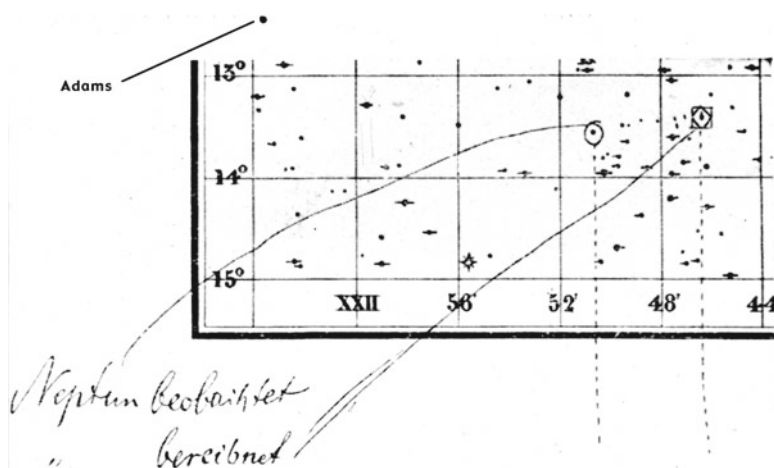


Chapter 2

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.

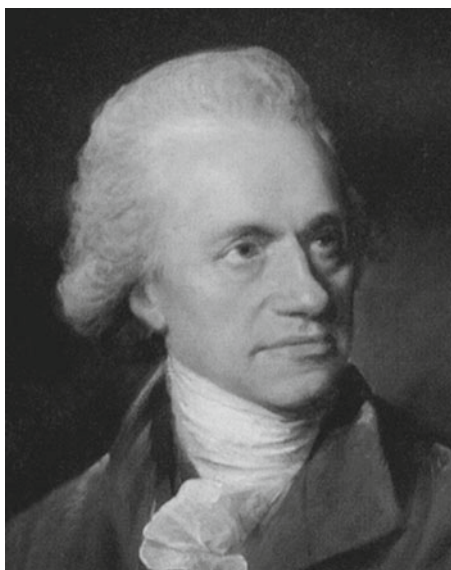


Fig. 2.1 William Herschel
(1738–1822)

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

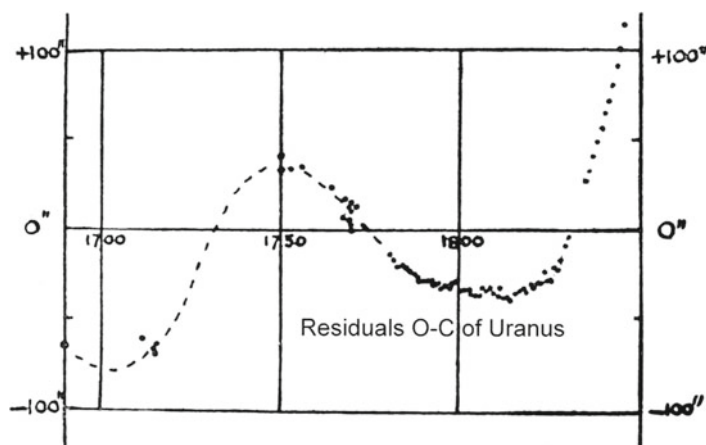


Fig. 2.5 An example of the discrepancies between the calculated longitudes of Uranus C and the observed ones O from 1690 to 1845 (From Le Verrier's paper in *Connaissance des temps* for 1849, additions, pp. 3–254, table pp. 129–136; calculations and drawing by André Danjon (1946).). Here, the calculated 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.

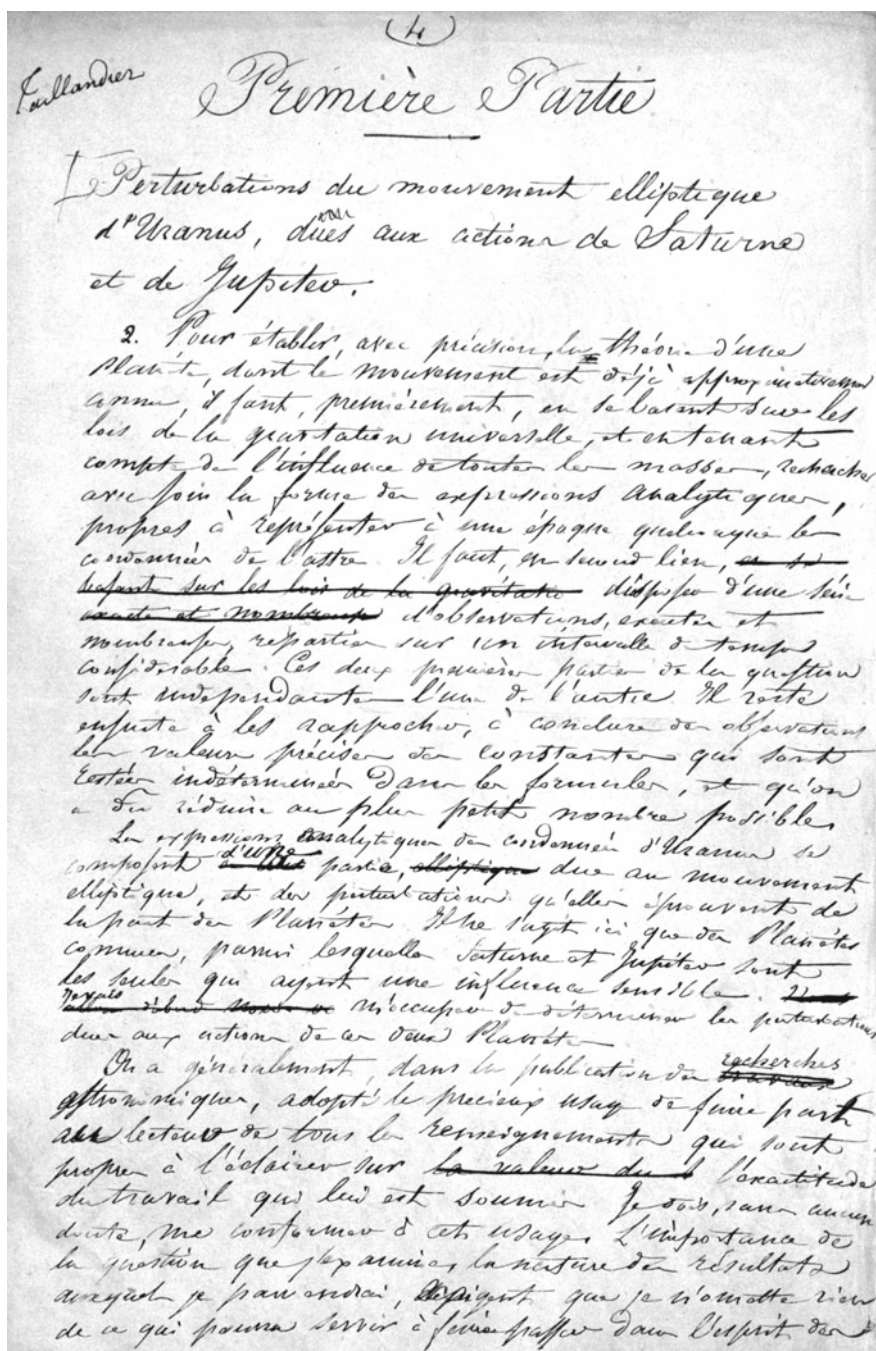


Fig. 2.6 Researches on the movements of Uranus (1846) (Autograph manuscript of Le Verrier [BOP, Ms 1063(27), 1st part, p. 5])

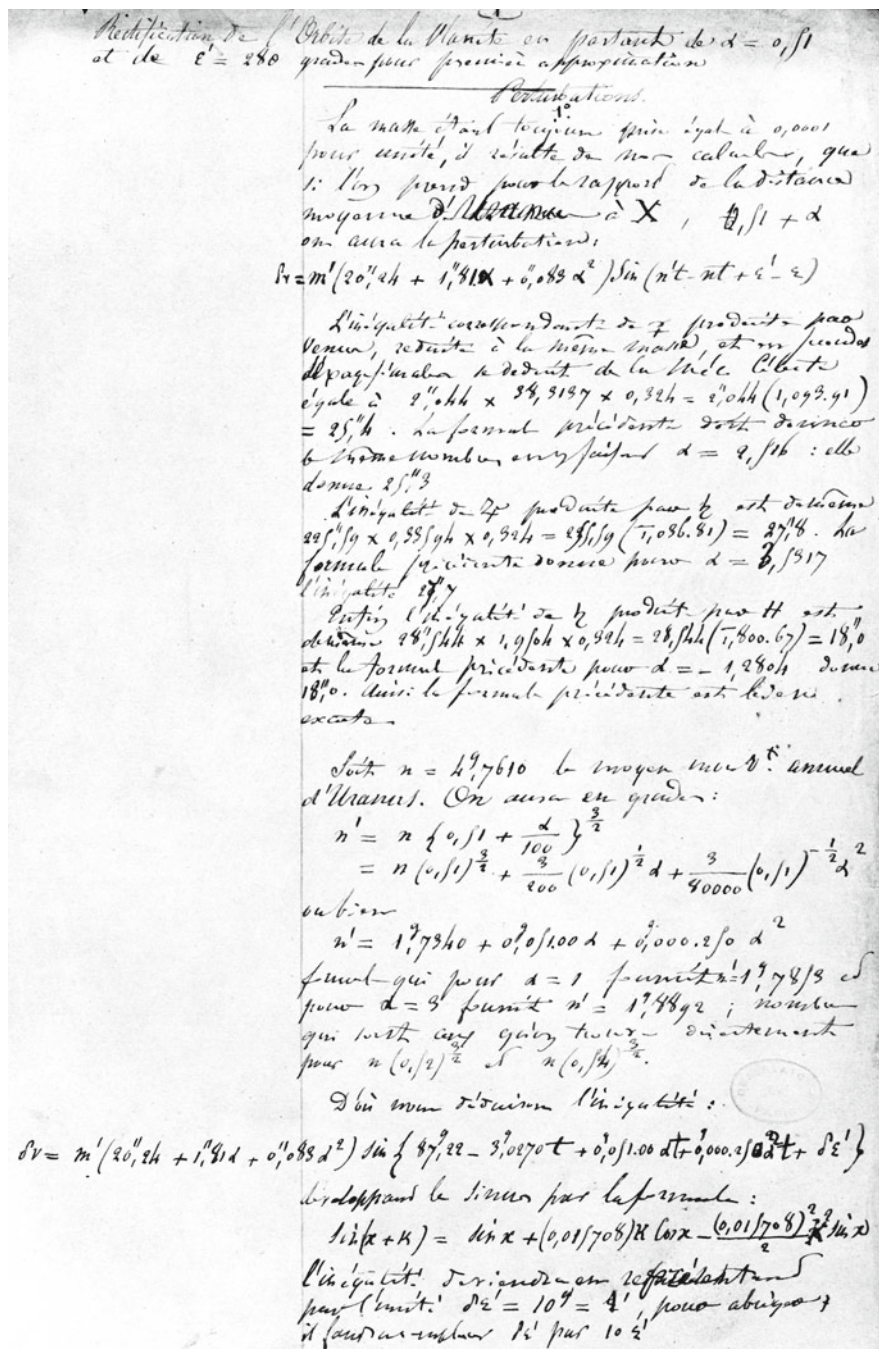


Fig. 2.7 Search for the perturbing body, 2^d approximation (1846) (Autograph manuscript of Le Verrier [BOP, Ms 1063(27), 5th part, p. 2])

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° 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 9½ 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 $9\frac{1}{2}$ 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.

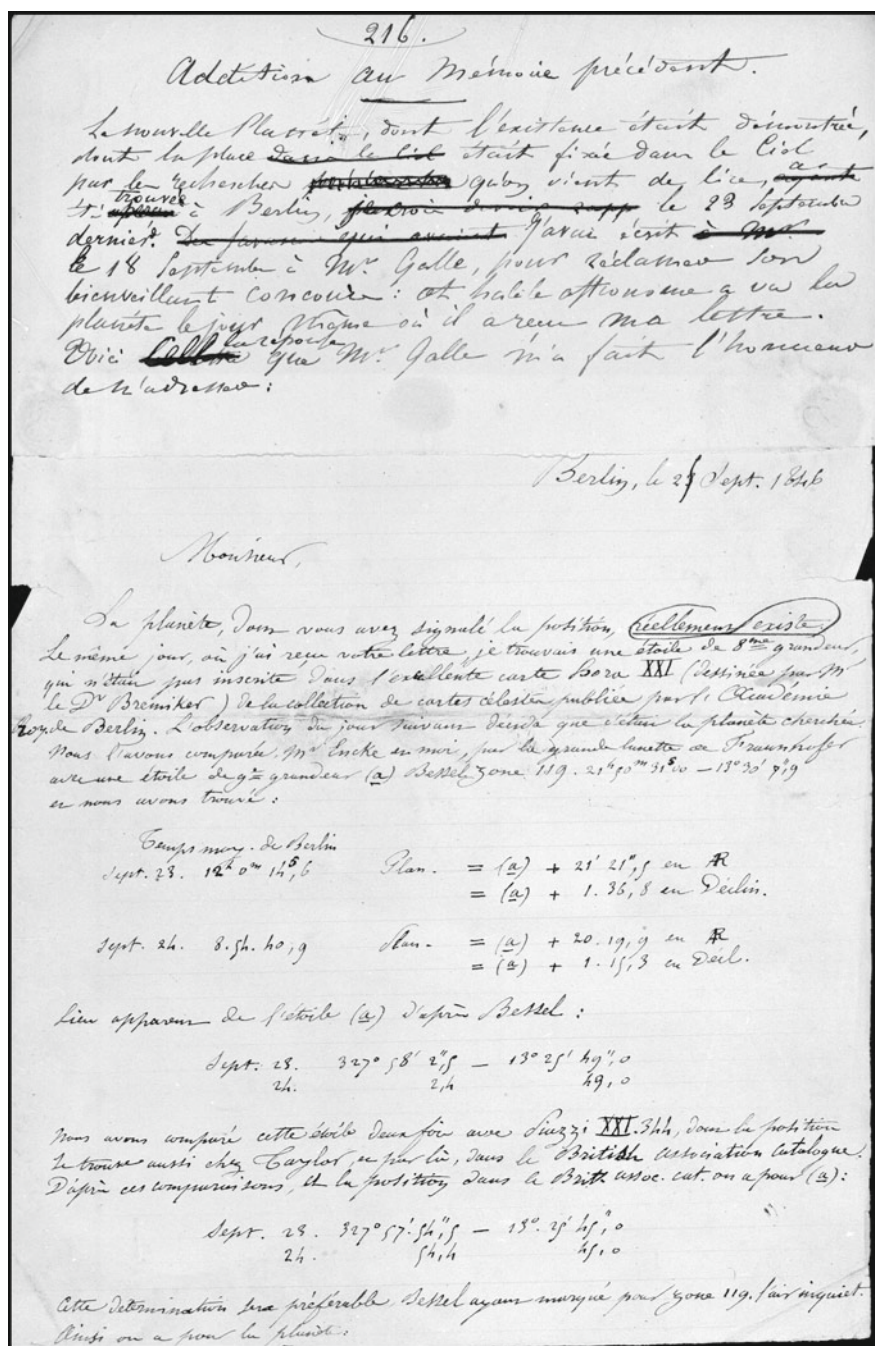


Fig. 2.9 (a) and (b) The letter of Galle to Le Verrier announcing the discovery of Neptune. This is a copy by an unknown hand, with autograph commentaries by Le Verrier

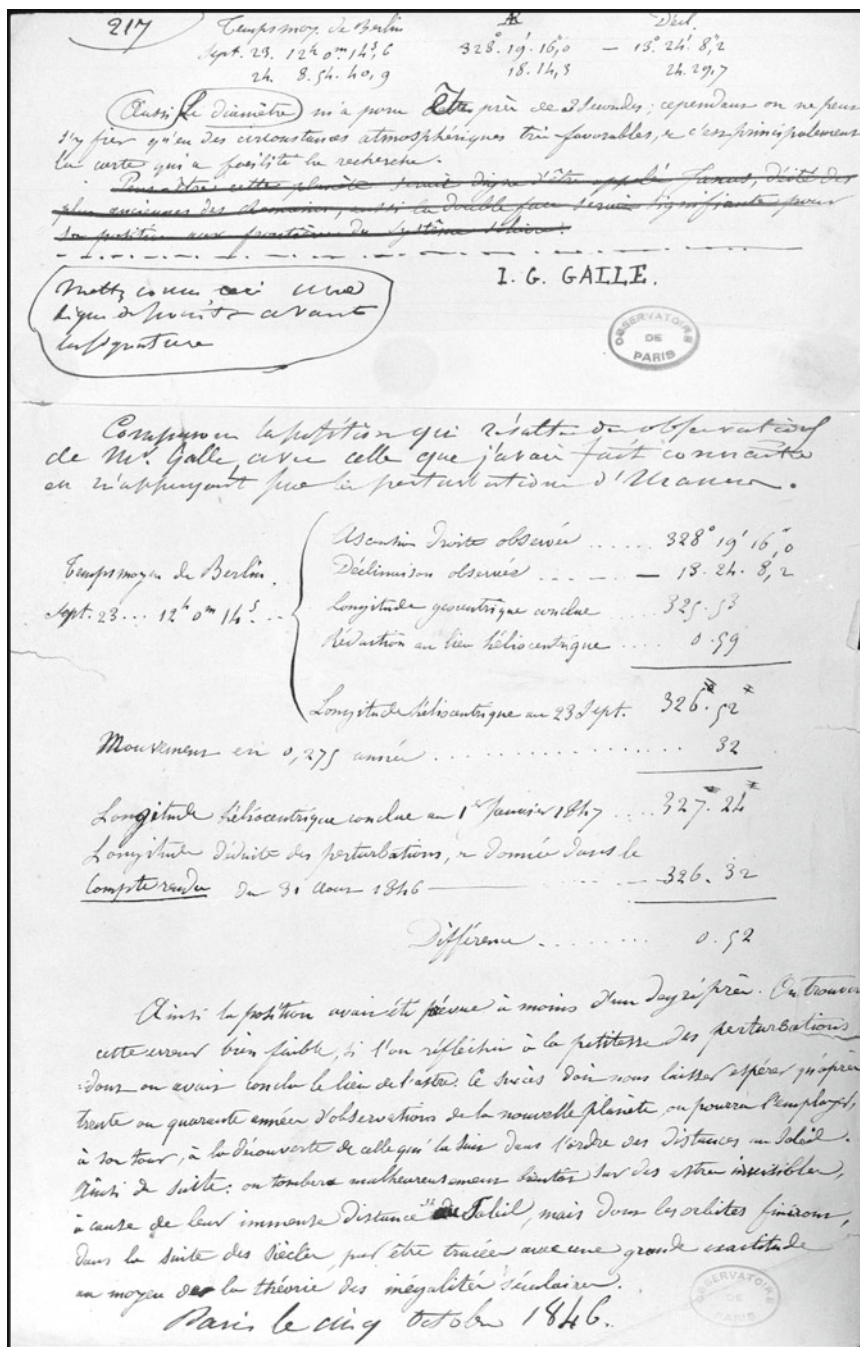


Fig. 2.9 (continued)

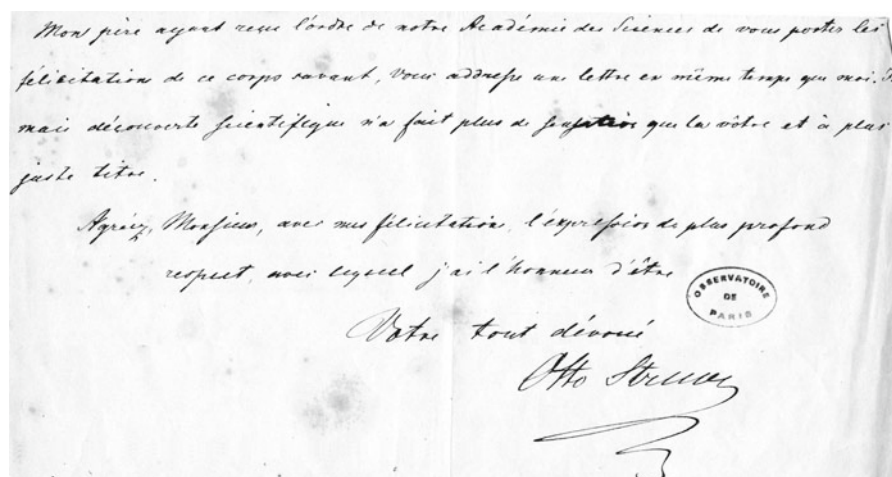


Fig. 2.10 The end of the congratulation letter from Otto Struve to Le Verrier for his discovery of Neptune

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*.

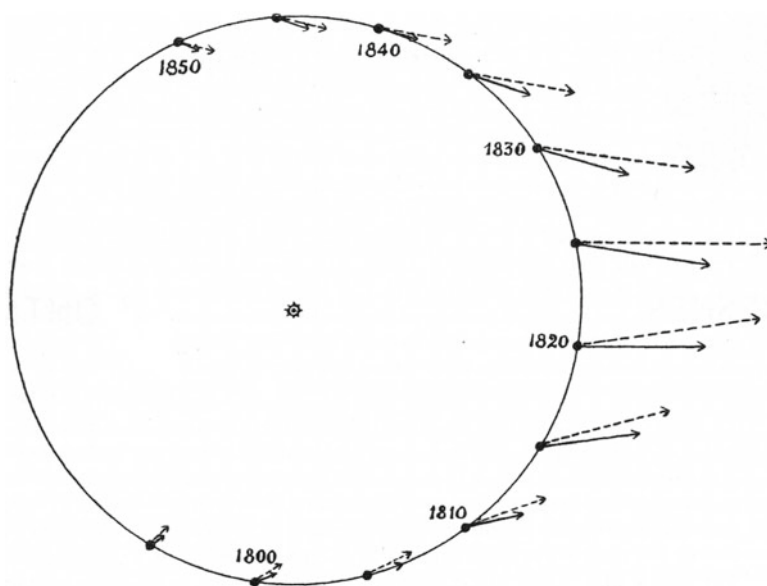


Fig. 2.14 Comparison of the perturbing force exerted on Uranus by Neptune at different epochs (*full arrows*) and by the hypothetical planet of Le Verrier (*dashed arrows*). One sees that the direction of the perturbing force is approximately correct (although the date of conjunction of Neptune with Uranus is too late by $1\frac{1}{2}$ 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.

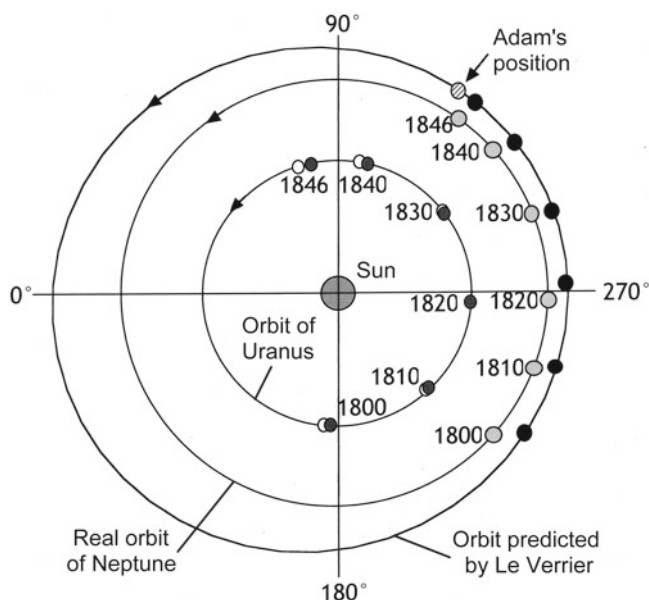


Fig. 2.15 The orbits of Uranus and Neptune. The axes mark longitudes as seen from the Sun. The position of Uranus in its orbit is indicated for different dates from 1800 to 1846 by the *dark grey circles*. The *empty circles* represent the positions that Uranus would have occupied if not perturbed by 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

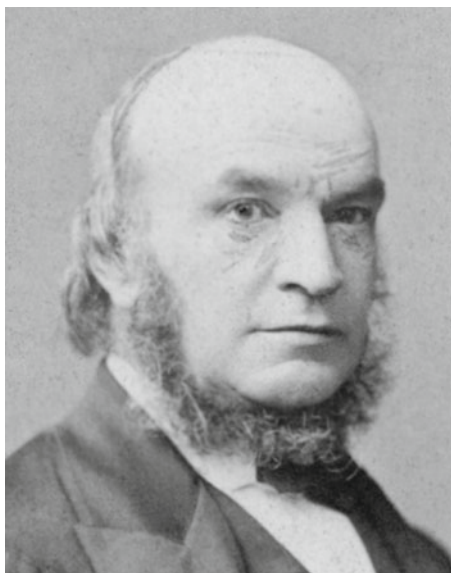
Mercury. It is remarkable that the work leading to the discovery of Neptune which served to establish his immense reputation (with the assistance of M. Arago) was nevertheless the worst of all his works: he did not dare introduce them to the *Annals of the Observatory*, where he has published all his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

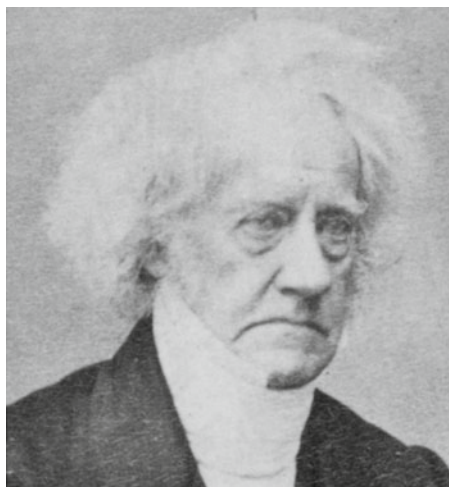
Another competitor actually did succeed: John Couch Adams (Fig. 2.16). Less than 2 weeks after Le Verrier had announced the discovery of the planet to the Academy of sciences, on 5 October 1846, Le Verrier received a letter from Airy who was just back from a trip in Germany. In this letter dated 14 October,²⁵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

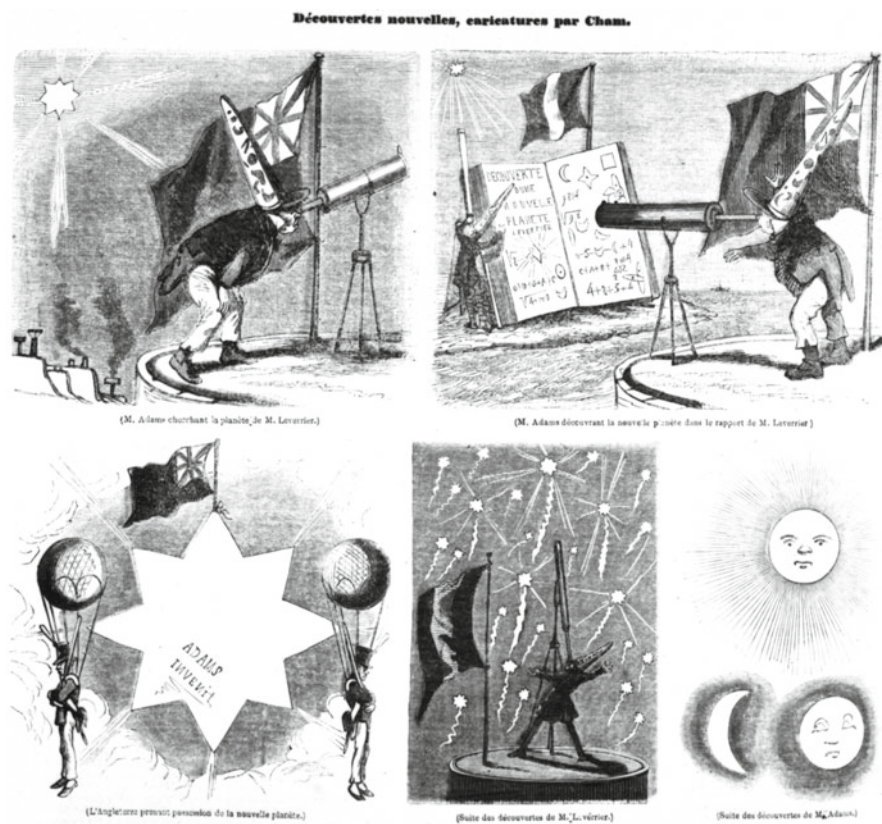


Fig. 2.18 Caricatures from Cham about the discovery of the new planet, published on 7 November 1846

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 Ferdinandea* [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.

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