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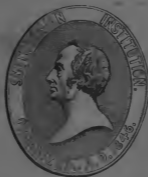


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*Francis Lieber*  
(Publication 18)

REPORT  
TO THE *from the Trustees*  
SMITHSONIAN INSTITUTION,  
ON THE  
HISTORY OF THE DISCOVERY  
OF  
NEPTUNE.

BY BENJAMIN APTHORP GOULD, JR.



WASHINGTON CITY:  
PUBLISHED BY THE SMITHSONIAN INSTITUTION.  
1850.



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UNIVERSITY OF CAMBRIDGE

## R E P O R T.

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IN a general account of the discovery of the planet Neptune, and of the remarkable circumstances which preceded and attended this discovery, it cannot at this late period be expected that any new views should be brought forward, or any material facts cited, which have escaped the notice of the eminent astronomers who have already written upon this subject. The facts are before the world. The numerical data are in all the books. The history is too strange to be forgotten by any one who has once studied it. In the following Report, therefore, it can scarcely be hoped that the facts should be more clearly arranged or more concisely presented than has already been done by Airy, Biot, and Herschel, or that any thing, bearing upon the history prior to 1847, be narrated, which cannot be found elsewhere.

The strange series of wonderful occurrences of which I am to speak is utterly unparalleled in the whole history of science; — the brilliant analysis which was the direct occasion of the search for a trans-Uranian planet, — the actual detection of an exterior planet in almost precisely the direction indicated, — the immediate and most unexpected claim to an equal share of merit in the investigation, made in behalf of a mathematician till then unknown to the scientific world, — and finally the startling discovery, that, in spite of all this, the orbit of the new planet was totally irreconcilable with those computations which had led immediately to its detection, and that, although found in the direction predicted, it was by no means in the predicted

place, nor yet moving in the predicted orbit. This series of events, together with the since developed theory of Neptune, constitute the subject of my Report.

The correctness of the prophecy, made<sup>1</sup> by a British writer within a few weeks of the discovery of the planet, that the future historian of astronomy would find it necessary to change his pen more than once while discussing this subject, will hardly now be called in question by the strongest partisans of any of those illustrious scientists, who have occupied themselves with the theories of Uranus and Neptune;—although the most important point at issue is a question very different from the one then anticipated.

There has never been a more complicated case presented for the sober judgment of the impartial historian; and the temptations to dwell upon the romance and poetry of the subject are extremely strong. I purpose, however, in the following history of the circumstances which led to, and have been connected with, the discovery of Neptune, to present the subject as simply as possible; leaving to others the philosophical considerations and poetic fancies which it suggests, and aiming only at clearness and impartiality. I shall arrange the Report as nearly as possible in the chronological order.

The planet Uranus was discovered by Sir William Herschel on the 13th of March, 1781, and, although at first supposed to be a comet, was before the end of the year recognized<sup>2</sup> as one of the primary planets of our solar system. Circular elements were first computed<sup>3</sup> during the summer of 1781, by Lexell, of St.-Petersburg, at that time in London; and others were soon after published in Russia,<sup>4</sup> France,<sup>5</sup> and Germany.<sup>6</sup> The computation

<sup>1</sup> *London Athenæum*, Nov. 21, p. 1191.

<sup>2</sup> *Mémoires de l'Acad. des Sciences, Paris*, 1779, p. 526; *Berliner Astronomisches Jahrbuch*, 1784, p. 215.

<sup>3</sup> *Acta Acad. Petrop.*, 1780, *Mem.*, p. 307.

<sup>4</sup> *Nova Acta Acad. Petrop.*, I, *Hist.*, pp. 72, 76, 81; *Acta Acad. Petrop.*, 1780, *Mem.*, p. 312.

<sup>5</sup> Lalande, *Mém. Acad. Sc. Paris*, 1779, p. 526.

<sup>6</sup> Klügel, of Helmstadt, *Berl. Ast. Jahrb.*, 1785, p. 193. Hennert, *ibid.*, p. 205.



of a planetary orbit was at that time a most laborious and troublesome process, by no means to be compared with the easy methods in use since Gauss gave<sup>1</sup> to the world the elegant and simple formulas of the "*Theoria Motus.*"<sup>10</sup> No elliptic elements were computed, therefore, until the year 1783, during which year elliptic orbits differing but slightly from each other were published by Méchain,<sup>2</sup> Laplace,<sup>3</sup> Caluso,<sup>4</sup> and Hennert;<sup>5</sup> and in the French and German astronomical Ephemerides for 1787 (published in 1784) were tables of the new planet. The name Uranus, originally proposed<sup>6</sup> by Bode, had at that time become almost universal upon the Continent, although in England the names "Herschel"<sup>7</sup> and "Georgium Sidus"<sup>8</sup> (or simply "The Georgian") were generally used until within a very few years, — the planet being still designated by the latter name in the British Nautical Almanac. The symbol adopted<sup>9</sup> with the name Uranus was that of platinum (♃), but in England and France the symbol ♃, formed from the discoverer's initial, is generally used.

In the mean time Bode, the Astronomer Royal of Prussia, had suggested<sup>10</sup> that Uranus might have been observed by astronomers before the discovery of its planetary nature, and consequently that earlier observations might be found by a proper search in the catalogues of fixed stars. This happy idea prompted him to study over the old star-catalogues, and his search was crowned with abundant success.<sup>11</sup> In August, 1781,

<sup>1</sup> March, 1809.

<sup>2</sup> *Hist. Acad. Berl.*, 1782, pp. 41, 49.

<sup>3</sup> *Mém. de Bruxelles*, T. 5, *Hist.*, p. xlix., *Mém.*, p. 43; *Journal de Paris*, May 31, 1783; *Berl. Ast. Jahrb.*, 1786, p. 247; *Conn. des Temps*, 1786, p. 3.

<sup>4</sup> *Ephem. Astron. Mediol.*, 1784, p. 199.

<sup>5</sup> *Berl. Ast. Jahrb.*, 1786, p. 223.

<sup>6</sup> *Berl. Gesellschaft Naturforschender Freunde*, March 12, 1782, III. p. 350.

<sup>7</sup> Proposed by Lalande, Dec. 22, 1781; v. *Mém. Acad. Paris*, 1779, p. 526; *Hist. Acad. Berlin*, 1782, p. 39.

<sup>8</sup> Proposed by Herschel, *Roy. Soc. Philos. Trans.*, 1783, p. 1.

<sup>9</sup> Proposed by Kochler, of Dresden; *Berl. Ast. Jahrb.*, 1785, p. 191; *Nouv. Acta Petrop.*, I. 69.

<sup>10</sup> *Berl. Ast. Jahrb.*, 1784, p. 218.

<sup>11</sup> See also Bode, *Vom neuentdeckten Planeten*, Berl., 1784.

he discovered<sup>1</sup> that a star<sup>2</sup> (No. 964) in Tobias Mayer's Catalogue, which had been observed<sup>3</sup> September 25, 1756, was not to be found in the place indicated, and that it had not been mentioned on various occasions when all the other stars of equal magnitude in the same vicinity had been observed. Uranus must have been nearly in that place at the same time, according to the orbits of Laplace and Méchain,<sup>4</sup> and the presumption became thus quite strong, that this supposed fixed star of Mayer was really the planet Uranus. In the same way Bode and Fixlmillner of Kremsmünster found,<sup>5</sup> in 1784, that a star observed<sup>6</sup> by Flamsteed, December  $\frac{4}{3}$ , 1690, and called by him 34 Tauri, was in all probability also Uranus. The same discovery seems to have been made and verified<sup>7</sup> independently in France by Lemonnier and Montaigne. Observations of the planet were thus obtained, which embraced an interval of more than an entire revolution, and from these two old observations in 1690 and 1756, and the two oppositions of 1781 and 1783, Fixlmillner computed elliptic elements,<sup>8</sup> which not only fully satisfied all the four places upon which they were based, but all the observations known. They were as follows:—

*Epoch, Jan. 1st, 1784.*

	°	′	″		days.
Mean anomaly,	297	9	25	Tropical period,	30587.37
Long. perihelion,	167	31	33	Mean distance,	19.18254
Long. asc. node,	72	50	50	Eccentricity,	0.0461183
Inclination,	0	46	20	Mean daily trop. motion,	42 <sup>o</sup> .3704

which, as the event has proved, were very near the truth.

<sup>1</sup> *Berl. Ast. Jahrb.*, 1784, p. 219; 1785, p. 189; Wurm, *Geschichte des Uranus*, p. 35.

<sup>2</sup> *Reduced Conn. des Temps*, 1778, p. 195; Fixlmillner, *Berl. Acad.*, 1783, *Hist.*, p. 15; Bessel, *Fundamenta Astronomiae*, p. 283.

<sup>3</sup> Mayer, *Opera Inedita*, ed. Lichtenberg, I. p. 72.

<sup>4</sup> *Hist. Acad. Berl.*, 1782, p. 40.

<sup>5</sup> *Hist. Acad. Berl.*, 1783, p. 15; *Berl. Astr. Jahrb.*, 1787, pp. 243, 247.

<sup>6</sup> *Hist. Coelestis Britann.*, II. p. 86, 2d ed.

<sup>7</sup> *Mém. Acad. Roy. des Sciences*, 1784, p. 353.

<sup>8</sup> *Hist. Acad. Berl.*, 1783, p. 19; *Berl. Astr. Jahrb.*, 1787, p. 249.

Wurm, of Nürtingen, computed,<sup>1</sup> at nearly the same time,<sup>2</sup> elements<sup>3</sup> very similar; and Fixmillner,<sup>4</sup> von Zach,<sup>5</sup> and others,<sup>6</sup> constructed tables. Those by the former are in the Berlin Astronomical Almanac for 1789 (published 1786). These tables, as above remarked, satisfied all the observations, and continued to represent the planet's course most satisfactorily until 1788, when a discrepancy between theory and observation became very apparent;<sup>7</sup> and Fixmillner was compelled to disregard Flamsteed's observation, and to calculate new elements and tables<sup>8</sup> from the oppositions since Herschel's discovery, and the single observation by Mayer. But at a later period, after Gerstner,<sup>9</sup> Lalande,<sup>10</sup> Oriani,<sup>11</sup> and Duval<sup>12</sup> had determined the perturbations by Saturn and Jupiter, and Delambre had published<sup>13</sup> his tables of Uranus, the discrepancies vanished, and the same elements were made to represent perfectly all the modern observations, and the two former ones of Mayer and Flamsteed. In 1788, Lemonnier discovered<sup>14</sup> that he had also observed<sup>15</sup> Uranus as a fixed star in 1764 and 1769. These observations of Uranus, made prior to Herschel's discovery of its planetary character, are called, by way of distinction, "ancient observations." Others have since been found, so that we now have twenty in all, viz. :—

<sup>1</sup> *Geschichte des Planeten Uranus*, p. 37; *Berl. Astr. Jahrb.*, 1788, p. 193.

<sup>2</sup> Summer of 1784.

<sup>3</sup> See Reggio, *Ephem. Astr. Mediol.*, 1784, p. 199.

<sup>4</sup> *Berl. Astr. Jahrb.*, 1789, p. 113.

<sup>5</sup> *Ibid.*, 1788, p. 217.

<sup>6</sup> Caluso, *Mém. de Turin*, 1787, pp. 113, 132, 137; Dom Nouet, *Conn. des Temps*, 1787, p. 176; Robison, *Trans. R. Soc. Edinb.*, 1788, Vol. I. p. 305.

<sup>7</sup> Wurm, *Gesch. des Uranus*, p. 38.

<sup>8</sup> *Berl. Astr. Jahrb.*, 1792, p. 159.

<sup>9</sup> *Ibid.*, 1792, pp. 214, 219.

<sup>10</sup> *Mém. de l'Acad. Sc. Paris*, 1787, p. 182.

<sup>11</sup> *Ephem. Astr. Mediol.*, 1790, 1791.

<sup>12</sup> *Mém. Acad. Berl.*, 1789; *Berl. Astr. Jahrb.*, 1793, 115.

<sup>13</sup> Computed 1789, crowned 1790. Wurm, *Geschichte des Uranus*, p. 89; Lalande, *Astronomie*, ed. 3<sup>me</sup>, Vol. I.

<sup>14</sup> See Lalande, *Acad. Sc. Paris*, 1789, p. 204; von Zach, *Conn. Soc. Reg. Gotting.*, 1789, p. 91.

<sup>15</sup> *Conn. des Temps*, 1821, p. 339.

- One<sup>1</sup> in 1690, by Flamsteed, Dec. 7<sup>h</sup>.  
 One<sup>2</sup> " 1712, " Flamsteed,  $\frac{\text{Apr. 9}}{\text{Mar. 22}}$ .  
 Four<sup>3</sup> " 1715, " Flamsteed,  $\frac{\text{March 4, 5, 10}}{\text{Feb. 21, 22, 27}}$ , Apr. 7<sup>h</sup>.  
 Two<sup>4</sup> " 1750, " Lemonnier, Oct. 14, Dec. 3.  
 One<sup>5</sup> " 1753, " Bradley, Dec. 3.  
 One<sup>6</sup> " 1756, " Mayer, Sept. 25.  
 One<sup>7</sup> " 1764, " Lemonnier, Jan. 15.  
 Two<sup>8</sup> " 1768, " Lemonnier, Dec. 27, 30.  
 Six<sup>9</sup> " 1769, " Lemonnier, Jan. 15, 16, 20, 21, 22, 23.  
 One<sup>10</sup> " 1771, " Lemonnier, Dec. 18.

Mr. Le Verrier has, however, found reason<sup>11</sup> to suspect the accuracy of Flamsteed's observation of  $\frac{\text{March 5}}{\text{Feb. 22}}$ , 1715, so that there are really but nineteen available ones.

The best tables of Uranus which existed before the masterly and accurate researches<sup>12</sup> of Le Verrier, in 1845 and 1846, were those<sup>13</sup> computed by Bouvard in 1821. Bouvard was acquainted with all the ancient observations which we know, excepting three by Flamsteed in 1715. In the introduction to his tables, he announced<sup>14</sup> that he had been utterly unable to find any elliptic orbit, which, combined with the perturbations by Jupiter and Saturn, would represent both the ancient and the modern observations. The best tables which he could obtain by the

<sup>1</sup> *Hist. Cæl. Brit.*, II. 86, 2d ed.; *Hist. Acad. Berl.*, 1783, p. 16.

<sup>2</sup> *Hist. Cæl. Brit.*, II. p. 537.

<sup>3</sup> *Ibid.*, pp. 549, 551; *Conn. des Temps*, 1820, pp. 409, 410.

<sup>4</sup> *Conn. des Temps*, 1821, p. 339.

<sup>5</sup> Bradley, *Astron. Obs.*, I. p. 155; Bessel, *Fund. Astr.*, p. 283; *Greenwich Plan. Reduct.*, I. p. 300.

<sup>6</sup> *Hist. Acad. Berl.*, 1783, p. 8; Bessel, *Fund. Astron.*, p. 284; *Conn. des Temps*, 1778, p. 195.

<sup>7</sup> Bouvard, *Conn. des Temps*, 1821, pp. 341, 342.

<sup>8</sup> *Ibid.*, pp. 341 - 343.

<sup>9</sup> *Ibid.*, pp. 341, 343 - 347.

<sup>10</sup> *Ibid.*, pp. 341, 347.

<sup>11</sup> *App. Conn. des Temps*, 1849, p. 125.

<sup>12</sup> *Comptes Rendus de l'Acad. des Sc.*, XXI. p. 1060; XXII. p. 907.

<sup>13</sup> *Tables Astronomiques*, Paris, 1821.

<sup>14</sup> p. ii.

use of both represented neither of them, in any way at all satisfactory. On the other hand, by using modern observations only, he was enabled to find elements which, although they gave errors amounting sometimes to 74" for the ancient observations, still satisfied all the modern ones comparatively well, — never differing more than 10" from theory, and generally much less.

"Such," said he,<sup>1</sup> "is the alternative which the formation of tables of the planet Uranus presents; — if we combine the ancient observations with the modern ones, the first will be passably represented, while the second will not be represented with the precision which they require; — but if we reject the former, and retain the latter only, the resultant tables will have all desirable precision for the modern observations, but will be incapable of properly satisfying the ancient ones. We must choose between two alternatives. I have thought it proper to abide by the second, as being that which combines the most probabilities in favor of the truth, and I leave it to the future to make known whether the difficulty of reconciling the two systems result from the inaccuracy of the ancient observations, or whether it depend upon some extraneous and unknown influence, which has acted on the planet."

He therefore summarily rejected the former observations, and founded his tables upon the latter alone, adducing arguments against the accuracy of the ancient observations, and forgetting how well they harmonized with one another, and had harmonized with the elements obtained soon after the discovery of the planet.

But a very few years after the publication of Bouvard's tables, important differences between theory and observation became<sup>2</sup> again manifest, and attracted the attention of astronomers.

Airy alluded,<sup>3</sup> in 1832, to these discrepancies, in his Report

<sup>1</sup> p. xiv.

<sup>2</sup> See *Ast. Nachr.*, IV. p. 56; VI. pp. 195, 209; *Königsberg Observ.*, 1826; *Cambridge (Eng.) Observ.*, 1826, 1830.

<sup>3</sup> *Reports of British Association*, I. p. 154.

to the British Association on the Progress of Astronomy, and mentioned that the tables, constructed only eleven years previously, were in error nearly half a minute of arc.

It is an easy thing to censure Bouvard for the readiness with which he abandoned the ancient observations, — now that we know that the discrepancies were caused by the action of an exterior planet, and that the maximum of error in the ancient observations amounted<sup>1</sup> only to nine seconds. But the illustrious Bessel, who, had his priceless life been spared a very little longer, would have seen his suspicions most fully confirmed, spoke<sup>2</sup> as follows, in a public lecture,<sup>3</sup> in the beginning of the year 1840, six years and a half before the discovery of Neptune: —

“In my opinion, Bouvard made much too light of the matter; inasmuch as, after he had found himself unable to reconcile the theory both with the ancient observations and the forty years’ series of modern ones, he contented himself with the remark, that the former were not so accurate as the latter. I have myself subjected them to a more careful investigation, and new calculation, and have thereby attained the full conviction, that the existing differences, which, in some cases, exceed a whole minute, are by no means to be attributed to the observations.”

Bessel had already<sup>4</sup> pointed out one error in Bouvard’s tables, in the equation depending on the mean longitude of Saturn minus twice that of Uranus. This error was small in comparison with the discordances to be accounted for, — but we thus see how early he was engaged upon the investigation.

The last sentence of Bouvard’s introduction, just quoted, which was, in fact, the first published suggestion that the discordance between the theory and observations of Uranus might be due to the influence of an unknown planet, is hardly definite enough to be viewed historically in that light. But the follow-

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<sup>1</sup> *Proc. Amer. Acad.*, I. p. 333.

<sup>2</sup> Bessel, *Populäre Vorlesungen*, p. 448.

<sup>3</sup> In Königsberg, Feb. 28th, 1840.

<sup>4</sup> *Astron. Nachrichten*, No. 48, II. p. 441.

ing extract from a letter,<sup>1</sup> written November 17, 1834, by the Rev. Dr. T. J. Hussey, of Hayes, to Prof. Airy, — now English Astronomer Royal and then Director of the Observatory of Cambridge, England, — although first published<sup>2</sup> since the discovery of Neptune, is the earliest written allusion to the subject with which I am acquainted.

“The apparently inexplicable discrepancies between the ancient and modern observations suggested to me the possibility of some disturbing body beyond Uranus, not taken into account, because unknown. . . . Subsequently, in conversation with Bouvard, I inquired if the above might not be the case: his answer was, that, as might have been expected, it had occurred to him, and some correspondence had taken place between Hansen and himself respecting it. Hansen’s opinion was, that one disturbing body would not satisfy the phenomena; but that he conjectured there were two planets beyond Uranus. Upon my speaking of obtaining the places empirically, and then sweeping closely for the bodies, he fully acquiesced in the propriety of it, intimating that the previous calculations would be more laborious than difficult; that, if he had leisure, he would undertake them and transmit the results to me, as the basis of a very close and accurate sweep. . . . I may be wrong, but I am disposed to think, that, such is the perfection of my equatorial’s object-glass, I could distinguish, almost at once, the difference of light between a small planet and a star. . . . If the whole matter do not appear to you a chimera, which, until my conversation with Bouvard, I was afraid it might, I shall be very glad of any sort of hint respecting it.”

As regards the opinion attributed in this letter to Prof. Hansen, I have the authority \* of that eminent astronomer himself

\* Prof. Hansen informs me, in a letter received since this account was written, that Mr. von Lindenau has been engaged upon a history of the discovery of Neptune. To his inquiries upon this subject, Prof. Hansen had replied in the following words, which he has also authorized me to make public:—

“Die mich betreffende Aeusserung in Airy’s Aufsatz (*Astr. Nachr.*, No. 535,

<sup>1</sup> *Notices R. Ast. Soc.*, VII. p. 123.

<sup>2</sup> Nov. 13, 1846.

for stating, that the assertion must have been founded on some misapprehension, as he is confident of never having expressed or entertained that belief.

In the *Notices* of the Royal Astronomical Society for November 13th, 1846, is a most important publication<sup>1</sup> by the Astronomer Royal, entitled, "An Account of some Circumstances historically connected with the Discovery of the Planet exterior to Uranus." In this paper is a series of extracts from letters, before unpublished, which furnish the testimony to a great part of the history anterior to the actual discovery. From the first letter<sup>2</sup> in the series, the preceding quotation was made.

Mr. Eugene Bouvard, nephew of the author of the Tables, wrote as follows<sup>3</sup> on the 6th October, 1837, from Paris, to the Astronomer Royal of England: — "My uncle has given me the tables of Uranus to reconstruct. In consulting the comparisons

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s. 136, und *R. Ast. Soc.*, VII. p. 123) ist jedenfalls unrichtig. Ich besitze freilich keine Copien von meinen Briefen an Bouvard, aber ich finde in seinen Briefen an mich, die ich wieder durchlas, als mir Airy's Aufsatz bekannt wurde, keine Spur, dass ich gegen ihn die von Hnssey mir in den Mund gelegte Aeußerung gemacht hätte. Auch war dies unmöglich, da ich meine darauf bezüglichen Arbeiten nicht so weit fortgesetzt habe, um darüber eine bestimmte Ansicht haben zu können. Ich kann möglicher Weise geschrieben haben, dass *vielleicht* die his dahin in der Bewegung des Uranns nicht erklärten Abweichungen von der Theorie nicht von einem, sondern von mehreren auf ihn einwirkenden, unbekanntem Planeten herrührten; aber dass *Ein* störender Körper die Abweichung *nicht* erklären könne, habe ich auf keinen Fall behauptet. Airy, dem ich kurz nach dem Erscheinen seines Aufsatzes darüber schrieb, antwortete, dass er in seinem nächsten Artikel über Neptun davon Gebrauch machen wollte. Es wäre mir lieb, wenn dieses auch in dem Ihrigen geschähe. Immer ist es meine feste Ansicht gewesen, dass die Anomalien in der Bewegung des Uranus nichts anders sind, als die Wirkung eines oder mehrerer oberhalb befindlichen Planeten, und ich befand mich darüber in Opposition mit Bessel und Nicolai, welche beide dieses für unmöglich hielten. Ersterer ist indess in seinen letzten Lebensjahren von dieser Ansicht abgegangen, und hat sich ernstlich mit der Aufsuchung dieses Planeten durch Rechnung beschäftigt. Ich hatte in den zwanziger Jahren angefangen mich mit dieser Aufgabe zu beschäftigen, — gab die Arbeit aber wieder auf, und habe sie seitdem wegen anderer Untersuchungen ganz aus dem Gesicht verloren. Eine Stelle, wo LeVerrier sagt dass die Urannsbeobachtungen der letzten 20 Jahren nothwendig waren, um ein sicheres Resultat zu erlangen, lässt es mich nicht bereuen, damals meine Arbeit liegen gelassen zu haben."

<sup>1</sup> *Notices R. Astron. Soc.*, VII. p. 121.

<sup>2</sup> *Ibid.*, p. 123.

<sup>3</sup> *Ibid.*, p. 125.



which you have made between observations of this planet and the calculations in the tables, it will be seen that the differences in latitude are very large, and are continually becoming larger. Does this indicate an unknown perturbation exercised upon the motions of this star by a body situated beyond? \* I do not know, but this is at least my uncle's idea."

Prof. Airy remarked in his reply,<sup>1</sup> that the error in latitude was very small; — that it was the errors in the longitude which were increasing with so fearful rapidity. And, a few months after, he showed<sup>2</sup> that the tabular radius-vector of Uranus was much too small. This result of observations at the quadratures was one to which Prof. Airy, both at that time and uniformly since, attached great importance.

It is from this period, that the definite belief of most astronomers in the existence of a trans-Uranian planet appears to date. Numerous mathematicians subsequently conceived the purpose of entering earnestly into laborious and precise calculations, in order to decide whether the assumption of an exterior cause of disturbance were absolutely necessary, and, if so, to determine from the known perturbations their unknown cause. The Astronomer Royal most justly expresses<sup>3</sup> himself "confident, that it will be found that the discovery is a consequence of what may properly be called a movement of the age; that it has been urged by the feeling of the scientific world in general, and has been nearly perfected by the collateral, but independent, labors of various persons possessing the talents or powers best suited to the different parts of the researches."

The problem became, from this time forth, one of the most important questions of Physical Astronomy. Astronomers in various countries busied themselves with it, and spoke of it without reserve.

\* "Cela tient-il à une perturbation inconnue apportée dans les mouvements de cet astre par un corps situé au-delà?"

<sup>1</sup> *Notices R. Astron. Soc.*, VII. p. 126.

<sup>2</sup> *Notices R. Astr. Soc.*, VII. p. 122.

<sup>3</sup> *Astr. Nachr.*, No. 349, XV. p. 217.

The first decided opinion, publicly expressed, was, I believe, that of Bessel, in the lecture<sup>1</sup> delivered at Königsberg on the 28th February, 1840; from which I have already quoted. He had, at that time, already repeated the calculations of the elder Bouvard, and convinced himself that the ancient and modern observations could not be reconciled by any modification of the elements; and that the differences could not be attributed to inaccuracy of instruments, or to methods of observation.

"From all these investigations," said he,<sup>2</sup> "I have arrived at the full conviction, that we have in Uranus a case to which Laplace's assertion\* is not applicable. . . . We have here to do with discordances, whose explanation can only be found in a new physical discovery. . . . Farther attempts to explain them must be based upon the endeavor to discover an orbit and a mass for some unknown planet, of such a nature, that the resulting perturbations of Uranus may reconcile the present want of harmony in the observations. If the motions of Uranus can actually be explained in this way, the lapse of time will raise this explanation to the rank of evidence, in the same degree in which it will exhibit the influence of this new power. And this, too, must exert influences upon the motion of Saturn, which are indeed smaller, but will, nevertheless, hardly be able to escape a special investigation, and will thus afford an independent confirmation of the existence of the new planet."

In continuing his lecture, Bessel dwelt long upon the labor and difficulty of the problem. He spoke of the painful amount of preparatory labor to be performed, alluded<sup>3</sup> to the patience and care with which his young friend and pupil, Flemming, had already reduced all the observations of Uranus, and, with fatherly kindness, he encouraged him in his work.

The labors of the talented and enthusiastic Flemming were

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\* *i. e.*, that the theory of gravitation entirely explained all motions observed in our solar system.

<sup>1</sup> Bessel, *Popul. Vorles.*, p. 408.

<sup>3</sup> *Ibid.*, p. 452.

<sup>2</sup> *Ibid.*, pp. 449, 450.

interrupted by his early death. Bessel himself was upon the point of commencing the investigation which he had proposed, when he sank beneath the weight of his labors; and "the prize toward which he had stretched forth his hand was wrested from him"<sup>1</sup> by the lingering and painful disease which closed the long series of his brilliant investigations, and removed him from the world.

Sir John Herschel stated publicly,<sup>2</sup> soon after the discovery of Neptune, that Bessel, while in England, in 1842, conversed with him concerning the probability that the motions of Uranus were disturbed by an exterior planet, and later in the same year, in a letter from Königsberg, intimated that he was engaged in researches relative to these perturbations. Professor Schumacher<sup>3</sup> has promised to publish Flemming's reduction of the Uranus-observations in the *Astronomische Nachrichten*.

Meantime Mr. Eugene Bouvard, in Paris, had been engaged upon the same labors. In May, 1844, he wrote again<sup>4</sup> to Mr. Airy, that he had not only reduced the observations of Uranus anew, but that he had reconstructed tables, and succeeded in satisfying the modern observations so nearly that the extreme of discordance amounted only to fifteen seconds, while the tables of his uncle gave places nearly two minutes out of the way.

But it must be observed, on the other hand, that the ancient observations were not represented with any degree of accuracy; and, as regards the modern ones, fifteen seconds, although but a small angle, — a single second of time, — is nevertheless five times the amount of error which we are warranted in assuming.<sup>5</sup>

Thus not only the investigations of Bessel previous to 1840, but the entirely independent ones of Bouvard, between 1837 and 1844, showed the impossibility of explaining all the observed motions of Uranus by known causes. The Royal Society of

<sup>1</sup> Schumacher, *Introduction to Bessel's Popular Lectures*, p. iv.

<sup>2</sup> *Letter to London Athenæum*, Oct. 3, 1846.

<sup>3</sup> *Introduction to Bessel's Lectures*, p. iv.

<sup>4</sup> *Airy's Account*, letter No. 5, *Notices Astr. Soc.*, VII. p. 127.

<sup>5</sup> *Proceedings Amer. Acad.*, I. p. 333.

Sciences of Göttingen had already proposed, as a prize-question,<sup>1</sup> in 1842, the full discussion of the theory of the motions of Uranus, with special reference to the cause of the large and increasing errors of Bouvard's tables.

The death of Flemming and the feebleness and illness of Bessel put a stop to their researches in Germany; and from this period we know of but two mathematicians, Messrs. Adams, of Cambridge University, in England, and Le Verrier, in Paris, who busied themselves with the problem. Mr. Adams states<sup>2</sup> that his attention was first called to the subject by reading, in the summer of 1841, Mr. Airy's Report on the Progress of Astronomy. Mr. Le Verrier undertook<sup>3</sup> the investigation at the request of Arago, the Director of the Observatory of Paris. That eminent scientist had doubtless been impressed by the computations of Bouvard with the necessity of the research; and the Astronomer Royal expresses<sup>4</sup> his conviction, that the knowledge of Bouvard's labors "tended greatly to impress upon astronomers, both French and English, the absolute necessity of seeking some external cause of disturbance."

Three months before the date of the last-cited letter of Bouvard, Mr. Adams had informed Mr. Airy, through Prof. Challis, that he was engaged in researches of a precisely similar nature. The letter<sup>5</sup> of Prof. Challis is dated February, 1844, and was written to obtain the reductions of the Uranus-observations which had been made at Greenwich. Airy immediately<sup>6</sup> forwarded the complete series of heliocentric errors of the Uranus-tables, — both in longitude and latitude, — for all the Greenwich observations from 1754 to 1830.

During the summer of 1845, Mr. Arago represented, as above stated, to Mr. Le Verrier, then colleague of the venerable

<sup>1</sup> *Abhandl. d. Königl. Gesellschaft*, II. p. x.

<sup>2</sup> Adams's "Explanation," p. 3, *Naut. Alm.*, 1851.

<sup>3</sup> Le Verrier's "Recherches," p. 4, *Conn. des Temps*, 1849.

<sup>4</sup> "Account," *Ast. Soc. Notices*, VII. p. 127.

<sup>5</sup> "Account," letter No. 6, p. 128.

<sup>6</sup> *Ibid.*, letter No. 7, p. 128.

and illustrious Biot, the transcendent importance of the question, and urgently pressed him to enter into a full discussion of the subject. Le Verrier did this, and presented his first paper<sup>1</sup> to the Academy of Sciences on the 10th November, 1845.

A fortnight previous, Mr. Adams had communicated<sup>2</sup> to Mr. Airy, that, according to his calculations, the observed inequalities of Uranus might be explained by supposing an exterior planet with a mass and orbit as follows:—

Mean distance (assumed nearly in accordance with Bode's law), . . . . .	38.4
Mean sidereal motion in 365.25 days, . . . . .	1° 30'.9
Mean longitude, October 1st, 1845, . . . . .	323° 34'
Longitude of the perihelion, . . . . .	315° 55'
Eccentricity, . . . . .	0.1610
Mass, that of the sun being unity, . . . . .	0.0001656

With these elements Mr. Adams gave a table of remaining errors of longitude for every three years of the modern observations, none of which exceeded two and a quarter seconds. The ancient observations were satisfied within 12", excepting Flamsteed's in 1690, which differed by 44".4, not having been used in the equations of condition. It was, however, probable, he thought, that a small change in the mean motion of the hypothetical planet would reduce this discordance materially.

Prof. Airy, in replying, desired<sup>3</sup> to know whether this assumed perturbation would explain the error of the radius-vector, as he considered that this trial would be an *experimentum crucis*. For some reason, no answer was received. This is deeply to be regretted, as, had an affirmative answer been given, Airy would undoubtedly<sup>4</sup> have procured the publication of Mr. Adams's results, and the painful discussion concerning priority would have been spared. As the case now stands, the question of priority depends upon another question,—whether Adams's communication of his results to the Astronomer Royal can be considered as a publication of them.

<sup>1</sup> *Comptes Rendus*, XXI. p. 1050.

<sup>2</sup> "Account," letter No. 12, p. 130.

<sup>3</sup> *Airy's Account*, letter No. 11, p. 129.

<sup>4</sup> *Ibid.*, p. 131.

The next letter of Mr. Adams, which has been printed,<sup>1</sup> is dated September 2, 1846. Le Verrier had, in the mean time, not only published<sup>2</sup> the memoir already alluded to, in which the perturbations of Uranus by Jupiter and Saturn are fully developed, calculated, and discussed, — but had communicated to the Academy two other most important papers. In one,<sup>3</sup> presented on June 1st, 1846, he proved<sup>4</sup> that the motions of Uranus could not be accounted for, except by introducing the perturbative influence of an unknown planet, for which he assigned an approximate place. In the other,<sup>5</sup> he found an orbit, a mass, and a more precise position for the disturbing planet. This was presented on the 31st August.

Mr. Airy mentions,<sup>6</sup> that on the 29th June, at a meeting of the Board of Visitors of the Greenwich Observatory, at which Sir John Herschel and Prof. Challis were present, he spoke of the extreme probability that another planet would be discovered within a short time; and assigned, as a reason for this belief, the coincidence between Mr. Le Verrier's results and those of Mr. Adams. He had addressed<sup>7</sup> a letter to Mr. Le Verrier, similar to that sent a year previously to Mr. Adams, to make inquiries about the radius-vector. Mr. Le Verrier answered<sup>8</sup> under date of June 28, stating that the errors of radius-vector must be accounted for, inasmuch as the equations of condition depended on observations at the quadratures as well as at the oppositions. Concerning the correctness of this inference, however, there appears room for discussion. Le Verrier called Airy's attention to the fact, that the position in quadrature in 1844, deduced by means of his formulas from the two oppositions which comprised it, only differed  $0''.6$  from the observed position, which proved, he said, that the error of radius-vector had entirely disappeared. This he considered as one of the strongest arguments in favor of the truth of his results. For,

<sup>1</sup> "Account," letter No. 20, p. 137.

<sup>2</sup> *Comptes Rendus*, XXI. p. 1060.

<sup>3</sup> *Ibid.*, XXII. p. 907.

<sup>4</sup> *Ibid.*, p. 911.

<sup>5</sup> *Comptes Rendus*, XXIII. p. 428.

<sup>6</sup> "Account," p. 133.

<sup>7</sup> Letter No. 13, p. 132.

<sup>8</sup> Letter No. 14, p. 133.

while in his first researches he only made use of oppositions, the quadratures were represented with all precision. "Le rayon vecteur," said<sup>1</sup> he, "s'est trouvé rectifié de lui-même sans que l'on l'eut pris en considération d'une manière directe. Excusez-moi, Monsieur, d'insister sur ce point. C'est une suite du desir que j'ai d'obtenir votre suffrage."

At Airy's suggestion,<sup>2</sup> Professor Challis had already commenced<sup>3</sup> a search for the planet on the 29th July, using a modification of a plan which Mr. Airy had drawn up. The date of the letter suggesting this search was July 9; that of the general plan was July 13. Le Verrier's memoir,<sup>4</sup> which assigned  $325^{\circ}$  as the probable longitude of the planet, was presented to the French Institute, as we have seen, on June 1st. Still, it does not appear that any search whatever had been instituted in the intervening time in any part of Europe or America;—indeed, there is no account of any search having been made excepting by Professor Challis, before the night of September 23.

It must, indeed, be confessed that astronomers in general did not seem to consider the theoretical results, published by Mr. Le Verrier, as necessarily indicating the *physical* existence and true position of such an exterior planet. Professor Challis alone—the only astronomer who entered into a systematic search for the planet, and the only one excepting Dr. Galle, the assistant at the Royal Observatory of Berlin, whom we know to have even looked for it—has assigned,<sup>5</sup> as a reason which deterred him from an earlier search, that it was "so novel a thing to undertake observations in reliance upon merely theoretical deductions; and that, while much labor was certain, success appeared very doubtful." But is there any practical astronomer, in a latitude permitting the search, who was not deterred by the same considerations. Even in Paris, that focus of science, with its many and powerful telescopes, with its numerous

<sup>1</sup> "Account," letter No. 14, p. 134.

<sup>4</sup> *Comptes Rendus*, XXII. p. 917.

<sup>2</sup> *Ibid.*, pp. 135, 136.

<sup>5</sup> *Ast. Soc. Notices*, VII. p. 145.

<sup>3</sup> *Ast. Soc. Notices*, VII. p. 145.

eminent astronomers, where Mr. Le Verrier was known and his brilliant genius appreciated, — not to allude to the American observatories, furnished with some of the finest and most powerful telescopes which have left the *ateliers* of Munich, — we have no information that any attempts\* were made to test the physical accuracy of Le Verrier's results, or that the planet was even looked for on one single evening.

A strange contrast to this apathy on the part of other astronomers is furnished by the demeanor of Le Verrier himself. Having fairly arrived at his results, he looked upon them as conclusive. His computations had been an earnest work. He had employed all his analytical powers, — and employed them, too, most successfully, — to refine the methods which he used, and to narrow the field of his inquiry; all his powers of application and numerical research, to insure precision; — and his indomitable perseverance, in carrying out his computations with full vigor, permitted him to omit no possible test of their accuracy. He proved that the observations of Uranus made it necessary to assume the existence of some unknown disturbing body. For the observations which he adopted as the basis of his calculations, he had assigned, *à priori*, the limits of error allowable; and he found that all the observations could be satisfied within these predetermined limits by the assumption of an exterior planet, moving in a given orbit, and having a given mass. The correctness of his results was dependent upon no empirical assumption. He gave them, therefore, fearlessly to the world, and staked his reputation upon their accuracy. This forms by no means the least part of his claims to the respect and admiration of scientists throughout the world. Had the planet not been found in the predicted place, Le Verrier would alone have borne the mortification. Neptune was discovered in almost precisely the direction assigned, and Le Verrier receives the admiration so justly due him.

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\* It is, perhaps, due to the Naval Observatory at Washington, to make some exception in its favor. *Astr. Nachr.*, XXVI. p. 65.



The mass and orbit given in the memoir of August 31st are<sup>1</sup> as follows:—

Semixis major, . . . . .	36.1539
Sidereal period, . . . . .	217 <sup>m</sup> .387
Eccentricity, . . . . .	0.10761
Equation of the center, . . . . .	7° 44' 44"
Longitude of perihelion, January 1, 1800, . . . . .	284 5 48
Mean longitude, . . . . .	240 17 41
Precession in 47 years, . . . . .	0 39 20
Mean sidereal motion in 47 years, . . . . .	77 50 3
Mean anomaly, 1847, January 1, . . . . .	34 1 56
Mean longitude, . . . . .	318 47 4
Mass, . . . . .	$\frac{1}{4322}$

The geocentric longitude, resulting from this orbit, for the end of September, 1846, was 325°. Le Verrier, in acknowledging the receipt of a memoir, made use of the opportunity thus afforded,<sup>2</sup> to request Dr. Galle to look for the planet. The letter reached Berlin<sup>3</sup> on the 23d September, and Galle, in complying with this request, found, on the same evening, a new planet in longitude 325° 53', or within 55' of the geocentric place<sup>4</sup> assigned by Mr. Le Verrier.

The remembrance of the enthusiasm excited by this discovery, of the amazement with which the tidings were received, not only by astronomers, but by almost all classes of the community, and of the homage paid to the genius of Le Verrier, is still fresh in the memory of all. Nations vied with one another in expressions of their admiration.<sup>5</sup> Arago, to whom the right of conferring a name upon the new planet was delegated<sup>6</sup> by Le Verrier, gave<sup>7</sup> to the planet the name of that geometer, with the symbol  $\Upsilon$ , deduced from the initials of the same. This

<sup>1</sup> *Recherches*, pp. 234–236; *Comptes Rendus*, XXIII. p. 432.

<sup>2</sup> *Astr. Nachr.*, XXV. p. 51.

<sup>3</sup> *Berl. Vossische Zeitung*, Sept. 25th, 1846.

<sup>4</sup> 324° 58'.

<sup>5</sup> *Comptes Rendus*, XXIII. pp. 959–960, etc.

<sup>6</sup> *Ibid.*, p. 662.

<sup>7</sup> *Astr. Nachr.*, XXV. p. 81.

name and symbol have not, however, generally prevailed,\* as they are at variance with the received nomenclature, in accordance with which the names of Roman deities have been uniformly selected; and the name *Neptune*, which, with its appropriate symbol ( $\Psi$ ), a trident, was originally proposed<sup>1</sup> by the *Bureau des Longitudes*, and immediately adopted<sup>2</sup> by Gauss, Struve, Encke, and Airy, has become almost universal.

The first public announcement of Mr. Adams's labors was in a London newspaper,<sup>3</sup> the *Athenæum*. In this journal, under date of October 1st, 1846, Sir John Herschel, commenting on the actual detection at Berlin of the long-expected planet, spoke, as before quoted, of Bessel's conversation with him, and subsequent letter in 1842, and alluded to the fact that Mr. Adams<sup>4</sup> had been engaged in an investigation similar to that of Mr. Le Verrier, and with similar results.

On the 13th November, the Astronomer Royal presented to the Astronomical Society of London the extremely valuable and important Account which has been so often quoted in my Report, and which must ever remain an integral part of this singular history. Professor Challis presented at the same time an ac-

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\* It seems quite desirable that astronomers in different countries should be unanimous in the adoption of fixed names and symbols. For each of the two planets of which this Report speaks, two names and two symbols are in use. The great preponderance of authority seems, however, decidedly in favor of the mythological names used in this Report, with their corresponding symbols,  $\S$  and  $\Psi$ . Le Verrier, who in 1846 announced<sup>5</sup> his fixed determination to call Uranus by the name of its discoverer, according to Lalande's proposition, has now happily abandoned<sup>6</sup> this unpopular idea; and even the distinguished Arago, who bestowed the name of the French geometer upon the new planet discovered in consequence of his computations, and publicly declared<sup>7</sup> his determination never to call it by any other name than Le Verrier, has yielded to the general usage of astronomers.

<sup>1</sup> Le Verrier, letter to Galle, Oct. 1, 1846, *Ast. Nachr.*, XXV. p. 194.

<sup>2</sup> Gauss, letter to Encke, Oct. 7, 1846, *Ibid.* Struve, *Bulletin Imp. Acad. Petersbourg*, Dec. 27, 1846. Challis, *Astr. Nachr.*, XXV. p. 313.

<sup>3</sup> *London Athenæum*, Oct. 3, 1846, p. 1019.

<sup>4</sup> See also Challis's letter of Oct. 15, in *London Athenæum* of Oct. 17, 1846.

<sup>5</sup> *Recherches*, p. 3, note.

<sup>6</sup> *Comptes Rendus*, XXVII. pp. 209, 273, et al.

<sup>7</sup> *Ibid.*, XXIII. p. 662.

count<sup>1</sup> of his search since the end of July; and Mr. Adams, a brief notice<sup>2</sup> of his computations. These papers are printed together in the Monthly Notices<sup>3</sup> of that Society.

During the month of November, 1846, Le Verrier published a complete account of all his investigations, giving in detail the processes by which he had arrived at the results previously made known in the *Comptes Rendus*. This memoir may justly claim a place among the most remarkable mathematical works of the age. It is entitled *Recherches sur les Mouvements de la Planète Herschel, dite Uranus*, and is published as an *Addition* to the *Connaissance des Temps* for 1849, occupying 256 pages. Mr. Adams gave in December a similar abstract of his computations, as a supplement of 31 pages to the Nautical Almanac for 1851.

It is not easy for those who are not versed in the study of Physical Astronomy, to form any adequate idea of the difficulty of the problem which Messrs. Le Verrier and Adams proposed to themselves. The difficulties in the development of the proper methods were exceedingly great, as any one might infer from the manner in which even Airy was accustomed<sup>4</sup> to speak of the problem. An investigation must indeed be eminently difficult and complicated, which that distinguished mathematician would regard as unfeasible. Not only the orbit and mass of the suspected planet, but the elements of Uranus also, were to be regarded as unknown quantities. The limits of error of the ancient observations were also undetermined, but must yet exercise an important influence on the result.

Le Verrier's memoir consists of three parts. The first of these<sup>5</sup> contains a complete investigation of the theory of Uranus, and corresponds to the first paper of the series in the *Comptes Rendus*. It is an investigation of the highest importance, apart from its relation to the problem of a disturbing planet, and is conducted with extraordinary ability. The theory of Uranus may, as Airy has said,<sup>6</sup> be considered as placed now for the first

<sup>1</sup> *Ast. Soc. Notices*, VII. p. 145.

<sup>2</sup> *Ibid.*, p. 149.

<sup>3</sup> Vol. VII. No. 9.

<sup>4</sup> Airy's "Account," *passim*.

<sup>5</sup> *Recherches*, t. 2, p. 6.

<sup>6</sup> *Account*, p. 131.

time upon a satisfactory foundation. The methods used, though essentially those of Laplace, Lagrange, and Poisson, have been modified in many respects, and it may with safety be asserted, that almost all of the modifications are improvements.

After a brief consideration of the general formulas of perturbations, Le Verrier proceeds<sup>1</sup> to what he calls the simultaneous determination of all the inequalities. The method adopted is such, that the inequalities are all obtained at the same time, and are so mutually dependent, that an error in any one part of the calculation vitiates the entire result. If, then, after the numerical computation is completed, any one part of the result is found to be exact, it may fairly be concluded that the remainder is exact also. By this method of "simultaneous determination," Le Verrier determined<sup>2</sup> all the perturbations by Saturn, carrying them out to the very smallest sensible terms;—in many cases even to terms whose coefficients amounted only to 0".01. The planetary elements<sup>3</sup> on which his researches were based, were taken from Bouvard's tables.

The whole computation of the perturbations by Saturn having been completed, Le Verrier proceeded to compute<sup>4</sup> a part of the same quantities by the method of Lagrange, in order to test the accuracy of his previous results. The actual steps of this second and most laborious calculation are not given, but the agreement<sup>5</sup> between the amount of the inequalities as obtained by the two different processes is wonderfully close, and reflects no small honor on the author's powers of numerical computation, and on the precision with which all the small terms were taken into account. The agreement of the parts thus doubly computed is a test of the accuracy of the whole work.

The perturbations by Jupiter, proportional to the first power of the mass, are determined<sup>6</sup> according to the ordinary methods; and, although not doubly computed, as in the case of Saturn, are carried out with great rigor.

<sup>1</sup> *Recherches*, § 5, p. 9.

<sup>2</sup> *Ibid.*, § 15, p. 16.

<sup>3</sup> *Ibid.*, pp. 16, 52.

<sup>4</sup> *Ibid.*, § 23, p. 52.

<sup>5</sup> *Ibid.*, §§ 26–28, pp. 37–41, etc.

<sup>6</sup> *Ibid.*, § 34, p. 51.

In the investigation<sup>1</sup> of the changes proportional to the square of the disturbing force, Le Verrier has neatly availed himself of the method used<sup>2</sup> by Laplace. The parts of these, which are not strictly secular, can without hesitation be rejected, and he has calculated<sup>3</sup> the values of the coefficients for 1800 and for 2300, and determined their values for intermediate years by simple interpolation. The discussion is completed by the consideration<sup>4</sup> of the influence of the changes of Saturn's elements, produced by the attraction of Jupiter. This is investigated with much care and elegance. The action of Saturn, as reflected through Jupiter, is not of sufficient importance to require computation.

The theory of Uranus was thus made complete, and needs now only the addition of the perturbations by Neptune, to accommodate it perfectly to the present demands of science.

In the second part<sup>5</sup> of Mr. Le Verrier's "Researches," the theory of Uranus, thus remodelled, is compared with the whole series of known observations, in order to discover how large the discrepancies between the observed and computed course of the planet actually were. In entering upon this division of his subject, the author shows<sup>6</sup> that the sum of the errors in the perturbations as given by Bouvard — considering those perturbations only whose value had completely changed during the period through which our observations extend, and making full allowance for the erroneous masses of Jupiter and Saturn, which Bouvard used — amounted to twenty-nine sexagesimal seconds.

Bouvard's tables appear, indeed, throughout this part of the investigation, in a singularly unfavorable light. Le Verrier has repeated<sup>7</sup> the whole of Bouvard's computations, and subjected his tables to a most searching scrutiny. The result must have surprised astronomers.

<sup>1</sup> *Recherches*, §§ 48 - 58.

<sup>2</sup> *Mécan. Cél.*, Bowditch's Trans., III. p. 283.

<sup>3</sup> *Recherches*, p. 61.

<sup>4</sup> *Ibid.*, § 50, p. 65.

<sup>5</sup> *Ibid.*, p. 89.

<sup>6</sup> *Ibid.*, § 61, p. 90.

<sup>7</sup> *Ibid.*, pp. 91 - 99.

The first inaccuracy alluded<sup>1</sup> to by Le Verrier is in the eccentricity of the orbit. In order to discover the value of the eccentricity which was used in constructing the tables, we can refer directly to Mr. Bouvard's Introduction,<sup>2</sup> or can deduce it from the table<sup>3</sup> of the equation of the center. These, however, do not agree with one another, and in order to decide which of them is erroneous, we must try, if possible, some third method of finding the eccentricity assumed. We are enabled to do this by the algebraical expression of the equation of the center, which is also given<sup>4</sup> in the preface. But, strange to say, we obtain in this way still a third value differing from both the others. The example, which Le Verrier gives<sup>5</sup> to illustrate this, is the value of the equation of the center for a mean anomaly of  $90^\circ$ .

The first value deduced from the preface is . . .	$5^\circ 93' 57.52''$
The second " " " " " . . .	$5 92 71.04$
The value given by Table X. is . . .	$5 93 48.00$

Mr. Le Verrier assures us, farther, that any attempt to decide definitely, by means of the radius-vector, what was the true eccentricity on which the tables were based, would lead only to the discovery of new discordances.

In the secular motion of the mean longitude, Tables I. and II. do not agree. The error in Table II. would, according to Le Verrier, give an error of  $21''.5$  in the computed place for Flamsteed's observation of 1690. In the formation of equations of condition, Bouvard appears<sup>6</sup> to have been equally unfortunate, both as regards the analytical and the numerical parts of the work. Finally, a series of typographical errors is given,<sup>7</sup> sixteen of them being in the single table of the equation of the center, and the majority of them errors of grave importance.

Le Verrier was thus compelled to repeat the whole work, and the catalogue of errors which he has given is a sufficient indica-

<sup>1</sup> *Recherches*, p. 92.

<sup>2</sup> *Tables*, p. ii.

<sup>3</sup> Table X. p. 95.

<sup>4</sup> *Tables*, p. xv.

<sup>5</sup> *Recherches*, p. 93.

<sup>6</sup> *Ibid.*, pp. 93-95.

<sup>7</sup> *Ibid.*, pp. 96, 97.

tion of his thoroughness and high standard of precision. And when we remember, that neither Airy<sup>1</sup> had detected these errors, nor Bessel published any thing concerning them, excepting the notice of the one term already referred to, we cannot but still more admire the searching rigor of Le Verrier's investigations.

The author has next given an ephemeris of the heliocentric<sup>2</sup> and geocentric<sup>3</sup> places of Uranus for several successive days, at the epochs of the ancient observations, and of those modern ones which he had chosen as most appropriate for the comparison. For these latter he did not construct normal places, but selected a series of two hundred and sixty-two of the best observations, some taken in opposition and some in quadrature, and suitably distributed over the interval between 1781 and 1845. These, together with the ancient observations,<sup>4</sup> were then compared<sup>5</sup> with the theory. The comparisons were made in right-ascension and declination, and the differences subsequently converted into differences of longitude and latitude. Each of these differences furnishes one equation, but, on account of the smallness of the errors in latitude,<sup>6</sup> the computation was founded upon the longitudes alone; and the mean equations of condition<sup>7</sup> used were those depending upon the longitudes after grouping together all the observations made at the same period.

By means of these Le Verrier was enabled to solve the important problem,<sup>8</sup> — “*Is it possible to satisfy the whole of the preceding equations by a proper determination of the values of the unknown quantities which they contain?*” He found, that the elements, furnished even by those equations of condition derived solely from the modern observations, were entirely incapable<sup>9</sup> of representing the course of the planet since its discovery; — the discordance in the mean positions deduced from ten observations in the years 1781 and 1782 amounting<sup>10</sup> to 20.5 sexagesimi-

<sup>1</sup> “*Account*,” p. 126.

<sup>2</sup> *Recherches*, § 71, pp. 100–110.

<sup>3</sup> *Ibid.*, § 73, pp. 112–124.

<sup>4</sup> Specially reduced, pp. 124–126, § 74.

<sup>5</sup> *Recherches*, § 77, pp. 129–136.

<sup>6</sup> *Recherches*, p. 137.

<sup>7</sup> *Ibid.*, pp. 138–141.

<sup>8</sup> *Ibid.*, p. 142.

<sup>9</sup> *Ibid.*, § 79, p. 144.

<sup>10</sup> *Ibid.*, p. 143.

mal seconds. It is impossible to believe that such an error actually exists, and we thus see that the theory is inadequate.

The same result is obtained by considering the data from two other entirely different points of view. In the *first* place, the consideration<sup>1</sup> of the equations of condition formed from the observations at eight equidistant epochs, comprehending an interval of ninety-eight years, and of the relations between their second differences, shows<sup>2</sup> that the resulting values of the variation of the mean motion are totally incompatible with one another, and that some change in the then existing theory was inevitably necessary. *Secondly*, from the relation between the correction of the elements, the mean anomaly at the epoch, and the error of the heliocentric longitude, Mr. Le Verrier forms<sup>3</sup> eighteen equations of condition; and, by an extremely elegant process,<sup>4</sup> deduces the amount of discordance between theory and observation, which could, under the worst possible combination of unfavorable circumstances, result from the errors to which the observations are subject. He allows<sup>5</sup> to each of the normal places from modern observations a possible error of four seconds, and to the position deduced from Flamsteed's three observations in 1715, a possible error of fifteen seconds. But even by assuming all the errors at their maximum, and all acting to diminish the discrepancy, Le Verrier shows that only 92" out of 356" can be accounted for, leaving a discordance of 264 sexagesimal seconds still unexplained.<sup>6</sup>

In the third part<sup>7</sup> of the work, which corresponds to the second memoir<sup>8</sup> in the *Comptes Rendus*, the author proceeds to the discussion of the great problem of the cause of the anomalies which he had proved to exist. In introducing this part of the Researches by some remarks on the difficulty of the problem, Le Verrier remarks,<sup>9</sup> that the unforeseen obstacles which he

<sup>1</sup> *Recherches*, § 80.

<sup>2</sup> *Ibid.*, p. 146.

<sup>3</sup> *Ibid.*, p. 147, § 81.

<sup>4</sup> *Ibid.*, §§ 82, 83, pp. 147 - 149.

<sup>5</sup> *Ibid.*, pp. 149, 150.

<sup>6</sup> *Recherches*, p. 150.

<sup>7</sup> *Ibid.*, p. 151.

<sup>8</sup> *C. R.*, XXII. p. 907.

<sup>9</sup> *Recherches*, p. 151.



encountered would more than once have deterred him from the farther prosecution of his labor, had he not been fully impressed with a conviction of its importance. The readers of the work cannot but be struck with this remark, for the impediments encountered and the apparent contradictions of the results would indeed have dismayed any less gifted mathematician. Entering upon the immediate subject, the author considers<sup>1</sup> in a cursory manner various hypotheses which have been suggested to account for the apparently anomalous motion of Uranus, and assigns his reasons for rejecting them. Astronomers must also be impressed with the argument<sup>2</sup> which his computations had furnished him, against the hypothesis, that a comet might have produced the disturbances in question. It is, that both the series of ancient observations and that of the observations since 1820 are alike incapable of according with elements deduced from the motion of the planet for the forty years immediately following its discovery; in other words, that between 1690 and 1845 *two* perturbations had occurred. As regards the hypothesis of an unknown planet, too, he inferred that it could not be within the orbit of Uranus, as its effect on Saturn must, in that case, have been more perceptible. "It is easy to conclude," said<sup>3</sup> he, "that its perturbative action would only be exerted at the particular time when it passed in the neighborhood of Uranus, and the small difference which there would be between the periods of revolution of the two bodies would prevent this circumstance from having taken place more than once in the period which the observations of the planet include. This consequence is contradictory to our deduction from the observations. The period comprised between 1781 and 1820 *shows no trace whatever* of large perturbations; and, on the other hand, can neither be connected with the previous nor with the subsequent observations."

This last was unquestionably written after the researches were completed. The planet of Le Verrier's theory must have acted

<sup>1</sup> *Recherches*, § 86.

<sup>2</sup> *Ibid.*, p. 152.

<sup>3</sup> *Ibid.*, p. 153; *C. R.*, XXII. p. 914.

on Uranus twice during the interval comprised between the observations of 1690 and 1845. But it is now known that only one maximum of perturbation by Neptune occurred within that period, namely, in 1822. With regard to the fact that the observations between 1781 and 1820 were capable of being perfectly represented by elliptic elements, the question naturally arises, whether osculating elements might not be found, capable of representing the motions of Uranus for any period of forty years, within moderate limits of error.

In case the perturbing body be exterior to Uranus, Mr. Le Verrier showed<sup>1</sup> that it could not be at any very remote distance, such, for example, as three times the mean distance of Uranus; for, in that case, the mass to be attributed must be so large as to affect Saturn very much more than the theory of that planet allows us to assume. It is most natural, then, to commence the first rough approximation, by assuming the new distance to be about double that of Uranus, — and the more so, as this distance would correspond to the curious empirical formula<sup>2</sup> of Wurm, Titius, and Bode, which represented, though in a very rough way, the distances of the planets from the sun. Le Verrier is entitled to praise for holding himself independent of this “law,” which, as Gauss long since showed,<sup>3</sup> is not only totally devoid of that precision which characterizes nature’s laws, but fails entirely when legitimately applied to Mercury. As it is, perhaps the influence of this ill-omened formula may have been instrumental in depriving both Le Verrier and Adams of the satisfaction of arriving, by theoretical means, at a knowledge of the elements and mass of Neptune, and thus making a physical as well as a mathematical discovery.

The problem to be considered was fortunately simplified, in some degree, by the fact, that the perturbations in latitude were very small. Uranus moves in a plane but slightly inclined to the ecliptic, and the same must, therefore, in all probability, be

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<sup>1</sup> *Recherches*, p. 153.

<sup>2</sup> Bode, *Berl. Astr. Jahrb.*, 1791, p. 188. Lalande, *Bibliographie*, p. 845.

<sup>3</sup> *Monatliche Correspondenz*, VI. p. 504.

true of the disturbing body. Le Verrier, then, assuming that the unknown planet moved in the ecliptic, proceeded to investigate the following questions<sup>1</sup> : —

“ *Is it possible that the irregularities of Uranus are due to the action of a disturbing planet, situated in the ecliptic at a mean distance double that of Uranus? And if so, where is this planet situated? What is its mass? What are the elements of the orbit which it describes?* ”

There is, as he says, but one route to follow in the discussion of this question. “ It is necessary<sup>2</sup> to form expressions for the perturbations, due to the new body, in functions of its mass and of the unknown elements of the ellipse which it describes ; we must introduce these perturbations into the coördinates of Uranus, calculated by means of the unknown elements of the ellipse which this planet describes. Putting the coördinates, thus obtained, equal to the coördinates observed, we must take as unknown quantities in the resultant equations of condition, not only the elements of the ellipse described by Uranus, but also the elements of the ellipse described by the disturbing planet whose position we seek.”

Taking, then, not the planet's coördinates, but the elements of its orbit, as the unknown quantities, Le Verrier follows<sup>3</sup> the course thus indicated, using the common formulas, and omitting all consideration of the terms proportional to the time which can be confounded with the mean motion, and of the constants which can be combined with the epoch. The first attempt at a solution was based upon eight equations<sup>4</sup> similar to those<sup>5</sup> to which we have before alluded. These were founded on observations<sup>6</sup> subsequent to 1747, all of Flamsteed's observations being neglected. The second differences were combined precisely as before. The six equations<sup>7</sup> thus obtained were reduced, by a dexterous elimination<sup>8</sup> of the epoch, eccentricity, and mean motion, to three

<sup>1</sup> *C. R.*, XXII. p. 915 ; *Recherches*, p. 154.

<sup>5</sup> *Ibid.*, § 80.

<sup>2</sup> *Recherches*, p. 154, § 88.

<sup>6</sup> *Ibid.*, p. 146.

<sup>3</sup> *Ibid.*, §§ 90-95.

<sup>7</sup> *Ibid.*, p. 162.

<sup>4</sup> *Ibid.*, p. 162, § 96.

<sup>8</sup> *Ibid.*, p. 164.

others,<sup>1</sup> whose constant terms were the same as those in the equations above alluded to. From these he obtained<sup>2</sup>  $\frac{171}{40}$  ( $m = 2.11$ ) as a first approximation to the mass. But from the numerous obstacles to accuracy which are manifest in the course of this solution, Le Verrier inferred<sup>3</sup> that the interval of ninety-eight years was not sufficient for his purpose, but that it was necessary to extend the interval as far as observations would permit, and to form the equations of condition with all possible rigor.

This is done in the second solution,<sup>4</sup>—a solution which is pre-eminently a discussion of limits, and a brilliant combination of ingenuity, of analytical skill, and of laborious calculation,—a solution which cannot be adequately described without departing from the popular form prescribed to me in this Report. Account is taken of all the allowable errors in the ancient observations, and “the field of inquiry narrowed with consummate skill.”<sup>5</sup> The epoch being assumed at the beginning of the present century, all the values of its mean longitude which, when substituted in the equations of condition, would give a negative mass, as well as those which would give a mass large enough to affect Saturn sensibly, were promptly rejected.<sup>6</sup> The expression obtained for the mass is in the form of a fraction;<sup>7</sup> the numerator and denominator of which were separately examined, the former by an elegant application of Sturm’s formula. The result<sup>8</sup> was, that the longitude of the epoch must be included between the limits  $97^\circ$  and  $190^\circ$ , or between  $263^\circ$  and  $359^\circ$ , in order to render the corresponding mass positive; and if those masses be rejected which the motion of Saturn forbids us to assume, Le Verrier found<sup>9</sup> that the longitude of the epoch must be comprised between  $108^\circ$  and  $162^\circ$ , or between  $297^\circ$  and  $333^\circ$ . But, after laborious calculation, he also found that neither of

<sup>1</sup> *Recherches*, p. 164.

<sup>2</sup> *Ibid.*, § 100.

<sup>3</sup> *Ibid.*, p. 165.

<sup>4</sup> *Ibid.*, §§ 101–124.

<sup>5</sup> Peirce, *Proc. Amer. Acad.*, I. p. 66.

<sup>6</sup> *Recherches*, p. 169.

<sup>7</sup> *Ibid.*, §§ 104–107.

<sup>8</sup> *Ibid.*, p. 174.

<sup>9</sup> *Ibid.*, § 112, p. 181.

these limitations will allow the places in 1690 and 1747 to be represented with tolerable accuracy, "so that the consequence which would seem to result from the discussion, thus conducted, would be, that it was impossible to represent the course of Uranus by means of the perturbative action of the new planet."

Le Verrier was fortunately undismayed by this result, although many a good mathematician and experienced computer would, for less reason, have abandoned his apparently unprofitable labor in despair. But even Le Verrier, according to Biot,<sup>1</sup> revolved the matter in his mind for three months without advancing a step. He discovered subsequently, as he next states,<sup>2</sup> that, by neglecting two little inequalities of the longitude, in themselves so small that one would suppose himself warrantable in omitting them with perfect impunity, all the details of the solution became different, and the errors in 1690 were completely changed. He found,<sup>3</sup> still farther, that in spite of the apparent limitations furnished above by the resulting negative value of the mass, the assumption of values outside these limits, for the longitude of the epoch, enabled him perfectly to represent the motions of Uranus. He arrived, in short, at the fundamental proposition,<sup>4</sup> "*that there was in the ecliptic but one single region in which the perturbing planet could be placed, so as to account for the motions of Uranus; that the mean longitude of this planet on the 1st of January, 1800, must have been between 243° and 252°.*" Continuing his numerical computations, and calculating,<sup>5</sup> for each one of the eighteen normal places, the values of the perturbations corresponding to different hypotheses as to the longitude of the epoch, Le Verrier found<sup>6</sup> "that all the observations of Uranus could be represented by means of the perturbative action of a planet whose mean longitude was 252° on the 1st of January, 1800, and whose eccentricity and perihelion longitude are determined by the formulas" already given. The corresponding longitude in 1847.0 would be 325°, and, as above stated, Le Ver-

<sup>1</sup> *Journal des Savans*, Jan., 1847.

<sup>2</sup> *Recherches*, p. 181.

<sup>3</sup> *Ibid.*, p. 182.

<sup>4</sup> *Ibid.*, § 114, p. 185.

<sup>5</sup> *Ibid.*, § 116, pp. 187, 188.

<sup>6</sup> *Ibid.*, p. 193.

rier closed his memoir of June, 1846, by expressing his hope that astronomers might detect the planet.

The fourth book<sup>1</sup> contains a more precise determination of the elements of the orbit, and, though the processes are given more in detail, is essentially the same as the memoir of August 31, in the *Comptes Rendus*.<sup>2</sup> In this book the limits of the real and the apparent place of the disturbing body are computed with still greater precision. The hypotheses as to the mean distance vary<sup>3</sup> from 36.2 to 39.2; those as to the longitude of the epoch, from 234° to 270°. For these different hypotheses, six in number, the coefficients of the equations of condition are minutely computed.<sup>4</sup> Thirty-three equations of condition are formed<sup>5</sup> by comparison with observed geocentric places. From these equations six unknown quantities, and among them the elements of the orbit of Uranus, are eliminated, and their places supplied by the unknown quantities representing the corrections of the mean distance, and of the longitude of the epoch. The resulting<sup>6</sup> thirty-three are solved<sup>7</sup> by the method of least squares, and give,<sup>8</sup> as the final solution, the orbit on page 21 of this Report.

Le Verrier, in thus assigning definitely the planet's position in the heavens, expressed<sup>9</sup> the belief, that its disc would be large enough to indicate its planetary character to the attentive observer. Uranus, at the distance 19, has an apparent diameter of 4". Assuming a position and mass of the new planet conformable to his computations, and its density the same as that of Uranus, Le Verrier inferred, that its apparent diameter in opposition must be about 3".3, and its specific brilliancy about one third that of Uranus.

The orbit and mass thus obtained represent all the ancient observations within eight seconds, except Flamsteed's in 1690, to

<sup>1</sup> *Recherches*, p. 196.

<sup>2</sup> *C. R.*, XXIII. p. 428.

<sup>3</sup> *Recherches*, p. 198.

<sup>4</sup> *Ibid.*, § 130, pp. 203-220.

<sup>5</sup> *Ibid.*, § 131, p. 222.

<sup>6</sup> *Ibid.*, § 135, p. 231.

<sup>7</sup> *Ibid.*, § 136, p. 233.

<sup>8</sup> *Ibid.*, §§ 137-141, pp. 234-236.

<sup>9</sup> *Ibid.*, p. 237; *C. R.*, XXIII. p. 430.

which Le Verrier allows<sup>1</sup> but little weight, being content with representing it within 20'.\*

The author next determines<sup>2</sup> the extreme limits within which the disturbing planet is necessarily situated, allowing<sup>3</sup> a large error to all the observations, — five seconds, for instance, to the modern ones, and twenty-five to Flamsteed's in 1690. The result is,<sup>4</sup> that *the mean distance must be between the limits 35.04 and 37.9*. Assuming the corresponding times of revolution as the extreme limits of the period, he finds,<sup>5</sup> by an ingenious geometrical process, the limits of the planet's place, which they give for 1847.

The fifth and last part<sup>6</sup> of the *Recherches* corresponds to the memoir presented to the Academy of Sciences on the 5th of October, after the discovery of the planet, but was evidently written before the welcome news had reached Mr. Le Verrier. In this he endeavors to deduce from the perturbations of the latitude the position of the place in which the new planet must move. He infers<sup>7</sup> that the observations in latitude concur with those in longitude in indicating the existence of a disturbing planet, that<sup>8</sup> the plane of this planet's orbit must be inclined about  $4^{\circ} 38'$  to that of the orbit of Uranus, and that<sup>9</sup> a single observation of the orbit of this new body would suffice to make known approximately the plane in which it moves. These deductions as to the plane of the orbit Le Verrier submitted with much diffidence, on account of the smallness of the perturbations in latitude from which they must be made.

Mr. Adams's investigation is of a totally different nature, though characterized by remarkable ability and mathematical

\* In order to do justice to this observation, it must be stated that Neptune, with the mass  $\frac{1}{1342}$ , used by Prof. Peirce, satisfies<sup>10</sup> this observation within a single second.

<sup>1</sup> *Recherches*, p. 238.

<sup>2</sup> *Ibid.*, §§ 143-148.

<sup>3</sup> *Ibid.*, p. 240, § 143.

<sup>4</sup> *Ibid.*, p. 240, § 144.

<sup>5</sup> *Ibid.*, pp. 242, 247.

<sup>6</sup> *Ibid.*, p. 250.

<sup>7</sup> *Ibid.*, p. 251.

<sup>8</sup> *Ibid.*, p. 251.

<sup>9</sup> *Ibid.*, p. 252.

<sup>10</sup> *Proc. Amer. Acad.*, I. 333.

power. In it the question of limits is neither discussed, nor in the least degree involved, but the problem is directly proposed, — “To find the most probable orbit and mass of the disturbing body which has acted on Uranus.” The question, whether it be necessary to assume the existence of such a body, is not discussed. The labors of Bouvard were supposed to have set this question at rest. Mr. Adams states,<sup>1</sup> that his first solution was attempted in 1843, assuming the orbit to be a circle, the distance, nearly \* in conformity with “Bode’s law,”<sup>2</sup> twice that of Uranus, and taking solely the modern observations into account. The errors of the tables were taken<sup>3</sup> from Bouvard’s equations of condition as far as the year 1821, and, for later dates, from Schumacher’s *Astronomische Nachrichten* and the Reductions of the Greenwich Planetary Observations.

Mr. Adams inferred<sup>4</sup> from his results that a good general agreement between theory and observation might be obtained, and therefore commenced<sup>5</sup> a more accurate investigation, the results of which were communicated to the Astronomer Royal in the letter<sup>6</sup> of October, 1845, above<sup>7</sup> referred to, and to Prof. Challis, some time<sup>8</sup> in the month of September.

Flamsteed’s observation of 1690 was entirely rejected.<sup>9</sup> The chief inequalities given by Bouvard were recomputed,<sup>10</sup> without the detection of any error, excepting the one pointed out by Bessel. Airy’s mass of Jupiter<sup>11</sup> was introduced in the place of the one used by Bouvard. Those inequalities depending on the square of the disturbing force, which had been pointed out by Hansen, were also recomputed.<sup>12</sup> The differences between the calculated and observed heliocentric longitudes were converted<sup>13</sup>

$$* x = 4 + 2^{n-2} . 3.$$

<sup>1</sup> “*Explanation*,” § 3, p. 4.

<sup>2</sup> *Berl. Astr. Jahrb.*, 1791, p. 189.

<sup>3</sup> *Expl.*, § 4.

<sup>4</sup> *Ibid.*, § 3.

<sup>5</sup> *Ibid.*, § 10 *et seq.*

<sup>6</sup> “*Account*,” p. 129.

<sup>7</sup> *Expl.*, p. 15.

<sup>8</sup> “*Account*,” p. 128, letter No. 6.

<sup>9</sup> *Ibid.*, p. 130. “*Explanation*,” § 25.

<sup>10</sup> “*Explanation*,” p. 5, § 7.

<sup>11</sup> *Mem. R. Astr. Soc.*, X, p. 47.

<sup>12</sup> *Expl.*, p. 6.

<sup>13</sup> *Ibid.*, p. 7, § 9.



into differences of *mean* longitude. Observations made near opposition were selected,<sup>1</sup> when possible, and the series of modern observations divided into groups, from which were deduced<sup>2</sup> normal places at intervals of three years. The equations of condition formed from these served as the basis of the entire computation.

Assuming, then, as a first hypothesis,<sup>3</sup> the mean distance of the new body to be twice that of Uranus, Adams computes<sup>4</sup> the value of the fundamental perturbations. The values obtained<sup>5</sup> are almost identical with those<sup>6</sup> of Le Verrier. But with regard to the influence of the third and fourth terms, Adams does not appear to have experienced the inconveniences by which Le Verrier states that he was so much impeded.<sup>7</sup> Both mathematicians agree,<sup>8</sup> however, in the rejection of the small perturbations of the second order, dependent on the square of the eccentricity of the disturbing planet. Taking the mean opposition in 1810, as the epoch, Mr. Adams has elegantly arranged<sup>9</sup> his equations of condition in such a way, that they separate themselves into two groups, each having<sup>10</sup> but five unknown quantities. The coefficients are then readily computed by summation. By eliminating<sup>11</sup> the unknown quantities in each group, it will, therefore, be comparatively easy, for any assumed value of the mass and longitude at the epoch, to obtain a correction by the method of least squares. But as the unknown quantities are the same in all the equations of each group, these equations may be added,<sup>12</sup> and the substitution of the elements of Uranus, already approximately known, gives values for the mass and epoch. The equations of condition drawn from the ancient observations, omitting that in 1690, are the suited<sup>3</sup> for the determination of

<sup>1</sup> *Explanation*, p. 5, § 6.

<sup>2</sup> *Ibid.*, pp. 6, 7.

<sup>3</sup> *Ibid.*, p. 8, § 12.

<sup>4</sup> *Ibid.*, § 12.

<sup>5</sup> *Ibid.*, p. 9.

<sup>6</sup> *Recherches*, p. 161, § 95.

<sup>7</sup> *Ibid.*, p. 181.

<sup>8</sup> Adams, *Expl.* §§ 11, 12; Le Verrier, *Recherches*, pp. 158, 201.

<sup>9</sup> *Expl.*, § 13.

<sup>10</sup> *Ibid.*, § 14.

<sup>11</sup> *Ibid.*, §§ 15 - 20.

<sup>12</sup> *Ibid.*, § 21, p. 13.

<sup>13</sup> *Ibid.*, § 23.

the eccentricity and perihelion longitude. This determination is very skilfully made, and the elements<sup>1</sup> furnished by the whole analysis are those communicated<sup>2</sup> to the Astronomer Royal in October, 1845.

Mr. Adams next proceeded<sup>3</sup> to repeat the whole investigation, assuming the mean distance to be 37.25, the first hypothesis having been 38.36. For the time when the computations on this hypothesis were made, no date is assigned. They were probably made during the summer of 1846, as the resulting elements were communicated<sup>4</sup> to Mr. Airy on the 2d of September of that year. They are as follows<sup>5</sup>:—

Epoch,	. . . . .	1810.328
Mean longitude,	. . . . .	264° 50'
Longitude of perihelion,	. . . . .	298° 41'
Eccentricity,	. . . . .	0.120615
Mean longitude, Oct. 6, 1846,	. . . . .	323° 2'
Mass,	. . . . .	$\frac{1}{5555}$

The theoretical corrections of the mean longitude on each hypothesis are next<sup>6</sup> given,—the parts due to the changes in the elements of Uranus and to the action of the hypothetical planet being written separately,—and these corrections are compared with observation.

The modern observations of Uranus are thus admirably represented<sup>7</sup> down to the year 1840. The ancient ones were represented within tolerable limits, excepting the observation in 1771, where the discrepancy amounts to 11".8 on the first, and 12".8 on the second hypothesis. Le Verrier has not given any comparison of this observation with his final orbit.<sup>8</sup>

In the observations since 1840, however, Mr. Adams did not find so satisfactory an agreement. The differences, as deduced from the three oppositions immediately preceding the investigation, were<sup>9</sup> by the two hypotheses,—

<sup>1</sup> *Expl.*, § 31.

<sup>2</sup> *R. Ast. Soc. Notices*, pp. 129, 151.

<sup>3</sup> *Expl.*, § 32, *et seq.*

<sup>4</sup> *R. Ast. Soc. Not.*, VII. pp. 138, 151.

<sup>5</sup> *Expl.*, §§ 47, 48, p. 25.

<sup>6</sup> *Ibid.*, §§ 49–51.

<sup>7</sup> *Ibid.*, § 52.

<sup>8</sup> *Recherches*, § 142, p. 238.

<sup>9</sup> *Expl.*, § 53, p. 28.

	I.	II.
1843,	7.11	5.77
1844,	8.79	7.05
1845,	12.40	10.18

The errors for these three years are, thus, about one fifth less in the second hypothesis. The first had assumed that the ratio of the mean distances of the two planets was equal to  $\sin. 30^\circ$ , the second that it was  $\sin. 31^\circ$ . Mr. Adams roughly inferred<sup>1</sup> from this consideration, that the true ratio would be about equal to  $\sin. 35^\circ$ , which gives a mean distance of 33.42.

The application of the Rule of Three to a problem so complicated as that which the orbit of the disturbing planet presented was, we are bound to believe, intended merely as a rude means of conjecture. It appears, therefore, surprising, that this inference should be dwelt upon as one of the merits of Mr. Adams's investigation, and as tending to show that the solution at which he arrived corresponded to the orbit and place of Neptune. Even had Mr. Adams intended to apply the rule of simple proportion, it is impossible that he should have founded it upon the comparison of three oppositions alone. Le Verrier has shown<sup>2</sup> that the assumption of even 35 as the mean distance would lead to *intolerable* discordances. Peirce has further proved<sup>3</sup> that an important change in the character of the perturbations takes place near the distance 35.3. It is, therefore, evident that no claims can be based upon the rough inference alluded to. And it is but just to assume that Mr. Adams would disclaim any intention to dwell upon this point, although stress has been laid upon it<sup>4</sup> by one of the most eminent of his countrymen.

After giving the formulas for the corrections of the tabular radius-vector of Uranus upon each of his hypotheses, Mr. Adams closes by stating,<sup>5</sup> that, on account of the shortness of the pe-

<sup>1</sup> *Expl.*, p. 29; "Account," p. 139.

<sup>2</sup> *Recherches*, p. 240.

<sup>3</sup> *Proc. Amer. Acad.*, I. 66.

<sup>4</sup> Herschel, *Outlines of Astr.*, p. 517, note.

<sup>5</sup> *Expl.*, § 60.

riod (98 years, the observation in 1690 having been rejected), and the smallness of the perturbations in latitude, all his attempts to fix the plane of the orbit had been unsatisfactory.

I have thus endeavored to give an account of the origin and progress of the theory of Uranus up to the discovery of Neptune, and the publication of those computations which had led astronomers to suspect its existence and direction. Of the discussion concerning priority which unfortunately arose, it is not necessary to speak. There cannot be the slightest doubt of the fact, that the several investigators were entirely independent of one another; but as many persons, especially those not professedly devoted to the pursuit of science, attach importance to the question of priority, I have endeavored to state the facts as impartially as possible, and thus to give the data by which any one may be enabled to judge for himself. The discussion which subsequently arose was of such a nature as to throw the controversy between the partisans of the French and of the English geometer entirely into the background.

Before passing from the discovery of Neptune to the subsequent development of the theory, it is proper to allude to the charts published by the Berlin Academy at the instance<sup>1</sup> of Bessel; inasmuch as these furnished the means by which Neptune was discovered at Berlin upon the first search. They have also directly contributed, within the last four years, to the detection of several members of our solar system. The chart<sup>2</sup> embracing the region in which Neptune was found had been drawn up with great accuracy by Dr. Bremiker, the eminent coadjutor of Prof. Encke, in the Berlin Astronomical Almanac. Although sometime printed, it had been published but a short time. Had Prof. Challis been in possession of this map, he would probably<sup>3</sup> have discovered Neptune on the 4th of August, as he observed it as a fixed star on that day,<sup>4</sup> while sweeping for the planet.

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<sup>1</sup> *Abhandl. d. Königl. Akad. Berl.*, 1824, p. iii.; *Astr. Nachr.*, IV. pp. 297, 437.

<sup>2</sup> Hour XXI.

<sup>3</sup> *Astr. Nachr.*, XXV. p. 102; *Ast. Soc. Not.*, VII. p. 146.

<sup>4</sup> *Ast. Soc. Not.*, VII. p. 148.

He observed it again, August 12. He also noted it September 29, as seeming to have a disc; but the news of the discovery at Berlin on the 23d arrived before the next fair evening.<sup>1</sup>

The slowness of the planet's motion of course rendered it impossible to find the true orbit, till after the lapse of considerable time, but elements upon the hypothesis of a circular orbit were computed within the first month by Adams,<sup>2</sup> Galle,<sup>3</sup> and Binet.<sup>4</sup> These agreed very nearly with one another, and coincided especially in showing the distance from the sun to be about 30. Mr. Adams afterwards computed<sup>5</sup> a second circular orbit, which gave the same result.

Mr. Valz, of the Marseilles Observatory, endeavored<sup>6</sup> early in the year 1847 to deduce the form of the orbit from the small arc described by the planet since its discovery. He found himself, however, unable to obtain a reliable value for the eccentricity, but assigned  $\frac{1}{2}$  as the result of his computations. At the same time he requested attention to the fact, that, in a letter to Arago concerning the perturbations of Halley's comet, published<sup>7</sup> Sept. 12, 1835, he had expressed his belief in the existence of a planet exterior to Uranus.

The means of obtaining elliptic elements was afforded by the fortunate discovery of an ancient observation of Neptune. This discovery was made independently by two astronomers, Mr. Walker in Washington, and Dr. Petersen in Altona, each of whom computed an approximate ephemeris for Neptune, and came to the conclusion that no observer except Lalande had catalogued in the vicinity of the planet.

The modes of research employed by Dr. Petersen and Mr.

<sup>1</sup> *Ast. Soc. Notices*, VII. p. 147.

<sup>2</sup> *Astr. Nachr.*, XXV. p. 106.

<sup>3</sup> *Ibid.*, XXV. pp. 192, 311; *Bericht d. Königl. Preuss. Akad.*, 1846, p. 280; *Ast. Soc. Not.*, VII. p. 148; *Astr. Nachr.*, XXV., p. 311.

<sup>4</sup> *Comptes Rendus*, XXIII. p. 798.

<sup>5</sup> *Ast. Soc. Not.*, VII. p. 148.

<sup>6</sup> *Comptes Rendus*, XXIV. p. 638.

<sup>7</sup> *Ibid.*, I. p. 130.

Walker were essentially different. Dr. Petersen<sup>1</sup> compared a chart of the stars of the *Histoire Céleste*, in a zone extending from 2° south to 4° north latitude, and from 14<sup>h</sup> to 17<sup>h</sup> right-ascension, directly with the heavens, and found that three stars observed by Lalande were no longer visible. On a second comparison, he saw that the recorded places of two of these were probably vitiated by a typographical error of one minute of time. The third star had been observed May 10th, 1795, and was entered<sup>2</sup> as follows in the *Histoire Céleste* : —

Mag.	Middle Thread.	Zen. Dist.
7.8	14 <sup>h</sup> 11 <sup>m</sup> 23 <sup>s</sup> .5	60° 7' 19"

Dr. Petersen then calculated from Galle's circular elements the position of Neptune for the time of this observation, and found an agreement sufficiently close to convince him that the missing star was Neptune.

Walker had, in the United States, arrived at the same result by a totally different investigation.<sup>3</sup> He made no use of the telescope. He had at hand a large collection of observations, from which he had already computed elements of sufficient accuracy to show that Lalande had only swept in the neighborhood of Neptune upon the 8th and 10th of May, 1795. For these nights he computed a *locus* of Neptune, by assuming different eccentricities, upon the two hypotheses of the present true anomaly being greater or less than 180°. The catalogue of Lalande's stars within this locus was then subjected to a rigid scrutiny. The stars which have been since observed, those more than 15' north or south of the planet's computed path, and those below the ninth magnitude, being rejected, there remained but one star in the list, and that was less than a minute north of the computed declination of Neptune for that right-ascension. This discovery was made on the 2d of February, and on the next day Mr. Walker communicated to the astronomers of the

<sup>1</sup> *Astr. Nachr.*, XXV. pp. 291, 303.

<sup>2</sup> *Histoire Céleste*, p. 158, obs. 8.

<sup>3</sup> *Washington Union*, Feb. 9, 1846; *Proc. Am. Phil. Soc.*, IV. p. 318; *Proc. Amer. Academy*, I. p. 57; *Astr. Nachr.*, XXV. p. 383.

Washington Observatory his conviction that the missing star was Neptune. Upon the 4th, the weather first became clear, and Prof. Hubbard of the Observatory had the gratification of finding<sup>1</sup> that the star was no longer in the place where Lalande had seen it.

It was thus rendered a moral certainty, by the independent labors of the American and the German astronomer, that Neptune had been observed by Lalande, and that the eccentricity of the orbit was very small. This enabled Mr. Walker to compute elliptic elements<sup>2</sup> during the month of February, 1847, which represented the motion of the planet with great accuracy; and Mr. Adams also communicated<sup>3</sup> elliptic elements to Prof. Schumacher, under date of May 19th. By means of the ephemerides of Neptune published<sup>4</sup> by these two astronomers, the planet was followed without difficulty through the opposition of 1847.

Mr. Walker and Dr. Petersen had both immediately written to Le Verrier to inform him of their researches, and Le Verrier communicated to the French Academy of Sciences, at the same meeting,<sup>5</sup> the results of the investigations at Washington and at Altona.

But in speaking of Mr. Walker's elements, he remarked<sup>6</sup> that "the small eccentricity which appeared to result from Mr. Walker's computations would be incompatible with the nature of the perturbations of the planet Herschel."

The hypotheses of Walker and Petersen received a striking confirmation from the examination of Lalande's MSS. by Mr. Mauvais, of the Royal Observatory, Paris. Mauvais discovered<sup>7</sup> that Lalande had observed the star on the 8th, as well as on

<sup>1</sup> *Proc. Amer. Acad.*, I. p. 64; *Proc. Am. Phil. Soc.*, IV. p. 318.

<sup>2</sup> *Proc. Amer. Acad.*, I. p. 67; *Proc. Am. Phil. Soc.*, IV. p. 319; *Astr. Nachr.*, XXV. p. 383.

<sup>3</sup> *Astr. Nachr.*, XXV. p. 399. See also *R. Ast. Soc. Not.*, June 11, 1847.

<sup>4</sup> *Astr. Nachr.*, XXV. pp. 51, 241.

<sup>5</sup> March 29, 1847.

<sup>6</sup> *Comptes Rendus*, XXIV. p. 531.

<sup>7</sup> *Comptes Rendus*, XXIV. p. 641; *Astr. Nachr.*, XXVI. p. 97.

the 10th of May. But in consequence of the non-accordance of the two observations, he had only inserted the latter in the *Histoire Céleste*, and annexed to it the (:) which he used to denote a doubtful observation. Both Mauvais and Peirce, after a careful reduction of both observations, found<sup>1</sup> that the discordance corresponded precisely to the motion of Neptune in two days, — so that the question of identity is now beyond dispute.

Meantime,<sup>2</sup> Prof. Peirce had taken a remarkable step. From the distance, 30, and consequent angular motion, without any hypothesis as to the character of the orbit, he arrived at the startling conclusion,<sup>3</sup> “that the planet Neptune is not the planet to which geometrical analysis had directed the telescope; that its orbit is not contained within the limits of space which have been explored by geometers searching for the source of the disturbances of Uranus; and that its discovery by Galle must be regarded as a happy accident.”

This conclusion, paradoxical as it might at first have appeared to many, was announced with a candor and moral courage only equalled by that of Le Verrier in his original prediction of the planet's place. The reasoning<sup>4</sup> by which Peirce defended his position deserves, even at this late day, the most careful consideration. It is so clear and convincing, that it would seem unnecessary to dwell upon it, were it not that even now astronomers<sup>5</sup> of high eminence do not hesitate to dispute the ground there taken; although the arguments adduced against it are rather of a popular than of a mathematical character.

Peirce first alluded<sup>6</sup> to the two fundamental propositions of Le Verrier, viz. : —

1st. That the planet's mean distance must be between 35 and 37.9;

<sup>1</sup> *Comptes Rendus*, XXIV. p. 666; *Proc. Amer. Acad.*, I. p. 149.

<sup>2</sup> March 16, 1847.

<sup>3</sup> *Proc. Amer. Acad.*, I. p. 65.

<sup>4</sup> *Ibid.*, pp. 65–68.

<sup>5</sup> Herschel, *Outlines of Astronomy*, § 776, p. 516.

<sup>6</sup> *Proc. Amer. Acad.*, I. p. 66.



2d. That the mean longitude for January 1st, 1800, must have been within the limits  $243^{\circ}$  and  $252^{\circ}$ ; and announced that he had convinced himself that, although neither of these was inconsistent with the observations made upon Neptune since its discovery, yet that the two combined were utterly irreconcilable with observation; — that if the first proposition were assumed as true, the mean longitude in 1800 must have been at least forty degrees from the limits of the second; — and that, if we adopted the second proposition, the time of revolution must be less by forty years than the inferior limit given by the first.

“It is not, however,” continued Peirce, “a necessary conclusion that Neptune will not account for the perturbations of Uranus; for its probable mean distance of about 30 is so much less than the limits of the previous researches, that no inference from them can be extended to it. An important change, indeed, in the character of the perturbations, takes place near the distance 35.3; so that the continuous law by which such inferences are justified is abruptly broken at this point, and it was hence an oversight in Mr. Le Verrier to extend his inner limit to the distance 35. A planet at the distance 35.3 would revolve about the sun in 210 years, which is exactly two and a half times the period of revolution of Uranus. Now if the times of revolution of two planets were exactly as 2 to 5, the effects of their mutual influence would be peculiar and complicated, and even a near approach to this ratio gives rise to those remarkable irregularities of motion which are exhibited in Jupiter and Saturn, and which greatly perplexed geometers until they were traced to their origin by Laplace. This distance of 35.3, then, is a complete barrier to any logical deduction, and the investigations with regard to the outer space cannot be extended to the interior.

“The observed distance, 30, which is probably not very far from the mean distance, belongs to a region which is even more interesting in reference to Uranus than that of 35.3. The time of revolution which corresponds to the mean distance 30.4 is not 168 years, being exactly double the year of Uranus, and the

influence of a mass revolving in this time would give rise to very singular and marked irregularities in the motions of this planet. The effect of a near approach to this ratio in the mean motion is partially developed<sup>1</sup> by Laplace in his theory of the motions of the three inner satellites of Jupiter. The whole perturbation arising from this source may be divided into two portions or inequalities, one of which, having the same period with the time of revolution of the inner planet, is masked to a great extent behind the ordinary elliptic motions, while the other has a very long period, and is exhibited for a great length of time under the form of a uniform increase or diminution of the mean motion of the disturbed planet.”

Peirce closed this most important paper by showing,<sup>2</sup> that, if the period of Neptune were more than 166½, and less than 169½ years, the conclusion was inevitable, that its period was precisely twice as long as that of Uranus.

Walker has since found<sup>3</sup> the period of Neptune to be 164.6 years. The two periods are therefore not precisely commensurable, but they approach commensurability so nearly that some of the terms<sup>4</sup> of the perturbations of Uranus require careful investigation, which, according to the theories of Le Verrier and Adams, would be merged in the longitude of the epoch and other elements of the elliptic motion.

In reference to the apparent inconsistency of the assertion, that the discovery was “a happy accident,” Peirce still farther showed<sup>5</sup> that the problem was susceptible of several solutions, decidedly different from one another, and from those of Le Verrier and Adams, and equally complete with theirs. “The present place of the theoretical planet,” to quote from his communication<sup>6</sup> of May 4th, 1847, to the American Academy, “which

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<sup>1</sup> *Mécanique Céleste*, Bowditch's Transl., I. p. 656; IV. p. 126.

<sup>2</sup> *Proc. Amer. Acad.*, I. p. 68.

<sup>3</sup> *Smithsonian Contributions*, II. App. I. p. 6.

<sup>4</sup> *Proc. Amer. Acad.*, I. p. 334.

<sup>5</sup> *Boston Courier*, April 30, 1847.

<sup>6</sup> *Proc. Amer. Acad.*, I. p. 144.

might have caused the observed irregularities in the motions of Uranus, would, in two of them, be about one hundred and twenty degrees from that of Neptune, the one being behind, and the other before, this planet. If Le Verrier or Adams had fallen upon either of the above solutions instead of that which was obtained, Neptune would not have been discovered in consequence of geometrical prediction. The following are the approximate elements for the three solutions, at the epoch of January 1st, 1847:—

	I.	II.	III.
Mean longitude, . . . . .	319°	79°	199°
Longitude of perihelion, . . . .	148°	219°	188°
Eccentricity, . . . . .	0.12	0.07	0.16

In each of these the mass is 0.0001187, and the period of sidereal revolution double that of Uranus.”

Continuing the investigation, Prof. Peirce arrived at another singular result. He found himself unable to reconcile the observed motions of Neptune with the assumption, that it was the chief source of the unexplained irregularities in the motion of Uranus. This result was in all candor acknowledged to the Academy, but with the remark<sup>1</sup> that he considered “it would be presumptuous in him to claim for his investigations a freedom from error which the greatest geometers have not escaped, especially in the face of the vastly improbable conclusion to which his analysis tended.” He subsequently<sup>2</sup> succeeded, as is well known, in entirely explaining the motions of Uranus by the action of Neptune, using the mass  $\frac{1}{2000}$ . The failure of the first attempt was in consequence of his not having carried the development of the disturbing function to a sufficient number of terms, but having contented himself in the first approximation with the number of terms used<sup>3</sup> by Le Verrier in his formulas. The numerical formulas of Le Verrier’s Researches can only

<sup>1</sup> *Proc. Amer. Acad.*, I. p. 145.

<sup>2</sup> *Ibid.*, I. p. 332.

<sup>3</sup> *Recherches*, § 94.

apply to mean distances within the limits assigned by that geometer.

During the summer of 1847, Peirce determined the approximate perturbations of Neptune by the other planets, and communicated<sup>1</sup> them to Walker in November. Walker, using these values, deduced a pure elliptic orbit, from the discussion of 689 observations, including those of Lalande. This orbit,<sup>2</sup> together with the normal places on which it was founded, and with the expressions for the heliocentric coördinates, was presented to the American Academy, December 7th. At the same meeting, Peirce communicated<sup>3</sup> the corresponding values of the entire perturbations of Neptune up to the terms depending on the cubes of the eccentricities. He used the masses of the planets assumed by Le Verrier in his theories of Mercury and Uranus, with the exception of the mass of Uranus, for which he took Lamont's determination.<sup>4</sup> The results were given both in the usual form and in that which Le Verrier had adopted in his theory of Mercury. Peirce also gave<sup>5</sup> the particular values of the perturbations of the true anomaly and radius-vector, for the epoch of Lalande's observations, and for every three months from October, 1846, to January, 1851. Through the labors of Peirce and Walker, the elliptic orbit of Neptune, with the numerical values of its perturbations by all the other planets, was thus known at the beginning of December, 1847, with such accuracy, that an ephemeris<sup>6</sup> founded on these data satisfied the observations of September, 1848, within one and a tenth seconds of time in right-ascension, and six and a half seconds in declination.<sup>7</sup>

Applying anew Peirce's second values for the perturbations,

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<sup>1</sup> *Proc. Amer. Acad.*, I. p. 285.

<sup>2</sup> *Ibid.*; *Proc. Amer. Phil. Soc.*, IV. 378.

<sup>3</sup> *Proc. Amer. Acad.*, I. pp. 287-295.

<sup>4</sup> *Mem. R. Ast. Soc.*, XI. p. 59.

<sup>5</sup> *Proc. Amer. Acad.*, I. pp. 294, 295.

<sup>6</sup> *Astr. Nachr.*, XXVII. p. 347.

<sup>7</sup> *Ibid.*, XXIX. p. 191.

and using still later observations, Walker presented,<sup>1</sup> March 6, 1848, a second pure elliptic orbit, which has represented<sup>2</sup> the course of Neptune so well up to the present time, as to render a nearer approximation unnecessary, if indeed it were possible.

The perturbations of Uranus by Neptune were also communicated<sup>3</sup> by Prof. Peirce on March 6th. This geometer had found<sup>4</sup> Neptune capable of entirely accounting for all the motions of Uranus, provided a mass of about  $\frac{1}{10000}$  be adopted.

The determination of the mass has been a problem of some difficulty. Soon after the discovery of the planet, Mr. Lassell, of Liverpool, discovered<sup>5</sup> a satellite. Observations of this satellite, made at Pulkowa, at Liverpool, and at Cambridge, have given masses\* for Neptune differing very considerably from one another. The question must be expected to remain unsettled for some time. Peirce seems to have provisionally adopted the mass ( $\frac{1}{15400}$ ) deduced by him from Cambridge observations alone.

The following table<sup>6</sup> of differences between the observed and calculated longitudes of Uranus is taken from Peirce's communication, and shows how well the motions of this planet now accord with theory. The first column contains the discrepancies which exist when the action of Neptune is not taken into consideration. The second and third give those which would remain, did the theoretical planets of Le Verrier and Adams actually exist, and act upon Uranus. The third column contains the discrepancies, after allowing for the influence of Neptune, supposing the mass to be that deduced by Peirce from the Cambridge observations alone.

\* Struve found from Pulkowa observations  $\frac{1}{14512}$ . Peirce, from English and American observations,  $\frac{1}{12120}$ ; from American observations alone,  $\frac{1}{15400}$ .

<sup>1</sup> *Proc. Amer. Acad.*, I. p. 331.

<sup>2</sup> *Smithsonian Contrib. to Knowl.*, II., Appendix I. p. 5.

<sup>3</sup> *Proc. Amer. Acad.*, I. p. 334.

<sup>4</sup> *Ibid.*, p. 332.

<sup>5</sup> *Astr. Nachr.*, XXVI. p. 165.

<sup>6</sup> *Proc. Amer. Acad.*, I. p. 333.

## RESIDUAL DIFFERENCES BETWEEN THE THEORETICAL AND OBSERVED LONGITUDES OF URANUS.

Date.	Without any exterior Planet.	By Le Verrier's Theory, with Mass = $\frac{1}{8122}$ .	By Adams's Theory, with Mass = $\frac{1}{2141}$ .	Introducing Neptune with Prisce's Mass = $\frac{1}{13443}$ .	
1845,	+ 6.5	- 0.3	+ 10.2	- 0.9	
1840,	+ 0.7	+ 2.2	+ 1.3	- 1.1	
1835,	- 4.5	- 0.8	- 1.2	+ 2.0	
1829,	- 7.8	- 2.2	+ 2.0	+ 0.8	
1824,	- 7.6	- 5.4	+ 1.7	- 2.0	
1819,	+ 3.8	+ 0.4	- 2.2	+ 1.0	
1813,	+ 4.5	- 0.9	- 1.0	- 0.3	
1808,	+ 3.8	+ 0.8	0.0	- 0.4	
1803,	- 3.4	+ 0.8	+ 1.6	+ 0.8	
1797,	- 6.7	- 1.0	- 0.5	+ 0.3	
1792,	- 7.8	+ 0.3	- 1.1	+ 0.3	
1787,	+ 2.0	- 1.2	- 0.2	- 0.5	
1782,	+ 20.5	+ 2.3	0.0	- 3.0	
Ancient. {	1769,	+ 123.3	+ 3.7	+ 1.8	- 6.0
	1756,	+ 230.9	- 4.0	- 4.0	+ 4.0
	1715,	+ 279.6	+ 5.5	- 6.6	+ 8.7
	1690,	+ 289.0	- 19.9	+ 50.0	+ 0.8

Beside that solution of the "inverse problem of the perturbations of Uranus," which Le Verrier and Adams obtained, we have seen that, *using their data*, there are a number of other solutions, one of which corresponds to the orbit and mass of Neptune. Had Le Verrier been in possession of observations intermediate to those which he used, he would not have arrived at so harmonious results. The fact, however, that Neptune does not correspond to his solution, cannot in the least detract from the merit or intrinsic value of his investigations. These may be looked upon as a complete discussion of two distinct problems.

In the first place, Le Verrier demonstrated the existence of a disturbing planet. He solved this problem completely by proving, not only that it was impossible to represent the motions of Uranus without the assumption of some unknown disturbing body, but that the perturbations were of that analytical form

which belongs to the influence of an exterior planet. In so far as this goes, Le Verrier may be considered the discoverer of Neptune.

In his solution of the second problem, that of the orbit and mass, he was not so successful. But, inasmuch as, by using all the observations within his reach, he found an orbit and mass capable of accounting for the observed motions of Uranus, he must be, in the opinion of mathematicians, legitimately entitled to all the admiration which he would have received had such a planet actually moved in that orbit. He omitted, it is true, the consideration of the terms depending on a near approach to commensurability; but this, although certainly a defect, cannot be considered as an error in the theory, for, within the limits where he had reason to suppose that the orbit was situated, these terms are almost uniformly negligible. His laborious and elegant researches have been crowned with brilliant success, and Mr. Le Verrier himself rewarded by the consciousness of having been the immediate occasion of the discovery of Neptune. And although the agreement of Neptune's direction at the time of discovery with the direction of the theoretical planet was but accidental, it almost seems as though the heavens strove to show themselves propitious, so happy was the accident, so wonderful the coincidence.

In order to show as clearly as possible the relative positions of Neptune, and of the theoretical planets of Le Verrier and Adams, I have computed the following table, which gives the true longitude and the radius-vector of each of the three, for every tenth year of the two centuries immediately succeeding Flamsteed's first observation of Uranus. It will be observed from this table, that the longitude of the planet of Le Verrier's theory coincided with that of Neptune in 1840; and that Neptune would be in conjunction with Adams's planet about the year 1856. The closest agreement of the radius-vectors was not far from the year 1830, and the greatest discordance in the years from 1710 to 1720, at which time the distance of Neptune was

about midway between that of Uranus and that of the two hypothetical planets.

Date.	TRUE LONGITUDE.			RADIUS-VECTOR.		
	Neptune.	Le Verrier.	Adams.	Neptune.	Le Verrier.	Adams.
1690,	341.1 <sup>o</sup>	65.1 <sup>o</sup>	81.3 <sup>o</sup>	29.92	39.09	40.69
1700,	4.4	79.2	94.5	29.84	39.63	41.30
1710,	26.7	93.0	107.3	29.79	40.89	41.66
1720,	49.1	106.5	119.8	29.77	40.04	41.74
1730,	70.8	120.2	132.0	29.80	39.85	41.48
1740,	93.8	134.0	145.5	29.86	39.40	41.09
1750,	116.0	148.2	158.9	29.95	38.71	40.39
1760,	138.0	163.0	172.6	30.04	37.84	39.46
1770,	160.1	178.9	187.3	30.10	36.93	38.37
1780,	181.8	195.1	202.7	30.22	35.68	37.15
1790,	203.5	212.7	219.2	30.28	34.58	35.91
1800,	225.9	231.4	236.8	30.30	33.57	34.74
1810,	246.8	251.2	255.5	30.28	32.78	33.75
1820,	268.5	270.9	275.2	30.23	32.64	33.06
1830,	290.2	291.4	295.5	30.15	32.29	32.77
1840,	312.0	312.0	315.9	30.02	32.63	32.91
1850,	334.2	332.0	335.9	29.96	33.32	33.47
1860,	356.4	351.0	355.0	29.87	34.26	34.37
1870,	18.7	8.9	14.1	29.81	35.43	35.49
1880,	41.1	25.8	29.9	29.77	36.48	36.73
1890,	63.4	39.4	45.7	29.78	37.99	37.97

Mr. Adams has taken no personal part in the controversies which have arisen since the discovery of Neptune, but has continued to devote himself to the pursuit of science. Mr. Le Verrier has published<sup>1</sup> several articles in the *Comptes Rendus*, in order to defend his claim to be considered the actual discoverer of Neptune, by showing that this planet might have been brought within the limits of his theory. In England, Sir John Herschel has taken<sup>2</sup> similar ground in favor of Mr. Adams.

<sup>1</sup> *Comptes Rendus*, 1848, Sept. 11th, Oct. 2d, etc.

<sup>2</sup> *Outlines of Astronomy*, pp. 309, 509-512, 516-518.



The argument of Le Verrier is,<sup>1</sup> that, since he was obliged to found his computations upon irregularities, which, on account of the probable inaccuracy of the observations, were uncertain by a tenth part of their whole value, it would be very natural that this want of precision should affect the positions thence deduced for the disturbing planet, and that these positions should themselves be wrong by their tenth part.

I should not have alluded to this reasoning had not Mr. Le Verrier published it, and it will perhaps even now be considered unadvisable to endeavor to refute it. According to this argument, an error of 3.0 would be allowable in the mean distance 30, — of 4.0 if the mean distance were 40, etc., — errors which would make the attraction of the planet to be exerted in a direction totally different from the true one. But even this allowance would not correct the error of the radius-vector in 1710 and 1890.

As a rejoinder to the last argument, Mr. Le Verrier says,<sup>2</sup> that, “when there are perturbations, he can tell where Neptune is,” but to demand that he “should do it long after the perturbative action has disappeared, is simply to exact of him what is impossible, — a species of miracle.” Yet at the time of the early observations, when the radius-vector of Neptune differed from that of the theoretical planet by ten times the radius of the earth’s orbit, Uranus was, according to Mr. Le Verrier’s theory, undergoing a perturbation by Neptune.<sup>3</sup> It is unnecessary to allude in this connection to Le Verrier’s deduction<sup>4</sup> from his computations, that the small eccentricity of Neptune’s orbit would be *incompatible* with the nature of the perturbations of Uranus.

With regard to the mass, Le Verrier uses a somewhat different argument. Assuming the mass to be, as Peirce has found,  $\frac{1}{1000}$  of what had been predicted, he shows<sup>5</sup> that this corresponds to an error of but a fifth in the diameter of Neptune. This is very true, — no schoolboy will deny it, — but it was the mass,

<sup>1</sup> *Comptes Rendus*, XXVII. p. 273.

<sup>4</sup> *Comptes Rendus*, XXIV. p. 531.

<sup>2</sup> *Ibid.*, p. 275.

<sup>5</sup> *Ibid.*, XXVII. p. 277.

<sup>3</sup> See Herschel’s *Outlines of Astronomy*, p. 517, § 776.

not the diameter, which he sought. He has farther alluded<sup>1</sup> to the fact, that, even now, astronomers are in doubt concerning the exact mass of Uranus, inasmuch as the masses deduced from its action on Saturn and from the periods of the satellites do not harmonize with each other; and has thence inferred that the same discordance should be expected between the masses of Neptune as deduced from the perturbations of Uranus and from the satellite observations. It is an interesting question, and one which still remains open, whether the discrepancies between the two computations of the mass of Uranus might not be reconciled by a proper investigation of the influence of Neptune upon Saturn. This is one of the most important questions, connected with Neptune, which remain undecided, and it is earnestly to be hoped that some one of the three illustrious geometers who have labored so faithfully upon the perturbations of Uranus by Neptune may now investigate the Saturn-perturbations produced by the new planet.

The only other point of Mr. Le Verrier's argument to which I will allude is that in which he says,<sup>2</sup> — "The orbit calculated by Mr. Walker, from a position in 1795, and the small arc observed since the discovery, can very well be erroneous by many degrees, either in 1887 or in 1757, and if I have admitted the positions which it has given for these epochs, it is solely by courtesy, and because it presents for me no inconvenience." Of this I may be permitted to say, that Mr. Walker's laborious and accurate investigations have given us the orbit of Neptune to a very high degree of precision, and deserve the gratitude and admiration of astronomers, — not such an imputation as this. It would be contrary to all probability should the place given by Mr. Walker's orbit for those years be false by two minutes.\*

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\* Walker's orbit represents the course of Neptune as well as can be possibly desired from 1795 to 1848, an interval of 53 years. The error in 1887 or 1757 would, according to the doctrine of chances, be to that in either of the years above named in the ratio  $65^3 : 27^3 = 5''.8 : 1''.0$ , and the resulting error, therefore, less than six seconds.

<sup>1</sup> *Comptes Rendus*, XXVII. p. 278.

<sup>2</sup> *Ibid.*, p. 327.

We have seen that it represented the observations so perfectly during the opposition of 1848, as to need no correction whatsoever.

The argument<sup>1</sup> of Sir John Herschel is mostly based upon the circumstance, that the directions of the real and of the hypothetical planet were nearly identical at the time of Neptune's discovery, and upon the fact that the radius-vectors were not very different at that time. But surely it cannot be considered as an analogy between the two orbits, that the perihelion of one was so near the aphelion of the other.

Reasoning like this seems, however, utterly inapplicable to researches of such nicety and analytical refinement as characterize those upon the perturbations of Uranus. It would allow to these investigations no other merit than the success with which Neptune's apparent place was approximately predicted. It is an effort to show that the uncertainty of the calculations was so great, that Neptune's perturbative influence may be included within their limits.

The debt of gratitude which astronomy owes to Le Verrier and to Adams may not be thus diminished. The arguments, which tend to prove that Neptune is the planet of their theory, can only be based upon the supposition of error in that theory, a supposition which I am unwilling to admit. Investigations conducted with the care and precision which characterized these must not be so lightly dealt with. The combined labors of Le Verrier and Peirce have incontrovertibly proved, that, by reducing the limits of error assumed for the modern observations to 3", there can be but two possible solutions of the problem. There are two different mean distances of least possible error, — one of which is 36, and the other 30. The one is included within the theory and limits of Le Verrier, and corresponds with Adams's solution; the other is the orbit of Neptune.

This simple view of the case — a view which it seems to me impossible for those not interested in the matter to avoid taking

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<sup>1</sup> *Outlines of Astronomy*, pp. 511, 516.

— reconciles all the computations and observations, as well as the discords and contentions. It does not detract in the slightest degree from the well-earned fame of the illustrious geometers, who had arrived at a solution of the problem, and I am not aware that it has ever been opposed by mathematical reasoning.

CAMBRIDGE, *July*, 1849.



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