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Account of some circumstances historically connected with the discovery of the Planet exterior to Uranus.  
By *G. B. Airy*, Astronomer Royal \*)

It has not been usual to admit into the Memoirs of this Society mere historical statements of circumstances which have occurred in our own times. I am not aware that this is a matter of positive regulation: it is, I believe, merely a rule of practice, of which the application in every particular instance has been determined by the discretion of those Officers of the Society with whom the arrangement of our Memoirs has principally rested. And there can be no doubt that the ordinary rule must be a rule for the exclusion of papers of this character; and that if a positive regulation is to be made, it must absolutely forbid the presentation of such histories. Yet it is conceivable that events may occur in which this rule ought to be relaxed; and such, I am persuaded, are the circumstances attending the discovery of the planet exterior to Uranus. In the whole history of astronomy, I had almost said in the whole history of science, there is nothing comparable to this. The history of the discoveries of new planets in the latter part of the last century, and in the present century, offers nothing analogous to it. Uranus, Ceres, and Pallas, were discovered in the course of researches which did not contemplate the possible discovery of planets. Juno, and Vesta, were discovered in following up a series of observations suggested by a theory which, fruitful as it has been, we may almost venture to call fanciful. Astræa was found in the course of a well-conducted re-examination of the heavens, apparently contemplating the discovery of a new planet as only one of many possible results. But the motions of Uranus, examined by philosophers who were fully impressed with the universality of the law of gravitation, have long exhibited the effects of some disturbing body: mathematicians have at length ventured on the task of ascertaining where such a body could be; they have pointed out that the supposition of a disturbing body moving in a certain orbit, precisely indicated by them, would entirely explain the observed disturbances of Uranus: they have expressed their conviction, with a firmness which I must characterise as wonderful, that the disturbing planet would be found exactly

in a certain spot, and presenting exactly a certain appearance; and in that spot, and with that appearance, the planet has been found. Nothing in the whole history of astronomy can be compared with this.

The principal steps in the theoretical investigations have been made by one individual, and the published discovery of the planet was necessarily made by one individual. To these persons the public attention has been principally directed; and well do they deserve the honours which they have received, and which they will continue to receive. Yet we should do wrong if we considered that these two persons alone are to be regarded as the authors of the discovery of this planet. I am 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 labours, of various persons possessing the talents or powers best suited to the different parts of the researches.

With this conviction, it has appeared to me very desirable that the authentic history of this discovery should be published as soon as possible; not only because it will prove a valuable contribution to the history of science, but also because it may tend to do justice to some persons who otherwise would not receive in future times the credit which they deserve. And as a portion of the history, I venture to offer to this Society a statement of the circumstances which have come to my own knowledge. I have thought that I could with propriety do this: not because I can pretend to know all the history of the discovery, but because I know a considerable part of it; and because I can lay claim to the character of impartiality to this extent, that, though partaking of the general movement of the age, I have not directly contributed either to the theoretical or to the observing parts of the discovery. In a matter of this delicacy I have thought it best to act on my own judgment, without consulting any

\*) In der Königl. Astronomischen Gesellschaft am 13<sup>ten</sup> November gelesen.

other person: I have, however, solicited the permission of my English correspondents for the publication of letters.

Without pretending to fix upon a time when the conviction of the irreconcilability of the motions of Uranus with the law of gravitation first fixed itself in the minds of some individuals, we may without hesitation date the general belief in this irreconcilability from the publication of *M. Alexis Bouvard's* Tables of Uranus in 1821. It was fully shewn in the introduction to the tables, that, when every correction for perturbation indicated by the best existing theories was applied, it was still impossible to reconcile the observations of *Flamsteed*, *Lemonnier*, *Bradley*, and *Mayer*, with the orbit required by the observations made after 1781: and the elements of the orbit were adopted from the latter observations, leaving the discordances with the former (amounting sometimes to three minutes of arc) for future explanation.

The orbit thus adopted represented pretty well the observations made in the years immediately following the publication of the tables. But in five or six years the discordance again growing up became so great, that it could not escape notice. A small error was shewn by the *Kremsmünster* Observations of 1825 and 1826: but, perhaps, I am not in error in stating that the discordance was first prominently exhibited in the *Cambridge* Observations, the publication of which from 1828 was conducted under my superintendance.

While still residing at Cambridge, I received from the *Rev. T. J. Hussey* (now *Dr. Hussey*) a letter, of which the following is an extract. It will be considered, I think, as honourable to that gentleman's acuteness and zeal. I must premise that the writer had lately passed through Paris.

Nr. 1. The *Rev. T. J. Hussey* to *G. B. Airy*.

[Extract.]

Hayes, Kent, 17 November, 1834.

„With *M. Alexis Bouvard* I had some conversation upon a subject I had often meditated, which will probably interest you, and your opinion may determine mine. Having taken great pains last year with some observations of Uranus, I was led to examine closely *Bouvard's* tables of that planet. 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. My first idea was to ascertain some approximate place of this supposed body empirically, and then with my large reflector set to work to examine all the minute stars thereabouts: but I found myself totally inadequate to

the former part of the task. If I could have done it formerly, it was beyond me now, even supposing I had the time, which was not the case. I therefore relinquished the matter altogether; but 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 have not heard from him since on the subject, and have been too ill to write. What is your opinion on the subject? If you consider the idea as possible, can you give me the limits, roughly, between which this body or those bodies may probably be found during the ensuing winter? As we might expect an excentricity [inclination?] approaching rather to that of the old planets than of the new, the breadth of the Zone to be examined will be comparatively inconsiderable. I may be wrong, but I am disposed to think that, such is the perfection of my equatoreal's object-glass, I could distinguish, almost at once, the difference of light of a small planet and a star. My plan of proceeding, however, would be very different: I should accurately map the whole space within the required limits, down to the minutest star I could discern; the interval of a single week would then enable me to ascertain any change. If the whole of this matter do not appear to you a chimæra, which, until my conversation with *Bouvard*, I was afraid it might, I shall be very glad of any sort of hint respecting it.“

My answer was in the following terms: —

Nr. 2. *G. B. Airy* to the *Rev. T. J. Hussey*.

[Extract.]

„Observatory, Cambridge, 1834, Nov. 23.

„I have often thought of the irregularity of Uranus, and since the receipt of your letter have looked more carefully to it. It is a puzzling subject, but I give it as my opinion, without hesitation, that it is not yet in such a state as to give the smallest hope of making out the nature of any external action on the planet. *Flamsteed's* observations I reject (for the present) without ceremony: but the two observations by *Bradley* and *Mayer* cannot be rejected. Thus the state

of things is this, — the mean motion and other elements derived from the observations between 1781 and 1825 give considerable errors in 1750, and give *nearly the same errors* in 1834, *when the planet is at nearly the same part of its orbit*. If the mean motion had been determined by 1750 and 1834, this would have indicated nothing: but the fact is, that the mean motions were determined (as I have said) independently. This does not look like irregular perturbation. The observations would be well reconciled if we could from theory bring in two terms; one a small error in *Bouvard's* excentricity and perihelion, the other a term depending on twice the longitude. The former, of course, we could do; of the latter there are two, viz. a term in the equation of the centre, and a term in the perturbations by Saturn. The first I have verified completely (formula and numbers); the second I have verified generally, but not completely: I shall, when I have an opportunity, look at it thoroughly. So much for my doubts as to the certainty of any extraneous action. But if it were certain that there were any extraneous action, I doubt much the possibility of determining the place of a planet which produced it. I am sure it could not be done till the nature of the irregularity was well determined from several successive revolutions."

It will readily be understood that I do not quote this letter as a testimony to my own sagacity; but I think it deserving of production, as shewing the struggle which was made twelve years ago to explain the motions of Uranus, and the difficulty which seemed to envelope the subject. With regard to my last sentence, I think it likely that the same difficulty would still have been felt, if the theorists who entered seriously upon the explanation of the perturbations had not trusted more confidently to *Bode's* law of distances than I did myself.

In the year 1836, having quitted the Observatory of Cambridge, I completed the reduction of the planetary observations made there during the years 1833, 1834, 1835, in such a form as to exhibit the heliocentric errors of the tabular places of Uranus, together with the effect of errors of the tabular radius vector. The memoir containing these reductions was subsequently printed in the *Memoirs* of this Society. The progress of the errors of the tables of Uranus was here clearly marked.

In 1837, I received from *M. Eugène Bouvard* a letter, from which I trust I may be permitted to make an extract. It will, I am certain, be received as creditable to the intelligence and industry of the astronomers of the Observatory of Paris.

Nr. 3. *M. Eugène Bouvard* to *G. B. Airy*.

[Extract.]

„Paris, ce 6 Octobre, 1837.

„Dans le peu de moments de loisir que me laissent mes fonctions, je m'occupe d'un travail que je crois n'être pas sans importance. Mon oncle [*M. Alexis Bouvard*] travaille à réfaire ses tables de Jupiter et de Saturne, en se servant des corrections apportées récemment aux éléments astronomiques. Il m'a cédé les tables d'Uranus à reconstruire. En consultant les comparaisons que vous avez fait des observations de cette planète avec les calculs des tables, on voit que les différences en latitude sont très-grandes et qu'elles vont toujours en augmentant. Cela tient-il à une perturbation inconnue apportée dans les mouvemens de cet astre par un corps situé au-delà? Je ne sais, mais c'est du moins l'idée de mon oncle. Je regarde la solution de cette question comme fort importante. Mais, pour réussir, j'ai besoin de réduire les observations avec la plus grande précision, et souvent les moyens me manquent."

The remainder of this letter relates principally to the reduction of observations.

The following are extracts from my answer: —

Nr. 4. *G. B. Airy* to *M. Eugène Bouvard*.

[Extract.]

„Royal Observatory, Greenwich, 1837, Oct. 12.

„I think that, probably, you would gain much in the accuracy of the reduced observations by waiting a short time before you proceed with that part of your labour. Some time ago, I presented to the Astronomical Society of London a very complete reduction of the observations of all the planets made at Cambridge in the years 1833, 1834, 1835. This paper will, as I expect, very shortly be printed. I have reduced the observations made at Greenwich in 1836 in the same manner: the volume containing these reductions will very soon be published. \* \* \* You may also know that I am engaged upon a general reduction of the observations of planets made at Greenwich, from the commencement of *Bradley's* observations to the present time. It may, perhaps, be a year before I can furnish you with the places deduced from these observations. \* \* \* With respect to the errors of the tables of Uranus, I think you will find that it is the longitude which is most defective, and that the errors in latitude are not at present increasing. To shew this, I set down a few of my results. \* \* \* You will see by this statement that the errors of longitude are increasing with fearful rapidity, while those of latitude are nearly stationary. \* \* \* I cannot conjecture what is the cause of these errors, but I am inclined,

in the first instance, to ascribe them to some error in the perturbations. There is no error in the pure elliptic theory (as I found by examination some time ago). If it be the effect of any unseen body, it will be nearly impossible ever to find out its place."

On the 24th of February, 1838, I addressed a letter to M. *Schumacher*, which is printed in the *Astronomische Nachrichten*, Nr. 349. In this letter it is shewn, by treatment of the results of the reduced observations of 1833, 1834, 1835, 1836 (to which allusion was made in my letter to M. *Eugène Bouvard*), that the tabular radius vector of Uranus was considerably too small. This deduction (which has been confirmed by the observations of all the subsequent years) has always appeared to me to be very important. It is, perhaps, worth while here to point out that the detection of this error arose, in the first place, from the circumstance that my observations of Uranus had not been confined to the mere opposition (as had too often been done), but had been extended, as far as possible, to quadratures; and, in the next place, from my having so reduced the observations as to exhibit the effect of error of the radius vector.

On the 14th of May, 1838, I transmitted to M. *Eugène Bouvard* the reduced observations of 1833, 1834, 1835, 1836; and referred him to the paper in the *Astronomische Nachrichten* which I have cited.

The following letter from M. *Eugène Bouvard* will shew how vigorously the attention of the astronomers of Paris was still directed to Uranus: —

Nr. 5. M. *Eugène Bouvard* to G. B. *Airy*.

[Extract.]

„Paris, ce 21 Mai, 1844.

„ \* \* \* Je viens aujourd'hui vous prier de me communiquer, si c'est possible, les ascensions droites et les déclinaisons d'Uranus depuis 1781 jusqu'en 1800. \* \* \* J'ai réduit moi-même toutes ces observations en m'en tenant aux élémens imprimés, mais je crains qu'il n'y ait quelques erreurs. Il y a surtout une telle incertitude sur les erreurs de collimation du quart de cercle depuis 1785 jusqu'en 1800, qu'il est presque impossible d'avoir une grande confiance dans les observations. \* \* \* Mon travail est fort avancé. Je suis arrivé à des résultats fort bons déjà, puisque je satisfais aux observations actuelles et aux premières de 1781, 1782 &c., à 15" de degré près en longitude: tandis que d'après les tables de mon oncle les erreurs sont de près de 2' de degré actuellement. Si je mettais de côté les observations de Maskelyne faites depuis 1785 jusqu'à 1796, mes tables pourraient satisfaire aux observations à 7" ou 8" près. Mais je crains que

cette période ne m'empêche d'y parvenir; et malheureusement c'est dans cette intervalle que les observations sont le plus défectueuses. \* \* \* D'après mes calculs, il faut changer considérablement les élémens elliptiques d'*Herschel*, surtout le moyen mouvement et le périhélie. J'ai déterminé aussi la masse de Saturne, et je la trouve très différente de celle que l'on admet; il faut l'augmenter beaucoup. Mais j'attendrai une nouvelle approximation pour être tout à fait sûr de ma détermination."

After some further correspondence, I transmitted to M. *Eugène Bouvard*, on June 27, 1844, the proof-sheets of the *Planetary Reductions*, containing the Right Ascensions and North Polar distances of Uranus; and M. *Bouvard*, in acknowledging the receipt of them, on July 1, 1844, pointed out an error in the refraction for June 15, 1819. I mention this to shew the extreme care with which M. *E. Bouvard's* collateral calculations had been conducted.

Although no allusion is made in the last letter to the possible disturbing planet, it would be wrong to suppose that there was no thought of it. In fact, during the whole of these efforts for reforming the tables of Uranus, the dominant thought was, „Is it possible to explain the motions of Uranus, without admitting either a departure from the received law of attraction, or the existence of a disturbing planet?" I know not how far the extensive and accurate calculations of M. *Eugène Bouvard* may have been used in the subsequent French calculations, but I have no doubt whatever that the knowledge of the efforts of M. *Bouvard*, the confidence in the accuracy of his calculations, and the perception of his failure to reconcile in a satisfactory way the theory and the observations, have tended greatly to impress upon astronomers, both French and English, the absolute necessity of seeking some external cause of disturbance.

I have departed from a strictly chronological order for the sake of keeping in connexion the papers which relate to the same trains of investigation. Several months before the date of the last letter quoted, I had received the first intimation of those calculations which have led to a distinct indication of the place where the disturbing planet ought to be sought. The date of the following letter is Feb. 13, 1844: —

Nr. 6. Professor *Challis* to G. B. *Airy*.

[Extract.]

„Cambridge Observatory, Feb. 13, 1844.

„A young friend of mine, Mr. *Adams*, of St. *John's* College, is working at the theory of Uranus, and is desirous

of obtaining errors of the tabular geocentric longitudes of this planet, when near opposition, in the years 1818—1826, with the factors for reducing them to errors of heliocentric longitude. Are your reductions of the planetary observations so far advanced that you could furnish these data? and is the request one which you have any objection to comply with? If Mr. *Adams* may be favoured in this respect, he is further desirous of knowing, whether in the calculation of the tabular errors any alterations have been made in *Bouvard's* Tables of Uranus besides that of Jupiter's mass."

My answer was as follows: —

Nr. 7. *G. B. Airy* to Professor *Challis*.

[Extract.]

„Royal Observatory, Greenwich, 1844, Feb. 15.

„I send all the results of the observations of Uranus made with both instruments [that is, the heliocentric errors of Uranus in longitude and latitude from 1754 to 1830, for all those days on which there were observations, both of right ascension and of polar distance]. No alteration is made in *Bouvard's* Tables of Uranus, except increasing the two equations which depend on Jupiter by  $\frac{1}{50}$  part. As constants have been added (in the printed tables) to make the equations positive, and as  $\frac{1}{50}$  part of the numbers in the tables has been added,  $\frac{1}{50}$  part of the constants has been subtracted from the final results.

Professor *Challis*, in acknowledging the receipt of these, used the following expressions: —

Nr. 8. Professor *Challis* to *G. B. Airy*.

[Extract.]

„Cambridge Observatory, Feb. 16, 1844.

„I am exceedingly obliged by your sending so complete a series of tabular errors of Uranus. \* \* \* The list you have sent will give Mr. *Adams* the means of carrying on in the most effective manner the inquiry in which he is engaged."

The next letter shews that Mr. *Adams* had derived results from these errors.

Nr. 9. Professor *Challis* to *G. B. Airy*.

„Cambridge Observatory, Sept. 22, 1845.

„My friend Mr. *Adams* (who will probably deliver this note to you) has completed his calculations respecting the perturbation of the orbit of Uranus by a supposed ulterior

planet, and has arrived at results which he would be glad to communicate to you personally, if you could spare him a few moments of your valuable time. His calculations are founded on the observations you were so good as to furnish him with some time ago; and from his character as a mathematician, and his practice in calculation, I should consider the deductions from his premises to be made in a trustworthy manner. If he should not have the good fortune to see you at Greenwich, he hopes to be allowed to write to you on this subject."

On the day on which this letter was dated, I was present at a meeting of the French Institute. I acknowledged it by the following letter: —

Nr. 10. *G. B. Airy* to Professor *Challis*.

„Royal Observatory, Greenwich, 1845, Sept. 29.

„I was, I suppose, on my way from France, when Mr. *Adams* called here: at all events, I had not reached home, and therefore, to my regret, I have not seen him. Would you mention to Mr. *Adams* that I am very much interested with the subject of his investigations, and that I should be delighted to hear of them by letter from him?"

On one of the last days of October, 1845, Mr. *Adams* called at the Royal Observatory, Greenwich, in my absence, and left the following important paper: —

Nr. 11. *J. C. Adams*, Esq. to *G. B. Airy*.

„According to my calculations, the observed irregularities in the motion of Uranus may be accounted for by supposing the existence of an exterior planet, the mass and orbit of which are 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, 1st October, 1845 ....	323 34
Longitude of Perihelion .....	315 55
Excentricity .....	0,1610
Mass (that of the Sun being unity) ....	0,0001656.

For the modern observations I have used the method of normal places, taking the mean of the tabular errors, as given by observations near three consecutive oppositions, to correspond with the mean of the times; and the Greenwich observations have been used down to 1830: since which, the Cambridge and Greenwich observations, and those given in the

Astronomische Nachrichten, have been made use of. The following are the remaining errors of mean longitude: —

Observation — Theory.

1780	+0 <sup>''</sup> 27	1801	—0 <sup>''</sup> 04	1822	+0 <sup>''</sup> 30
1783	—0,23	1804	+1,76	1825	+1,92
1786	—0,96	1807	—0,21	1828	+2,25
1789	+1,82	1810	+0,56	1831	—1,06
1792	—0,91	1813	—0,94	1834	—1,44
1795	+0,09	1816	—0,31	1837	—1,62
1798	—0,99	1819	—2,00	1840	+1,73

The error for 1780 is concluded from that for 1781 given by observation, compared with those of four or five following years, and also with *Lemnier's* observations in 1769 and 1771.

„For the ancient observations, the following are the remaining errors: —

Observation — Theory.

1690	+44 <sup>''</sup> 4	1750	—1 <sup>''</sup> 6	1763	—5 <sup>''</sup> 1
1712	+6,7	1753	+5,7	1769	+0,6
1715	—6,8	1756	—4,0	1771	+11,8

The errors are small, except for *Flamsteed's* observation of 1690. This being an isolated observation, very distant from the rest, I thought it best not to use it in forming the equations of condition. It is not improbable, however, that this error might be destroyed by a small change in the assumed mean motion of the planet.“

I acknowledged the receipt of this paper in the following terms: —

Nr. 12. *G. B. Airy* to *J. C. Adams*, Esq.

„Royal Observatory, Greenwich, 1845, Nov. 5.

„I am very much obliged by the paper of results which you left here a few days since, shewing the perturbations on the place of Uranus produced by a planet with certain assumed elements. The latter numbers are all extremely satisfactory: I am not enough acquainted with *Flamsteed's* observations about 1690 to say whether they bear such an error, but I think it extremely probable.

„But I should be very glad to know whether this assumed perturbation will explain the error of the radius vector of Uranus. This error is now very considerable, as you will be able to ascertain by comparing the normal equations, given in the Greenwich observations for each year, for the times before opposition with the times after opposition.“

I have before stated, that I considered the establishment of this error of the radius vector of Uranus to be a very important determination. I therefore considered that the trial,

whether the error of radius vector would be explained by the same theory which explained the error of longitude, would be truly an *experimentum crucis*. And I waited with much anxiety for Mr. *Adams's* answer to my query. Had it been in the affirmative, I should at once have exerted all the influence which I might possess, either directly, or indirectly through my friend Professor *Challis*, to procure the publication of Mr. *Adams's* theory. \*)

From some cause with which I am unacquainted, probably an accidental one, I received no immediate answer to this inquiry. I regret this deeply, for many reasons.

While I was expecting more complete information on Mr. *Adams's* theory, the results of a new and most important investigation reached me from another quarter. In the *Compte Rendu* of the French Academy for the 10th of November, 1845, which arrived in this country in December, there is a paper by M. *Le Verrier* on the perturbations of Uranus produced by Jupiter and Saturn, and on the errors in the elliptic elements of Uranus, consequent on the use of erroneous perturbations in the treatment of the observations. It is impossible for me here to enter into details as to the conclusions of this valuable memoir: I shall only say that, while the correctness of the former theories, as far as they went, was generally established, many small terms were added; that the accuracy of the calculations was established by duplicate investigations, following different courses, and executed with extraordinary labour; that the corrections to the elements, produced by treating the former observations with these corrected perturbations, were obtained; and that the correction to the ephemeris for the present time, produced by the introduction of the new perturbations and the new elements, was investigated, and found to be incapable of explaining the observed irregularity of Uranus. Perhaps it may be truly said that the theory of Uranus was now, for the first time, placed on a satisfactory foundation. This important labour, as M. *Le Verrier* states, was undertaken at the urgent request of M. *Arago*.

In the *Compte Rendu* for June 1, 1846, M. *Le Verrier* gave his second memoir on the theory of Uranus. The first part contains the results of a new reduction of nearly all the existing observations of Uranus, and their treatment with reference to the theory of perturbations, as amended in the former memoir. After concluding from this reduction that the

\*) Here the Astronomer Royal explained to the meeting, by means of a diagram, the nature of the errors of the tabular radius vector.

observations are absolutely irreconcilable with the theory. M. *Le Verrier* considers in the second part all the possible explanations of the discordance, and concludes that none is admissible, except that of a disturbing planet exterior to Uranus. He then proceeds to investigate the elements of the orbit of such a planet, assuming that its mean distance is double that of Uranus, and that its orbit is in the plane of the ecliptic. The value of the mean distance, it is to be remarked, is not fixed entirely by *Bode's* law, although suggested by it; several considerations are stated which compel us to take a mean distance, not very greatly differing from that suggested by the law, but which nevertheless, without the suggestions of that law, would leave the mean distance in a most troublesome uncertainty. The peculiarity of the form which the investigation takes is then explained. Finally, M. *Le Verrier* gives as the most probable result of his investigations, that the true longitude of the disturbing planet for the beginning of 1847 must be about  $325^\circ$ , and that an error of  $10^\circ$  in this place is not probable. No elements of the orbit or mass of the planet are given.

This memoir reached me about the 23d or 24th of June. I cannot sufficiently express the feeling of delight and satisfaction which I received from it. The place which it assigned to the disturbing planet was the same, to one degree, as that given by Mr. *Adams's* calculations, which I had perused seven months earlier. To this time I had considered that there was still room for doubt of the accuracy of Mr. *Adams's* investigations; for I think that the results of algebraic and numerical computations, so long and so complicated as those of an inverse problem of perturbations, are liable to many risks of error in the details of the process: I know that there are important numerical errors in the *Mécanique Céleste* of Laplace; in the *Théorie de la Lune* of Plana; above all, in *Bouvard's* first tables of Jupiter and Saturn; and to express it in a word, I have always considered the correctness of a distant mathematical result to be a subject rather of moral than of mathematical evidence. But now I felt no doubt of the accuracy of both calculations, as applied to the perturbation in longitude. I was, however, still desirous, as before, of learning whether the perturbation in radius vector was fully explained. I therefore addressed to M. *Le Verrier* the following letter: —

Nr. 13. *G. B. Airy* to M. *Le Verrier*.

„Royal Observatory, Greenwich, 1846, June 26.

„I have read, with very great interest, the account of your investigations on the probable place of a planet disturbing the motions of Uranus, which is contained in the *Compte*

*Rendu de l'Académie* of June 1; and I now beg leave to trouble you with the following question. It appears, from all the later observations of Uranus made at Greenwich (which are most completely reduced in the Greenwich Observations of each year, so as to exhibit the effect of an error either in the tabular heliocentric longitude, or the tabular radius vector), that the tabular radius vector is considerably too small. And I wish to inquire of you whether this would be a consequence of the disturbance produced by an exterior planet, now in the position which you have indicated?

„I imagine that it would not be so, because the principal term of the inequality would probably be analogous to the Moon's variation, or would depend on  $\sin 2(v-v')$ ; and in that case the perturbation in radius vector would have the sign — for the present relative position of the planet and Uranus. But this analogy is worth little, until it is supported by proper symbolical computations.

„By the earliest opportunity I shall have the honour of transmitting to you a copy of the Planetary Reductions, in which you will find all the observations made at Greenwich to 1830 carefully reduced and compared with the tables.“

Before I could receive M. *Le Verrier's* answer, a transaction occurred which had some influence on the conduct of English astronomers.

On the 29th of June, a meeting of the Board of Visitors of the Royal Observatory of Greenwich was held, for the consideration of special business. At this meeting, Sir *J. Herschel* and Professor *Challis* (among other members of the Board) were present; I was also present, by invitation of the Board. The discussion led, incidentally, to the general question of the advantage of distributing subjects of observation among different observatories. I spoke strongly in favour of such distribution; and I produced, as an instance, the extreme probability of now discovering a new planet in a very short time, provided the powers of one observatory could be directed to the search for it. I gave, as the reason upon which this probability was based, the very close coincidence between the results of Mr. *Adams's* and M. *Le Verrier's* investigations of the place of the supposed planet disturbing Uranus. I am authorised by Sir *J. Herschel's* printed statement in the *Athenæum* of October 3, to ascribe to the strong expressions which I then used the remarkable sentence in Sir *J. Herschel's* address, on September 10, to the British Association assembled at Southampton. „We see it [the probable new planet] as *Columbus* saw America from the shores of Spain. Its movements have been felt, trembling along the far-reaching line of our analysis, with a certainty hardly

inferior to that of ocular demonstration.“\*) And I am authorised by Professor *Challis*, in oral conversation, to state that the same expressions of mine induced him to contemplate the search for the suspected planet.

M. *Le Verrier's* answer reached me, I believe, on the 1st of July. The following are extracts from it: —

Nr. 14. M. *Le Verrier* to G. B. *Airy*.

[Extract.]

„Paris. 28 Juin, 1846.

„ \* \* \* Il a toujours été dans mon désir de vous en écrire, aussi qu'à votre savante Société. Mais j'attendais, pour cela, que mes recherches fussent complètes, et ainsi moins indignes de vous être offertes. Je compte avoir terminé la rectification des éléments de la planète troublante avant l'opposition qui va arriver; et parvenir à connaître ainsi les positions du nouvel astre avec une grande précision. Si je pouvais espérer que vous aurez assez de confiance dans mon travail pour chercher cette planète dans le ciel, je m'empresserais, Monsieur, de vous envoyer sa position exacte, dès que je l'aurai obtenue.

„La comparaison des positions d'Uranus, observées dans ces dernières années, dans les oppositions et dans les quadratures, montre que le rayon de la planète, calculé par les tables en usage, est effectivement très-inexact. Cela n'a pas lieu dans mon orbite, telle que je l'ai déterminée; il n'y a pas plus d'erreur dans les quadratures que dans les oppositions.

„Le rayon est donc bien calculé dans mon orbite; et, si je ne me trompe, M. *Airy* désirerait savoir quelle est la nature de la correction que j'ai fait subir à cet égard aux tables en usage?

„Vous avez raison, Monsieur, de penser que cette correction n'est pas due à la perturbation du rayon vecteur produite actuellement par la planète troublante. Pour s'en rendre un compte exact, il faut remarquer que l'orbite d'Uranus a été calculée par M. *Bouvard* sur des positions de la planète qui n'étaient pas les positions elliptiques, puisqu'on n'avait pas pu avoir égard aux perturbations produites par la planète inconnue. Cette circonstance a nécessairement rendu les éléments de l'ellipse faux, et c'est à l'erreur de l'excentricité et

à l'erreur de la longitude du périhélie qu'il faut attribuer l'erreur actuelle du rayon vecteur d'Uranus.

„Il résulte de ma théorie que l'excentricité donnée par M. *Bouvard* doit être augmentée, et qu'il en est de même de la longitude du périhélie; deux causes qui contribuent, à cause de la position actuelle de la planète dans son orbite, à augmenter le rayon vecteur. Je ne transcris pas ici les valeurs de ces accroissements, parceque je ne les ai pas encore avec toute la rigueur précise, mais je les aurai rectifié avant un mois, et je me ferai un devoir, Monsieur, de vous les transmettre aussitôt, si cela vous est agréable.

„Je me bornerai à ajouter que la position en quadrature, déduite en 1844 des deux oppositions qui la comprennent, au moyen de mes formules, ne diffère de la position observée que de 0"6; ce qui prouve que l'erreur du rayon vecteur est entièrement disparue.

„C'est même une des considérations qui devront donner plus de probabilité à la vérité de mes résultats, qu'ils rendent un compte scrupuleux de toutes les circonstances du problème. Ainsi, bien que je n'aye fait usage dans mes premières recherches que des oppositions, les quadratures n'ont pas laissé de se trouver calculées avec toute l'exactitude possible. Le rayon vecteur s'est trouvé rectifié de lui-même, sans que l'on l'eût pris en considération d'une manière directe. Excusez-moi, Monsieur, d'insister sur ce point. C'est une suite du désir que j'ai d'obtenir votre suffrage.

„Je recevrai avec bien du plaisir les observations que vous voulez bien m'annoncer. Malheureusement le temps presse; l'opposition approche; il faut de toute nécessité que j'aye fini pour cette époque. Je ne pourrai donc pas comprendre ces observations dans mon travail. Mais elles me seront très-utiles pour me servir de vérifications; et c'est ce à quoi je les employerai certainement.“

It is impossible, I think, to read this letter without being struck with its clearness of explanation, with the writer's extraordinary command, not only of the physical theories of perturbation but also of the geometrical theories of the deduction of orbits from observation, and with his perception that his theory ought to explain all the phenomena, and his firm belief that it had done so. I had now no longer any doubt upon the reality and general exactness of the prediction of the planet's place. My approaching departure for the Continent made it useless for me to trouble M. *Le Verrier* with a request for the more accurate numbers to which he alludes; but the following correspondence will shew how deeply his remarks had penetrated my mind.

(Fortsetzung folgt.)

\*) This sentence is copied from the written draft of the speech. Sir *J. Herschel* appeared to suppose that the sentence had not been reported in the public journals as spoken. I did, however, see it so reported in an English newspaper, to which I had access on the Continent.



# ASTRONOMISCHE NACHRICHTEN.

N<sup>o</sup>. 586.

Account of some circumstances historically connected with the discovery of the Planet exterior to Uranus.  
By *G. B. Airy*, Astronomer Royal  
(Fortsetzung).

About a week after the receipt of *M. Le Verrier's* letter, while on a visit to my friend the Dean of Ely, I wrote to Professor *Challis* as follows: —

Nr. 15. *G. B. Airy* to Professor *Challis*.

„The Deanery, Ely, 1846, July 9.

„You know that I attach importance to the examination of that part of the heavens in which there is \* \* \* \* \* reason for suspecting the existence of a planet exterior to Uranus. I have thought about the way of making such examination, but I am convinced that (for various reasons, of declination, latitude of place, feebleness of light, and regularity of superintendence) there is no prospect whatever of its being made with any chance of success, except with the Northumberland Telescope.

„Now I should be glad to ask you, in the first place, whether you could make such an examination?

„Presuming that your answer would be in the negative, I would ask, secondly, whether, supposing that an assistant were supplied to you for this purpose, you would superintend the examination?

„You will readily perceive that all this is in a most unformed state at present, and that I am asking these questions almost at a venture, in the hope of rescuing the matter from a state which is, without the assistance that you and your instruments can give, almost desperate. Therefore I should be glad to have your answer, not only responding simply to my questions, but also entering into any other considerations which you think likely to bear on the matter.

„The time for the said examination is approaching near.“

In explanation of this letter, it may be necessary to state that, in common, I believe, with other astronomers at that time, I thought it likely that the planet would be visible only in large telescopes. I knew that the Observatory of Cambridge was at this time oppressed with work, and I thought that the undertaking — a survey of such an extent as this seemed

likely to prove — would be entirely beyond the powers of its personal establishment. Had Professor *Challis* assented to my proposal of assistance, I was prepared immediately to place at his disposal the services of an efficient assistant; and for approval of such a step, and for liquidation of the expense which must thus be thrown on the Royal Observatory, I should have referred to a Government which I have never known to be illiberal when demands for the benefit of science were made by persons whose character and position offered a guarantee, that the assistance was fairly asked for science, and that the money would be managed with fair frugality. In the very improbable event of the Government refusing such indemnity, I was prepared to take all consequences on myself.

On the 13th. of July, I transmitted to Professor *Challis* „Suggestions for the examination of a portion of the Heavens in search of the external Planet which is presumed to exist and to produce disturbance in the motion of Uranus,“ and I accompanied them with the following letter: —

Nr. 16. *G. B. Airy* to Professor *Challis*.

„Royal Observatory, Greenwich, 1846, July 13.

„I have drawn up the enclosed paper, in order to give you a notion of the extent of work incidental to a sweep for the possible planet.

„I only add at present that, in my opinion, the importance of this inquiry exceeds that of any current work, which is of such a nature as not to be totally lost by delay.“

My „Suggestions“ contemplated the examination of a part of the heavens 30° long, in the direction of the ecliptic, and 10° broad. They entered into considerable details as to the method which I proposed; details which were necessary, in order to form an estimate of the number of hours' work likely to be employed in the sweep.

I received, in a few days, the following answer: —

Nr. 17. Professor *Challis* to *G. B. Airy*.

[Extracts.]

„Cambridge Observatory, July 18, 1846.

„I have only just returned from my excursion. \* \* \* I have determined on sweeping for this hypothetical planet. \* \* \* With respect to your proposal of supplying an assistant I need not say any thing, as I understand it to be made on the supposition that I decline undertaking the search myself. \* \* \* I purpose to carry the sweep to the extent you recommend.“

The remainder of the letter was principally occupied with the details of a plan of observing different from mine, and of which the advantage was fully proved in the practical observation.

On August 7, Professor *Challis*, writing to my confidential assistant (Mr. *Main*) in my supposed absence, said, —

Nr. 18. Professor *Challis* to the Rev. *R. Main*.

[Extract.]

„Cambridge Observatory, August 7, 1846.

„I have undertaken to search for the supposed new planet more distant than Uranus. Already I have made trial of two different methods of observing. In one method, recommended by Mr. *Airy* \* \* \* I met with a difficulty which I had anticipated. \* \* \* I adopted a second method.“

From a subsequent letter (to be cited hereafter), it appears that Professor *Challis* had commenced the search on July 29, and had actually observed the planet on August 4, 1846.

Mr. *Main*'s answer to the other parts of this letter, written by my direction, is dated August 8.

At Wiesbaden (which place I left on September 7), I received the following letter from Professor *Challis*: —

Nr. 19. Professor *Challis* to *G. B. Airy*.

[Extract.]

„Cambridge Observatory, Septb. 2, 1846.

„I have lost no opportunity of searching for the planet; and, the nights having been generally pretty good, I have taken a considerable number of observations: but I get over the ground very slowly, thinking it right to include all stars to 10-11 magnitude; and I find, that to scrutinise, thoroughly, in this way the proposed portion of the heavens, will require many more observations than I can take this year.“

On the same day on which Professor *Challis* wrote this letter, Mr. *Adams*, who was not aware of my absence from England, addressed the following very important letter to Greenwich: —

Nr. 20. *J. C. Adams*, Esq. to *G. B. Airy*.

„St. John's College, Cambridge, Sept. 2, 1846.

„In the investigation, the results of which I communicated to you last October, the mean distance of the supposed disturbing planet is assumed to be twice that of Uranus. Some assumption is necessary in the first instance, and Bode's law renders it probable that the above distance is not very remote from the truth: but the investigation could scarcely be considered satisfactory while based on any thing arbitrary; and I therefore determined to repeat the calculation, making a different hypothesis as to the mean distance. The eccentricity also resulting from my former calculations was far too large to be probable; and I found that, although the agreement between theory and observation continued very satisfactory down to 1840, the difference in subsequent years was becoming very sensible, and I hoped that these errors, as well as the eccentricity, might be diminished by taking a different mean distance. Not to make too violent a change, I assumed this distance to be less than the former value by about  $\frac{1}{30}$ th part of the whole. The result is very satisfactory, and appears to shew that, by still further diminishing the distance, the agreement between the theory and the later observations may be rendered complete, and the eccentricity reduced at the same time to a very small quantity. The mass and the elements of the orbit of the supposed planet, which result from the two hypotheses, are as follows: —

	Hypothesis I.	Hypothesis II.
	$\left(\frac{a}{a^{\text{T}}} = 0,5\right)$	$\left(\frac{a}{a^{\text{T}}} = 0,515\right)$
Mean Longit. of Planet 1st Oct. 1846	325° 8'	323° 2'
Longitude of Perihelion . . . . .	315 57	299 11
Eccentricity . . . . .	0,16103	0,12062
Mass (that of Sun being 1) . . . .	0,00016563.	0,00015003

„The investigation has been conducted in the same manner in both cases, so that the differences between the two sets of elements may be considered as wholly due to the variation of the fundamental hypothesis. The following table exhibits the differences between the theory and the observations which were used as the basis of calculation. The quantities given are the errors of mean longitude, which I found it more convenient to employ in my investigations than those of the true longitude.

## Ancient Observations.

Date.	(Obs. — Theory.)		Date.	(Obs. — Theory.)	
	Hypoth. I.	Hypoth. II.		Hypoth. I.	Hypoth. II.
1712	+6"7	+6"3	1756	— 4"0	— 4"0
1715	—6,8	—6,6	1764	— 5,1	— 4,1
1750	—1,6	—2,6	1769	+ 0,6	+ 1,8
1753	+5,7	+5,2	1771	+11,8	+12,8

## Modern Observations.

1780	+0,27	+0,54	1810	+0,56	+0,61
1783	—0,23	—0,21	1813	—0,94	—1,00
1786	—0,96	—1,10	1816	—0,31	—0,46
1789	+1,82	+1,63	1819	—2,00	—2,19
1792	—0,91	—1,06	1822	+0,30	+0,14
1795	+0,09	+0,04	1825	+1,92	+1,87
1798	—0,99	—0,93	1828	+2,25	+2,35
1801	—0,04	+0,11	1831	—1,06	—0,82
1804	+1,76	+1,94	1834	—1,44	—1,17
1807	—0,21	—0,08	1837	—1,62	—1,53
1810	+0,56	+0,61	1840	+1,73	+1,31

„The greatest difference in the above table, viz. that for 1771, is deduced from a single observation, whereas the difference immediately preceding, which is deduced from the mean of several observations, is much smaller. The error of the tables for 1780 is found by interpolating between the errors given by the observations of 1781, 1782, and 1783, and those of 1769 and 1771. The differences between the results of the two hypotheses are exceedingly small till we come to the last years of the series, and become sensible precisely at the point where both sets of results begin to diverge from the observations; the errors corresponding to the second hypothesis being, however, uniformly smaller. The errors given by the Greenwich Observations of 1843 are very sensible, being for the first hypothesis +6"84, and for the second +5"50. By comparing these errors, it may be inferred that the agreement of theory and observation, would be rendered very close by assuming  $\frac{a}{a_1} = 0,57$ , and the corresponding mean longitude on the 1st October, 1846, would be about  $315^{\circ}20'$ , which I am inclined to think is not far from the truth. It is plain also that the eccentricity corresponding to this value of  $\frac{a}{a_1}$ , would be very small. In consequence of the divergence of the results of the two hypotheses still later observations would be most valuable for correcting the distances, and I should feel exceedingly obliged if you would kindly communicate to me two normal places near the oppositions of 1844 and 1845.

„As *Flamsteed's* first observation of Uranus (in 1690) is a single one, and the interval between it and the rest is so large, I thought it unsafe to employ this observation in for-

ming the equations of condition. On comparing it with the theory, I find the difference to be rather large, and greater for the second hypothesis than for the first, the errors being +44"5 and +50"0 respectively. If the error be supposed to change in proportion to the change of mean distance, its value corresponding to  $\frac{a}{a_1} = 0,57$ , will be about +70", and the error in the time of transit will be between 4<sup>s</sup> and 5<sup>s</sup>. It would be desirable to ascertain whether *Flamsteed's* manuscripts throw any light on this point.

„The corrections of the tabular radius vector of Uranus, given by the theory for some late years, are as follows: —

Date.	Hypoth. I.	Hypoth. II.
1834	+0,005051	+0,004923
1840	+0,007219	+0,006962
1846	+0,008676	+0,008250

„The correction for 1834 is very nearly the same as that which you have deduced from observation, in the *Astronomische Nachrichten*; but the increase in later years is more rapid than the observations appear to give it: the second hypothesis, however, still having the advantage.

„I am at present employed in discussing the errors in latitude, with the view of obtaining an approximate value of the inclination and position of the node of the new planet's orbit; but the perturbations in latitude are so very small that I am afraid the result will not have great weight. According to a rough calculation made some time since, the inclination appeared to be rather large, and the longitude of the ascending node to be about  $300^{\circ}$ ; but I am now treating the subject much more completely, and hope to obtain the result in a few days.

„I have been thinking of drawing up a brief account of my investigations to present to the British Association.“

Mr. *Main*, acting for the Astronomer Royal in his absence, answered this letter as follows: —

Nr. 21. The Rev. *R. Main* to *J. C. Adams*, Esq.

„Royal Observatory, Greenwich, 1846, Sept. 5.

„The Astronomer Royal is not at home, and he will be absent for some time; but it appears to me of so much importance that you should have immediately the normal errors of Uranus for 1844 and 1845, that I herewith send you the former (the volume for 1844 has been published for some time), and I shall probably be able to send you those for 1845 on Tuesday next, as I have given directions to have the computations finished immediately. If a place (geocentric)

for the present year should be of value to you, I could probably send one in a few days."

In acknowledging this letter, Mr. *Adams* used the following expression: —

Nr. 22. *J. C. Adams*, Esq. to the Rev. *R. Main*.

[Extract.]

St. John's College, Cambridge, 7th Sept. 1846.

„I hope by to-morrow to have obtained approximate values of the inclination and longitude of the node.“

On the same day, Sept. 7, Mr. *Main* transmitted to Mr. *Adams* the normal places for 1845, to which allusion was made in the letter of Sept. 5.

On the 31st. of August, M. *Le Verrier's* second paper on the place of the disturbing planet (the third paper on the motion of Uranus) was communicated to the French Academy. I place the notice of this paper after those of September 2, &c. because, in the usual course of transmission to this country, the No. of the *Comptes Rendus* containing this paper would not arrive here, at the earliest, before the third or fourth week in September; and it does not appear that any earlier notice of its contents was received in England.

It is not my design here to give a complete analysis of this remarkable paper: but I may advert to some of its principal points. M. *Le Verrier* states that, considering the extreme difficulty of attempting to solve the problem in all its generality, and considering that the mean distance and the epoch of the disturbing planet were determined approximately by his former investigations, he adopted the corrections to these elements as two of the unknown quantities to be investigated. Besides these, there are the planet's mass, and two quantities from which the excentricity and the longitude of perihelion may be inferred; making, in all, five unknown quantities depending solely on the orbit and mass of the disturbing planet. Then there are the possible corrections to the mean distance of Uranus, to its epoch of longitude, to its longitude of perihelion, and to its excentricity; making, in all, nine unknown quantities. To obtain these, M. *Le Verrier* groups all the observations into thirty-three equations. He then explains the peculiar method by which he derives the values of the unknown quantities from these equations. The elements obtained are, —

Semi-axis Major .....	36,154	(or $\frac{a}{a^1} = 0,531$ )
Periodic Time .....	217 <sup>y</sup> 387	
Excentricity .....	0,10761	
Longitude of Perihelion .....	284°45'	
Mean Longitude, 1 Jan. 1847..	318 47	
Mass $\frac{1}{9300} =$	0,0001075	
True Heliocentric Longitude 1 Jan. 1847	326°32'	
Distance from the Sun .....	33,06	

It is interesting to compare these elements with those obtained by Mr. *Adams*. The difference between each of these and the corresponding element obtained by Mr. *Adams* in his second hypothesis is, in every instance, of that kind which corresponds to the further change in the assumed mean distance recommended by Mr. *Adams*. The agreement with observations does not appear to be better than that obtained from Mr. *Adams's* elements, with the exception of *Flamsteed's* first observation of 1690, for which (contrary to Mr. *Adams's* expectation) the discordance is considerably diminished.

M. *Le Verrier* then enters into a most ingenious computation of the limits between which the planet must be sought. The principle is this: assuming a time of revolution, all the other unknown quantities may be varied in such a manner, that though the observations will not be so well represented as before, yet the errors of observation will be tolerable. At last, on continuing the variation of elements, one error of observation will be intolerably great. Then, by varying the elements in another way, we may at length make another error of observation intolerably great; and so on. If we compute, for all these different varieties of elements, the place of the planet for 1847, its locus will evidently be a discontinuous curve or curvilinear polygon. If we do the same thing with different periodic times, we shall get different polygons; and the extreme periodic times that can be allowed will be indicated by the polygons becoming points. These extreme periodic times are 207 and 233 years. If now we draw one grand curve, circumscribing all the polygons, it is certain that the planet must be within that curve. In one direction, M. *Le Verrier* found no difficulty in assigning a limit; in the other he was obliged to restrict it, by assuming a limit to the excentricity. Thus he found that the longitude of the planet was certainly not less than 321°, and not greater than 335° or 345°, according as we limit the excentricity to 0,125 or 0,2. And if we adopt 0,125 as the limit, then the mass will be included between the limits 0,00007 and 0,00021; either of which exceeds that of Uranus. From this circumstance, combined with a probable hypothesis as to the density, M. *Le Verrier* concluded that the planet would have

a visible disk, and sufficient light to make it conspicuous in ordinary telescopes.

M. *Le Verrier* then remarks, as one of the strong proofs of the correctness of the general theory, that the error of radius vector is explained as accurately as the error of longitude. And finally, he gives his opinion that the latitude of the disturbing planet must be small.

My analysis of this paper has necessarily been exceedingly imperfect, as regards the astronomical and mathematical parts of it: but I am sensible that, in regard to another part, it fails totally. I cannot attempt to convey to you the impression which was made on me by the author's undoubting confidence in the general truth of his theory, by the calmness and clearness with which he limited the field of observation, and by the firmness with which he proclaimed to observing astronomers, „Look in the place which I have indicated, and you will see the planet well.“ Since Copernicus\*) declared that, when means should be discovered for improving the vision, it would be found that Venus had phases like the Moon, nothing (in my opinion) so bold, and so justifiably bold, has been uttered in astronomical prediction. It is here, if I mistake not, that we see a character far superior to that of the able, or enterprising, or industrious mathematician; it is here that we see the philosopher. The mathematical investigations will doubtless be published in detail; and they will, as mathematical studies, be highly instructive: but no details published after the planet's discovery can ever have for me the charm which I have found in this abstract which preceded the discovery.

I understand that M. *Le Verrier* communicated his principal conclusions to the astronomers of the Berlin Observatory on September 23, and that, guided by them, and comparing their observations with a star-map, they found the planet on the same evening. And I am warranted by the verbal assurances of Professor *Challis* in stating that, having received the paper on September 29, he was so much impressed with the sagacity and clearness of M. *Le Verrier's* limitations of the field of observation, that he instantly changed his plan of observing, and noted the planet, as an object having a visible disk, on the evening of the same day.

My account, as a documentary history, supported by

\*) I borrow this history from *Smith's Optics*, sect. 1050. Since reading this Memoir, I have however, been informed by Professor *De Morgan*, that the printed works of Copernicus do not at all support this history, and that Copernicus appears to have believed that the planets are self-luminous. — G. B. A.

letters written during the events, is properly terminated; but I think it advisable, for the sake of clearness, to annex extracts from a letter which I have received from Professor *Challis* since the beginning of October, when I returned to England.

Nr. 23. Professor *Challis* to G. B. *Airy*.

[Extract.]

„Cambridge Observatory, October 12, 1846.

„I had heard of the discovery [of the new planet] on October 1. \* \* \* I find that my observations would have shewn me the planet in the early part of August, if I had only discussed them. I commenced observing on July 29, attacking first of all, as it was prudent to do, the position which Mr. *Adams's* calculations assigned as the most probable place of the planet. On July 30, I adopted the method of observing which I spoke of to you. \* \* \* In this way I took all the stars to the 11th magnitude in a zone of 9' in breadth, and was sure that none brighter than the 11th escaped me. My next observations were on August 4. On this day \* \* \* I took stars here and there in a zone of about 70' in breadth, purposely selecting the brighter, as I intended to make them reference-points for the observations in zones of 9' breadth. Among these stars was the planet. A comparison of this day's observations with a good star-map would most probably have detected it. On account of moonlight I did not observe again till August 12. On that day I went over again the zone of 9' breadth which I examined on July 30. \* \* \* The space gone over on August 12, exceeded in length that of July 30, but included the whole of it. On comparing [at a later time] the observations of these two days, I found that the zone of July 30 contained every star in the corresponding portion of the zone of August 12, except one star of the 8th magnitude. This, according to the principle of search, which in the want of a good star-map I had adopted, must have been a planet. It had wandered into the latter zone in the interval between July 30 and August 12. By this statement you will see, that, after four days of observing, the planet was in my grasp, if only I had examined or mapped the observations. I delayed doing this, partly because I thought the probability of discovery was small till a much larger portion of the heavens was scrutinised, but chiefly because I was making a grand effort to reduce the vast number of comet observations which I have accumulated; and this occupied the whole of my time when I was not engaged in observing. I actually compared to a certain extent the observations of July 30 and August 12, soon after taking them,

more for the sake of testing the two methods of observing adopted on those days than for any other purpose; and I stopped short within a very few stars of the planet. After August 12, I continued my observations with great diligence, recording the positions of, I believe, some thousands of stars: but I did not again fall in with the planet, as I took positions too early in right ascension. \* \* \* On Sept. 29, however, I saw, for the first time, *Le Verrier's* last results, and on the evening of that day I observed strictly according to his suggestions, and within the limits he recommended; and I was also on the look-out for a disk. Among 300 stars which I took that night, I singled out one, against which I directed my assistant to note „seems to have a disk,“ which proved to be the planet. I used on this, as on all other occasions, a power of 160. This was the third time I obtained an approximate place of the planet before I heard of its discovery.“

This letter was written to me purely as a private communication, but I have received permission from Prof. *Challis* to publish it with the rest.

Before terminating this account, I beg leave to present the following remarks: —

First. It would not be just to institute a comparison between papers which at this time exist only in manuscript, and papers which have been printed by their authors; the latter being in all cases more complete and more elaborately worked out than the former.

Second. I trust that I am amply supported, by the documentary history which I have produced, in the view which I first took, namely, that the discovery of this new planet is the effect of a movement of the age. It is shewn, not merely by the circumstance that different mathematicians have simultaneously but independently been carrying on the same investigations, and that different astronomers, acting without concert, have at the same time been looking for the planet in the same part of the heavens; but also by the circumstance

that the minds of these philosophers, and of the persons about them, had long been influenced by the knowledge of what had been done by others, and of what had yet been left untried; and that in all parts of the work the mathematician and the astronomer were supported by the exhortations and the sympathy of those whose opinions they valued most. I do not consider this as detracting in the smallest degree from the merits of the persons who have been actually engaged in these investigations.

Third. This history presents a remarkable instance of the importance, in doubtful cases, of using any received theory as far as it will go, even if that theory can claim no higher merit than that of being plausible. If the mathematicians whose labours I have described had not adopted Bode's law of distances (a law for which no physical theory of the rudest kind has ever been suggested), they would never have arrived at the elements of the orbit. At the same time, this assumption of the law is only an aid to calculation, and does not at all compel the computer to confine himself perpetually to the condition assigned by this law, as will have been remarked in the ultimate change of mean distance made by both the mathematicians, who have used Bode's law to give the first approximation to mean distance.

Fourth. The history of this discovery shews that, in certain cases, it is advantageous for the progress of science that the publication of theories, when so far matured as to leave no doubt of their general accuracy, should not be delayed till they are worked to the highest imaginable perfection. It appears to be quite within probability, that a publication of the elements obtained in October 1845 might have led to the discovery of the planet in November 1845.

I have now only to request the indulgence of my hearers for the apparently egoistical character of the account which I have here given; a character which it is extremely difficult to remove from a history that is almost strictly confined to transactions with which I have myself been concerned.

Beobachtungen des Neptun am Passageninstrumente zu Göttingen.

1846			1846		
	M. Zt. Gött.	Gerade Aufst.		M. Zt. Gött.	Gerade Aufst.
Septbr. 28	9 <sup>h</sup> 23' 57" 1	328° 13' 34" 8	Octbr. 21	7 <sup>h</sup> 52' 14" 0	327° 54' 11" 2
29	9 19 56,5	12 23,4	22	7 48 16,2	53 42,9
Octbr. 4	8 59 56,1	7 8,4	25	7 36 23,0	52 21,0
6	8 51 56,4	5 10,9	Novbr. 2	7 4 47,2	50 12,3
10	8 35 59,0	1 43,0	3	7 0 50,8	50 3,9
15	8 16 4,1	327 57 51,3	17	6 5 54,7	51 44,5
20	7 56 12,0	54 43,5	Decbr. 1	5 11 24,7	59 57,3

B. Goldschmidt.