had to do only with the difficulty one always has in gaining acceptance for new ideas,” wrote Le Verrier.¹⁷ Despite the difficulties, the international meteorological service came into being, in effect, on 2 November 1857, at which time the *Bulletin météorologique* (it would later be renamed the *Bulletin météorologique international*), contained observations of 14 sites in France and five sites in other countries: Brussels, Geneva, Madrid, Rome, and Turin. Before the year was out, Vienna, Lisbon and St. Petersburg were added, but not England, where all the telegraphic lines were in the hands of private companies. (The main difficulty Le Verrier encountered, indeed, was to get free transmissions, which was impossible in England, at least for now.) Of course, the *Bulletin* was sent by telegraph to each foreign correspondent. One can imagine that this work was quite considerable for the telegraphers, who could only send some hundred letters or characters per minute.

¹⁷ °Ann. *OP, Observations* 19 (1863), p. 84.

Warning the Ports

“The Observatory, since [the end of 1857], has been in the process of realizing its goal of warning the ports, which goes beyond anything that has been done elsewhere for several years,” wrote Le Verrier, with satisfaction. But he warned: “If it now finds itself falling behind, and temporarily surpassed in execution, this will certainly not be its fault.” What did he mean by this last remark? Quite simply, the Navy was dragging its feet. It would not approve anything until 1859 despite the good intentions, or at least apparent good intentions, of the minister, the admiral Ferdinand Alphonse Hamelin. It was on this occasion that Le Verrier wrote a statement that has become famous:

There are those who do and have done; there are others who do nothing but allow others to do something; the worst type, but unfortunately the most common, are those who do nothing and do not want others to do anything.

The operation of the service was described in a long letter Le Verrier wrote to his colleague Airy, director of the Greenwich Observatory, on 4 April 1860¹⁸:

Each day [at 7 in the morning] our ports take note of the state of the atmosphere and the sea, and the readings of the barometer and the thermometer, and send this information by telegrams furnished by the Navy to Paris. Immediately, the various ports then receive communications about the state of the atmosphere and the sea in the regions which concern them. Accordingly, Cherbourg receives from Dunkirk, Le Havre and Brest. Brest in turn receives from Dunkirk, Cherbourg, Rochefort, and Bayonne. The port of Toulon is informed by Cette [nowadays Sète], Marseille, and Antibes....

In the afternoon at 3 o'clock, the ports again inform Paris of the state of the atmosphere and of the sea, but omit readings of the barometer and thermometer which are included in the morning transmission. Immediately these transmissions of 3 o'clock are forwarded to the relevant ports.

Later in the letter Le Verrier proposes an exchange of data with England, which would take effect in June of 1860. He began also a copious correspondence with FitzRoy in an effort to spell out this exchange, a correspondence that would only end with the suicide of FitzRoy on 30 April 1865. He also exchanged information with the ports of Spain, Italy, and Austria (which at the time possessed Venice and Trieste), and Belgium and the Netherlands, but always without forecasts: it was left to each of them to assess the situation themselves from the information they received.

In 1861, the international meteorological network was complete: it extended from St. Petersburg to Haparanda in Norway, Valentia in Ireland, Cadiz, Naples and Constantinople. But the system of alerts for ports, “the ultimate goal for which [the international service] was formed,” remained embryonic. Le Verrier wanted to do more. He didn’t want to limit his service to an exchange of data with and between ports but to create within the Observatory a bureau of forecasting storms and creating warnings for the ports. A similar bureau had already been organized in England

¹⁸ Reproduced in *Historique des entreprises météorologiques*, pp. 15–18.

by FitzRoy, who wore himself out making weather predictions which were often wrong, for the data was insufficient for the purpose, but which were nevertheless appreciated when they were correct.¹⁹ On 30 October 1862, Le Verrier addressed another letter to the minister overseeing the project, requesting him to finally authorize the creation of such an office: “Since 1860, there has been no follow through on our demands, and according the inevitable law of all scientific affairs, what doesn’t progress falls backward.” On 31 January 1863, he received finally what appeared to be an encouraging letter from the minister of the Navy, telling him that he would put at the disposal of the Observatory two navy officers who had been requested, “after he had verified their goodwill and their capacity,” in order to set up a true department for forecasting and announcing oncoming storms. This was actually a rather hypocritical response, since the minister of the Navy had already created on 18 February 1859 a meteorological service that duplicated Le Verrier’s, probably owing to distrust of him, followed in 1861 by a service for forecasting storms that would use the methods practiced by FitzRoy right up until his death.²⁰

Le Verrier, who knew full well what the Navy was doing, was no fool, and realized that he could not do everything he would have liked. He persisted, however, in 1863 in operating and making functional a partial service forecasting winds and tides. This service seemed to be well received by the chambers of commerce of the ports in which it operated, and by the maritime and civil authorities of Cherbourg and of the *département* of la Manche; the latter, one imagines, would not think of going against the wishes of the president of the general Council of their *département*. Finally, at the end of 1863, there were no less than 74 ports involved in this system.

In 1864, there was a relaxing of the stance of the Navy, which now invited Le Verrier to submit directly his telegrams to military ports. But this is as far as things went.

One finds in a document on the history of meteorological enterprises, written by Le Verrier, many pages on the problem of furnishing warnings to ports, in which he cast himself in the leading role. The reasons for his misunderstandings with the Navy were several: his intransigence and obstinacy, the importance which he gave to terrestrial phenomena, which did not interest sailors, and especially the fact that the naval officers were reluctant to work with or depend on civilians. This, however, was not a problem in England and the United States, where the naval meteorological services were directed by sailors.²¹

¹⁹ See Javelle et al. (2000), pp. 101–103; it is following a wreck in 1859 that, like in France, this bureau was created.

²⁰ This service would work until 1875.

²¹ Le Verrier writes in his *Historique des entreprises météorologiques* for 8 January 1862, p. 25: “We hope that one could obtain the concurrence of the Navy for the establishment of a special bureau. [...] The Navy would hopefully not consider us as strangers. The closest ties always existed between Navy and Astronomy, and the Imperial Observatory works at making them even closer. This call was not better received than the preceding ones.”

An International Service in Full Swing

Despite its limitations, the international service of Le Verrier functioned well. At the end of 1863, it comprised 65 stations with which the Observatory was in daily telegraphic correspondence, 21 in France and 46 elsewhere in Europe.²² Two years later, there were only 59 stations.²³ It would be tedious to consider in detail the development and functioning of this service. We content ourselves with recounting several highlights.

Between 25 July and 26 August 1863, a daily telegraphic dispatch of forecasts for agriculture was transmitted through diplomatic channels from Paris to an association of owners of large lands in Mecklembourg in northern Germany: it was the beginning of a system of warnings for agriculture, which would, however, soon collapse, as the telegraphic office at Rostock began demanding payment for the service. Such a service would not be realized by the Observatory until 1876, and then only for France. Meanwhile, several individuals such as Nicolas Antonin-Hélène Poincaré (uncle of the mathematician), civil engineer for the *département* of the Meuse, edited forecasts for the farmers of the region based on information provided by the Observatory, and even a system of signals from its branch in the Vosges to warn of possible flooding; the necessary information would be sent to it from the Observatory, with particular attention given to the progress of storms.

The first synoptic maps related to the meteorological situation in Europe were disseminated regularly in the daily bulletin starting 16 September 1863; the bulletin was four pages, the annual subscription was 36 francs (Figs. 9.8 and 9.9). From 1866, isobaric maps were beginning to be sent by telegraph even to foreign sites, no doubt in the form of a sequence of numbers. Each bulletin summarized the general meteorological conditions, with indications on their possible progression. The forecasts gradually became better over the years, and in 1870 the term “probable weather” first appeared. Le Verrier even made some private forecasts on request, such as for the celebrated photographer Nadar who was preparing perhaps for a balloon ascension²⁴ (Fig. 9.10). It is evident that these forecasts were made based on analogy to past meteorological situations; physical considerations did not play any role. Moreover, they had limited value, for the bulletin could hardly reach users more than a day’s distance from the Observatory.

In 1864, Le Verrier decided to enrich the network of meteorological stations in France, and appealed to the *Écoles normales d’instituteurs* (training schools for teachers) which were found in the major city of each *département*. The idea wasn’t entirely new, for several schools already made meteorological observations on a regular basis. This was the case, most notably, in the Academy of Metz. On 13 August, the Minister of Public Instruction sent out, at Le Verrier’s request, a circular to the

²² For a list of these stations and details of how the international meteorological service was organized, see °*Ann. OP, Observations* 19 (1864), pp. 79–87.

²³ °*Ann. OP, Observations* 21 (1865), pp. 62–74.

²⁴ We could not find any mention of a balloon flight of Nadar in South-East France.

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France
5 francs par an.
Etranger
6 francs de poste en plus. 310 Numéros - 2 forts volumes in-folio par an.
Abonnements chez M. Chauvin, lithographe, rue d'Ulm, 8. Page 1.

Bulletin international
de l'Observatoire Impérial de Paris.
Janvier 1864. Vendredi 4.

Statatmosphérique de l'Europe à 8 h. du matin.

Stations	Pression	Temp.	Vents inférieurs	Etat du ciel	Etat de la nuit	Vents, force à 5 h. a.	Corr.	Hauteur
Paris	754.2	2.3	E faible	Couv. pluv.	"	SE modéré	"	2.00
Dunkerque	759.3	-0.3	SE très-faible	Nonageux	Belle	SE modéré	"	10.0
St. Pierre	748.0	3.0	SE fort	Pluie	Agitée	ESE très-fort	"	10.0
Charbourg	754.5	6.0	E très-fort	Couvert	Très-agité	SE fort	"	10.0
Caen	757.3	6.0	ONO faible	Nonageux	Houleuse	OSO ass. fort	"	10.0
Reims	757.0	7.0	NO très-fort	Couvert	Grosse	SO fort	"	10.0
Metz	"	"	O ass. fort	Couvert	Un peu agité	N presq. nul	"	10.0
Strasbourg	757.6	5.9	N très-faible	Couvert	Calme	ONO presq. nul	"	10.0
Amsterdam	756.0	6.0	NO modéré	Couvert	Un peu agité	SE tr. faible	"	10.0
Bruxelles	755.0	6.3	O presq. nul	Pluie	"	"	"	10.0
Antwerp	756.9	6.3	O presq. nul	Pluie	"	"	"	10.0
Lyon	756.4	7.0	S presq. nul	Pluie	"	"	"	10.0
Marseille	757.7	"	E presq. nul	Pluie	"	"	"	10.0
Nice	752.5	6.3	SO presq. nul	Couvert	"	"	"	10.0
Genoa	757.7	5.1	O presq. nul	Nonageux	"	"	"	10.0
Barcelona	758.8	7.0	ONO presq. nul	Pluie	"	"	"	10.0
Valencia	757.1	8.6	ONO presq. nul	Pluie	Houleuse	O fort	"	10.0
Bayonne	761.0	10.0	NO modéré	Pluie	Grosse	O très-fort	"	10.0
Bordeaux	758.4	6.8	O presq. nul	Beau, pas nuage	"	"	"	10.0
Antibes	"	"	SO très-faible	Pluie	Grande d'E	O presq. nul	"	10.0
Boulogne	"	4.0	SE ass. fort	Pluie	Belle	"	"	10.0
Genève	757.7	2.3	NO faible	Couv. pluv.	Grande boule	NO très-fai.	+0.62	"
Lausanne	755.5	-1.1	ENE faible	Tr. nuages	Calme	ENE faible	+1.43	"
Stockholm	766.1	-9.6	ONO presq. nul	Nonageux	"	"	"	"
Copenhagen	764.6	-1.8	N presq. nul	Serein	"	"	"	"
Copenhague	765.9	-5.2	ENE très-faible	Pur	Nonageux	"	"	"
Helsinki	"	-3.0	E faible	Serein	Brumelle	SE faible	"	"
Tienne	761.9	-8.7	ESE faible	Nonageux	"	"	"	"
Vienna	756.4	-12.3	NO très-faible	Couvert	"	"	"	"
Prague	762.2	-9.1	NE presq. nul	Nonageux	"	"	"	"
Helsingfors	761.2	-16.0	NNO presq. nul	Serein	"	"	"	"
Helsinki	759.9	-8.3	SO presq. nul	Couvert	"	"	"	"
Oslo	768.7	3.2	SSE presq. nul	Très-nuage	Agitée	"	"	"
Salerno	761.3	12.8	NO très-faible	Nonageux	Houleuse	"	"	"
Tomar	763.5	7.6	ESE presq. nul	Nonageux	Houleuse	"	"	"
Lisbonne	768.5	10.1	NO presq. nul	Un peu nuage	"	"	"	"
Alicante	764.9	10.4	O faible	Tr. nuages	Calme	"	"	"
Groningue	765.6	-4.0	NE nul	Beau	Calme	"	"	"
Leiden	763.8	-2.0	E	Serein, gelée	Orageuse	"	"	"
Bilbao	762.6	9.8	NO très-faible	Couvert, pluie	Grande boule	"	"	"
Greenwich	759.2	0.8	ENE ass. fort	Nonageux	"	"	"	"
Lisbon	765.6	-8.9	E presq. nul	Brumelle	"	"	"	"
Madrid	766.6	3.9	O presq. nul	Nonageux	"	"	"	"
Barcelone	758.7	9.0	NO modéré	Beau	Brumelle	"	"	"
Barcelon	758.0	1.0	NO	Nonageux	"	"	"	"
Palermo	759.9	12.9	SO tr. faible	Pluie	Un agité	SE très-fai.	"	"
Ancone	759.8	7.0	SE tr. faible	Couvert	Calme	ONO très-fai.	+5.60	"
Ancone	754.3	-5.6	NE modéré	Pluie	Grande boule	"	"	"
Rome	754.7	4.5	E presq. nul	Pluie	"	"	"	"
Rome	757.8	2.2	S presq. nul	Couvert	Agitée d'N.	N	"	"
Rome	753.0	-9.1	NNE tr. faible	Nonageux	Grande nuage	"	"	"
Naples	754.9	2.0	"	Pluie	"	"	"	"
Valencia	757.8	6.7	NO faible	Ass. nuage	Calme	NO peu fort	+1.52	"
Bruxelles	759.6	-1.6	NE faible	Nonageux	"	"	"	"
Schlesbourg	758.8	-10.9	N presq. nul	Nonageux	"	"	"	"

Fig. 9.8 A page of the *Bulletin météorologique* for 1st January 1864, with data transmitted by telegraph by the stations of the international network

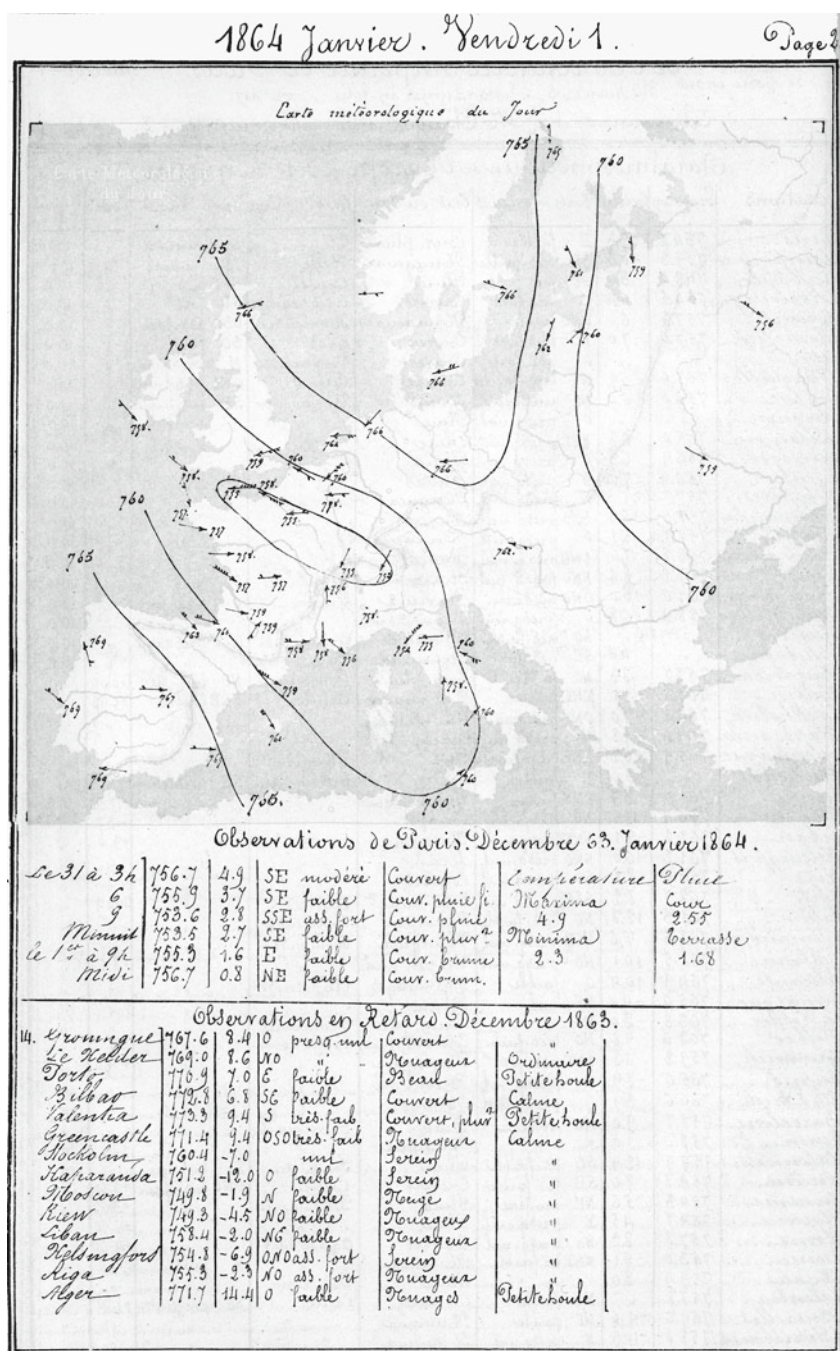


Fig. 9.9 Map of the meteorological situation for 1st January 1864 at 8 a.m., with meteorological data for Paris and late meteorological data from the preceding day. Lines of equal pressure (isobars, figures in millimeter of mercury) are drawn on the map, and the strength and direction of the wind are indicated by symbols similar to those in use today

Fig. 9.10 Autograph manuscript of Le Verrier for a telegram containing a meteorological forecast for Nadar, who was perhaps preparing a manned balloon flight

L'air ambiant } 10^h 10^m Nadar
 2 jours
 L'air
 L'air est Offert. à Nadar Lyon.
 urgente
 Embellie soutenue.
 Vents ~~Musclé~~ de Nord Ouest
 Manœuvrez vers l'Italie, ou
 à cause des Alpes descendez
 Vallée du Rhône.
 Le Verrier
 Expédié à 10^h 11.
 Dr Henry

prefects of the *départements*, which forwarded it to the various schools; 60 schools out of 78 responded affirmatively right away, the rest would follow in due course. They were provisioned with standard instrumentation, at a cost of 250 francs, which included a barometer of Fortin, a thermometer for lows, a thermometer for highs, a psychrometer [for measuring humidity], a rain gauge and a weather vane (notable is the absence of an anemometer, the speed of winds being only estimated at this time). Regular coordinated observations began on 1 June 1865. But even this was not good enough for Le Verrier, who wanted a station in every *canton*!²⁵

In 1864, Le Verrier also decided “to extend [meteorological] observations to the surfaces of the oceans and inland seas.” Indeed, “our maps only include Europe, which is inadequate; they contain nothing concerning what happens on the surface of the Atlantic Ocean, which is so much the more to be regretted given that most of the storms that assail us seem to take form in these regions.” To do this he asked ship captains to organize observations and communicate them to the Observatory which

²⁵ Each French *département* is divided in several tens of *cantons*.

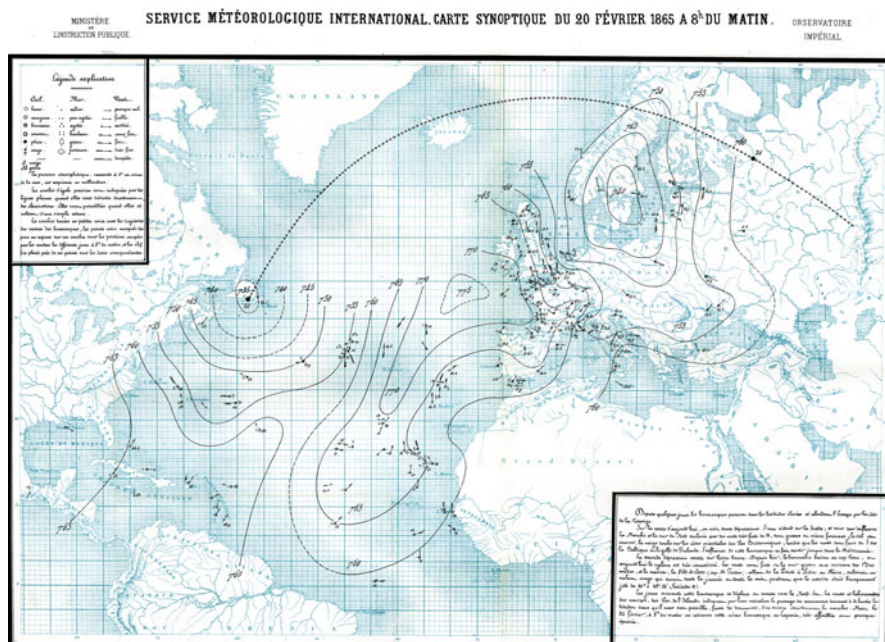


Fig. 9.11 Synoptic map of the meteorological situation of Europe and North Atlantic for 20 February 1865 at 8 a.m. This map was drawn *a posteriori* from observations by sailors and by ground stations. The curve with small crosses indicate the movement of a pressure low from 20 to 26 February

would analyze them. It is to be understood, of course, that the means available at the time did not permit the transmission of the measurements while navigating at sea, but it was hoped that by constructing maps showing the conditions in former situations, useful information could be gleaned allowing forecasts by analogy. Le Verrier established with Mouchez, who would be his successor as director of the Observatory, model forms that were to be filled out by captains of vessels. In order to motivate them to respond to his wishes, Le Verrier from 1865 on awarded, through his Scientific Association, prizes of 300 francs for meteorological observations made at sea. This initiative was successful. He received numerous observations and even, thanks to Buys-Ballot, copies of many journals of Dutch ships, and published several maps of the meteorological situation of Europe and the Atlantic on particular days in the past (Fig. 9.11).

The Scientific Association relieved, moreover, the economic straits of the Observatory by financing the publication of the *Atlas des orages* (Atlas of Storms) of 1865 (Fig. 9.12), the *Atlas météorologique de l'Observatoire impérial* in 1866, and, in 1867, the first *Atlas des mouvements généraux de l'atmosphère* for 1864. The latter, which went through subsequent editions, summarized the origin and laws of disturbances of the North Atlantic based on the observations collected from ships, as well as of the adjoining continents.

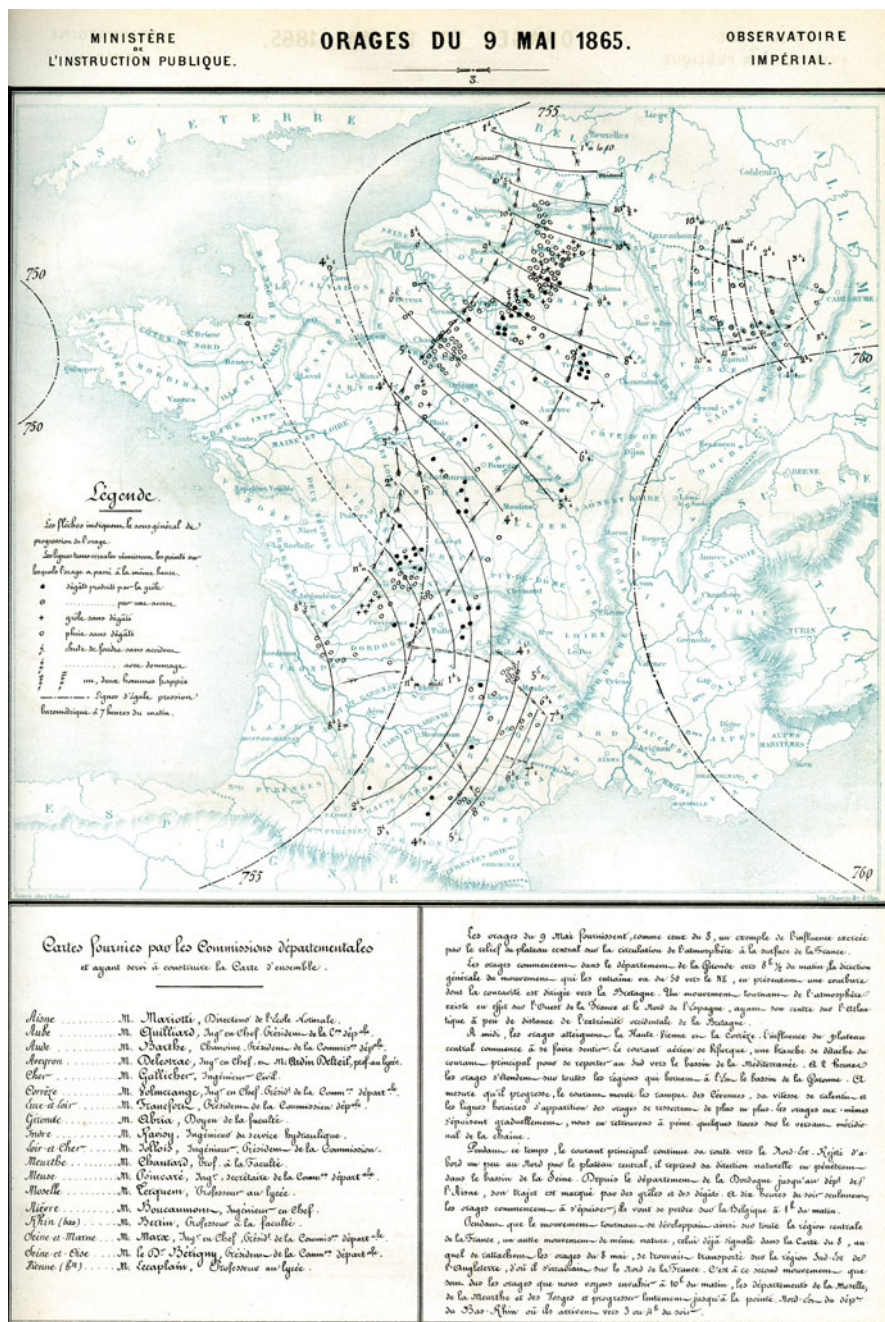
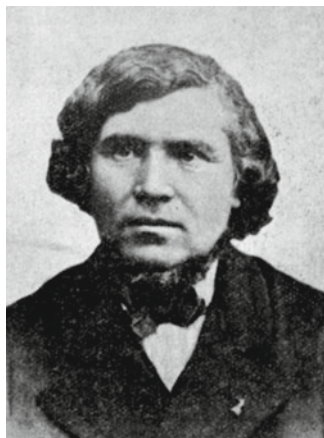


Fig. 9.12 A plate from *Atlas des orages* (Paris, 1865) showing a map of the storms of 9 May 1865. The small symbols indicate the location of lightning falls and of hail or rainfalls, the full lines the displacement of the storms hour-to-hour and the dot-line curves the isobars at 7 a.m. (pressure in millimeters of mercury)

Fig. 9.13 Hippolyte Marié-Davy (1820–1893)



The growing abundance of data made the work of synthesis which had to be carried out at the Observatory increasingly difficult, and exhausted in turn each of the men responsible for it: Liais until 1857, then Desains until his expulsion in 1861, then Hippolyte Marié-Davy (Fig. 9.13). As the latter refused to be 24 h out of every 24 at Le Verrier's beck and call, and didn't want to live at the Observatory, Marié-Davy was probably subject to the worst bullying by the Director. It is remarkable that in spite of all he managed to discharge his duties in a regular manner. The other astronomers complained, not without reason, of the excessive emphasis, in their view, given to meteorology in the activities of the Observatory.

Le Verrier realized, however, that the Observatory couldn't do everything, and so proposed in 1865 to create meteorological commissions in the *départements* to take charge of studying storms. The prefects generally were favorably disposed toward this request, and it is from the work of these Commissions that the *Atlas des orages* came about. In 1866, the study of storms and hail and the observations of the teaching schools were combined to make up the *Atlas météorologique*, which led in 1872 to the *Atlas physique et statistique de la France*; there would be seven editions of this publication during the period from 1865 to 1876. There would also be an *Annuaire* (annual) for meteorology, summarizing in a compact and popular format the observations of the year, in the same spirit as the one published by the Bureau of longitudes. All of this merely replicated, in improved form, what had been done previously. None of it was of much use in forecasting.

Difficulties and Opposition

We have already described in great detail the internal quarrels at the Observatory, which made life difficult for everyone, and culminated in Le Verrier's revocation in 1870. These quarrels, needless to say, affected his meteorological projects as they



Fig. 9.14 The meteorological observatory of Montsouris around 1906

did everything else. But there were also external problems arising from his desire to control all of French meteorology. The Meteorological Society in France, which he refused to participate in during its organization, was not willing to be relegated to the background. In 1855, Renou and another member of the Society, Léonce Elie de Beaumont, publicly declaimed against Le Verrier. In 1868, tired or irritated by the pretensions of the Observatory director, the Minister of Public Instruction, Victor Duruy, asked Renou to draw up a study of the meteorological institutions in France and overseas; Renou concluded that the Paris Observatory, now situated in the middle of a vast city, was not an ideal place from which to make meteorological observations. Instead he proposed the creation of a special observatory, outside the conurbations of the metropolis (this was exactly the same period in which the transfer of certain astronomical activities outside the metropolis was being considered). Based on this study, a commission, presided over by Charles Sainte-Claire Deville, in which Marié-Davy participated, decided the following year to create a meteorological observatory in the park of Montsouris, which at the time was relatively isolated. The city of Paris, which since 1864 published climatologic observations in *Bulletin of municipal statistics*, acquired from the Baron Jules de Lesseps, at a cost of 150,000 francs, the palace of Bardo (Fig. 9.14), a small replica of the palace of the Bey of Tunis which had been featured at the *Exposition universelle* of 1867. The city put it at the disposal of the minister of Instruction on 1 April 1869. A budget of 60,000 francs was provided to the new observatory. Sainte-Claire Deville was named director. There was obviously some overlap here with the Paris Observatory.

This was the situation that Delaunay found when he was called upon to take charge of the Paris Observatory on 3 March 1870. Renou and Sainte-Claire Deville wanted all meteorological activities transferred to Montsouris. But now Marié-Davy, finally rid of Le Verrier's persecutions and head of the international service,

had no desire to move there. Moreover, he had little esteem for the work being done at Montsouris. This put Delaunay in an embarrassing position: personally, he would have liked to see the Observatory devoted entirely to astronomy, but he also sympathized with Marié-Davy's point of view, and was reluctant to give up meteorology to the French meteorological society which had ensconced itself at Montsouris. In July 1870, he wrote to the Minister of Public Instruction²⁶:

Keeping meteorology here signifies the death of our astronomical observatory, but it would likewise be the death of meteorology were it to fall into the hands of Sainte-Claire Deville.

The war of 1870 led to a suspension of activity at Montsouris, while Marié-Davy and his assistants Claude Emile Fron and Boinot, who followed the government to Tours and then to Bordeaux, managed to continue without interruption the international service and the publication of its Bulletin. With peace restored, hostilities between the meteorologists resumed, but the Minister offered a solution meant to satisfy everyone. The meteorological observatory of Montsouris was annexed to the Paris Observatory, and Marié-Davy was named director on 1 April 1872. A position in the office of Inspector general for the meteorology in France and Algeria was created for Sainte-Claire Deville, as compensation for the loss of Montsouris. As for Renou, he received the directorship of the Laboratory of meteorological researches in the park of Saint-Maur, which was attached to the *École pratique des hautes études* (Practical School of advanced studies²⁷) Observations there began on 1 November 1872. This observatory is still in existence today, although its role and activities have changed; it belongs now to the Institute of Earth Physics of Paris (IPGP).

The Fleeting Return of Meteorology to the Observatory and the Creation of the Central Meteorological Bureau

One of Le Verrier's first acts upon his return to the Observatory in 1873 was to insist on the return of the meteorological service to the Observatory. The decree of 13 February 1873 satisfied him in this regard by transferring to the Observatory "the weather warning service and the study of large-scale atmospheric movements." The reinstallation took place in effect on 17 May, and on this date the weather warning service resumed. Marié-Davy, who had no desire to be dependent again on Le Verrier, remained at Montsouris, which became the "municipal Observatory of the *département* of the Seine." He insured that it functioned in an efficient way. In 1876, its local meteorological network consisted of 20 stations. However, the disputes among the meteorologists continued as before. France sent no delegates to the first

²⁶ Cited by Fierro (1991), p. 114.

²⁷ This school was founded by Victor Duruy in 1868 in order to promote research at the university and to develop practical teaching, hence its name which subsists today.

international meteorological Congress in Vienna in September 1873, the government judging it preferable not to flaunt the national infighting before the representatives of the rest of the world. In any event, the Congress would not succeed in creating an international meteorological organization. That was not accomplished until the Congress of Rome in 1879.

In July 1874, the meteorological service of the Observatory was split into two parts, assigned respectively to Rayet, who also presided over the studies of terrestrial magnetism, and to Fron. But it was necessary to wait for funding, and not until January 1875 were the 30,000 francs promised the year before by the Minister of Public Instruction disbursed, and only then did the international meteorological service (which it was hoped would soon include Algeria) begin functioning properly. In particular, the service providing warnings to the ports was expanded after the suppression by decree on 24 December 1875 of the rival service of the Navy. The forecasts remained however imprecise and impressionistic. For example the telegram sent to the ports of the English Channel on 30 December 1876 reads as follows²⁸:

A depression [area of low pressure] yesterday morning over the North Sea, new depression west Ireland; bad weather to be feared over the English Channel and the Ocean.

As for the ports of the Mediterranean, they received the following notice: "Lowering of 3 mm southern Europe with calm weather; new depression western Ireland." The depression however could hardly be of interest to them. It was only in 1876 that the weather notices to the ports contained forecasts of wind direction and magnitude.

Le Verrier revived in 1873 the departmental weather commissions, by asking them to organize among themselves regional commissions "endowed with a life of their own." They were each to publish a general Atlas underwritten by departmental subscriptions. But difficulties would soon surface: certain *départements* went it alone, others did not organize themselves in quite the way Le Verrier wanted. He had recommended in effect that the meteorological regions be identical to the academies, however the Council of the Observatory did not follow this recommendation but deferred to the status quo in order to avoid problems. Independently of this organization, two meteorological observatories were built in the mountains, one by general de Nansouty at Pic du Midi de Bigorre, at 2,877 m altitude, established in 1873 by the Society Ramond, and that of Puy-de-Dôme, which followed in 1876 (Fig. 9.15).

In this latter year, despite the progression of his illness, Le Verrier had the vigor to start up again, on 1 May, the agricultural warnings service, which he had dreamed of for many years. As early as 1873 he had attempted to distribute to the agricultural communes (townships) meteorological advice through the departmental commissions, but without success. Now he decided to send them directly to the communes wanting them. The only stipulation was that they possess a barometer. This time things went well. By 1 July 1877, 931 communes benefited from the meteorological warnings. There were 1,500 by May 1878. Confronted with this rapid expansion,

²⁸ Cited by Javelle (2000), p. 103.



Fig. 9.15 The meteorological observatory at Puy-de-Dôme under construction in 1873

the administration of telegraphic lines required the communes to pay for the transmissions. This, of course, had the effect of causing a rapid decline in the service.

The Council of the Observatory also had in mind extending to Algeria the meteorological service. Le Verrier declared that this did not seem possible, since Fron, on whose shoulders this extension would fall, was already buckling under the demand of the other work demanded of him. The Council took no notice of Le Verrier's objection, however. A report produced by Belgrand was adopted during the last meeting held during Le Verrier's lifetime, on 17 May 1877. Le Verrier's death on 23 September partially put an end to 22 years of entrenched warfare between the factions of French meteorologists.

His death, however, produced a void, which the Minister tried to fill by the creation of a new institution. He formed for this purpose a commission, which recommended profound reform. The report reads in part²⁹:

The members of the commission unanimously recognize that the current organization of the meteorological service cannot give to these scientific studies the development which they require and that major improvements are needed.... The large majority of the commission thinks, moreover, that a service so important, whose details are so numerous, ought not to remain dependent on an astronomical observatory. Without this essential separation, the only outcome will be failure, no matter what the undertaking. It is quite evident, indeed, that there is no scientific connection between astronomy and meteorology. As long as meteorological science does not have its own official representation, as long as it is not absolutely independent, nothing will happen.

²⁹ Cited by Fierro (1991), p. 197.

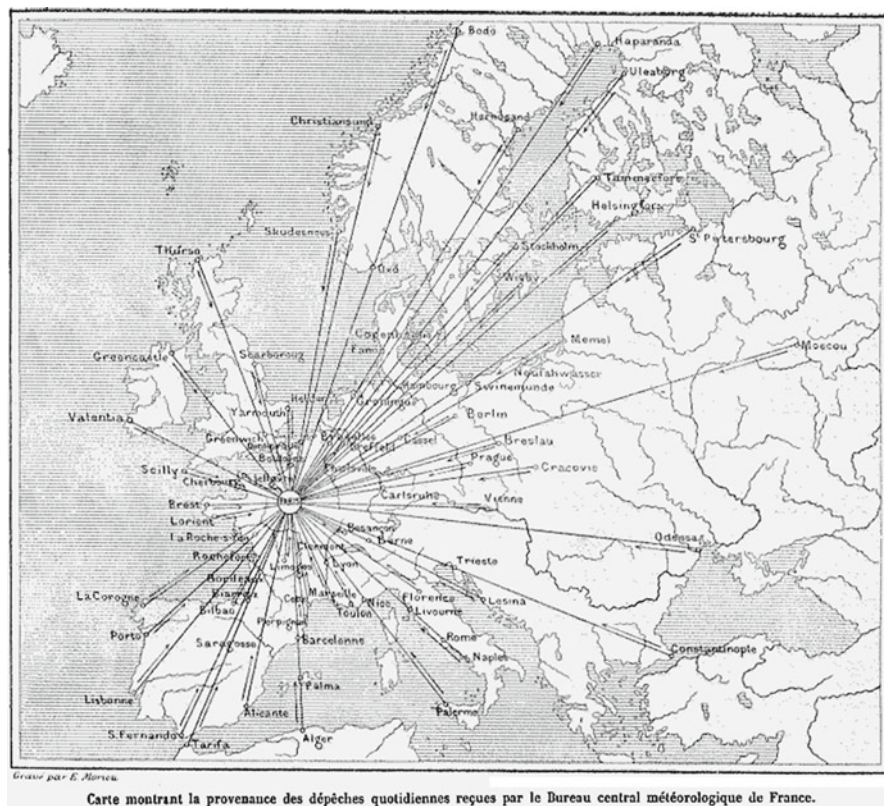


Fig. 9.16 Map of the daily telegraphic relations of the central meteorological bureau in 1878

Following the recommendation of this commission, the Minister of Public Instruction, now Agénor Bardoux, decreed on 14 May 1878 that the meteorological service of the Paris Observatory be transformed into a central Bureau of meteorology, the predecessor of the present Météo-France. The director was the physicist Eleuthère Mascart, who was assisted by a council presided over by Hervé Mangon, deputy and member of the rural economics section of the Academy of sciences. Fron would direct the weather warning service from its creation to 1904. The central meteorological Bureau left the Observatory on 6 October 1878 and installed itself at 60 rue de Grenelle. French meteorology was thus finally and definitively separated from astronomy. Nevertheless, its organization for a long time bore the impress of Le Verrier's institutions.³⁰ In particular, it benefited from the impressive system of telegraphic exchanges which Le Verrier had succeeded in putting in place (Fig. 9.16).

³⁰ Also, the meteorological commissions of the departments and the meteorological observations in the Écoles normales would survive for many years the death of Le Verrier.

Le Verrier and the Astronomical Theory of Climate

Le Verrier left his mark not only on French meteorology, but on international meteorology, through the creation of the international meteorological service, which permitted diffusion of meteorological data and a rudimentary 24-hr forecast for all of Europe. It was owing to his remarkable talents as an administrator that Le Verrier was able to accomplish this. Rather curiously, however, he seems not to have attempted to interpret meteorological phenomena in terms of physical causes, in contrast to other foreign researchers. One finds, in a small work published in 1880 by Brault, Chief of the meteorological service of charts and plans of the Navy, the following words, in which he attempts to account for this deficiency in the great scientist³¹:

The meteorologist is someone who works rather than someone who knows: his merit consists above all in understanding well how to carry out his role; for, as is the case with this science, as with so many others, one needs pioneers, that is laborers ... who clear the land by their labor, until the time when this land, well prepared and richly seeded, under the influence of some genius, allows small shoots to grow up which ripen into fruit and finally lead to an abundant harvest.

This is the way Le Verrier envisaged meteorology, and he did not find it beneath him to prepare the harvest for the future.

If he had carried out meteorology alone in his office, as he had done astronomy all his life, Le Verrier would no doubt have been able to take giant strides forward in the resolution of all the great questions which meteorology poses. But all these major questions require a legion of laborers, and it was this legion of laborers that he tried, in vain, to assemble. Le Verrier did not have the means to carry out all he intended, and in the end he abandoned the effort.

Nevertheless, there is one aspect where Le Verrier could have applied without great difficulty his intimate knowledge of celestial mechanics: this was the question of the influence on climate of variations in the parameters of the Earth's orbit due to the action of the other planets. Indeed, the precession of the equinoxes produces a progressive change in the date on which the Earth passes perihelion relative to the seasons and a continual variation in the duration of the seasons, as is explained in Fig. 9.17. Variations in the eccentricity of the Earth's orbit and variations in the inclination of the axis of the Earth's rotation relative to the ecliptic add their effects. These variations in turn lead to changes in the amount of energy received from the Sun, and in the Earth's temperature. This is the basis of the astronomical theory of climate developed by Milutin Milankovitch between 1915 and 1938, and taken up again recently by numerous researchers, most notably the Belgian André Berger.³² This theory explains well the alternations between glacial epochs and interglacial periods well known to geologists and paleontologists. For example, quoting Jean-Claude Duplessy:

³¹ Brault (1880).

³² See e.g. http://en.wikipedia.org/wiki/Milankovitch_cycles.

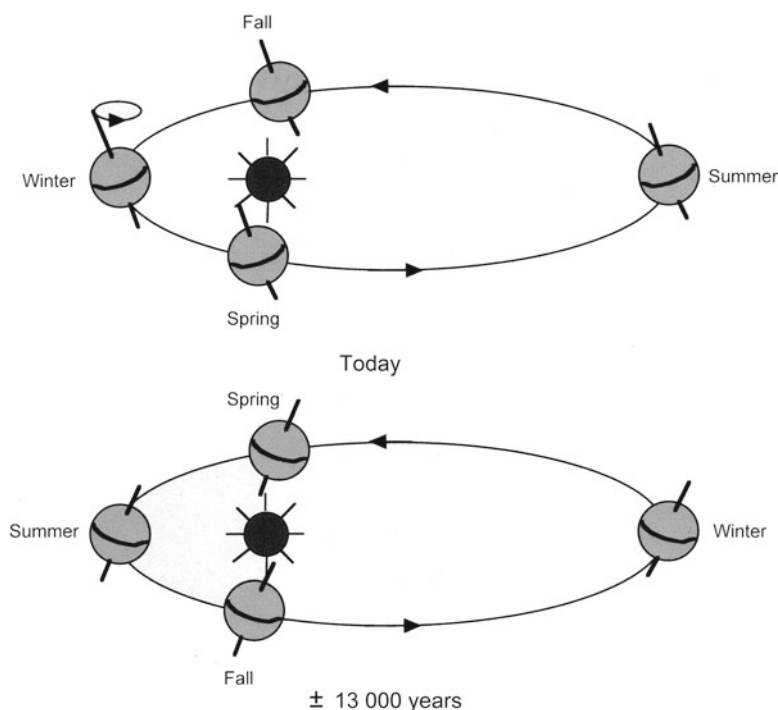


Fig. 9.17 Precession and climate. Precession rotates the axis of the Earth in 25,000 years as indicated at the top left of the figure. (The terrestrial orbit is drawn here with a very exaggerated eccentricity.) The position of the Earth is represented for solstices and equinoxes (remember that at the equinoxes the Sun is in the plane of the terrestrial equator, represented as a *bold line*). At present, the Earth is near its perihelion at the beginning of January, thus close to the winter solstice in the Northern hemisphere, while its largest distance to the Sun occurs in summer. Fall and winter therefore are shorter than spring and summer. But 13,000 years ago or 13,000 years in the future, the situation is reversed, because the axis of the Earth has turned by 180° . The Earth is then closest to the Sun near the summer solstice in the Northern hemisphere, and fall and winter in the Northern hemisphere are longer than spring and summer

Some 125,000 years ago the eccentricity of the Earth's orbit was close to 4% [current value 1.67%], the obliquity of its axis was sensibly greater than today [$23^\circ 48'$ compared to $23^\circ 26'$], and the Earth drew closest to the Sun in summer [currently in winter]. This configuration led to a distribution in insolation [influx of solar energy] during the summer for the higher latitudes of the Northern hemisphere 13% greater than the current value and inaugurated the last interglacial period.

We have not found in Le Verrier's papers any indications of a direct interest in this question. However, John Herschel, beginning in 1830, had laid the foundations of an astronomical theory of climate, but this foundational text³³ seems to have been little known in France. Arago, who thought a great deal about climate, examined the

³³ Herschel, J.W.: *Transactions of the Geological Society of London* 3, 2d series, pp. 293–299 (1832).

question in 1834, and concluded for an absence of effect.³⁴ However, there had already been noted in the colder regions of the globe fossils characteristic of hot regions, and Louis Agassiz had demonstrated in 1837, from his study of moraines and boulders, the existence in the past of a glacial age, though at the time he had no way of dating it. Some years later, in 1842, the mathematician Alphonse Joseph Adhemar had published in Paris a book³⁵ in which he attempted to explain glaciers in terms of the precession of the equinoxes.

In 1854, Jean Reynaud reexamined the question with a great deal of common-sense, observing that it was necessary to consider separately what happens in the Northern hemisphere and what happens in the Southern.³⁶

Le Verrier, as we have seen, did not read much, and then only the work of the most celebrated authors. Most likely he was completely unaware of John Herschel's article (of which there is an offprint in the library of the Observatory, but Le Verrier barely read English!), and he was also most likely ignorant of the books of Adhemar and Reynaud, probably even of Agassiz. His principal preoccupation was the long-term stability of the planetary orbits under the effects of mutual gravitational attraction: this preoccupation led him to calculate, beginning some time before 1855, variations in the orbital parameters of the Earth and other planets from 100,000 years ago to 100,000 years in the future³⁷ (Fig. 9.18). If Le Verrier did not attempt to go further than this into the past or future, it was because he was aware that uncertainties in the masses of the planets rendered such calculations dubious. (Indeed, his calculations do not agree well with modern ones.)

The Scottish physicist James Croll was the first to draw conclusions from these orbital variations. His work, published in 1864, must also have been unknown to Le Verrier.³⁸ Croll considered the problem in very general terms. He pointed out that it was the combined effects of the eccentricity in the Earth's orbit and of the longitude of perihelion (tied to precession), and what is new compared to John Herschel, also in the obliquity of the ecliptic (i.e., the inclination of the Earth's axis of rotation to its orbital plane) which determined the insolation (the energy flux from the Sun) at a given latitude of the Earth and which could therefore lead to long-term changes in the climate. For him, a diminution of the energy received from the Sun in winter at high latitudes of the Northern hemisphere ought to encourage the accumulation of snow and therefore a period of more severe cold; an eccentric orbit, and a winter solstice close to aphelion (the point in the Earth's orbit farthest from the Sun) would lead to a long cold winter and a short hot summer, hence to a glacial period.

Similar ideas would be circulated right up to the end of the nineteenth century, but not all geologists and meteorologists were convinced. Some even thought that a

³⁴ *Arago F., *Œuvres complètes*, t. 8, pp. 206–213.

³⁵ Adhémar, J.A.: *Révolution de la mer, Déluges Périodiques*. Paris, chez l'auteur. 2d edition (1870) available on <http://books.google.fr> (1842).

³⁶ *Reynaud, J.: *Philosophie religieuse: terre et ciel*, pp. 409–425. Furne, Paris (1854).

³⁷ °Ann. OP 2 (1856), p. [29] (=350); see for the very technical discussion p. 149 à 185.

³⁸ Croll, J.: *Philosophical Magazine* 28, 121–137 (1864).

<i>Éléments de l'orbite de la Terre.</i>				
Époques.	Longitudes		Inclinaisons.	Longitudes du nœud.
	Excentricités.	du périhélie.		
— 100 000	0,0473	316° 18'	3° 45' 31"	96° 34'
— 90 000	0,0452	340. 2	2.42.19	76.17
— 80 000	0,0398	4.13	1.18.58	73.47
— 70 000	0,0316	27.22	1.13.58	136. 8
— 60 000	0,0218	46. 8	2.36.42	136.29
— 50 000	0,0131	50.14	3.40.11	116. 9
— 40 000	0,0109	28.36	4. 3. 1	91.59
— 30 000	0,0151	25.50	3.41.51	66.49
— 20 000	0,0188	44. 0	2.44.12	41.34
— 10 000	0,0187	78.28	1.24.35	16.39
0	0,0168	99.30	0. 0. 0	0. 0
+ 10 000	0,0115	134.14	1.14.26	148.15
+ 20 000	0,0047	192.22	2. 7.46	124.29
+ 30 000	0,0059	318.47	2.33.19	100.29
+ 40 000	0,0124	6.25	2.27.53	75.31
+ 50 000	0,0173	38. 3	1.51.54	48.13
+ 60 000	0,0199	64.31	0.51.52	10.47
+ 70 000	0,0211	71. 7	0.34.35	220.38
+ 80 000	0,0188	101.38	1.45.40	170.15
+ 90 000	0,0176	109.19	2.40.56	139. 3
+ 100 000	0,0189	114. 5	3. 2.57	109.57

Fig. 9.18 Variations of the parameters of the orbit of the Earth, 100,000 years before and 100,000 years after 1850, from Le Verrier. The inclination of the orbit refers to that in 1850. One sees that the long-term changes are far from negligible

long cool summer and a short mild winter in the Northern hemisphere would occasion the onset of an Ice Age. It was Milankovitch whose arguments more or less won the day for Croll's ideas, and who also gave them definitive form. Starting with Le Verrier's theory of the Earth's motion, he calculated the solar radiation for various latitudes for the last 500,000 years. His calculations have subsequently been repeated, with better data, principally by André Berger. Figure 9.19 shows for example how the combined effect of different changes in the Earth's orbit produces variations in the mean insolation in June for a given latitude, here 65° North; one can make similar calculations for different latitudes and for different periods of the year.³⁹ The modeling of climate shows that what happens in the Northern

³⁹ See examples in Berger, A.: *Rev. Geoph. and Astron. Astrophys.* **26**, 624–657 (1988). For calculations over a much longer time span (–20 Ma to +10 Ma), see Laskar, J. et al.: *Astron. Astrophys.* **270**, 522–533 (1993).

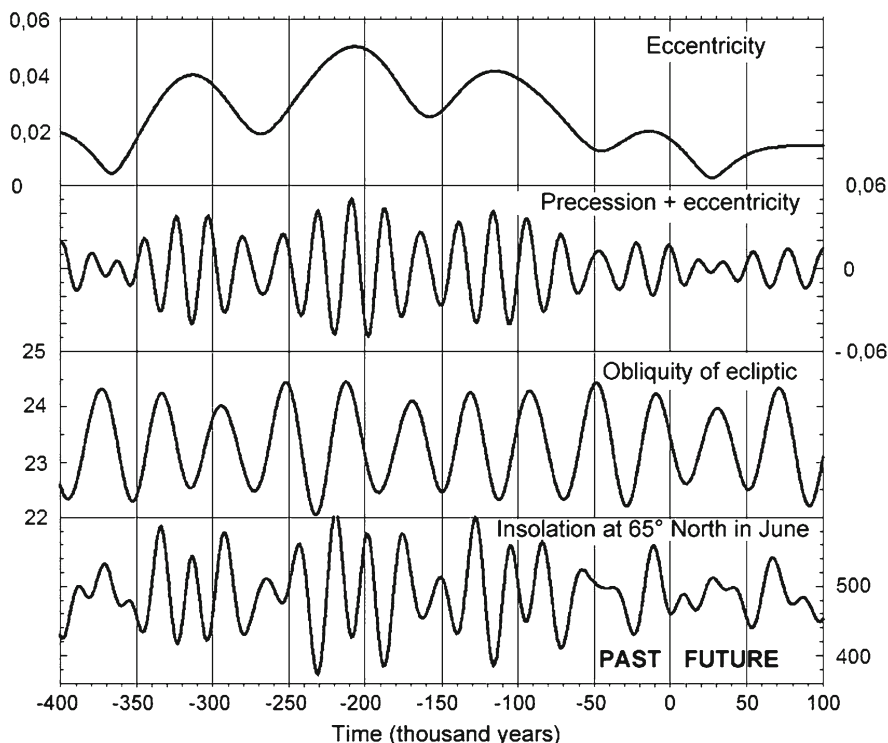


Fig. 9.19 Variations of the mean insolation in June at latitude 65° North. (From Berger, op. cit., as well as the following figure.) Note that time, in 1,000 years, runs from right to left, the origin being the present time. The *three upper curves* give the variations of the parameters of the terrestrial orbit: eccentricity; relative variations of the Earth-Sun distance in June due to the combined effect of precession and eccentricity; obliquity of the ecliptic. The *lower curve* gives the variations of the insolation, in W/m^2 outside the atmosphere

hemisphere has the greatest effect, because it contains much more of the Earth's total land masses than does the Southern hemisphere.

Figure 9.20 compares variations in insolation in elevated Northern latitudes calculated by such a model, with that of the ratio of the isotopes $^{18}\text{O}/^{16}\text{O}$ in the Antarctic ice, which furnishes an indirect estimation of the mean temperature of the globe. The correlation is excellent: the astronomical theory of climate finds itself well-confirmed over a span of many thousands of years. The figure shows that a slight cooling is expected over the next 5,000 years; this will be followed by a slight warming, then by more significant fluctuations until a new glacial age is established in some 60,000 years from now. In the shorter term, other factors, such as solar activity, may produce changes in temperature of a few degrees, not to mention human factors (anthropogenic climate change). However, these factors have nothing to do with Le Verrier's work on the orbit of the Earth.

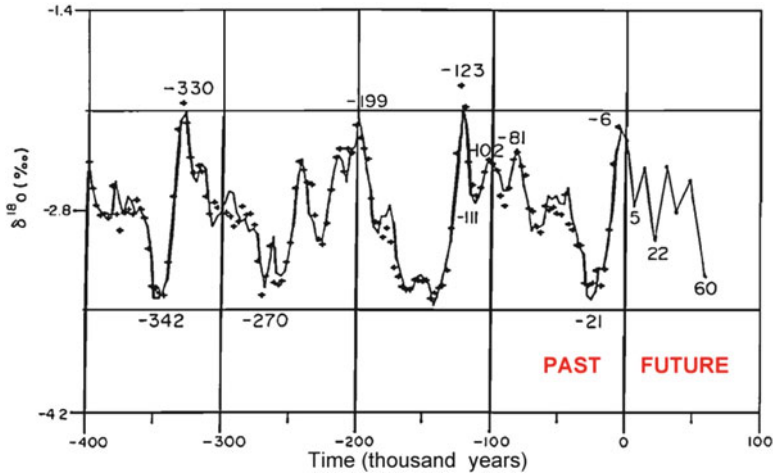


Fig. 9.20 Climatic variations during the last 400,000 years, and prediction for the next 60,000 years, ignoring the changes due to man. The observations (*crosses*) correspond to the $^{18}\text{O}/^{16}\text{O}$ isotopic ratio measured in the ice obtained by drilling at the Vostok station in the Antarctic. This ratio is given as the deficit, given in 1/1,000, with respect to a reference value which is the average ratio in oceanic water. It is an indirect measure of the average temperature of the Earth. The total range (distance between the *horizontal lines*) corresponds to about 15° . The line is the prediction of a model which assumes that the climatic system has a time-lag of 3,000 years with respect to its perturbations. One sees that if one ignores other effects (which are very important) the temperature of the Earth should decrease during the next 5,000 years

Chapter 10

The Legacy of Le Verrier



Le Verrier discovers the planet Neptune by his calculations, September 1846: plan for a painting on the ceiling of the East circular hall of the Paris Observatory, by Edmond Louis Dupain (1889). Le Verrier, standing, points to Neptune while Glory (or Fame) brings a laurel crown. The names of some of the other persons visible on the painting are indicated at the bottom right corner. To the left, at the desk, are Otto Struve and Arago under the statue of Laplace, with the large dome of the East tower behind them. In the foreground, at the bottom, is possibly Faye; the person with the powder wig seated at the other table may be Lalande or Lemonnier. Galle looks at Neptune through a telescope

The vision that we have of a historical person is biased by the fact that we have difficulty positioning him in his epoch, with which we may not be very familiar, but more especially because we are prone to neglect certain sides of the person and emphasize other sides, whether they be good or ill. Our estimation thus becomes somewhat Manichean. For Le Verrier, this is certainly the case. It is not easy to reconcile his scientific genius with the problematical human side. Also it should not be surprising that writers have tended to minimize, according to their inclinations, one or the other aspect. We should, for our part, acknowledge that we have had difficulty in achieving objectivity. But then perhaps no historian can ever approach his subject with complete objectivity. The one point beyond dispute about Le Verrier is his scientific genius: his brilliance is clear from a reading of his works, and was even agreed by his worst enemies, such as Delaunay, Flammarion and Liais, though at times they tried to minimize its scope. But let us see how his contemporaries and successors have looked on Le Verrier after his death.

The Funeral Orations and the Obituary Notices

Le Verrier must not have been very pious, for Fizeau, who was a good Catholic, had to incite him “to conform his religious practice to his sentiments.” Communion was given to him on 29 June 1877 by the *curé* of Saint-Sulpice, that is nearly 2 months before his death on 23 September.¹

Several discourses were given at Le Verrier’s interment, which took place on 25 September at the Montparnasse cemetery, near the Observatory (Fig. 10.1)²: the speakers were, of course, unanimous concerning the quality of his scientific work, and particularly waxed eloquent about the discovery of Neptune, a triumph of celestial mechanics which had an enormous impact on the public and which had raised the public’s esteem for science. We cite, by way of example, an extract of the discourse of Jean-Baptiste Dumas, permanent secretary of the Academy of sciences, which appeared in part in *La Nature* for 13 October 1877³:

Accepting with solid good sense the laws of attraction as correct, he worked out all their consequences. It was in this way, by an admirable and convincing analysis, he discovered in space a small [sic] unknown planet; he waited as if he held it in his hands; he marked out in the sky its path and position and the point it ought to occupy on 1 January 1847, as if he directed its course. The manner in which this body was found in the firmament with a telescope, at the very point where it had been predicted by mathematical analysis, is well known.

The emotion was universal. But Le Verrier was not the only one to benefit: his associates, rivals, scientists from all countries also were beneficiaries. Through the recognition of what he had achieved, and the glory he had achieved by it, the public confidence in science rose to a level that had never, perhaps, been attained before.

¹ Centenaire de la naissance de Le Verrier, pp. 91–92; see also Levert et al. (1977), pp. 169–170.

² All these discourses are reproduced in *CRAS 85 (1877), pp. 579–596.

³ **La Nature*, 2^e semestre 1877, pp. 305–307.

Fig. 10.1 The tomb of Le Verrier at the Montparnasse cemetery



It was not exactly the moment to recall Le Verrier's character and the difficulties that ensued from it. Dumas nevertheless alluded, in passing, to the difficulties that he had known:

The truth that he had pursued with so much passion during his sojourn on this Earth, through so many agitations and troubles, he knows finally in its entirety in the serenity of eternal life and in the peace of his tomb.

The discourse given by Yvon Villarceau on behalf of the astronomers recalled in objective fashion the relationship of Le Verrier to astronomy, and made a brief allusion, the only one in all these discourses, to his meteorological activity, which the astronomers had always looked at askance:

It would be unjust not to mention the creation of the meteorological warning service, which Le Verrier installed on a grand scale. His great scientific authority was no doubt necessary to realize such an institution; thus can be explained the concentration, in the hands of one person, of everything of interest to both astronomers and meteorologists of the time.

As for Tresca, he did not hesitate at all in his funeral eulogy to enter into Le Verrier's character, and did not attempt to disguise its unsavory aspects:

Impatient and brusque while working out his exalted speculations, intolerant of any distraction, he was, on the other hand, agreeable and affable in all the other areas of his life. Overt contradiction of his opinions did not disturb him; he welcomed it, as long as he could convince himself that it proceeded only from candor. But the very openness of his nature could sometimes express itself through a complete want of courtesy, not justified by the circumstances, and then Le Verrier no longer seemed to be the same person: in place of his erstwhile charm, he seemed aloof, disdainful, and abrupt in manner to the point of being hurtful. He was, above all, always implacable to whatever he regarded as specious, or mindless, work.

The audience certainly waited for revenge from Faye. But he spoke only a few words on behalf of the Bureau of longitudes, whose relations with Le Verrier had always been execrable. He acquitted himself gracefully on this occasion:

Others have fought bitterly against indifference and neglect; he had the good fortune to encounter everywhere admirers, even among his rivals.

As for Janssen, who gave the final discourse, he was careful not to speak of anything except Le Verrier's astronomical work.

There were many articles that came out in journals, scientific and otherwise. Their authors discussed more freely than the preceding orators Le Verrier's character and his conflicts with others. Here, for example, is an extract from a carefully balanced article written probably by Louis Figuier in the *Année scientifique*⁴:

In the polemics that he had to endure as well as in those he provoked, he did not always observe the usual courtesies. He sometimes made himself the advocate of bad causes....

But asperities of Le Verrier's character have no doubt been much exaggerated: for what person, at the head of a large administration, does not create enemies? It is from the accounts of his enemies that Le Verrier's character has been turned into a caricature, a portrait that is surely exaggerated. But it is not our intention to set it right here.

All the critiques, all the passionate attacks, that have been directed against Le Verrier, would have been better aimed, it seems to us, had they been against the bureaucrat rather than against the man. When Delaunay replaced Le Verrier as the director of the Paris Observatory, the same complaints, recriminations, and attacks were lodged against him: the cacophony of diatribes continued; the only thing that changed was the name of the bureaucrat.

Most of the attacks against Le Verrier, of which the political journals made obliging echoes, came from employees who had either quit the Observatory by order of the director or of their own volition [here, the author was obviously thinking of Flammarion]. But people unfamiliar with the work and the administration of the Observatory were always well received. As to the relations we ourselves have had with M. Le Verrier, we have always found him to have all the desirable qualities of affability, gentleness, and good manners. No doubt those who complained of his irascibility and bad manners must have experienced them. As for us, we can no less attest from experience his warm qualities of heart and spirit.

Two other obituaries were published in the *Annales de l'Observatoire de Paris*, but only in 1880.⁵ The first, read before the Academy on 10 March 1879, was by the mathematician Joseph Bertrand, who had known Le Verrier very well. This time, nothing was held back: the text recalled to life the insupportable atmosphere at the Observatory before 1870:

The theory of the first four planets ... was completed in an atmosphere of difficult administrative cares, the torments of hostile surveillance, the troubles of many covert contradictions, and the hindrances of outright ruptures and hostile declarations. I neither wish nor need to evoke the memory of a stubborn siege lasting fifteen years. It is useless for me to try to sort out plausible truths in the incessant and sustained accusations with which people have disparaged and blackened the intemperate and bizarre doings of a despotic character. All anger is unjust, but it never believes itself to be so. All hatred is blind, but it imagines itself clear-sighted. The most implacable accusers, the adversaries who were the rudest in

⁴ **L'Année scientifique*, 1877, pp. 522–532.

⁵ °*Ann. OP, Mémoires* 15 (1880) pp. 3–22 et 23–43.

their quick invectives, were always the most convinced that they had the truth entirely on their side....

As to the other article, that of Tisserand, it was devoted completely to the beautiful researches into the planetary system that Le Verrier had pursued with so much success over four decades. One can still read it with profit, because it is a very clear exposition by a master. That same year, 1880, Gaillot, who had effectively helped Le Verrier with his calculations in his final years, published in *La Nature* a long article which is also of interest⁶. He concludes with a maxim which captures the essence of Le Verrier's work:

Every discrepancy reveals an unknown cause and could lead to a discovery.

On the other side of the Channel, where there was still keen interest in Le Verrier's researches, obituaries appeared that were very detailed. Edwin Dunkin, an astronomer at Greenwich who would become in 1884 president of the Royal Astronomical Society, wrote immediately after Le Verrier's death a eulogy⁷ in which he made some allusion to the problems at the Observatory at the end of 1860s:

He [continued his researches] in silence year after year, but not without numerous difficulties, private and public, which perturbed him on occasion. These problems increased a great deal over time, given his dismissal from the Observatory in 1870 and the political changes that came about in succeeding years. The problems became so acute during the war between France and Germany that for a while it was doubtful whether he would ever be able to return to his work. His health suffered a great deal during this period of extreme stress; but upon his reinstallation at the Observatory in 1873, he resumed immediately his researches on the planets with his erstwhile vigor.

Dunkin mentioned also with praise Le Verrier's geodesic activity, in which he himself had participated, as well as his meteorological work, which was passed over almost entirely in silence in France – no doubt because the dissensions among meteorologists had not yet been resolved:

The establishment by Le Verrier of the daily meteorological bulletin has shown itself to be of great scientific importance. The concentration of all the meteorological observations of France and the neighboring countries in the hands of such an authority could not but produce results of very great value concerning the climate of Europe and the West.

In February 1878 John Russell Hind, Superintendent of the Nautical Almanac Office in Greenwich who was present at Le Verrier's funeral and knew him very well, gave a detailed presentation of his work to the Royal Astronomical Society.⁸ He stressed, of course, the discovery of Neptune, concerning which he spoke in a calm and impartial way about the respective contributions of both Le Verrier and Adams, but also about his study of comets: no doubt this was a subject of particular interest to him. He mentioned the problem of the anomalous advance of the perihelion of Mercury, citing Adams:

⁶ *La Nature*, 2^e semestre 1880, pp. 102–107.

⁷ *The Observatory* (1877) 1, pp. 199–206.

⁸ *Monthly Notices of the Royal Astronomical Society* 38 (1878) pp. 155–166.

The theory of the planet had been established with so much care, and its transits across the solar disk furnished observations so precise that there could be no doubt of the reality of the phenomenon: and the only way of accounting for it seems to be to assume, with M. Le Verrier, that there exist either a number of small planets or a certain quantity of diffuse material circulating around the Sun inside of Mercury's orbit.

Hind hardly mentioned Le Verrier's difficulties with his personnel and the authorities. The little he did say gave only a hint of the truth:

In 1870, due to disagreements with the Observatory personnel, [Le Verrier] ceased his connections with it for some time, but after the regrettable death of Delaunay in 1872, he was reinstated, and directed the institution for the rest of his life.

It appears from these texts that nothing that took place in Paris was unknown to the astronomers in England.

The Statue of Le Verrier

Upon Le Verrier's death, there were thoughts of erecting a statue of him near the Observatory. The Academy and the Scientific Association that Le Verrier himself had founded, launched at the beginning of 1878 an international subscription. Fizeau was placed in charge of it. The Council general of the département of La Manche, over which Le Verrier had presided, decided to contribute 2,000 francs. Finally, the subscription collected a sum of 31,559.65 francs, the equivalent of 130,000 dollars in today's currency. This was enough to finance the statue, but where should it be placed? Here's what Fizeau said about the subject in his inaugural discourse:

However, it was necessary to determine, in concert with the public authorities, the site where the statue would be erected: plans to erect the statue in a public place near the Observatory met with the approval of the minister of the Interior and the prefect of the Seine; it was fully expected that the city of Paris would quickly authorize the project, since it would permit, without any expense on its part, the embellishment of a boulevard with a work of art of the first order dedicated to the honor of the sciences.

But, we regret to say, the expectations of the committee have not been realized; for all the trouble taken, despite multiple attempts, success has proved elusive. Obstacles that cannot be overcome over the course of several years must be regarded as insurmountable.

Without seeking out the likely motives of such a decision, let us recall that Le Verrier had been nominated director of the Observatory under the Empire; he was a senator, but under the Empire he had also been brusquely disgraced; it was only under the Republic that he returned to the Observatory where he soon ended his career. We also know that Le Verrier was religious [especially thanks to Fizeau himself!]; but who would reproach him for this? That he had a proud character, a brusque and intimidating manner to those who opposed him. But do these things still distress us? We will only note the astonishment that the decision has caused among all the friends of the sciences.

Louis Figuier was much more explicit⁹:

It is known that the municipal Council did not want to approve the location for the statue under the pretext that Le Verrier was not republican; a fact which is perfectly well known.

⁹**L'Année Scientifique* (1890) pp. 575–576.

Fig. 10.2 The statue of Le Verrier by Chapu (1889) in the Northern courtyard of the Observatory

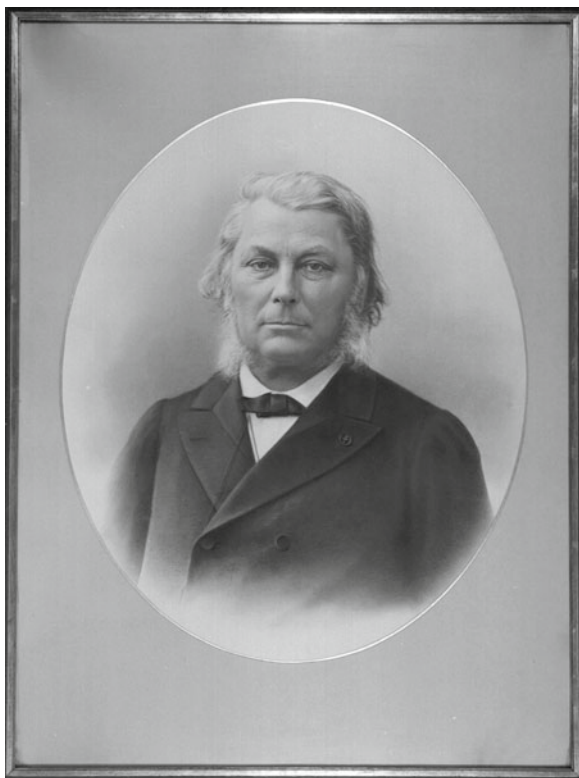


Finally, the subscription committee received on 4 August 1888 authorization to put the monument in the northern courtyard of the Observatory, therefore outside the domain of the city of Paris. The statue, which was nearly completed, was erected on a high pedestal; it is still to be found there today (Fig. 10.2). The ensemble was conceived by Lucien Magne, Le Verrier's son-in-law, and another architect named Gennys, and was realized by the sculptor Henri Michel Antoine Chapu. The existence of catacombs under the site required reinforcement of the foundations, and the addition by Chapu of two bas-reliefs to the plinth caused a delay of almost another year. The statue was inaugurated on 27 June 1889 by the Minister of Public Instruction Fallières, the future president of the Republic, with six discourses. Fizeau spoke for the subscription Committee, Joseph Bertrand for the Academy, Mouchez (Fig. 10.3) on behalf of the Observatory, Tisserand for the Bureau of longitudes, Cornu as a representative of Otto Struve, the director of the Observatory of Pulkova, and finally Fallières.

These addresses¹⁰ are interesting. Otto Struve said, "Now we can regard the image of Le Verrier free of the passions that surrounded him while alive."

¹⁰ *Discours prononcés à l'occasion de la cérémonie d'inauguration de la statue de Le Verrier sous la présidence de M. Fallières, ministre de l'Instruction publique et des Beaux-arts, à l'Observatoire le jeudi 27 juin 1889 (1889) Paris, Firmin-Didot.

Fig. 10.3 Admiral Mouchez (1821–1892), the successor of Le Verrier at the directorship of the Paris Observatory



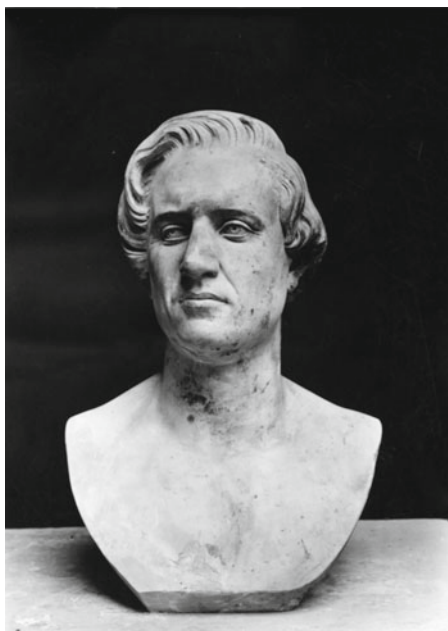
Mouchez still regretted (or feigned regret) that the name Le Verrier had not been given to the planet he discovered:

Because of a regrettable ingratitude on the part of contemporary astronomers, and an even more culpable indifference among French astronomers, the name of Le Verrier, justifiably given by Arago to the new planet so marvelously discovered, was not retained. However, never has such an honor, which would have for all time inscribed in the skies the name of the illustrious astronomer and retained his fame to a far posterity, been more merited; it would have certainly have been much more justified than the custom that attaches the most obscure observer to a new comet, found in the field of his telescope by sheer luck.

Mouchez mentions at length this time the meteorological activity of Le Verrier; but he bore a grudge on this subject toward his colleagues in the Navy, commenting: “Unfortunately, if good ideas abound in France, practical sense, and the will to make use of them, are often found lacking with us: and so it happened, as it often does with us, that here again foreigners [in this case Admiral FitzRoy] were the first to realize this grand project, which however demonstrates for us the value of these ideas.”

Tisserand’s discourse, concerned with Le Verrier’s discoveries in celestial mechanics, is a model of popularization of a difficult subject. We have cited extracts from it at various places in this book. As for the discourse of Otto Struve, he drew on the memories of his father to comment upon the arrival of Le Verrier at the Observatory:

Fig. 10.4 Bust of Le Verrier by Pradier. In this 1946 photograph, the bust, slightly damaged by the war, had not yet been restored



When he was appointed director of the Observatory, he had never been seriously engaged in practical astronomy: up to that time, as he himself said, he had scarcely looked in a telescope.... But an even more serious difficulty awaited him: forty years ago, the Observatory of Paris still enjoyed the traditional renown acquired through the works of Cassini, and it was known to be inhabited by illustrious savants; but it had languished, scarcely equipped with the instruments so necessary to modern science, and risked perishing for a lack of sufficient resources.

Once the directorship was handed to Le Verrier, new blood began to circulate through the Observatory's veins. With that clarity of vision that always distinguished him, Le Verrier traced a plan of operations and carried it out with a force of personality that sometimes, in the eyes of his contemporaries, went too far, but of which we nowadays enjoy the benefits.

The Centennials of the Birth of Le Verrier and of the Discovery of Neptune

Nothing was done in 1896 on the occasion of the fiftieth anniversary of the discovery of Neptune, apart from minor articles in the press¹¹; not until 1911, the centennial of Le Verrier's birth, were ceremonies held in his remembrance. At Saint-Lô, the city of his birth, the press mobilized to have commemorative activities organized, but the city showed little enthusiasm. Le Verrier had been quite forgotten, and the bust of Pradier (Fig. 10.4) had been relegated to the commissions gallery of the

¹¹ See for example Parville, H. de (1896) *Le cinquantenaire de Neptune*, *La Nature*, 2^e semestre 1896, pp. 22–23.



Fig. 10.5 Ceremony at Saint-Lô (Manche) on the eve of the centenary of the discovery of Neptune, on 22 September 1946. The bust of Le Verrier by Pradier, extracted from the ruins of the town hall which was almost entirely destroyed, is at the center. Immediately to the right, André Danjon, director of the Paris Observatory

Town Hall, and “commonly used as a hat rack.” The prefect had to ask the local archaeological society to organize a ceremony and banquet.¹²

At Paris, there were no festivities either, but the Academy of sciences honored his memory by the publication of a remarkable brochure,¹³ due essentially to Guillaume Bigourdan but unsigned, in which are printed many original documents concerning the life and works of the “illustrious astronomer” communicated by Le Verrier’s daughter, Lucile Magne. There is no commentary of importance, but there is an almost complete bibliography of Le Verrier’s publications.

The centennial of the discovery of Neptune occurred in 1946, while France was still painfully recovering from the ravages of the Second World War. At Saint-Lô, the archaeological society organized new festivities, which were modest because the city was in ruins: on 22 September, a meeting took place in front of the ruins of the Town Hall (Fig. 10.5), a Mass was celebrated and a banquet took place at 1 in the afternoon, at which the mayor, Georges Lavalley, recalled that Le Verrier had been a member of the general Council, but a contested member. Lavalley asserted that the complaints against him were “too violent to be sincere.... Someday, perhaps, someone will write the life of Le Verrier. Justice would be done to him.” This

¹² See for details Levert et al. (1977), pp. 177–179.

¹³ *Centenaire de la naissance de U.-J.-J. Le Verrier* (1911) Paris, Gauthier-Villars; most of the letters reproduced in this pamphlet are preserved at the BOP.



Fig. 10.6 Invitation to the commemorative ceremony of the centenary of the discovery of Neptune, and to the exhibition devoted to this discovery (This exposition was very complete: see *Catalogue de l'exposition "Le Verrier et son temps"* (1946))

role fell to Françoise Lamotte,¹⁴ but she could only verify that the colleagues of Le Verrier had complained of the “grand airs and difficult moods of the president.” In the afternoon, the director of the Observatory of Paris, André Danjon, gave a speech on the discovery of Neptune.

Another commemorative ceremony took place in Paris (Fig. 10.6), but also in England at London and Cambridge: a sign of definitive reconciliation between the former rivals that was facilitated by the victory of the Allies, with French delegates going to London and Cambridge to honor the memory of Adams, while some days later, on 18 October 1946, the Astronomer Royal Sir Harold Spencer Jones and the Cambridge professor Frederick John Stratton, went to Paris to join the festivities there. A medal was struck for the occasion (Fig. 10.7).

André Danjon gave a talk in which he laid out the work of Adams and Le Verrier.¹⁵ He gave another on 26 October at Brussels before the Belgian astronomical Society.¹⁶ This one was more interesting and more detailed than the Parisian speech; Danjon played the role of historian in positioning the discovery of Neptune in its context, and in explaining its importance seen through the eyes of a person in 1946. There is nothing to add to his text, which is still relevant:

It would be easy to find in the history of the sciences many discoveries that were more useful, in the common sense of the word, than the discovery of Neptune; there are others that

¹⁴ Levert et al. (1977), pp. 115–117.

¹⁵ Danjon (1946).

¹⁶ Danjon (1946c).



Fig. 10.7 Medal of the centenary of the discovery of Neptune. The verso alludes to the position of Neptune in the constellation of Capricorn

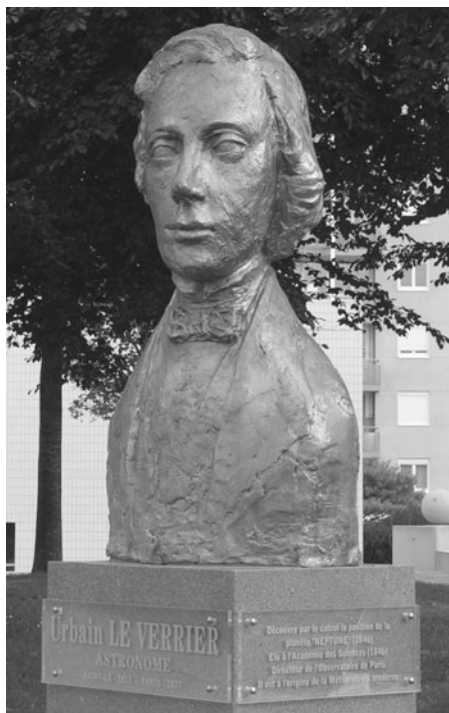
have been more important for the development of science and the progress of the human spirit. But one would search in vain to find any whose reverberations were greater, whose echoes were more persistent, or whose impact on its contemporaries were more pronounced. Was all the emotion, to which the written record of the time bears witness, justified? The public, one needs to realize, was particularly captivated by the mysterious character of the art used to reveal Neptune, which made this planet appear like a magician's ball conjured, as it were, from a spell book of numbers and formulae. This magical operation was one in which the astronomer appeared in the role of a sorcerer. But this same public readily lent its ear to the suggestions of the initiated who commented to the public about this prodigious discovery, which came as such a surprise, and explained to the public that celestial mechanics had triumphantly extricated itself from a very grave crisis: that, during several dozens of years, it had found itself helpless in explaining sensible irregularities in the movements of Uranus, but that the cause of these disorders was finally identified, the new planet being alone responsible. At a moment in which the infallibility of celestial mechanics was threatened, it found its honor defended, and found itself definitively established. In a word, the discovery of Neptune was a triumph of scientific determinism, and even though immediate practical utility could not be expected, it should be regarded as a striking manifestation of the power which science gives mankind over nature. All thoughtful spirits could feel the importance of such an event: the disciples of Auguste Comte found in it arguments in favor of positivism, and praised its novelty, while for their part, the spiritualists saw with deep satisfaction the existence of a supreme order of the Universe affirmed.

Sixty years after the ceremonies at Saint-Lô, the city finally made up for its relative indifference to its most illustrious citizen by erecting in July 2004, in the garden of the cultural Center, a bronze bust by the sculptor Louis Derbré (Fig. 10.8). On the pedestal is written:

Urbain Le Verrier, astronomer, Saint-Lô—1811—Paris—1877
 discovered by calculation the position of the planet Neptune (1846)
 elected to the Academy of sciences (1846)
 Director of the Paris Observatory
 Creator of modern meteorology.

On 23–25 September 1996, 150 years after the discovery of Neptune, there took place at the Paris Observatory the “Journées 1996, Systèmes de référence spatio-temporels: hommage à Laplace.” An observation of Uranus and Neptune, which

Fig. 10.8 Bust of Le Verrier
by Louis Derbré in Saint-Lô
(2004)



were close together at the time, was planned for the participants, but the weather did not allow it.

Le Verrier and French Astronomy

We have seen that Le Verrier reorganized the Paris Observatory on his arrival in 1854. Here is what Danjon says about this in his Parisian discourse of 1946:

Not since the epoch of Louis XIV had anyone known how to give to the Observatory a suitable charter, and to endow it with personnel required to carry out work of a routine nature by means of equipment that was both powerful and accurate. This was the challenge [Le Verrier] undertook and pushed through with success, though not without stepping on the toes of some of his personnel, who had grown soft and accustomed to the habits of an anarchic regime. Le Verrier knocked over all obstacles, and achieved his goal. Hard on others, as he was on himself, he pursued his goal directly, and did not worry about the disgruntled ones.

For us, the astronomers of France, [the discovery of Neptune] has had lasting effects, by drawing out of the shadows him who would reorganize our observatories, give us our equipment, and create a framework under which, apart from a few minor alterations, we still live today. But time spares no work, and Le Verrier, a positive spirit, would be the first to denounce nowadays the decrepit and obsolete parts of his administrative work.... What we

ought to retain of Le Verrier's work is not obedience to the letter of the rules he dictated, nor adherence to the lists he left behind of the needs he saw for astronomy. What we ought to retain is his will to maintain the equipment of the observatories to make possible the progress of science. His one unvarying preoccupation was to supply the initiates to astronomy with all the necessary requirements. And it was this innate taste for large and difficult things, pushed relentlessly to their accomplishment, which characterized everything he did.

And Danjon, who had just been named director of the Paris-Meudon Observatory, added:

The successor to Le Verrier has no intention to do otherwise than to remain in the same path our illustrious predecessor traced out, in seeking to maintain our national observatory in a condition so as to be ready and able to participate in the progress of astronomy.

In fact, Le Verrier was the first director of the Paris Observatory who had the free reign to make it operate effectively. None of his predecessors, moreover, had the title of director, with some short exceptions. Before the Revolution, the Observatory was under the tutelage of the Academy of sciences, and afterwards, of the Bureau of longitudes. These two organizations did not refrain from exercising their prerogatives. The last head of the Observatory before Le Verrier, Arago, despite his charisma, was unable to fully impose his views; moreover, the Bureau of longitudes and Arago himself, seriously ill during his final years, let the institution deteriorate. It would be unfair, however, to forget the efforts made by Cassini and by Arago to provision the Observatory with suitable instruments for observing. Arago's instruments weren't negligible, and thanks to him the Observatory would have been in possession, shortly after Le Verrier's arrival, of the largest refractor of the period, had not the objective deteriorated irremediably. As for the two large instruments pushed for by Le Verrier, the 120 cm reflector and the 75 cm refractor, one was spoiled, the other was given up. On the other hand, Foucault's 80 cm telescope, set up at Marseille, and the meridian circle offered to the Paris Observatory by Bishoffsheim were lasting successes. At Paris, the disposition of things at the death of Le Verrier is summarized in Fig. 10.9.

Though not mentioned by Danjon, one reproach to Le Verrier might be his near-exclusive interest in astronomy and celestial mechanics. It's true that Danjon himself shared this interest; but, in contrast to Le Verrier, he saw, beginning in the 1950s, that the new specialties belonging to astrophysics – and in particular, radio astronomy – promised a great future. As for Le Verrier, he neither supported nor even recognized the nascent field of astrophysics, and the few efforts made by his staff to employ the new techniques of observation of spectroscopy and photometry were not encouraged. It's likely that the Observatory would have had a very different orientation if Arago had had the force to better organize and impose his ideas, and if he had found at the Observatory astronomers worthy of him. But all of his initiatives were snubbed upon Le Verrier's arrival. Astrophysics in France was stillborn until Janssen took it up again, and then outside of the dominant institutions. An extenuating circumstance for Le Verrier was that in his time the major foreign observatories devoted themselves almost exclusively to astrometry, with

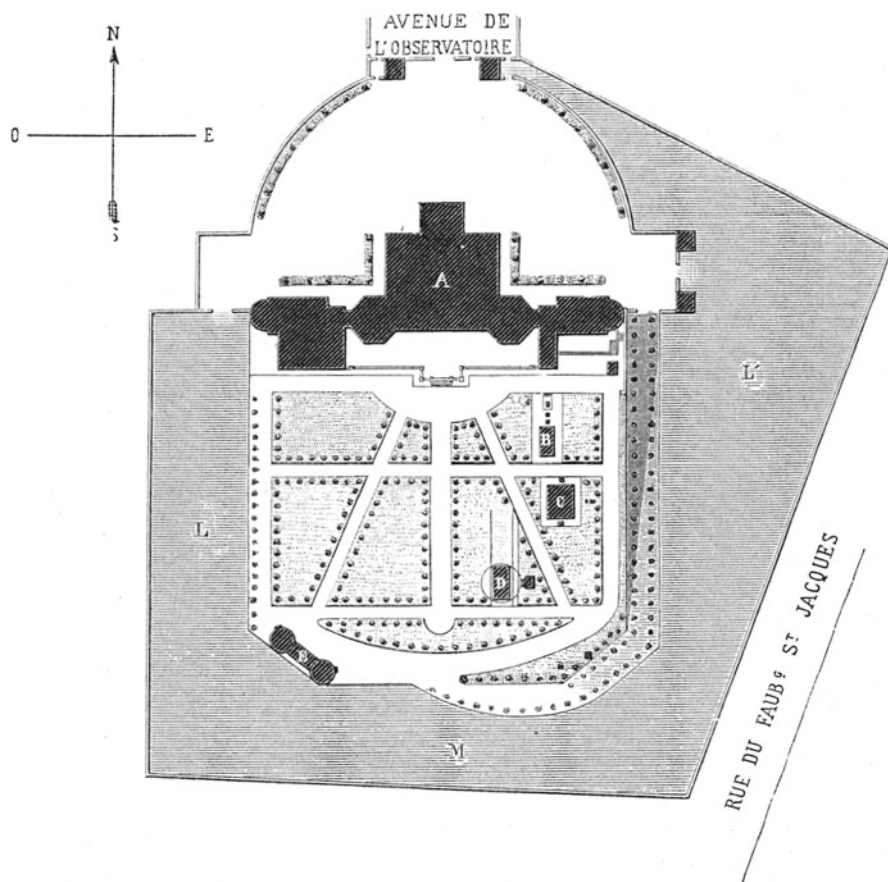


Fig. 10.9 Plan of the Paris Observatory in 1878, just after the death of Le Verrier. *A* the main building, with on the East tower the 38 cm unusable equatorial and on the West tower the 32 cm equatorial. In the East wing, the old observation rooms, and in the West one the director's apartments. *B* Foucault's siderostat. *C* Bishoffsheim's meridian circle. *D* the 120 cm reflector, barely usable. *E* the twin domes, each one containing a 24 cm equatorial. *LL'* moats, *M* moat to be filled up

astrophysics being the purview of only a few isolated individuals like Huggins and Secchi. The Paris Observatory was not exceptional in this regard.

From the foundation of the Paris Observatory until the end of Le Verrier's reign, its astronomers not only carried out astronomy but also geodesy, terrestrial magnetism, and meteorology. Far from withdrawing from these, Le Verrier continued them, despite numerous difficulties, and even developed meteorology to a considerable degree at the Observatory, unheeding of the competition. The envious, the jealous, and many of the astronomers did not fail to criticize him for this. After his death, the Observatory quickly stopped occupying itself with anything except astronomy

proper. The geographical service of the army had to operate without the assistance of astronomers, and meteorology and terrestrial magnetism became departments of other institutions, such as the Central Bureau of meteorology. Here is what admiral Mouchez had to say in January 1879 on the matter in his plan for the Observatory¹⁷:

...The meteorological observations of the Paris Observatory have lost much of their interest ever since specialized meteorological observatories have multiplied everywhere, even in Paris, with superior instruments and more standardized and systematic means for making observations than those at the Observatory....

If it is desired to keep the study of magnetic phenomena at the Observatory, a considerable sum would have to be allocated for the purpose. But I believe the funds are small, and would be better put to use for astronomy whose needs increase more rapidly than the budget allotted to it.... It is necessary to specialize more and more in the study of sciences and to concentrate our efforts when we are so limited in our means of action and resources. This is the only way to assure progress....

This position is certainly understandable, though also rather theoretical, since in fact the Observatory would continue for some time to make meteorological and magnetic observations. The other observatories specializing in geodesy and meteorology and terrestrial magnetism were concerned with public service, and existed mainly to support practical needs. They would, for instance, take charge of the distribution of time. Even today, the astronomers still count service tasks among their duties.

In astronomy Le Verrier and his successors until World War II, despite incontestable qualities, limited themselves for the most part to positional astronomy and celestial mechanics. They had reasons for this. Though certain astronomers, following Arago, posed questions regarding the physics of the Sun, stars, and nebulae, and concerned themselves with atmospheric phenomena and the origins of the terrestrial magnetic field and the heat of the Earth, the theoretical attainments of the time were too undeveloped to provide any but the most speculative notions. Even the efforts of Janssen did not lead in France to the emergence of real astrophysics, other than solar. It was to be left to the United States, where huge instruments capable of observing the extragalactic universe were built and where a pragmatic enthusiasm prevailed over traditionalism, and in England and Germany where physicists extended their concepts to the wider Universe in close liaison with observational astronomers, to pioneer astrophysics in the years between the World Wars.

It might be regretted that French physicists, and the French university system in general, were not much interested in astrophysics, and that astronomers, whose specialty was classified as a branch of mathematics and not physics, remained so long in the enclosures of their ivory towers. Jean Perrin, visionary that he was, recognized the situation, and in 1936 established the Institute of Astrophysics and the Observatory of Haute-Provence for the purpose of developing astrophysics outside of a university system that he judged to be backward. The change of spirit took

¹⁷ Mouchez, E.: *Rapport annuel sur l'état de l'Observatoire de Paris*, pp. 11–12. Gauthier-Villars, Paris (1879).

time, however, and was only realized in 1950. By then it was acknowledged that only advantages could come from the melding of fundamental astronomy and astrophysics, and that each came mutually to the aid of the other. At the present time, the distinction of the Institute of Astrophysics and the astronomical observatories is more theoretical than real. The comparison of meteorological and geophysical phenomenon on the Earth with those of other planets and satellites, and of the Sun with the other stars, has also opened up extremely productive fields of investigation. Nowadays astronomy, geophysics, and to some extent meteorology are grouped together in the CNRS in the National Institute for sciences of the Universe (INSU), and some observatories, such as those of Midi-Pyrénées and Grenoble, encompass these different branches. In this way they mark a return, though informed by an entirely different spirit, to the organization of Le Verrier.

Erratum to:

Le Verrier—Magnificent and Detestable Astronomer

James Lequeux

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Appendix: Life and Works of Le Verrier in His Time

Appendix A (continued)	
Life	Scientific and political events
1811, 12 March. Birth of Urbain-Jean-Joseph Le Verrier in Saint-Lô	
1831. Enters École polytechnique	1821. Publication by Bouvard of his <i>Tables d'Uranus</i> , where he notices unexplained anomalies in the motions of the planet
1833. Leaves École polytechnique and enters the tobacco administration	1830. Arago is elected <i>secrétaire perpétuel</i> of Academy of sciences
1836. Le Verrier resigns from administration	1834. Arago is nominated <i>directeur des observations</i> of the Paris Observatory
1837. <i>Répétiteur</i> at École polytechnique. Marriage with Lucile-Marie-Clotilde Choquet	1835. First paper, on the chemistry of phosphorus
1846. Elected to Academy of sciences. Professor at the University, adjunct member of Bureau des longitudes	1839. First papers on the stability of the Solar system
1849. Deputy of La Manche at the legislative Assembly. Professor at École polytechnique	1843. Works on the orbit of Mercury
	1844. Memoirs on the comets
	Works on the anomalous movement of Uranus, leading to the discovery of Neptune on 23 September
	1848. Revolution, founding of the 2 ^d Republic
	1851. <i>Coup d'état</i> of Louis-Napoléon Bonaparte The electric telegraph is open to the public

1852. Senator of the Empire. President of the General council of La Manche		Louis-Napoléon Bonaparte becomes emperor as Napoléon III
1854. Director of the Paris Observatory	1853. Works on minor planets, theory of the motion of the Earth (completed in 1858)	Founding of the French meteorological society
	Reorganisation of the Observatory	Death of François Arago
	Measurement of the longitude difference between Paris and Greenwich with the electric telegraph	
	1856. Measurement of the longitude difference between Paris and Bourges	
	Creation of the national meteorological network	
	1857. First meteorological forecast in France.	
	Creation of the international meteorological network	
	1859. Le Verrier discovers the anomalous advance of the perihelion of Mercury, which would later give the first verification of General Relativity	
	1861. Theory of the motions of Venus and Mars	1860, 18 July. Total solar eclipse in Spain, attended by Le Verrier and Foucault
		1862. Foucault and Eichen's build a 80 cm reflector with a silvered glass mirror for the Marseille Observatory, which becomes in 1863 a branch of the Paris Observatory.
	1863. First synoptic meteorologic maps of Europe	Foucault makes the first accurate measurement of the velocity of light
	1864. Founding of the <i>Association scientifique</i>	
		1867. Discovery by Wolf and Rayet of the emission-line stars which bear their names
		(continued)

Appendix A (continued)	
Life	
Works and scientific career	
1868. Beginning of problems with the authorities	Scientific and political events
	18 August. Total solar eclipse in Siam, attended by Stephan, Rayet and Tisserand
	1869. Founding of the meteorological observatory of Montsouris, which becomes in 1872 a branch of the Paris Observatory
1870. Dismissal of Le Verrier	Delaunay replaces Le Verrier
	War with Prussia
	1871. Defeat of France, and founding of the 3 ^e Republic
	1872. Death of Delaunay
1873. Second nomination as director of the Observatory	Reorganisation of the meteorological service. The Montsouris meteorological observatory is separated from the Paris Observatory
	1876. Theory of the movements of Jupiter, Saturn, Uranus and Neptune, and tables for Jupiter and Saturn
1877, 23 September. Death of Le Verrier	Tables of Uranus and Neptune
	The mirror of the 120 cm reflector is found to be bad
1889. Inauguration of the statue of Le Verrier at the Observatory	1878. The meteorological service is separated from the Observatory and becomes the <i>Bureau central météorologique</i>
1894. Exhibition <i>Le Verrier et son temps</i> at the Paris Observatory	1911. For the centenary of Le Verrier's birth, the Academy of sciences publishes many documents and an almost complete list of his publications
2011. Exhibition <i>Le Verrier, les coulisses de la découverte de Neptune</i> at the Paris Observatory (also virtual exhibition on http://expositions.obspm.fr)	

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