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AN HIATUS IN HISTORY: THE BRITISH CLAIM FOR NEPTUNE'S CO-PREDICTION, 1845–1846

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> The public would hardly take such a reason as that I have mentioned to be the true reason for his [Adams] not answering your questions, and I fear therefore that a hiatus must remain in history.
>
> Challis to Airy, 19 December 1846

NEPTUNE AND THE CONCEPT OF DISCOVERY

The planet Uranus was discovered in 1781 by William Herschel, as a byproduct of a systematic sky-survey of the brighter stars; and, once he had reported his observation (of what he thought be a comet), it then took about half a year before it gradually became evident that it must be a new planet. The discovery of Neptune in 1846 was a quite different kind of event: the words of the poet John Keats

> … or like some watcher of the skies
> When a new planet swims into his ken[2]

penned in 1820, really apply to only that one moment in history: at the Berlin Observatory, when Johann Galle and Heinrich d'Arrest found, over the night of 23/24 September, a *predicted* new planet that was not on their star-map. The known solar system then expanded by some fifty percent, after "the most magical predictive-math event in the history of the oldest science", to quote Dennis Rawlins.[3] It was a moment of a kind that had not been expected, whereby the obscure mathematics of perturbation-theory pinpointed the position of a new sphere in the sky.

Britain's 'Neptune file' (Figure 1) of the Royal Observatory at Greenwich (RGO), compiled by the Astronomer Royal George Biddell Airy (1801–92, see Figure 2), has a central role in any narration of the history of this event, if only because it contains so many of the essential documents, either originals or as copies. It strangely went missing in the mid-1960s, then finally re-appeared in 1999. The present re-telling of the story has been stimulated by this startling re-appearance of the Neptune file after it had been inaccessible to scholars for something over three decades. The present author is involved in a thorough archiving of all the relevant correspondence, and so this can claim to be the first narrative of the Neptune discovery, or of the British claim to co-prediction, that is based upon a comprehensive assessment of the data.[4]

Hitherto-unpublished documents, it will here be argued, throw new light on the subject, and their scrutiny *fails to endorse* the status traditionally assigned to John Couch Adams (1819–92, see Figure 3) as co-predictor of Neptune's position: the

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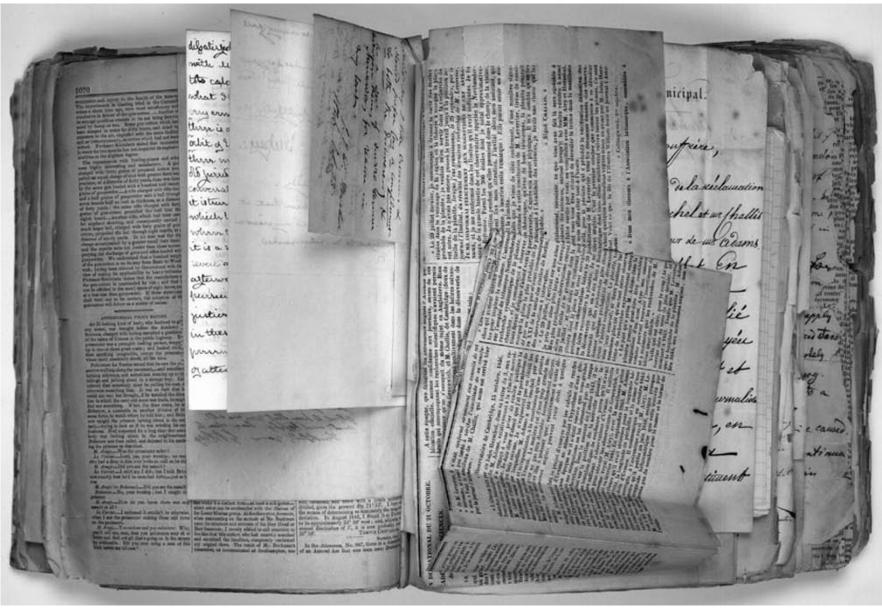

FIG. 1. The Neptune file, as collated by Airy, containing all his correspondence relevant to the asteroid Astraea (1845) and the discovery of Neptune; now kept in the Cambridge University Library, after its return from Chile. By permission of the Syndics of Cambridge University Library.

conclusions he reached prior to the prediction of Neptune's longitude in June 1846 by the French theoretical astronomer Urbain Le Verrier (1811–77, see Figure 4) were in essence private rather than public statements, while the solutions he communicated after this date ranged over twenty degrees of celestial longitude. Once the planet was found in the heavens, however, Adams then became the first to ascertain Neptune's distance, the position of its nodes, and its orbital inclination.

Le Verrier summarized his research concerning the new planet and predicted its position at the Academy of Sciences in Paris in June 1846; and the prediction was then published in *Comptes rendus*, the Academy's weekly journal. If a scientific prediction needs to be in some degree public, had any such been made in Britain, prior to this published French prediction? If so, can we hope to know what it was, or does it merely appear as having been constructed in retrospect? *Nothing* appeared in print to substantiate a British claim to co-prediction until over seven weeks *after* the discovery of Neptune in Berlin. "Already the word 'discovery' begins to break down under the weight of meaning laid upon it",[5] complained Sir John Herschel to the Rev. Richard Sheepshanks, in the wake of the intense Anglo-French priority disputes that had arisen.

The concept of discovery may be viewed as a construction made within a given research community[6] after the event.[7] A fairly small 'Cambridge network' soon

FIG. 2. George Biddell Airy *c*. 1850, portrayed by G. Cook, from the original in the National Maritime Museum Picture Library, at Greenwich.

came to control the narrative to an extraordinary degree[8], of how a new planet came to be co-discovered by two persons, more-or-less synchronously. The key figures in the British story hailed from just two adjacent Cambridge colleges, Trinity and St John's. Airy gave his "Account" of the events to the meeting of the Royal Astronomical Society (RAS) in November 1846, and this became accepted Europe-wide. Britain's Royal Society, the RAS, and the British Association for the Advancement of Science (BAAS) came to accept a single, coherent narrative. This involved alleged British predictions made in 1845 being "within a degree" of where the planet was finally found (which was, it will here be argued, by no means the case), but it did not involve predictions by the same person in the summer of 1846 that ranged over

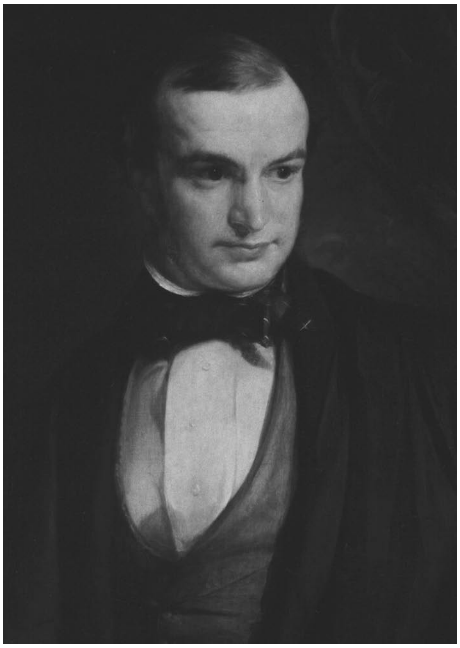

FIG. 3. Portrait of John Couch Adams at St John's College, painted by Thomas Mogford in late 1850 when Adams was a Fellow and Tutor at the college. By kind permission of the Master and Fellows of St John's College, Cambridge.

something like twenty degrees; no-one lied, but it merely came to be agreed that the latter were not on the agenda. Adams is normally described as a co-discoverer of Neptune, a view here challenged.

For the traditional narrative, see Tom Standage's *The Neptune file*.[9] The reader may also wish to have to hand a copy of Patrick Moore's *The planet Neptune*,[10] which republished many of the essential documents, also available in the author's website www.ucl.ac.uk/sts/nk/neptune.

THE RETURN OF BRITAIN'S NEPTUNE FILE

By the early 1990s the fact that the main bulk of the Neptune papers had been missing for nearly thirty years had been noted,[11] but none of the articles published in September 1996 on the occasion of the 150th anniversary of Neptune's discovery mentioned this fact.[11] On 8 October 1998, a mere week or so before the RGO was to be closed down and have its telephones disconnected, a telephone call from the observatory at La Serena in Chile inquired as to whether some historic RGO documents might have gone missing. Olin J. Eggen, the distinguished stellar astronomer who had been chief assistant to the Astronomer Royal Richard van der Riet Woolley at the RGO in the early 1960s, had just died, and the documents were, fortunately,

FIG. 4. Medal commemorating the death of Le Verrier, engraved and cast by Alphée Dubois in 1884. By kind permission from the Observatoire de Paris library.

noticed in his apartment at the Chilean Institute of Astronomy. He had twice denied having them, in correspondence with an RGO official and a journalist, but observatory officials found amongst his effects a large quantity of priceless historical documents and various old astronomy books which he had removed from the RGO archives at Herstmonceaux in the mid-1960s, without anyone noticing. In a sense he 'borrowed' the RGO file in order to complete the *Dictionary of scientific biography* entries on George Biddell Airy and the sometime professor of astronomy at Cambridge, James Challis (1803–82),[13] both of which he did, prior to 1971. But it is not easy to see how he could have been given permission to take the RGO file to Australia, which is where he went. The current RGO Archivist Adam Perkins recovered the documents safely in 1999, having them shipped in two large tea-chests (totalling 105 kg) back to England.

The recent recovery of these documents permits for the first time a comprehensive account. The relevant Neptune-discovery papers may shortly be published, though there may have been reasons why this has hitherto been deemed undesirable. The contents of the RGO's Neptune file had not even been itemized — unlike the smaller file kept by the Cambridge University Observatory Library (now at the Institute of Astronomy), where Challis did his sky-search.[13] Thus scholars laboured under a considerable disadvantage in relating the epic tale. For example, Allan Chapman's account[15] deplored the way William Smart's centenary essay[16] had cited none of his sources, adding: "One suspects that he may have got the information from the -missing 'RGO Neptune file'."[17] *A propos* a letter from Airy to the Cambridge geologist Adam Sedgwick of 4 December 1846, Chapman commented that he had not been able to trace the original of the letter cited by Smart, which was in fact in the missing RGO file[18] (this is where Airy complains of having received a "bloody nose" from the controversy and defends his alleged failure to receive Adams in September 1845). Chapman found, concerning Smart's essay, that "its main value lies in the citation and publication at length of several letters (probably in the RGO archives) which were still available in 1947 ..."; and, concerning the account by H. Spencer-Jones, *John Couch Adams and the discovery of Neptune*, he commented that, "Though citing several of Airy's letters, Spencer-Jones fails to give their archival location".[19] Morton Grosser's book, *The discovery of Neptune*,[20] contained "important errors of fact" in Chapman's view, due to a "failure to notice certain Airy manuscripts".[21] Grosser's book made *no reference* to the RGO's Neptune file.

This essay quotes for the first time in full what had until recently been regarded as the lost (or partially lost) letter of Airy to Sedgwick of 8 December 1846, where Airy gave vent to his ire over the position in which he found himself, after the failure of Challis's sky-search. Smart, while holding the John Couch Adams Astronomy chair at Cambridge, composed his 1947 centenary article on the discovery evidently without having access to this letter,[22] suggesting he too had not seen the RGO's Neptune file, and he cited only Sedgwick's reply to it. Airy's strongly-worded letter may help us to appreciate why the British Neptune file has sometimes appeared as inaccessible. As related by Smart, Sedgwick wrote to Airy that Adams had conducted himself "very like a simpleton",[23] whereas the original letter had "... like a very simpleton",[24] which conveys a different shade of meaning.

The crucial letter from Airy to Challis of 9 July, requesting initiation of the sky-search, had its opening sentence quoted as "You know that I attach importance to the examination of that part of the heavens in which there is a ... reason for suspecting the existence of a new planet", by Airy before the RAS meeting in November.[25] (The letter may be perused in full at www.ucl.ac.uk/sts/nk/neptune/corr.html.) He had deleted the phrase "possible shadow of",[26] not wishing to seem too doubtful! This censored letter had been so quoted in the 1904 account by W. Turner, assistant to the Astronomer Royal,[27] as if even he did not have access to the RGO's Neptune file when composing his account, ten years after Adams's death. The recent biography of Adams[28] quotes the time-honoured letters all with the same gaps in them as were created by Airy a century and a half ago, as likewise did the books by Grosser (1962) and Moore (1988).[29]

An account should be based upon documents either dateable or made public on dates agreed upon. Hitherto, British accounts have placed an undue emphasis upon undated and unaddressed scraps of paper, lacking any covering letters, produced by the interested parties only in order to salvage their reputations more than a year after they supposedly received them — and seven weeks after the discovery of Neptune, at the RAS meeting of 13 November where the British claims were presented. Retrospective prediction is a wonderful thing, but such documents will here be assigned a secondary status, without our necessarily accepting their claims at face value. The note that Airy claimed Adams had given to him in October 1845, with the elements of the new planet's position, lacked any date when first published in November 1846. Later it appeared as having written at its top, in a hand other than Adams (presumably Airy's), the month and year.[30] Thus the evaluation of this -document, traditionally viewed as pivotal to the whole case, involves to say the least a temporal dilemma. This document has now returned from its exile in Chile, which has to be an improvement.

Such errors point towards the remarkable extent to which historians have failed to access the primary source material. What we may call the 'crown jewels' document, the note allegedly presented by Adams to Airy in the autumn of 1845 and containing his prediction of the new planet's place, ended with the stirring words, "... assumed mean motion of the new planet", whereas the version of this letter that Airy presented before the RAS on 13 November 1846,[31] and which has been since then widely reprinted (for example in Grosser's *The discovery of Neptune*), ended merely with "... of the planet", a curious adjustment. Again, James Challis averred that he had noted "It seems to have a disc" in his notebook beside his observation of what, retrospectively, turned out to be Neptune.[32] The latter cited this in his November 1846 report to the RAS, since when it never fails to crop up in re-tellings of the story. One does not hear cited the less heroic-sounding words that Challis in fact wrote,[33] which were: "~~last one~~ seemed to have a disc", with part of them scratched out.

ASTRONOMICAL PRELUDE

Tables for the planet Uranus were first published by Alexis Bouvard at Paris in 1821, in the midst of an ongoing debate over the way its observed positions were not fitting well into a Kepler-ellipse orbit. There was a problem in reconciling the pre-discovery observations of the planet Uranus, made by astronomers such as John Flamsteed and Pierre Lemonnier, with more exact modern observations. The debate concerned discrepancies of a mere arcminute or so in its celestial longitude. Airy maintained his view, when conjectures arose as to whether a trans-Uranian planet could be causing these discrepancies, that there were nowhere near enough data for inferring the position of such a planet.[34] It is of interest that 1821 was in fact the year in which Uranus and Neptune were conjunct in the heavens, i.e. at the same celestial longitude, and this was relevant to extensive computations performed a couple of decades later. Eugène Bouvard (nephew of Alexis) presented to the Paris Academy his own revised and improved tables of Uranus in September 1845, thanks in large degree to high-quality data that Airy had been sending him, but his work was never published.

Airy's textbook on the subject for students (1834) had explained how the radius vector of a planet (i.e. its solar distance) was affected by conjunction with a planet exterior to itself, e.g. Jupiter as it passed by Saturn: "The effect then of a force in the direction of a planet's motion, which increases the planet's velocity, is to increase the size of the orbit."[35] A transfer of angular momentum increased its potential energy, pushing it into a higher orbit. Three years later Airy published, in the German journal *Astronomische Nachrichten* (*AN*), his estimation of this increase, which he had found by computing Uranus's position at its twice-yearly quarter positions (i.e., Earth–Sun–Uranus square): "The tables of Bouvard give the radius vector of Uranus too small by a quantity considerably greater than the Moon's distance from the Earth"; but without comment as to why this should be so.[36] Airy here alluded to Bouvard's Tables of 1821.[37]

Once he became Astronomer Royal in 1835, Airy commenced publishing in the *Nautical almanac* daily predictions of Uranus's radius-vector in both geocentric and heliocentric co-ordinates to eight-figure accuracy, as well as its right ascension and declination to 1/100th of an arcsecond.[38] He thereby stepped up the accuracy of the almanac's predictions of Uranus's positions by three orders of magnitude. He later published his observed daily RA and dec. Uranus values, with an "Error of tables" column.[39] This may help us to appreciate why Airy, in his *AN* article, should have believed he could express the radius-vector to an accuracy of thousandths of an AU (astronomical units, the mean distance of the Earth from the Sun), when the Bouvard tables carried considerably larger errors. The accuracy of Airy's transit measurements, whereby the daily time of Uranus's passing across a wire gave its position in the heavens, could not have been greater than several arcseconds, because the timing of the transit could not have been more exact than 0.1 to 0.2 of a second. Thus Airy's published volumes may have presumed unduly on the exactitude of his data. Both Le Verrier and Adams relied primarily upon British data for their work.[40]

Louis Wartmann at Geneva had earlier claimed to have found a new planet. His observations, reported in 1831, were published five years later, in 1836.[41] He put the planet at the Bode's-Law distance of 38.8 AU from the Sun; noting that it had not moved for a while, he averred that this showed its retrograde station (owing to the yearly retrograde loop which outer planets are seen to make, it would then appear as immobile). He lost it the next year, in 1832. Wartmann gave its position as 315° 27′ RA and –17° 28′ dec. for 6 September 1831, equivalent to 314° (14°Aquarius) when Uranus was in fact at 313°. The view expressed by Sheehan and Baum is that "in fact Wartmann had simply 'discovered' Uranus".[42]

Bode's Law is an empirical rule that indicates the mean distances of planets from the Sun.[43] For the outer planets it more or less doubles the distance between each orbit: thus Uranus's orbit was 19 AU, and Bode's Law predicted that of Neptune to be about 38 AU. There was (and is) no scientific basis for the existence of this law, but it had hitherto defined the planetary distances from the Sun fairly well.

At the time of Neptune's discovery, Wartmann's planet "remained an enigma".[44] The Harvard mathematician Benjamin Peirce told the American Academy of Arts and Sciences in Boston on 12 November 1845 that Wartmann's planet could not have been more than eight or ten degrees from Leverrier's computed orbit for Neptune.[45] Wartmann's observations had been made a decade after the Uranus-Neptune conjunction, by which time the two bodies were nine degrees apart. The British astronomer John Hind found its alleged position enigmatic,[46] while the German

Theodore Oppolzer may be regarded as having had the last word on the subject,[47] with his impressively bizarre conclusion: "it is remarkable that subtraction of the approximate precession in a century removes the discrepancy" between Wartmann's star and Uranus — that is, they were just over one degree apart.

By the 1840s the planet Uranus had been observed over some three-quarters of its 84-year orbit, following its discovery in 1781 by William Herschel. Around the time of its conjunction with Neptune it remained some twenty arcseconds in advance of its expected Kepler-orbit position, then around 1830 it started to lag behind its expected position, and by the 1840s it was a whole arcminute behind.[48] The attaining of its aphelion (furthest distance from the Sun) in 1843 would further have assisted astronomers in ascertaining its orbit. Neptune was at its brightest and most visible each year at solar opposition (i.e., in the alignment Neptune–Earth–Sun), which was then falling on 20 August.

In 1842 Urbain Le Verrier published two notes concerning the perturbations of Uranus,[49] followed in 1844 by an analysis of the planetary orbits including that of Uranus.[50] Three more swiftly-unfolding steps of public statements[51] at the Paris Academy of Sciences then followed. On 10 November 1845, his "Première mémoire sur la théorie d'Uranus" was given to the Paris Academy, concerning how Jupiter and Saturn were affecting the orbit of Uranus, and clearly demonstrating that known factors could not account for Uranus's position.

On 22 September 1845, Airy attended a meeting of the French Institute, where he learnt that the Paris Observatory director François Arago had deputed Le Verrier to investigate the possible cause of Uranus's perceived irregularity. Airy did not then meet Le Verrier, but he and Arago, the observatory directors respectively of Greenwich and Paris, spent a couple of days together at the Institute and then at the Paris Observatory.[52] Airy would have been keen to hear how Eugène Bouvard's presentation to the Academy a month earlier had revised and improved upon his uncle's Uranus parameters; after all, twelve letters had passed between Eugène Bouvard and Airy in the period 1837–44 as recorded by the RGO's Neptune file, and a substantial amount of the highest quality data was thereby sent to Paris to be used by both Bouvard and LeVerrier. Four years before his appointment to the post of Astronomer Royal in 1835, Airy had mentioned in a speech to the BAAS how no single ellipse could accommodate both the ancient (pre-discovery) and modern positions for Uranus,[53] and he later published his estimate of its radius-vector anomaly:[54] he was Britain's top expert on the subject. No British astronomer was more competent to appreciate the perturbation theory involved.

In the summer of 1846, both Adams and Le Verrier assigned to the hypothetical planet a *mean distance* almost exactly double that of Uranus's 19 AU, in accordance with Bode's Law — whereas Neptune turned out to have, instead, twice the *orbit-period* of Uranus, at only 30 AU mean distance. Thus the sole planet to have its position predicted on the basis of Bode's Law turned out to be the only one in our solar system not adhering to it! Its orbit was remarkably close to being circular, with an eccentricity ($e = 0.009$) lower than that for any other planet except Venus, and a mere one-fifth that of Uranus, leaving the problem as to why both Adams and Leverrier should have imagined it to be more than an order of magnitude larger.

Adams computed a wildly inaccurate longitude for its perihelion (a planet's closest approach to the Sun) of around 315°, while it was actually at 061°. Historians have struggled with the question, as to how a prediction derived from Uranus's perturbation using a distance estimate so greatly in error, eccentricity an order of magnitude too large, and a radically displaced apse line, came to be within degrees of the true position. We may here hope to avoid the scepticism of Benjamin Peirce, who proposed to the American Academy of Arts and Sciences in New York that the close agreements of the two predictions with the found planet were mere coincidences.[55] Smart[56] and other authors[57] have been concerned to refute this view, insisting that the discovery was no mere synchrony (as was Clyde Tombaugh's later finding of Pluto in accord with Percival Lowell's predictions), but based upon valid deduction.

Against Peirce's view one notes, firstly, that a recent doctoral thesis contains a recomputation of Le Verrier's huge perturbation-equations, carrying through their solutions to a higher order of accuracy, and has found that his computation, historically cited as accurate to within 52′, then became *more* exact, to within 16′ of celestial longitude (as measured along the ecliptic).[58] Secondly, I ascertained that the attractive force that Le Verrier's hypothetical planet would have had (from its mass/distance^2) during the 1821 Uranus-Neptune conjunction (about which the historic computations were centred), was within 25% of that of the real Neptune. Although Le Verrier's final estimated distance was much too large, yet he gave it a mass also too large, so that his mass/distance^2 ratio indicating gravity pull was within a reasonable proximity to that of the real Neptune. For these reasons, we may incline to the view that Le Verrier's prediction really was the awesome miracle of deductive logic that he believed it to be.[59]

In mid-1846, with credulity-straining synchrony, Le Verrier and Adams each attempted to improve his computation by (a) shrinking his initial Bode's-Law orbit and (b) putting his hypothetical planet close to an imagined perihelion, thereby bringing it nearer and rendering its speed more comparable to that of Neptune. They may have sensed that they were not at liberty further to reduce the planet's mean radius, owing to a problem that arises around 35–36 AU (the perturbation-equations become unstable, as pointed out by Peirce as a plank for his sceptical argument[60]). As both Adams and Le Verrier decreased their mean orbit radii by way of improving the fit of their theories with the observational data, they would have reached this limit beyond which their equations would become misleading. For this reason, each could further decrease the distance of his hypothetical planet only by placing it at or near the perihelion on a large-eccentricity orbit; which in mid-1846 they both did (Appendix I).

On 1 June 1846 Le Verrier announced his heliocentric longitude prediction of the new planet at 325° for epoch 1 January 1847, assuming a circular orbit and a Bode's-Law mean distance of 38 AU; the *Comptes rendus* containing this was said to have been read by Airy on 26 or 27 June. Then, on 31 August 1846, Le Verrier made his final prediction, for 326° 32′ heliocentric longitude, at the same epoch, but with a reduced mean distance of 36.1 AU, and an eccentricity of 0.11.[61] It took two to three weeks from the dates of these séances for the Paris Academy *Comptes rendus* to reach England.

Three pre-discovery letters from LeVerrier concerned his prediction, of which the first two emphasized the importance of completing his computations around or before the yearly solar-opposition date of the new planet. The first to Airy (28 June 1846) stated: "If I could hope that you will have enough confidence in my work to seek this planet in the sky, I would hasten, Sir, to send the exact position as soon as I obtain it"[62] (Airy declined the offer!). The computations were not then completed. Then on 8 September an extensive manuscript explaining the mathematics of his perturbation theory was transmitted to Heinrich Schumacher, the editor of the *Astronomische Nachrichten*, who published it in three issues of his journal, starting on 12 October.[63] An American source commented, "As the communication was written before the actual discovery of the planet, it will be read with great interest, showing clearly that the distinguished mathematician had the fullest confidence in his elaborate computations, and spoke of the planet with as much certainty before its discovery, as we now speak of it after two months of examination".[64] Le Verrier's accompanying letter to Schumacher hoped that this excerpt would "inspire sufficient confidence in astronomers" to induce them to have a look at "that portion of the heavens, where we shall undoubtedly discover a new planet". His celestial latitude computations were still not yet complete owing to "an indisposition which has retarded my movements"; however, this effect would be "very small" (i.e., the planet was close to the ecliptic). This was the best time of year to search because, although it was just past the opposition period, the nights were growing longer. Then on 18 September 1846, Le Verrier penned the historic letter to Galle at the Berlin observatory.[65] He apologised for not having replied for a year to an enquiry Galle had made (concerning Mercury), and asked whether he would be prepared to "take a few moments" to examine a region "where there could be a new planet to discover". Its diameter of around three arcminutes would be, he explained, "readily distinguishable with a good telescope".

The two predictions are properly compared using true heliocentric longitude: i.e., the position of the planet in the zodiac, as it would appear if seen from the Sun at the date of discovery (as portrayed by John Herschel in his 1849 *Outlines of astronomy,* see Appendix II). 'True' helio-longitude will indicate the position on an elliptical path, whereas Adams gave his results to Airy as 'mean' helio-longitude, i.e. the position on a notional circular path with uniform motion, this being a preliminary stage of the calculation. These mean and true orbit-positions could diverge markedly owing to the highly elliptical orbits used.[66]

ADAMS GOES TO GREENWICH

On that same 22 September 1845 when Airy was attending the French Institute meeting in Paris, Challis wrote him a letter of introduction, commending to his attention the researches of the 26-year-old postgraduate mathematician, John Couch Adams. This letter indicated that Adams wished to write a letter to Airy about his findings: "Mr Adams ... has arrived at results, which he would be glad to communicate with you personally.... 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."[67] Airy replied on 29 September, after this meeting failed to take place, likewise expressing a hope that Adams would write such a letter: "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."[68] In this respect, they were both disappointed.

Why would Adams have wanted to visit Greenwich? He was a close friend of Professor Challis, who was in charge of what was England's most powerful refracting telescope, and lived just down the road, but no suggestion seems to have been put to the latter that he should direct his telescope at any point in the sky. Adams wanted an introduction to meet — not write a letter to — Britain's busiest and most eminent astronomer. One possible answer, seldom given, is that he wanted to discuss the theory involved with Airy.

Richarda Airy, heavily pregnant, had remained behind while her husband visited France from 25 August to 26 September, with his sister and sister-in-law;[69] and it is likely that she would have been at home for Adams's first visit, a day or two before her husband returned. "I quite well recall Adams' calling", she said a year later. She had seen "his card brought into the room"[70] when Airy was out, perhaps on his morning stroll — however, this was *after* Airy's return and her two letters on the subject show *no* recollection of the first visit. Adams had thus arrived and presented Challis's introductory letter, perhaps to the butler, but without Mrs Airy's knowing: he seems to have had a knack for being hardly noticed. Our sole evidence for this first visit, is Airy's above-mentioned letter to Challis, regretting that the meeting failed to take place, presumably after he had seen Challis's letter of introduction (given to the butler?). On Adams's second visit, early in the morning of Tuesday 21 October 1845 on his journey back to Cambridge from Cornwall, he again delivered a note without the occupants of the house meeting him: this time it was a "short statement of the results at which I had arrived" (he informed his parents, two days later).[71]

Following the latter's second visit, Airy wrote to Adams on 5 November, and again requested a letter from him:[72] the Astronomer Royal failed to see why the validity of Bode's Law should be regarded as self-evident, and inquired (a) why Adams should be taking for granted its predicted value of 38 AU, and (b) whether the unknown planet could account for Uranus's anomalies in the radius vector. Clearly, these were central theoretical issues involved in the prediction, the latter being a topic especially associated with Airy, on which he had twice gone into print. Adams was indeed fortunate to have Britain's Astronomer Royal probing him on these key issues. Again, no reply was forthcoming — although Adams did draft a (hitherto-unnoticed) reply, never sent, which merely averred that Bode's Law "has a claim to be first tried"; adding, "The paper I then left [early in the morning] contained merely a statement of the results of my calculations".[73] Later, Adams commented rather disingenuously (to James Glaisher at the Greenwich Observatory) that he had not replied because he thought the radius vector query to have been "trivial".[74]

Airy's letter thanked Adams for "the paper of results … shewing the perturbations on the place of Uranus ...". Adams wanted to show the Astronomer Royal his perturbation values as being the bases for his computation, Airy being the acknowledged expert on the subject. The three-page note that Airy first produced in November 1846 — claiming to have received it thirteen months earlier — did not have these, but instead had tables of "Observation-Theory" obtained by subtracting Adams's computed helio-longitudes from those observed (i.e., positions for Uranus at its opposition to the Sun, each derived from several observations) , which would have had a limited value for persons ignorant of the theory that Adams was using, his theory having been explained to no-one prior to 13 November 1846. The perturbation values for Uranus were hovering around an arcminute, whereas the residuals in Adams's lists were around one or two arcseconds.[75] We can hardly assume that Airy would confuse 'perturbations', i.e., deviations from the Kepler-ellipse orbit, on which he was an expert, with an arcane list of 'errors'.[76] The note Airy later produced had over two hundred digits on it, with residual values given to hundredths of an arcsecond: was that the "short statement" that Adams left?

Challis's letter to Airy of 22 September 1845 did not hint that he had been given any document, or been told of orbit parameters, by the young Adams, but merely stated that the latter "has arrived at results, which he would be glad to communicate with you personally". Adams had indeed reached a prediction for the unknown planet in September 1845, as recorded in his notebook,[77] which triggered his approach to Challis that begins our story; that solution was for an angular difference between two 'mean' planets (i.e., in mean helio-longitude) for a moment in the year 1810 — which would have required considerably more computation before anyone could be told where to look.

Much later on, after the discovery, Challis claimed that he had been given a specific prediction,[78] as will be examined later, and this I found to match fairly well with his notebook record. A geocentric longitude he cited was three degrees away from the then-position of the planet, which, while no doubt impressive, would not have sufficed to find it.[79] Such later-appearing claims should, I suggest, be kept quite separate from the historical record for 1845: then, Adams's sequentially-paginated and intact notebook does *not* take the computation any further but instead delves into an intensive quest for radius vector perturbations: only on 2 September of the following year did he produce a properly-derived 'solution'.[80] As discussed earlier, the huge eccentricities that Adams was using meant that the step from mean to true helio-longitude was considerable, and this could have been what daunted him.

Historians have struggled with the credibility of the traditional tale,[81] whereby Challis, after writing the letter of introduction, was supposed to have made no inquiry afterwards as to how Adams, this sensitive young fellow-Cantabrian, had fared. After receiving two letters from the Astronomer Royal, Adams is supposed to have decided that the question being asked about his analysis was not worth answering!

If we follow Smart in having the three characters meet in December 1845 — and possibly also next 13 February at the RAS's Annual General Meeting — and then again in April for Adams's Biela's comet presentation at the RAS,[82] would they not they have aired these issues? In their retrospective account of events, both Challis and Airy conveyed the impression that they had scarcely communicated with Adams since the briefly-recorded events of 1845, while simultaneously endeavouring to give him credit for one of the greatest predictions ever made. Why would they have wanted to do that?

In Paris, on 10 November 1845, Le Verrier presented his "Premier mémoire sur la théorie d'Uranus" to the Academy, concerning irregularities in its orbit, and discussed the pull of Jupiter and Saturn upon it.[83] Airy remarked that only after its publication could it "... be truly said that the theory of Uranus was now, for the first time, placed on a satisfactory foundation".[84] In early December Airy visited Cambridge and stayed with Challis, the Plumian Professor of Astronomy, at the Observatory. They had both by then received the new *Comptes rendus* describing Le Verrier's paper, presumably the stimulus for Airy's visit. Smart has Adams also present,[85] and given, as we shall see, the frequency of his visits to the Observatory, this make sense. These three had each in his time been 'senior wrangler' (top mathematician of the year) and they had been undergraduates at adjacent Cambridge colleges: Challis at Trinity, Airy and Adams at St John's.[86]

Adams's manuscripts from the autumn of 1845 are, as Sampson found, well--preserved, but they greatly contradict the above comment about the "trivial" nature of Airy's question. They contain altogether eight dated computations from the autumn of 1845 until the new planet's discovery, three of which concern the radius-vector of Uranus (the others were on 18 September 1845, 15 and 16 December 1845, and 20 August and 1September 1846). Two dated pages of 28 November and 24 December 1845[87] are both entitled "Additional perturbation of the log of Radius Vector"; they show a successful finding of the relevant integral, but do not attempt to compute it. There follows an eight-month gap on the subject, until a page dated 1 September 1846 entitled "Calculation of additional perturbations of radius vector for 1836, 1840 and 1846" discovers these magnitudes, as being 0.0049, 0.0069 and 0.0082 AU respectively. He had thereby obtained an increment for 1836 comparable to that which Airy had published in his 1838 *Astronomische Nachrichten* report (0.0048 AU); after this, on 2 September 1846, he penned his first-ever letter on the subject of Neptune, to Airy. The letter cited these radius-vector perturbation values that he had determined the previous day, showing the key importance that the attaining of this solution had for him. At last, he could answer Airy's query.

A year later, in December of 1846, Adams averred concerning the note he had left for Airy: "I did think that the Astronomer Royal would have communicated my results among his correspondents. I took all that for granted and considered it a publication."[88] This characteristically vague and diffuse remark indicates a strange attitude concerning publication: why should such be Airy's business, if the two had not even met on that occasion and if Adams had lacked the courtesy to reply to

Airy's letter? The notion that the Astronomer Royal had an obligation to publicize an undated, unaddressed scrap of paper put through his letter-box from someone he had never met is a mere absurdity. He had been given no hint that Adams wanted it to be "published". This remark, as we shall see, aroused Airy's ire. Did Adams have an impediment to writing letters as Challis claimed (see the citation in our epigraph), by way of explaining a shortage of documents supportive of the British claim? The public would never believe this, Challis admitted to Airy, so an "hiatus in history" would have to remain. Challis always appears as ready to justify Adams's behaviour.

ADAMS'S DIARY FOR MARCH 1846

If we suppose that Adams had made a definite prediction in the autumn of 1845, we then face the problem of accounting for his having shown *no further interest* in the subject over a gap of something like eight months — until well after Le Verrier's first prediction in June. No letter either from or to him mentions the subject, and his manuscripts lack any record of his perturbational equations over the eight-month interval between Christmas Eve 1845 and 20 August 1846.[89] Adam Sedgwick (writing to Airy in December 1846) expressed an honest bewilderment verging on incredulity over the issue: "... when about June last Leverrier published one of the results Adams had obtained before [in September 1845] why, in the name of wonder, was not all Europe made to ring with the fact that a B.A. of Cambridge had done this (& *more than this*) ten months previously?"[90] Adams gave a highly unconvincing answer to this question when, many years later,[91] he explained that his reticent behaviour was in order that "his country" should have "the full credit of the discovery".

Adams's diaries are largely missing over this crucial period,[92] except that a portion for the month of March 1846, remains — never quoted, oddly enough, or even mentioned, by historians. It gives a day-by-day account of the life of a Cambridge astronomer, showing his close liaison with James Challis, working at the Observatory just outside town. The diary-fragment has two days for 1844 and fifteen for March 1846, of which about half allude to Challis and/or the Observatory at Cambridge.[93] Adams appears as grappling with cometary paths, and working on the problem of atmospheric refraction *for Airy*. For 12 February 1844, for example: "Prof Challis brought one of the Notices of the Astr. Soc^{y}. containing my results respecting the Comet." In January 1846 there appeared "a phenomenon which struck every astronomer with amazement", to quote Herschel:[94] a comet divided into two. Such a thing was hitherto unheard-of and created a sensation.[95] But quickly Adams, as his diary records for 9 March 1846, "solved the equations for determining the relative distance and velocities of the two heads of Biela's comet and communicated the results to Professor Challis, in the evening for his communication to the Philosophical Society" (in Cambridge, of which Challis was then President[96]). The next day Adams improved his comet calculations, receiving some more data from Challis, and then went in the evening to the Observatory to view the comet. 14 March found him weary: "Did not do much today, feeling rather tired. Walked as far as Observatory with Challis & then went to the Courts" (tennis?); he does not walk *to* the Observatory to meet Challis, as one might expect. Challis was then living at the Observatory, as the astronomy professor there, while Adams lived at St John's College a mile or so away. He reports on his work to Challis, on another day Challis comes round to his house, and on another Adams stays at the Observatory "till late". On 19 March, he is "finding the differential effect of refraction on two objects observed near each other by an equatorial", where he "worked some cases given by Airy". One is surprised at the way all previous commentators have ignored the diary of the co-predictor of Neptune over the year of its discovery. "Adams was his own private friend", wrote Mrs Airy to Challis.[97] The recent biography of Adams starts quoting from his diaries only a decade later,[98] and, in common with earlier accounts, gives no hint of any special friendship between Adams and Challis.

Adams "hardly destroyed anything he wrote", as Glaisher observed.[99] If conjecture may be permitted as to why all of Adams's 1845 and the rest of the 1846 diaries are missing, this was because he had no wish for a record of his close liaison with Challis to endure, but did want to record his working on "Biela's comet" and the finding of his method to clear atmospheric refraction from telescope observations. These were used (presumably) by Challis and Airy for their telescopes, and both were recorded in his March entries. In February he corresponded with the astronomer John Russell Hind about the comet, receiving on the 28th a letter from Hind[100] before in April giving his address to the RAS about it.[101]

The traditional tale always dwells upon Adams as a man of "extreme modesty", as "a modest and retiring fellow".[102] In January 1844, the 24-year old Adams had tackled Faye's comet and communicated a paper to the RAS's journal, before he became a member; on 15 October 1844, a letter of his was published in *The Times*, about the orbit of De Vico's comet. Then in September and October 1845, he strolled over to the Astronomer Royal's residence without any appointment, expecting to be given an audience – twice! Do these suggest modesty? A hitherto unnoticed letter from Herschel throws doubt upon this: in January 1846, he wrote to Adams, enclosing a book about the theory of ancient eclipses; and he discussed a correction to the motion of the lunar nodes, which implied an error in Laplace's celestial mechanics: an arcane and difficult issue. Herschel's letter expressed the hope that it might lead Adams to "research worthy of your powers".[103] This implies that, at the BAAS meeting of June 1845 held in Cambridge, Adams approached Britain's most distinguished astronomer, and became involved in an abstruse discussion about lunar theory and eclipses in Antiquity! Then in April 1846, barely five months after being elected a Fellow of the RAS,[104] it was he who addressed the RAS on the contemporary issue of "Biela's comet", after it split into two in the night sky. In October, upon the discovery of the new planet, he seized the opportunity to ascertain its elements — and lent his name to a proposal to call it "Oceanus". Adams appears in our story as a most confident individual, even though there is somewhat of a tendency for people to forget having met him! Only later on do statements like, "The timidity of Adams was truly astonishing" (Sedgwick[105]) appear, as part of the British retro-construction of history, and this has become part of the received image of Adams. He only appears "timid", however, if one assumes that he had made definite predictions, in the autumn of 1845.

FROM THE SKY-SEARCH TO THE BAAS MEETING

When on 1 June 1845 Le Verrier gave his first longitude prediction of the new planet, this stimulated a brief search at the Paris Observatory, abandoned in the first week of August.[106] It also triggered, unknown to Le Verrier — or to the rest of the world — a more clandestine search.[107] On 23 or 24 June, Airy heard about Le Verrier's report, and then on the 26th, a letter from Airy to Le Verrier questioned the latter about this report, putting the same query as he had earlier asked Adams about the radius vector, but making no mention of Adams.[108] The day before, he had sent off a letter to William Whewell[109] averring that he had received two planet-predictions, first one by Adams and now that published by Le Verrier, and that these were in substantial agreement. No doubt, it was the receiving of such estimations of the new planet's predicted position from two sources that led the Cambridge Observatory to commence its search.

Le Verrier replied on 28 June, concerning whether the radius vector of Uranus was being perturbed by the new planet, affirming that the correction Airy had alluded to "… n'est pas dû à la perturbation du rayon vecteur", and mentioning to M. Bouvard's calculations.[110] By taking account of the unknown planet's disturbance, he explained, one could better discern the elliptical orbit of Uranus, its eccentricity, and its longitude of perihelion. He further added: "If I may be allowed to hope that you will have enough confidence in my work to search for the planet in the sky, I will hasten, Sir, to send you its exact position as soon as I have calculated it." This letter so impressed Airy with its authority and air of confidence that he had, he informed the RAS, "no longer any doubts upon the reality and general exactness of the planet's place".[111] He did not, however, avail himself of Le Verrier's kind offer.

On 6 June, the RGO's Board of Visitors (that is, directors), a group of a dozen or so men that included Herschel, Airy and Challis, held its annual meeting at Greenwich; a further "adjoined meeting" was held at the Admiralty on the 29th.[112] Did Airy then boldly inform this group of the "extreme probability of now discovering a new planet in a very short time", as he alleged in his 13 November presentation to the RAS? There are two extensive sets of records of this meeting, and they lack any hint of such a startling remark.[113] The ensuing correspondence between him and Challis is far from supporting the notion. In his letter of 9 July 1845, Airy advised Challis that there was a "possible shadow of reason for suspecting the existence of a planet exterior to Uranus" — but he deleted that doubtful phrase from the letter when giving his November RAS address, as being too plainly incompatible with the great confidence he was then claiming to have felt, back in June. After making a tentative enquiry concerning whether Challis might be prepared to attempt the sky-search, the letter adds, "Presuming that your proposal would be in the negative…", and offers as inducement the loan of an assistant. This note appears (I suggest) as clearly the first time the topic has been aired between them: "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.…" A further letter followed, more urgent in tone. Challis's two-page reply on 18 July accepted the offer, treating the 9 July note as the first time he had heard of the proposal, and makes no allusion to the Board of Visitors' meeting.

That meeting on 29 June was specifically concerned with a proposal of Airy's for improved international collaboration between European observatories, and his being given a special status on the Board should this proposal be accepted (his forthcoming 'European tour' being not unconnected with this). How likely is it that, in the context of such a meeting, a proposal for a sky-search should be mooted, concerning which Airy had conspicuously not informed Le Verrier nor any other European astronomers? Airy may at the meeting have mentioned the idea, informally, to Herschel.

The astronomer Peter Hansen, then director of Denmark's Seeberg observatory and a leading world expert in celestial mechanics and perturbation theory,[114] stayed in Airy's house from 10 June to 4 July 1845. Adams used Hansen's equations in his locating of the new planet.[115] During the launch of the sky-search, was the advantage of having such an expert to hand appreciated? It rather appears that Hansen was never informed of the matter. When on 2 July Airy was walking with Hansen in Cambridge, they encountered Adams while crossing St John's bridge,[116] but nothing of Adams's work was mentioned. Adams could not even speak up when he met the celestial mechanist whose equations he was using.

The sky-sweep began at Cambridge on 29 July 1846 using Britain's biggest refractor, and it lasted two months. Challis began his sky-search "guided by a paper drawn up for him by Mr Adams",[117] to quote a contemporary source. At the RAS presentation on 13 November 1846 Challis emphatically claimed that this paper had directed his search.[118] This centrally important document, never hitherto published and ignored by successive generations of scholars,[119] will here be referred to as Adams's July ephemeris. Prepared in June/July of 1846 on one sheet, its second half comprised a series of predictions for the new planet, in right ascension and declination for five dates at intervals from 20 July to 8 October 1846. This appears as a mere transcript from Le Verrier's 1 June paper in *five* respects: it used a circular orbit;[120] it adopted the Bode's-Law radius of 38 AU; the orbit used had no inclination, i.e. it was in the ecliptic plane; the longitude on 29 August 1846 was given as 325°, the same value as Le Verrier's; and, lastly, its five columns spanned ±10°, the same confidence limit as had been expressed in Le Verrier's paper. The 325° centre position which it advocated (cited by Challis at the RAS presentation) was 3° away in helio-longitude from the position he was later to claim had been given to Airy the previous October and was not a solution present in his manuscripts. Thus Adams provided Challis with hypothetical planetary positions from 335° to 315° heliocentric longitude, a range of 20°.

Above these columns, a quite different computation appears, ostensibly more complex in having both a node position and an inclination (to the ecliptic), yet still with a circular orbit, giving a result differing by ten degrees: 336°.4 "nearly". Its solution is thus presented as having an order of magnitude greater accuracy. This computation was Adams's original work. From this page Challis would thereby gain the impression of an uncertainty in position of, say, twenty degrees. The -helio-longitude of 336°.4 for the hypothetical planet was derived not from perturbation theory, but from two supposedly "lost" stars, the one given as no. 1007 in the British Catalogue, recorded by Flamsteed as being in Gemini but never seen again,[121] and the other published in 1836 in *Comptes rendus* by Wartmann. The implication was that, because they had become lost, they were early detections of the new planet. Adams here showed more confidence in a dubious "Wartmann's star" computation than in his own -perturbation-equations, over which he had then been labouring for several years. One could argue that Adams in July 1846 mistook Uranus for Neptune, he being the sole British astronomer on record as having in some degree accepted Wartmann's claim. (A major term in Adams's perturbation-equations had the wrong sign, while the other terms in his computation conspired to minimize what would otherwise have been a serious error as Sampson showed;[122] Adams may have suffered paralysing doubt concerning whether he had erred.)

Later on, Challis was explicit as to why he had believed it necessary to conduct what was the most extensive sky-search in history up to that time, involving the logging of more than three thousand stars in the process: "I lost the opportunity of announcing the discovery by deferring the discussion of the observations ... little suspecting that the indications of theory were accurate enough to give a chance of discovery in so short a time."[123] Challis wrote to the Rev. Robert Main, assistant at the RGO, on 7 August 1846 describing his sky-search method.[124]

On 2 September, immediately following Le Verrier's final statement of his predicted position-elements, two letters were (almost simultaneously) sent to Airy, by Challis and Adams, only the latter of which concerns us. Just as Leverrier's final solution reduced his mean distance assumption, thereby abandoning Bode's Law, so likewise did Adams: "In the investigations the results of which I communicated to you last October, the mean distance of the supposed perturbing planet is assumed to be twice that of Uranus." Acknowledging that such an assumption could appear arbitrary, he added: "I therefore determined to repeat my calculations making a different hypothesis as to the mean distance. The eccentricity also resulting from my former calculations was far too large to be probable.... The result is very satisfactory, and appears to show that by still further diminishing the distance the agreement between the theory and the latest observations could be rendered complete ... the eccentricity reduces at the same time to a very small quantity."[125]

This is the *first* expression we have from Adams of confidence in the results of his own calculations, writing as if his circular-orbit July 1846 ephemeris given to Challis had never been. Adams then distinguished two "hypotheses", the first being that which he had allegedly given to Airy in October of the previous year (Appendices I and II). His second hypothesis, although worked through to a higher level of accuracy, was *less* accurate, the first being within two degrees and the second, three (Appendix II).[126] Two lists of his 'residuals' were compared in this letter, derived from each hypothesis. The historian Robert Grant considered that "These errors were smaller by the second hypothesis than by the first".[127] The means and standard deviations of the two data sets, there given in arcseconds, I found to be:

Hyp. I: 2.6 ± 5.6" for pre-1780 (n = 8), 0.025 ± 1.21" for post-1780 (n = 22),
Hyp. II: 1.1 ± 6.2" for pre-1780 (n =8), 0.027 ± 1.20" for post-1780 (n =22).

As expected, the pre-1780 observations gave much larger error-values, while the later ones imply an accuracy of one-fiftieth of an arcsecond in the Uranus-longitudes (credible for a mean value?). It is doubtful whether the error-values derived from Adams's second hypothesis can be described as smaller than those of the first.

After presenting these two hypotheses, Adams's letter added a further preferred option of reducing the mean radius further, such that $a/a_1 = 0.57$ (a being the mean radius of Uranus, *viz.* 19.2 AU, and a_1 that for the predicted planet). This put it at a mere 33.6 AU mean radius, for which he estimated that the corresponding longitude would be 315° 20′, adding "which I am inclined to think is not far from the truth". This last phrase, consistently ignored by historians, scuppers the claim that Adams had validly predicted the new planet's position. He has here swung *twenty degrees* from the position he advocated two months earlier in the July 1846 ephemeris. The calculations are in trouble, when so great a difference in predicted longitude results from varying the orbit radius.[128] (Later, Adams excused himself to Airy: "In my letter of Sept 2nd, I inferred that the mean distance used in my first Hypothesis [Hyp I] must be greatly diminished, but I rather hastily concluded that the change in the mean long. deduced would be nearly proportional to the change in the assumed mean distance."[129]) The implication of this letter was, to quote Rawlins: "Adams himself was fully aware that he had ultimately pointed Challis way off the correct longitude for Neptune."[130]

If we compare Le Verrier's final prediction in Paris on 31 August 1846 with Adams's 'Hyp II' penned on 2 September, the remarkable common features appear as: (a) predicted longitudes were within 4–5°;[131] (b) eccentricity values were similar (0.108 for Le Verrier and 0.121 for Adams), both an order of magnitude too high; and (c) their apse line values concurred within 15° (284° for Le Verrier and 299° for Adams — neither near Neptune's actual value of 61°). These coincidences reflect a similarity in mathematical procedure. Letters took a day or so to cross the channel, e.g. Airy's letter to Le Verrier of 26 June, inquiring about the radius vector, elicited a reply on the 28th; but Adams's letter seems to have been stimulated by his discovery on 1 September of the radius vector increment, as comparable to that earlier published by Airy, this enabling him at last to answer Airy's question.

Following his 2 September 1846 letter, Adams was sent some further Uranus data from the RGO, and replied on the 7th with gratitude because such data enabled "the correction to be applied to the assumed mean distance", adding that "tomorrow" he hoped to have approximate values of the inclination and longitude of the node.[132] The Adams manuscripts dateable to this period show several disparate solutions for these, a result that may have paralysed further endeavour.[133] No-one was informed of any such solution. Adams's July ephemeris used (as we saw) a Bode's-Law circular orbit as had Le Verrier, and its first value given was based on 'Wartmann's star' ten degrees away. Then, two dated letters from the first week of September indicated that Adams was grappling with the problem, expressing an enthusiasm and a sense that his computations were beginning to get somewhere, though with typical Adamsian vagueness: he hopes "tomorrow"[134] to have ascertained certain parameters, which seem never to have been sent. He has obtained a result differing by some ten degrees from the French solution, but this time in the other direction. Accounts of Neptune's discovery invariably omit the first of these solutions (using 'Wartmann's star'), while citing the second (2 September) without any comment on the huge extent to which the solutions were there sliding around from an arbitrary variation of mean distance. Challis's sky-sweeps were covering nine arcminutes at a time, owing to the high magnification he was using, so that was a considerable displacement.

Initially only an inner circle of Cantabrians seem to have known of the sky-search; however, a letter from Hind to Challis of 16 September 1846 says that he had "recently heard" of Challis's search,[135] suggesting that news of it had been circulating that week at the BAAS meeting at Southampton. The extensive archives of the BAAS meeting, which began on 9 September, contain no allusion to any planet-quest.[136] Adams said that he had intended to deliver a paper at this BAAS meeting,[137] but was not scheduled to do so. Evidence is lacking for such a vitally important Adams document, or even for his having been present there: if he attended, he passed through it like a ghost.

The usual story has Adams turn up only on the last day of the week-long event, Wednesday the 16th, and "dismayed to find" that he was too late, as the conference was then winding down and the Maths/Physics section had finished the day before.[138] Herschel as BAAS president, during his valedictory address on 10 September, claimed to have uttered the stirring words (echoing his father's great discovery): "We see it [the 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 ocular demonstration"[139] — although no paper reported them. On hearing of the new planet's discovery, Herschel wrote to *The Athenaeum* affirming that he had uttered these words, and that they comprised evidence for the British case. Did they? He had for, example, recently received a letter from the Astronomer Royal for Scotland, Charles Piazzi Smyth, about "the planet that is expected to be found beyond Uranus" which could have stimulated his comment.[140] The usual story has his remark occasioned by Airy's bold proclamation at the 29 June Board of Visitors meeting. A few months after Neptune's discovery, Herschel said to Sheepshanks about Adams, "I know nothing of him",[141] in the context of discussing how very much England owed to Adams. We may suppose that Herschel had forgotten his meeting with Adams at the previous year's BAAS meeting, but we can hardly suppose that he would *also* forget such a meeting at the 1846 session. At that week-long conference, if he had spoken those stirring but unreported words in relation to Adams but without naming him, which seemed to be the gist of his *Athenaeum* letter, it is hard

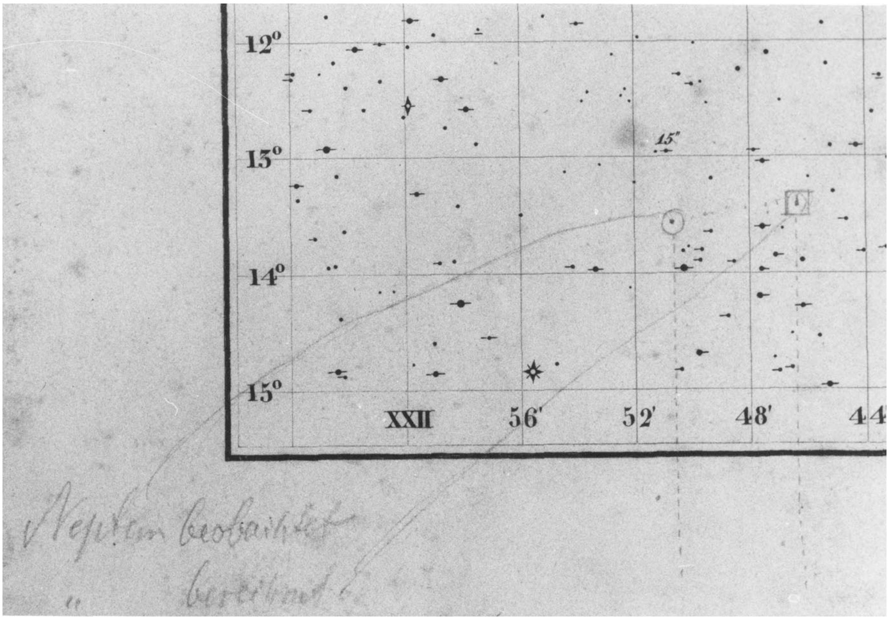

FIG. 5. A part of the 22nd Berlin star-map prepared by Carl Bremiker, and used by Heinrich Louis d'Arrest and Johann Galle in their discovery of Neptune. A circle marks their identification of the planet's position on the night of 23 September 1846, and a square indicates Le Verrier's prediction 52 arcminutes away

to imagine that he would not at some stage have become involved in conversation with Adams — had he been present. Thus Herschel's claim that he spoke the historic words, and that they pertained to Adams, does set up a problem *re* the latter's presence or otherwise at the BAAS meeting. Challis, in contrast to Adams, was scheduled to speak at the meeting — but pulled out.[142]

DISCOVERY AND "LE VOL À LA PLANÈTE"

Johann Galle at the Berlin Observatory received a letter from Le Verrier on 23 September 1846 telling him where to look. Using the new Berlin star-map, in just half an hour (Figure 5) he found the new planet around midnight, shortly before it was due to set, in the buttocks of Aquarius and right next to Saturn. Le Verrier probably received the letter from Johann Galle ("the planet whose position you indicated, really exists") on Monday 28 September,[143] too late for him to announce his news to the weekly meeting of the Academy of Sciences held on that day.[144] He instead gave it to two newspapers, who carried it on the 30th, in which he proposed the name, Neptune.[145] Thus Le Verrier had proposed and published its name, before England (with one exception, see below) had even heard of its discovery.[146] He then wrote to several European observatories on 1 October,[147] proposing both this name and its symbol, the Trident.

Hind became the first Briton to see the new planet, on the evening of 30 September — having received a letter from Brunnard in Berlin that morning — and he immediately wrote to Challis, Adams, Main and Herschel announcing its discovery. To Adams, he wrote: "Understanding from Prof. Challis that you are occupied about the planet of Le Verrier, I think you will be gratified to learn ...",[148] etc. This candid statement from one in a position to know viewed Adams as being "occupied" with the matter — and that was all. "I have easily viewed a disc this evening", he added. The next day his letter to *The Times* announced the "Discovery of Le Verrier's Planet",[149] whence the news reverberated across England.

On 1 October, James Glaisher, a senior RGO worker, wrote (in Airy's absence) to *The Illustrated London News*, stating merely that M. Le Verrier's prediction had been confirmed by Dr Galle in Berlin. Two days later, he wrote again, this time describing Challis's prolonged sky-search and even the "seems to have a disc" remark which Challis stated he had inscribed adjacent to Neptune.[150] Glaisher explained that "about four months ago" both Adams and Le Verrier had "concluded, independently, from theoretical calculations" the position of a perturbing planet. These two mathematicians, he added, "agreed in fixing 325º of heliocentric longitude as the most probable position of the perturbing planet ...", which position was "very nearly" true. This second letter of his was identical in substance with the first public announcement by Challis, writing to *The Cambridge Chronicle* on 1 October.[151] He too announced that Adams and Le Verrier had both arrived at their similar conclusions "four months" earlier in June, having both "agreed on fixing on 325 degrees of heliocentric longitude as the most probable position, which has proved to be very little different from the actual". This newspaper was published in Cambridge on the 3rd, so either a copy was rushed to Greenwich that day, or Glaisher met Challis to discuss the matter.

Both Challis at Cambridge and Glaisher at Greenwich went public with a story based upon events around June–July 1846: Adams's July ephemeris had advocated 325º of helio-longitude for the beginning of September and clearly Challis had this in front of him when he wrote. The *illusion* of agreement between the English and French predictions, and perhaps also with the newly-found planet, to within one degree was thus propounded — whereas the only thing within one degree was the concordance between Le Verrier's prediction and that of the new planet's celestial longitude. These agreed within 52′ (Neptune was found at 327º, Leverrier's prediction was for 326º, and Adams's alleged predictions were around 329º helio-longitude:[152] see Appendix II).

On 5 October Challis wrote to Arago praising Le Verrier's achievement with no mention of Adams. Thus, in the first five days after the news, Challis went public — to his local paper, to the RGO and to Paris — with a version of events radically different from his later story. He then changed his mind: carelessly, he explained to the *Cambridge Chronicle* two weeks later (17 October),[153] he had written his last letter "without consulting memoranda", and then came out with the now-familiar story concerning events of September 1845. If even the person doing the sky-search could not recall that Adams's prediction was supposed to have been made earlier than that of Le Verrier, without consulting "memoranda" to recall the fact, it is hard for us to have much faith in his later-constructed tale. It rather sounds as if Adams and Challis met together after the latter's first *Cambridge Chronicle* letter had appeared, to discuss the question of immortal fame.

Chapman has alluded to the "impeccably honest" Challis,[154] but this was not the French view. On 5 October Challis wrote to Arago about his sky-search: "Je me conformai strictement aux suggestions de cet astronome [Le Verrier], et je me renfermai dans les limites qu'il avait indiquées", and he extolled the glory of Le Verrier's discovery with no hint of any competition;[155] then on the 17th his letter to *The Athenaeum* advanced the priority of Adams. Arago published these two letters, in *Comptes rendues*, declaring them to be incompatible. Leverrier expressed his forthright opinion on this matter, twice, to Airy: "Challis — blanc en Angleterre, noir en France",[156] he declared, adding a week later that Challis had conducted a "two-faced correspondence".[157]

Airy stopped by at Altona on 5 October, to visit Schumacher, the editor of *Astronomische Nachrichten*.[158] He there read through an extensive manuscript submitted by Leverrier, explaining in full the predicted orbit and the mathematics of his perturbation-theory whereby the discovery had been accomplished. Did Airy inform Schumacher that he was about to support a British claim to co-discovery, no details of which had yet emerged? Airy, skilled in the mathematics of celestial mechanics, was one of the few who could understand Le Verrier's analysis. This pre-discovery paper perused by Airy appeared in the *Astronomische Nachrichten* on 22 October.[159]

Airy wrote letters to both Challis and Adams on 14 October concerning the report he was to give at the 13 November RAS meeting, using phrases such as: "The matter being one of delicacy, I will not compromise any one ..." to the former,[160] and "it would be wrong for me to compromise any one" to the latter,[161] and asking permission to cite their correspondence. On that same day he wrote to Le Verrier saying, "You are to be recognized, beyond doubt, as the real predictor of the planet's place",[162] adding that

> ... no person in England will dispute the completeness of your investigations, the sagacity of your remarks on the points it was important to observe, and the fairness of your moral convictions as to the accuracy and certainty of the results. With these things we have nothing which we can put in competition. My acknowledgement of this will never be wanting; nor, I am confident, will that of any other Englishman who really knows the history of the matter.[163]

These were words, complained Sir David Brewster, "which no Englishman can peruse without unmingled pain".[164] This unduly candid letter did indeed cause Airy much trouble and he complained (unreasonably) that the French should not have published it, as they did.[165]

On 16 October, a four-page shocked letter from Le Verrier to Airy expressed indignation over news of the British claim (revealed to him in the letter of 14 October): "Why has Mr Adams kept silent for four months? Why had he not spoken since June if he had good reasons to give? Why did he wait until the planet was seen in the telescope?" He alluded to a 26 June letter from Airy, which "shows that he had no precise information on the position of the planet ... that he was even surprised that I should place it there, where I had, for so placed it appeared to him that it did not take into account the radius vector ...". He found it odd that Herschel "raises a claim in favour of historical documents", as there did not appear to be any in support of the British claim. "Is it enough to have undertaken researches on a subject in order to claim to share the result?" he asked, pertinently, answering with a resounding: "Non!"[166]

It dawned on Challis that he had seen Neptune twice, on 4 and 12 August 1846, so he announced that an Englishman had made the first observations.[167] On 17 October, he gave a detailed account in *The Athenaeum* of the British sky-search as (supposedly) based upon Adams's work, and he and Adams proposed the blissful-sounding name "Oceanus" for it![168] Patriotic fervour was aroused, which soon rebounded upon Challis with a vengeance. His letter promised that "Mr Adams' investigations will, in a short time, be published in detail".[169] The explanations traditionally given for this delay dwell upon Adams's shyness, modesty and reticence, which may not be readily compatible with the way he was endorsing a name for the planet, as well as swiftly ascertaining and publishing its orbital elements.

When this letter reached France, the storm broke. On 19 October, at a turbulent meeting of the Academy of Sciences, François Arago, director of the Paris Observatory, described Adams's work as "clandestine", on the grounds that there had been

> neither mention of any publication of Adams's work, nor example of it shown. This circumstance alone suffices to end the debate. There exists only one rational and just way to write the history of science: it is to rely exclusively upon publications bearing a definite date: abandoning that, all is confusion and obscurity.[170]

(Arago then dismissed Challis's and Adams's name "Oceanus" for the new planet, so it had taken only two days for *The Athenaeum* of 17 October to reach Paris.) A report of this meeting appeared in the 21 October issue of the Parisian newpaper *Le National*, entitled "Le Vol à la Planète", commenting: "The fact is that the three foremost British astronomers have organised a dismal plot to steal M. Leverrier's discovery", adding, "Non seulement M Airy ne possèdent pas des calculs anglais semblables à ceux de M. Leverrier". After itemizing what was known from Britain, it concluded, "Voilà pour la périod qui a précedé la verification definite des predictions de M Leverrier, toutes les documents émaneés des observations Anglais".[171]

John Herschel's diary mentioned the Neptune issue on 25 October: "Wrote to the editor of *The Guardian* in reply to M. LeVerrier's savage letter",[172] adding "These Frenchmen fly at one like wildcats".[173] Le Verrier, in the (London) *Guardian*, had objected to what he saw as unfairness in the British position: "The planet was already discovered, before Mr Adams had printed a line on the subject. And yet Sir J. Herschel puts in his claim." Among men of science, Le Verrier explained, there ought to remain only "friendly rivalry", and this ought not to hinder "but cement the frank and brotherly friendship of those who cultivate it".[174] Trying to smooth matters, Herschel's reply alluded to a letter he had received from Adams, "candidly recognizant of M. Leverrier's priority in the discovery".[175] We have no record of any such letter from Adams, indeed the *Calendar* of Herschel's correspondence[176] lists no letter whatsoever from Adams to Herschel in the wake of Neptune's discovery.

On 31 October 1846, more than a month after the discovery, *The Athenaeum* explained with British *sang-froid* why it would be wrong to rush any statement about British priority, or the production of documents on which it would, in due time, be seen to depend: "Mr Adams' claim, whatever it might be, should not be lost by an early statement of facts upon proof of which it is to rest — they [the French] have hurt themselves, not us. The facts of discovery are not fleeting ... they rest upon records on paper.... [Adams's claim] is brought forward ... in the shape of a statement to be substantiated as soon as the calculations and observations can be published."[177] Did such "records of paper" ever exist? And why did the world have to wait until Adams's calculations could be published, before hearing what his predictions were supposed to have been? Adams's elements for Neptune were published,[178] four weeks *before* his supposed 'predictions' for the hypothetical planet were made public at the RAS's meeting.

John Hind, who was a British expert on the discovery of minor planets, appears as a well-informed witness to the course of events. He was in charge of a private observatory at Regent's Park, having previously worked at the RGO. Adams wrote to him as soon as he had resolved the orbit of "Biela's comet" in February, after discussing his results with Challis, and three letters remain from Hind to Adams in 1846, more than from anyone else to Adams for that year.[179] Hind also corresponded with Challis about the sky-search: Leverrier's paper of 31 August had stated that the new planet should be visible as a disc, and Hind wrote to Challis advising him to take note of this — which the latter eventually did, although too late. Around 14 September 1846 he informed an assistant at the Paris Observatory that Challis and he were trying to locate the new planet,[180] which was presumably the first the French heard about it. Hind's candid, open approach formed a marked contrast to that of the Cantabrians. But, once he noticed the hidden agenda that was unfolding, Hind protested angrily to the RAS's secretary, the Rev. Richard Sheepshanks: "the Cambridge people ... do their best for their own.... I am sure you must have noticed the inexcusable secrecy observed by all those acquainted with Mr Adams' results ... [a] secrecy which I hold to deprive him of all share in the discovery & I am very glad to find that I am not the only one who thinks so."[181] Years later, Hind wrote the RAS's orbituary of Le Verrier. He was clearly the best-informed 'non-Cantab' of the turbulent events.

MAPLESS IN CAMBRIDGE?

Three weeks after hearing of the discovery of the new planet, it dawned on Challis that he had the perfect excuse: he hadn't got the map, so no wonder he couldn't find it. In *The Athenaeum* of 17 October 1846 he explained the method used for his sky-search which had begun on 29 July: "Not having hour XXI of the Berlin star-maps — of the publication of which I was unaware — I had to proceed on the principle of comparison of observations made at intervals" ("Hours" here alludes to the Greenwich Hour Angle, whereby 360° are divided into 24 'hours' so that each map spans 15° of Right Ascension). In other words, not having a suitable star-map, he had instead to use a comparison method on a day-by-day basis, to see whether any stars had moved. That was why he had logged over three thousand stars, down to 11th magnitude. This theme was developed in his RAS presentation on 13 November. How different everything would have been, he explained, if only he could have had the map: "If I had had this map a first sweep would have been unnecessary: I should have compared my field of view with the map at once."[182] (A "first sweep" here alludes to an initial recording of the star-positions.) This argument was used in an editorial in *The Athenaeum* on 5 December: "It was a mere question of luck at the last; and if the Cambridge Library had possessed the twenty-first hour of the Berlin star-maps, Adams and Le Verrier would have changed places."[183]

Neptune was found in Berlin using this map, produced by the Berlin observer Carl Bremiker, which, although ready for publication in 1845, had not yet been mailed out to subscribers. In Cambridge, Challis had the older "Hour 22" Berlin map published in 1833, produced by Argelander. The modern constellation-boundaries had not yet been drawn, and the constellation-images had all gone, so these maps were just grids, with many points drawn in as the stars. Airy, when he advised Challis to perform the sky-search in the summer, had alluded to this Hour 22 sky-map.[184] Neptune was going retrograde during this time of its solar opposition, moving from Hour 22 back into the Hour 21 zone, transitioning into the new Hour 21 star-map towards the end of August.

There were thus four weeks of Challis's sky-search, during which Neptune was on the Hour 22 map in his possession. For most of his quest for Neptune, he had the right map. The correct elements (provided by Adams) of the new planet were first published by Challis, in the above-cited 17 October letter, so one can hardly argue that he did not know its co-ordinates. In that same letter he proudly cited the two positions in which he had logged what subsequently turned out to be Neptune:

Aug 4th, R.A. 21h 58m Aug 12th, R.A. 21h 57m.

The Hour 22 map, which Challis presumably had in front of him, extended from 21 hours 56 mins to 23 hours 4 mins: these maps had 'four-minute' (i.e. one degree) overlap zones. The two positions he quoted were manifestly present on the Argelander map. His argument, that he lacked the right map, was contradicted in the same letter by his own data.

THE RAS MEETING

On 3 November, Challis informed Airy that Adams was still unwilling to organize his report to the RAS meeting of 12 November: "I am sorry to say that I can give no hopes of Adams being able to undertake the Astronomical Report. He is moderator this year, and this with his College duties takes his time. I am in difficulties about this Report, and should be glad to see some means of getting out of it."[185] Far from looking forward to the opportunity of at last presenting his historic equations before the astronomical community, and thereby securing his portion of immortal fame, Adams was signalling an unwillingness to become involved — as too was Challis! Airy, as we shall see, found this attitude of his Cambridge colleagues stressful. Challis is here speaking on Adams's behalf, a not uncommon feature of this story.

Airy distinguished himself at the November RAS presentation: the RAS's historian described his performance as "extraordinarily effective", adding that "no-one can possibly doubt its facts".[186] Airy there stated, of Le Verrier's June memoir, "... the place which it assigned to the disturbing planet was the same, to within one degree, as that given by Mr Adams's calculations, which I had perused seven months earlier".[187] In fact, they were nearly four degrees apart (see Appendix II). Was that deception? It was something very like it. The myth of Adams as the 'first discoverer' of Neptune came to depend heavily on the notion of the two astronomers as having arrived at the *same* result. Fifty years later in 1896, James Glaisher, son of the above-mentioned Glaisher, restated this central British illusion: "The position thus assigned by Le Verrier to the disturbing planet differed by only one degree from that given by Adams in the paper which he had left at the Royal Observatory more than seven months before."[188] That misconception has encouraged historians in the view that they need not bother with astronomical details concerning mean versus true helio-longitude — Adams having given his predictions as the former while the latter is required for finding the planet. Our main characters, however, as senior wranglers and Cambridge astronomy lecturers, cannot be suspected of having had difficulty in this matter.

Adams, at the November RAS meeting, for the first time released his computation results to the public, impressing those present with his deep grasp of perturbation mathematics.[189] He there presented the identical list of yearly 'residuals' of his Uranus-perturbation values as had allegedly been presented to Airy thirteen months previously, and then sent again to Airy in his letter of 2 September 1846[136] — not differing by so much as a hundredth of an arcsecond. The temporal location of these vast computations was not entirely clear, as to whether they had all been performed before the discovery or perchance the previous year. Three perturbation-computations for 1843, 1844 and 1845 were included, on which his argument for the 'Hyp II' solution being preferable to the 'Hyp I' was made to rest; he had not done these in the previous year, and only the first of them was included in his 2 September letter, for which he now reached a different value, so clearly he had been continuing his calculations since that date.

Only the 325° and 323° longitude (mean, heliocentric) values of Adams's letter of 2 September were presented, without mention of the true, heliocentric positions of these solutions, presumably to avoid drawing attention to the fact that his second solution was *less* accurate than his first. Le Verrier had written to Galle with both mean and true longitude of the hypothetical planet, i.e. he told the astronomer where to look; whereas, all Adams claimed to have given Airy was the celestial longitude of a fictitious, circular-motion 'mean' planet — as he stated both in his 2 September letter and in his RAS presentation. Challis told the RAS that he had received, fourteen months earlier, a geocentric-longitude position, but Adams's testimony did not endorse that.

A CLOSE LIAISON, AND ADAMS'S UNDATED PREDICTIONS

Only after the discovery of Neptune, in his *Athenaeum* letter of 17 October 1846, did Challis claim that he had received parameters of "an assumed exterior planet" from Adams in September 1845; then, at his 13 November RAS presentation, this became "a paper" that Adams had given him. No-one is on record as having seen this document *during Challis's life*, or of hearing any details of its contents (except merely that it alluded to "the New Planet" as Challis informed the Cambridge Syndicate on 12 December).[190] Adams's RAS address emphasized the great moment when, after years of solitary toil, "… I communicated to Professor Challis, in September 1845, the final values which I had obtained for the mass, heliocentric longitude, and elements of the orbit of the assumed planet". Are we to assume that these "final values" of Adams's Herculean labours, as handed to his close friend and colleague, the top Cambridge astronomer, were of so little interest that no-one asked what they were? We can nowadays view what is presumably the document,[191] which has the month and year (but no day) written on it in what is presumably Challis's hand, reminding us of the document that Airy likewise produced for the RAS meeting. In his 17 October *Athenaeum* letter, Challis explained that "The same results, somewhat corrected, he [Adams] communicated in October to the Astronomer Royal", a sentence conspicuously echoed by Adams's RAS presentation a month later: "The same results, slightly corrected, I communicated in the following month to the Astronomer Royal." The problem is, that *in no way* can one of these documents be viewed as a "slightly corrected" version of the other, because they represent fundamentally different computations. The basic parameters — eccentricity, the apse line position, the mass and longitude of the hypothetical planet — all differ between the two documents, the only thing they have in common being the assumed Bode's-Law radius. The very high eccentricities, tenfold that of Earth's orbit, tend to make it more likely that Challis's document was composed later than Airy's: Airy's document gave an eccentricity of 0.16 while Challis's document had 0.14, then by September 1846 Adams put it at 0.12 for his 'Hyp II'. Adams's hypothetical planet (like that of Le Verrier) was close to its perihelion, so, had he decided to increase its eccentricity in this substantial manner between September and October (i.e. from 0.14 to 0.16), the planet's distance from the Sun would have contracted, and this would have had major ramifications for the perturbation-terms, which would all have needed re-computing. His work could hardly have been described as "finished", with so large a change made to the results within weeks.

The undated Adams prediction-document found amongst Challis's letters ("no. 32") is folded into four and fits neatly into a small envelope adjacent to it ("no. 31", the only envelope in this archive), addressed to Challis by Adams. Together with this envelope is a (hitherto-unmentioned) short note on a small piece of paper folded to fit into it, simply stating: "M Verrier recommends exploring for the new Planet the region of the Ecliptic from 321° to 325° heliocentric Longitude." David Dewhirst, who archived this Institute of Astronomy file, noted that he did not think that this note was in Adams's handwriting; however, two American colleagues of the present writer[192] examined this note and agreed that, in fact, it was in Adams's hand: who else, after all, could have written it? The contents of this envelope therefore give us, I suggest, the immediate (and shocked) response of Adams upon reading Le -Verrier's June prediction, in the *Comptes rendues*. This is his first dateable letter upon the subject of the new planet.

The core problem of this story is Adams's apparent reticence, if indeed he had just made the most remarkable prediction in the history of astronomy. Sedgwick, after having "a very long chat" with Adams on 6 December 1846, commented that he "has acted like a bashful boy rather than like a man who has made a great discovery". I endorse the thesis proposed by Dennis Rawlins, that Adams had *no* confidence in *any* solutions he reached, prior to 2 September 1846,[193] mere weeks before the discovery. Only after Le Verrier had made his second public prediction did Adams try decreasing the mean orbit distance, abandoning Bode's Law, and thereby gaining results in which he manifestly felt some confidence — enough to send a letter, dated and signed! On the traditional view, as expressed for example by Smart, there is the well-nigh unanswerable question as to why Adams waited for seven weeks after the discovery, before the world could hear what the British prediction was supposed to have been.

Writing in 1904, Professor Herbert Turner of Oxford assumed a distance between the two Cambridge astronomers, by way of trying to make sense of the traditional tale[194] — a view contradicted, as we have seen, by Adams's diary. At the November 1846 RAS presentation, Challis presented Adams's predictions as being more or less unvarying, thereby taking the blame for the British failure and ensuring that he would be forever remembered thereby. People believed Challis's tale because, as Sampson put it in the RAS's official history of the events, no-one could invent a story of so feeble a performance.[195] On 12 December 1846, Challis reported to the Observatory Syndicate that Adams "has at once done honour to the University, and maintained the scientific reputation of the country"; adding that, between September 1845 and the midsummer of 1846, "... I had little communication with Mr Adams respecting the New Planet".[196] Are we to believe that, although meeting several times a week, the two of them scarcely discussed any new-planet predictions, and did not lift a finger to seek for it through the telescope, prior to the arrival of Leverrier's prediction? Such and other comments which Challis made were calculated to give the impression of a distance between them.

AIRY'S IRE

As the image of Adams as the heroic but reticent first predictor of Neptune's position began to take shape, Airy realized that it depended upon his being prepared to accept 'blame'. Had he somehow not received Adams in October of 1845, and had he ignored his priceless insights? Turning over likewise in his mind Challis's awesome ineptitude in failing to find the star over a six weeks' search, he finally blew his top — to his old friend the Cambridge geologist, Adam Sedgwick, on 8 December 1846. The provocations sent to him by the latter have already been quoted. Airy comes the nearest here to telling the truth, as far as was permitted by his status as the top government scientist.

> I showed your yesterday's letter to my wife — received last evening. I have no doubt that the facts of Adams' call were as you & Adams have made out. My wife seems to have a notion of his card being brought to her in my absence, but nothing further is known. I was at the end of October 1845 busy almost every day at the Gauge Commission and on October 29th my boy Osmund was born. I am ashamed to mention these things; I have no misgivings whatsoever of the spirit with which you have entered into the matter [this correspondence]; but I must have a very low opinion of those who have so taken it up that my old friend has felt himself obliged to question me as if I were a criminal. In regard to the publishing of Adams' priority: I refer you to what I said in my last letter, and only add that if you were not so serious about it, I should think it very ludicrous. I will put your propositions into the form, in which there is not the most trifling exaggeration.
>
> Sedgwick's Theory, & Rules thereon founded
> 1. Every Cambridge man is a Baby, and cannot walk out except he has a Nurse to trot him out.

2. Only extra-Cantabs are eligible as Nurses. No Resident, not even a Plumian Professor, is competent to this office.
3. Simple nomination of an Extra-Cantab. by a baby imposes on such extra-Cantab., nolente volente [willy-nilly], all the duties and responsibilities of Nurse.
4. The regular duty of Nurse is, to divine the unexpressed wishes of the Baby to walk, and then to take him out.
5. The responsibility of the Nurse is not removed even though the baby take a fit of the pique (?) and refuse to answer questions, or though the baby refuse to clothe himself in what the Nurse considers to be a proper walking dress.

> I do not enter into any details about Adams' notion that the examination of the effect of the radius vector was unimportant. It now suffices for my guidance that I thought it important and still think so. Perhaps it might be sufficient for your persuasion, to tell you that Leverrier also thought it important.[197]
>
> And here finishes my Cambridge discussion. The next blow will probably be from Paris....[198]

The notion that the publishing of Adams's priority claim had been Airy's duty is dismissed as "ludicrous". Airy here appears as the nurse who must lead out the recalcitrant (Cantab.) babes for walks, select their dresses and even "divine their unexpressed wishes". One feels that Airy never apprehended the extent to which he himself wrote the script for the Neptune saga: no British case would have existed without the decisions he took.

This reply, which has been viewed as "satirical and deeply contemptuous",[199] needs to be placed within the context of an intense nine-letter correspondence between Sedgwick and the Airys, over the week 3–10 December 1846, during which time seven letters went back and forth, grumbling from Sedgwick and livid from Airy, plus a couple more from Richarda Airy.[200] Sedgwick's reply on the 9th conceded that Adams had acted "like a very simpleton", then Airy's letter by return reached its mysterious climax about "... such a train of circumstances that you have been compelled to ask questions which no gentleman if free would ask and which no gentleman could be expected to answer". What was this train of circumstances, and what were the terrible questions, that should neither be asked nor answered? There was *something* he didn't want to be queried upon. The truth (I suggest) is that he had not been in possession of the fabled document from Adams during the past year, though political pressure and national sentiment obliged him to aver the contrary. Adams had turned up unannounced in the week before Airy's wife was having her ninth baby (29 October 1845), and when a prosecution was just commencing of a senior assistant at the RGO, on a charge of incest plus murder of the incest offspring, where Airy's chief assistant Robert Main was required to give evidence.[201] The trial of this case at the Old Bailey kept generating lurid newspaper headlines, linked to the Observatory. It is hardly surprising if Airy's memory was a little hazy over this period.

Airy later claimed that he had been "severely abused by both English and French",[202] in the *sole paragraph* of his 600-page autobiography that concerned Neptune. The debate was stressful. The RAS's secretary Richard Sheepshanks commented: "I have been struck with the sort of fatality, which has burned us to the very heart. The most prudent precautions & best schemed places of observation have failed from causes which no-one could foresee and which few will now allow for."[203] It is not often that an astronomer reports being "burned to the heart" by "a sort of fatality". According to Sophie de Morgan, Sheepshanks appeared "though amusingly sarcastic in his letters, temperate in his public behaviour. If he had not been so at this time, the concussion in the Society might have ended in complete disruption".[204] Herschel found that the perceived failure of British astronomy "has really made me ill" and had "given me more pain and grief than any national event...".[205] In the same week as the above letter by Airy, *The Athenaeum* editorial accused François Arago of "mania", and of having a "distorting mirror of national bias".[206] When Airy's brother William visited St John's at Cambridge that same week, he heard so much about Airy's having "snubbed" Adams that he ended up dreaming about Neptune.[207] Concerning the soon-impending decision that the RAS had to make about awarding its yearly medal, an anxious letter from Herschel to Sheepshanks alluded to the "disastrous combination of circumstances" thereby generated, concluding: "burn this."[208] Charles Babbage, a dissident voice amongst RAS members, was one of the group of RGO Visitors who met on 29 June (his protest in *The Times* on 15 March 1847 has been cited). In a formal letter to the RAS council he stated: "I have clearly examined all the accessible papers relative to the new Planet and I am *very reluctantly* at the last moment compelled by my medical adviser to forego my intention of explaining personally my views to the General meeting."[209] These high stress levels are, I suggest, pertinent to the manner in which the British case was constructed, retrospectively.

EARLY 1847

Airy, in the first week of January 1847, voted *against* a motion to award the prestigious yearly gold medal of the RAS to Le Verrier. The stern bye-laws of that society mandated a 3:1 majority at least for a medal to be awarded. The outcome of the vote was 10:5 in favour, so Airy had almost exerted a casting vote. A proposal to amend the bye-laws just for that year in order to permit the awarding of two medals had been defeated (this motion is especially associated with the name of Charles Babbage, but judging from the correspondence extant it seems that most members would have favoured it). From hailing Le Verrier as a second Copernicus, Airy had moved two months later to voting against awarding a medal to him. Could not the next year's medal be awarded to Adams? Intense stress built up, as RAS members apprehended that their Society was incapable of awarding any medal at all that year. Babbage in *The Times* denounced those who had voted against the motion: "I much regret that the small minority of that council should have availed themselves of a privilege conferred upon them by the Society to prevent the awarding of medals to any discovery not eminently fit for them, into a means of preventing any such award in the strongest case which has yet occurred during the existence of the Astronomical Society."[210] The RAS (I suggest) just did not have enough time, from the publication of Adams's work in December 1846 to the finalizing of its once-yearly medal-award in January 1847, to cope with the turmoil going on.

On the night of 2 February 1847, the Washington Observatory verified that a star given in Lalande's *Histoire celeste* and present on a Berlin star-map was *not* in the heavens.This 'missing star' had been observed by Lalande on the night of 8 May 1795. The American astronomer Sears Cook Walker, who had proposed this act of negative-detection, inferred that it must have been the newly-found planet (the British astronomers Hind and Adams had likewise started searching through the massive *Histoire celeste* database, but had, for whatever reason,[211] drawn a blank). Walker thereby became the first astronomer to compute a realistic orbit for the new object. His results were communicated on 8 February.[212] Its mean distance he found to be 30.2 AU, its eccentricity 0.0088, its perihelion 0° and its period 166.4 years (the modern values being 30.05 AU, 0.0086, 45° and 164.8 years, respectively). Remarkably, Lalande had predicted in 1801 that something of this sort would happen: "Herschel discovered one by accident, and when another [primary planet] shall be discovered, it will be found in our fifty thousand stars, thus giving at once the means of determining its period of revolution."[213] Walker was grateful that he had been able to fulfil this prediction. It took a month for the slow-travelling steamship to convey the news to Europe.

James Challis had published Adams's first computations of the found planet in his *Athenaeum* letter of 14 October 1846, with the distance of the new planet from the Sun given as 30.05 AU. This happens to be its mean radius to four-figure accuracy and one sometimes finds Adams being given credit for ascertaining this,[214] but no such claim was made in Challis's letter. Adams had also found the tilt of the orbit relative to the ecliptic as 1° 45′, this being correct to within an arcminute, and the position of the node-axis to be 129° 43′, this being within two degrees of the modern value. On 12 March the RAS's *Monthly notices* printed a mean orbit of the new planet from Adams, 30.2 AU,[215] but such an estimate, based on observing merely a fraction of 1% of its complete orbit, was not worth a great deal. In April, using the Lalande 'missing star' position, Adams was prepared to express the eccentricity of the orbit as 0.0093.[216] Thus Adams deserves credit for (a) being the first to find the orbit-radius of the new planet, and (b) locating the nodes of its orbit and its accompanying angle of tilt. He thereby became the first European to publish the mean orbit-radius of Neptune, although there was no sound basis for his result, as Airy observed.[217]

The great debate went on throughout 1847, concerning the difference between the actual orbit of the new planet and the mathematical predictions. The American astronomers began to appreciate how the perturbation caused by the 2:1 ratio in the orbit periods of Uranus and Neptune, which is larger than any other resonance in the solar system, was the primary interaction between these two spheres: this had been wholly unsuspected by Adams or Le Verrier, so in what way could their work have been valid?[218] The American astronomer Benjamin Peirce expressed his view that the discovery was a mere "happy accident"[219] and Sears Walker endorsed this, saying that "The present visible planet Neptune is not the mathematical planet to which theory had directed the telescope. None of its elements conform to the theoretical limits".[220] Even Le Verrier finally came round to appreciating the force of Herschel's argument, that, were it not for the concordance of Adams's prediction with his own, he would have had a hard job defending the logic of his discovery. It was, Herschel explained, *edifying for science*, and promotive of its public esteem, that there had been two independent but concordant predictions, eliminating any suggestion of a merely fortuitous coincidence.[221]

The resolution of this stormy debate turned out to depend upon the newly-discovered moon of Neptune. Triton was found by the amateur observer William Lassell of Liverpool, whose telescope[222] was powerful enough to allow him to discern the orbit of Neptune's moon before anyone else.[223] As this object gradually came into focus for astronomers, it permitted an estimate of Neptune's mass, and thereby an estimate of the pull that it exerted upon Uranus became feasible, in the spring of 1848. In February 1848, as the barricades went up around the Paris Observatory with revolution on the streets, did the Neptune debate conclude? There has been "such an uproar here", Le Verrier wrote to Herschel in July 1848, owing to the librarian of the Observatory , M. Babinet, advocating Peirce's "happy accident" thesis.…[224]

CONCLUSION

> M. Leverrier is the man who has done the work, and to him the honour is due.... The attempt by Mr Adams was merely a rude sketch, which he was prevented from perfecting by the attempt to determine the latitude as well as the longitude of the body sought for. In trying to find out the inclination of the plane of the planet's orbit, he wandered from the object directly before him; and the -calculation probably got entangled in nodes which Mr Adams could not untie. M. Leverrier, from the remoteness of the planet, concluded the plane of its orbit would so nearly coincide with that of Uranus as to allow of its being assumed to be the same. He thus, like another Alexander, cut the Gordian knot: this saved time and gave him the victory ... he gave no notice of the inclination of the orbit, nor of the line of the nodes.

This was the verdict of John Taylor, writing in the *Liverpool Echo*, on 11 December 1846.[225] James Challis wrote to the London *Guardian* newspaper at the height of the controversy, in November 1846, in the wake of a rebuke he had received from Le Verrier: "I had no intention of putting any claim to discovery, either for Mr Adams or myself. The facts I stated were, as I thought, sufficient to show that no such claim could be made."[226] Adams's researches were, he explained, "spontaneous and independent". Making them public should not take "in any degree" from "the honour of M. Le Verrier's discovery"[227] (the British astronomers Airy and Herschel were indignant at Challis for expressing such a view). But this was not a view that prevailed.

The Cambridge mathematician Adams was the "first theoretical discoverer" of Neptune, affirmed Friedrich Struve in January 1847, director of the Russian observatory at Pulkova.[228] Sir David Brewster went a step further in May 1847, endorsing Adams as having been "the first discoverer of the New Planet".[229] In the course of describing what he alluded to with some hyperbole as "a discovery certainly the most remarkable in the annals of science",[230] he characterized Le Verrier as "the second theoretical discoverer of the new planet".[231] He expressed what has become the canonical view, that Adams had completed his labours, and given his results to two of Britain's most eminent astronomers, before Le Verrier had even begun working on the subject. Airy had for seven months "kept in his pocket the elements and place of the new planet". Only seven months?

Charles Babbage wrote to *The Times* in the context of this debate, saying that "The modern law relating to discoveries is that they take their date from the time of their first publication to the world",[232] thereby echoing the French view earlier expressed by Jean Baptiste Biot: "The discovery belongs to him who proclaimed and published it to all.… This is the common, unwritten law, without which no scientific title could be assured",[233] but Brewster rejected this: priority of invention or discovery did not and should not hinge upon publication. Instead, he endorsed the position taken by Airy, whose *Athenaeum* letter had opposed Babbage's view:[234] why, Brewster explained, Babbage's view would hand to Leibniz the honour of first discovering calculus, a conclusion which "has never yet been drawn". The Royal Society decided in November 1846 to award its annual Copley Medal to Le Verrier, an act which Brewster denounced as a "violation of historical truth" and a "compromise of principle".

Airy presented in November 1846 a version of events that was fairly well-balanced (in this writer's view), in his address to the Royal Astronomical Society, and this gained a wide degree of acceptance. It gave a greater credit to Le Verrier than to Adams, with the former extolled as a "philosopher" and the latter being cast merely as "the industrious mathematician". This was before Adams's work had appeared in print, an event that subsequently helped to swing the balance. In the following year Brewster's staunchly pro-British version of the discovery came to prevail, with the result that severe blame fell upon the Astronomer Royal. It became the accepted narrative, whereby Adams the young Cambridge genius had attained his conclusion before anyone else, by his diligent private study, and, but for the sad ineptitude of those to whom he entrusted his findings, would have been honoured as the sole discoverer. Adams welcomed Brewster's essay as "a splendid article about the New Planet".[235] The present account reevaluates this perspective, focusing as it does upon the English co-prediction claim, and covering the events from the autumn of 1845 to early 1847. This may be the first account the reader has encountered in which 'blame' does not fall upon Airy — on the contrary, he appears as having more or less constructed the British case, for England's sake.

Now that the document with the supposedly original solution given by Adams to Airy in 1845, the "physical basis of Britain's priority claim" as Rawlins has called it,[236] has returned from Chile, a re-evaluation of its significance is called for. The thorny problems attending it have been alluded to: as originally published in November 1846[237] it had no date at its top and it ended with the rather dull words, "... of the planet". There is no record of Airy's having shown this document to anyone else or even intimating that he owned it in the thirteen months during which he was supposedly in possession of the most remarkable astronomical prediction ever made.[238] Even in the weeks after the planet had been found, no-one stepped forward to claim that Britain's Astronomer Royal was to be seen with a document demonstrating an agreement within several degrees of the Adams and Le Verrier predictions. When the 'original' was produced, it had a month and year inscribed at the top by way of dating, in a different handwriting from the rest, presumably Airy's, and ended with the startling words, "... of the new planet". Given Airy's meticulous propensity to date every document around him, how likely is it that he would write only the month and not the day, unless indeed he was doing so long after the supposed date?

There were two occasions on which Airy assessed the relative merits of the predictions of Adams and Le Verrier. The first has been cited, in the letters to Le Verrier portraying him as the "real predictor of the planet's place", and expressing confidence that anyone "who really knows the history of the matter" would agree (letters of 14 and 21 October 1846). Then, a month later, addressing Britain's Royal Astronomical Society on 13 November, he compared Le Verrier with Copernicus:

> since Copernicus ... 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 [Le Verrier's memoir] which preceded the discovery.[239]

A stark contrast is here made with Adams as the mere "industrious mathematician" who published his results only after the discovery. One might have expected the British people to accept the firm and twice-stated views of their own Astronomer Royal upon this subject, but, alas, they did not — far from it. Airy was severely castigated on all sides, to the extent that the Neptune affair "seemed unduly to overshadow him for the rest of his life".[240]

Airy's Account was "full of the liveliest admiration for Le Verrier, and almost completely destitute of but the barest recognition of Adams's achievement", as Smart complained.[241] Historians have viewed this attitude as problematic, whereas the present account has not claimed to know better than Airy on this matter. His judgement is here viewed as having been soundly based, both from his comprehensive acquaintance with the historical characters and his grasp of the theoretical issues involved. There were certain things he could not say, given his public role, but that is a different matter.

Like a double-headed comet burning in the night sky, the twin talents of Adams and Le Verrier homed in on the unseen sphere. Britain had found the law of gravity, predicted Halley's comet, found Uranus, and was it not merely through Airy's ineptitude (as Brewster and a train of others have averred) that it did not also find the eighth planet, that final giant sphere in the depths of space? It was not, as has here been explained. Adams had indeed done his work, but in private, whereas in public he vacillated, and was never quite there when the occasion demanded.

Challis might well have been the first to find Neptune, if only he had allowed himself to be guided by Le Verrier's prediction, instead of being led astray by Adams's vacillating dilettantism. As news of the failed British sky-search leaked through to the public, a storm of indignation erupted. Airy and Challis were in imminent danger of becoming pilloried as figures of ridicule, from which they endeavoured to extract themselves by developing a British prior-prediction claim, far beyond what was warranted. They constructed Adams as a British mathematical hero, but this turned out to have exacted a terrible cost from their own reputations. For him to become that hero-figure, they were doomed to take 'the blame', victims of a national fervour which they themselves had kindled. It was a severely ironic outcome, far from what they had planned.

Years later, Airy issued a strict edict, that “No paper whatsoever is to be destroyed” within the RGO, and that, should any piece of paper be unwanted by any person, “it is to be delivered to me”.[242] His behaviour came to be regarded as somewhat obsessional in this regard, and humorous tales developed. Was this the working of his conscience, as a result of a certain piece of paper he was once supposed to have, and on which the reputation of British science was seen to depend, but which he had, regrettably, mislaid?

APPENDICES

I. *Distance estimates (in AU) of predicted planet*

	Mean Dist.	Perihelion Dist.	Discovery Dist.	Eccentricity	Anomaly
Le Verrier final	36.2	32.3	33.1	0.107	42°
Adams Hyp. 1	38.4	32.2	32.3	0.161	11°
Adams Hyp. 2	37.5	33.0	33.4	0.120	27°

Adams gave his values as fractions of an unstated Uranus mean-radius,[243] so reconstructions vary slightly.[244]

II. *Discovery-day longitude estimates*

In order to compare accuracies, the predictions have here been converted to true helio-longitude at the discovery date, 23 September 1846. Adams’s ‘Hyp. I’ prediction, of uncertain date, and his ‘Hyp. II’ prediction, of September 1846, are thereby compared with Leverrier’s of June and August 1846. The true helio-longitude values obtained are identical with those of Rawlins.[245] Similar computations were made by Herschel[246] and Biot[247] (Adams also gave a ‘Hyp I’ mean helio-longitude in his letter of 2 September to Airy, as 325° 8′ for 1 October 1846). The true helio-longitude of Neptune on the day of discovery was 326° 57′.

	Adams 1	Adams 2	Leverrrier 1	Leverrier 2
Mean helio-long.	323° 34′	323° 2′	325°	318° 47′
Epoch	1 Oct. 1845	1 Oct. 1846	1 Jan. 1847	1 Jan, 1847
Orbit period (yrs)	237.6	227.4	237.6	217.4
Motion to discovery day	1° 28′	–0° 2′	–0° 24′	–0° 27′
Mean helio-long. at discovery day	325° 2′	322° 59′	324° 35′	318° 19′
Perihelion	315° 55′	299° 11′		284° 45′
Anomaly	9° 7′	23° 48′		33° 34′
Eccentricity	0.161	0.121	0	0.108
Equation of centre	3° 38′	6° 27′	0	7° 39′
True helio-long.	328° 41′	329° 27′	324° 35′	325° 58′
Error	1° 44′	2° 30′	–2° 21′	–0° 58′
Herschel’s estimate, 1852	328° 42′	329° 25′		326° 0′

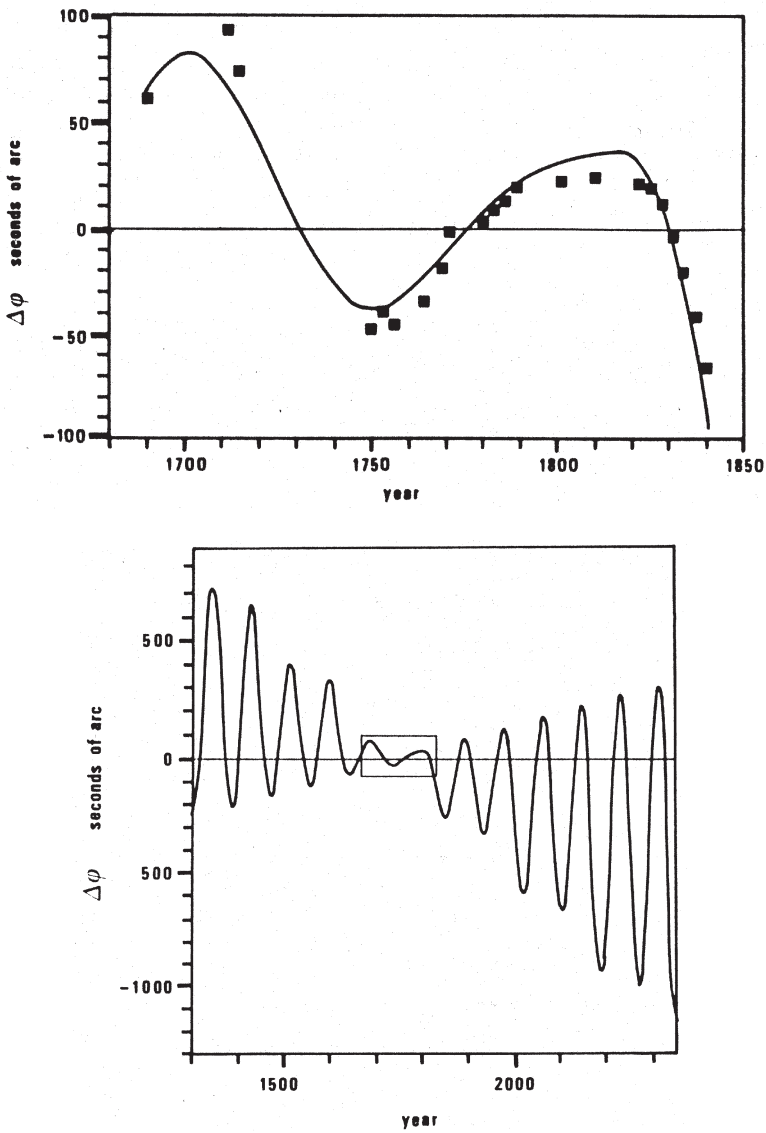

FIG. 6. The perturbations of Uranus: (*above*) 'residual perturbations' of Uranus over the period 1690–1846 (its deviation in arcseconds of celestial longitude from its expected Kepler-ellipse orbit), as ascertained by Adams, these being essentially the same data as Le Verrier had used. (*below*) a modern reconstruction of how the modern perturbation-equations predict the perturbation of Uranus by the pull of Neptune; the reconstruction spans a millennium, and its central box indicates the realm of the historically-observed perturbations. Reprinted with permission from H. M. Lai, C. C. Lam and K. Young, "Perturbation of Uranus by Neptune: A modern perspective", *American journal of physics*, lviii (1990), 950. © 1990, American Association of Physics Teachers.

III. *Uranus's perturbation-values 1690–1840*

The perturbations of Uranus's orbit as used by Adams and Le Verrier[248] are shown in Figure 6. The shape of this curve made no sense to *any* historian or astronomer[249] until the 1990 paper by the Chinese physicists Lai, Lam and Young.[250] The modern equations for Uranus's perturbation by Neptune are an order of magnitude larger than those found by the historical characters. The principal term for the perturbation in celestial longitude of Uranus by Neptune has a maximum of 52 arcminutes over 4,200 years.[251] The 2:1 ratio between their periods is inexact by nearly two percent, and this generates 'beats' over a long-period rhythm. The Lai study showed how the perturbation of Uranus by Neptune had reached a *minimum* over this historical period and not a maximum as had been supposed. (For earlier, incorrect perturbation-theory, making displacement symmetric about the 1821 Uranus-Neptune conjunction, see, e.g., Grant.[252]) The modern terms, for the amplitude of perturbation in the celestial longitude of Uranus due to Neptune, are an order of magnitude greater than that with which the historical characters imagined that they were dealing.[253] The perception of this fact, for whatever reason, does not appear prior to the 1990s.

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REFERENCES

ABBREVIATIONS

CR: Comptes rendus hebdomadaires des séances de l'Académie des Sciences (Paris).

MNRAS: Monthly notices of the Royal Astronomical Society

MRAS: Memoirs of the Royal Astronomical Society

ONRAS: Occasional notes of the Royal Astronomical Society

SP: The scientific papers of John Couch Adams, ed. by W. G. Adams, i (Cambridge 1896)

AM: Adams Manuscripts at Truro Municipal Archive, Cornwall

COA: Cambridge Observatory Archives §D.3 (at CUL)

CUL: Cambridge University Library
JCL: Adams MSS at St John's College, Cambridge
McA: The McAllister Collection of copied letters at St John's College, Cambridge

RGO: Royal Greenwich Observatory
RGON: The RGO's 'Neptune file' now in the CUL (all Airy letters in RGON are faded press copies, often only half legible)
RS:HS: John Herschel letters at the Royal Society

1. Letter, Challis to Airy, 19 Dec. 1846, RGON: McA, 33.12.f.12–14.
2. John Keats, *On first looking into Chapman's Homer*, 1820.
3. *Dio,* ix/1 (1999), 5: "Celestial mechanics' most miraculous day."

4. See www.ucl.ac.uk/sts/nk/neptuneletters.htm for summaries of 435 Neptune-discovery related letters.
5. Letter, Herschel to Sheepshanks, 17 December 1846, RS:HS 16:50.
6. Simon Schaffer, "Scientific discoveries and the end of natural philosophy", *Social studies of science,* xvi (1986), 387–420, p. 401.
7. For the concept of 'discovery' as socially-constructed in retrospect, see Helge Kragh and Robert Smith, "Who discovered the expanding universe?", *History of science,* xli (2003), 141–62, p. 142.
8. Robert Smith, "The Cambridge network in action: The discovery of Neptune", *Isis,* lxxx (1989), 395–422.
9. Tom Standage, *The Neptune file* (London, 2000).
10. Patrick Moore, *The planet Neptune: An historical survey before Voyager* (Cambridge, 1986, 1998).
11. Ian Ridpath, "Who stole the Neptune papers?", *Popular astronomy,* January 1988, 5; Dennis Rawlins, "The Neptune conspiracy: British astronomy's post-discovery discovery", *Dio,* ii/3 (1992), 115–42; Dennis Rawlins, "The 'theft' of the Neptune papers", *ibid.,* iv/2 (1994), 92–102. I have greatly benefitted from Rawlins's thesis on the role of Adams in the discovery.
12. Patrick Moore, "The hunt for Neptune", *Sky & telescope*, xcii/3 (March 1996), 42–43; William Sheehan and Richard Baum, "Neptune's discovery 150 years later", *Astronomy,* xxiv/9 (September 1996), 42–49; and Allan Chapman, "The story of a failed discovery", *Astronomy now*, x/9 (September 1996), 42–45.
13. Olin J. Eggen, entries on Airy and Challis in *Dictionary of scientific biography*. Secretary E. MaCauliffe at La Serena Observatory, Cerro Tololo, Chile, phoned the RGO.
14. Moore, *Neptune* (ref.10), 116–19 (list compiled by David Dewhirst).
15. Allan Chapman, "Private research and public duty: George Biddell Airy and the search for Neptune", *Journal for the history of astronomy,* xix (1988), 121–39.
16. W. M. Smart, "John Couch Adams and the discovery of Neptune", *ONRAS,* ii (1947), 33–88.
17. Chapman, "Private research" (ref. 15), 139, n. 54.
18. *Ibid*., 139, n. 57. It was however also present in the McAlister collection: McA 34.17.1.
19. *Ibid.,* 136, n. 6.
20. Morton Grosser *The discovery of Neptune* (Cambridge, MA, 1962; based on his Ph.D. dissertation of 1961).
21. Chapman, "Private research" (ref. 15), 126.
22. Smart, "Adams" (ref. 16), 74.
23. *Ibid*.
24. Letter, Sedgwick to Airy, 9 December 1846, 2, RGON, McA 33:10.
25. G. B. Airy, "Account of some circumstances historically connected with the discovery of the planet exterior to Uranus", *MNRAS,* vii (1846), 121–44, p. 135; G. B. Airy, "Account ...", *MRAS,* xvi (1847), 385–414, p. 402 (reproduced in Moore, *Neptune* (ref. 10), 87–106).
26. Letter, Airy to Challis, 9 July 1846: COA no. 2; RGON.
27. H. Turner, *Astronomical discovery* (London, 1904), 65.
28. H. M. Harrison (a descendent of John Couch Adams), *Voyager in time and space: The life of John Couch Adams* (Lewes, Sussex, 1994).
29. Moore, *Neptune* (ref. 10), 104.
30. SP, pp. lvi–lviii; H. Spencer Jones, *John Couch Adams and the discovery of Neptune* (Cambridge 1947), 15–18; or see the author's website www.ucl.ac.uk/sts/nk/neptune/crown.htm.
31. Airy, "Account" (ref. 25, 1846), 130; Moore, *Neptune* (ref. 10), 100.
32. J. Challis, "An account of observations undertaken in search of the planet discovered at Berlin on Sept

23, 1846", *MRAS,* xvi (1847), 415–26, p. 423; Airy, "Account" (ref. 25, 1846), 145–9, p. 147.

33. Challis's sweep book, COA, xxxix, 53.
34. Letters, Airy to Hussey, 23 November 1834, and Airy to Bouvard, 12 October 1837, RGON.
35. G. B. Airy, *Gravitation: An elementary explanation of the principal perturbations in the solar system* (London, 1834), 38.
36. Letter, Airy to Schumacher, 24 February 1838, published as "Schreiben des Herrn *Airy*, Astronomer Royal, an den Herausgeber", *Astronomische Nachrichten,* xv (1838), cols 217–20. See esp. col. 219 (on the radius-vector of Uranus).
37. A. Bouvard, *Tables astronomiques publiées par le Bureau des Longitudes de France contenant les tables de Jupiter, de Saturne et d'Uranus construites d'après la théorie de la mécanique celeste* (Paris, 1821). Bouvard's nephew Eugène Bouvard had continued the endeavour and briefly described new tables that he had constructed in "Nouvelles tables d'Uranus", *CR,* xxi/9 (1 September 1845), 524–5. For a discussion of the younger Bouvard's never-published and now lost study of Uranus's motions, see E. Lias, *L'histoire de la découverte de la planète Neptune* (Leipzig, 1892); also R. Baum and W. Sheehan, *In search of planet Vulcan: The ghost in Newton's clockwork universe* (London, 1997), 85–86.
38. *The nautical almanac and astronomical ephemeris for the year 1834* (1833) was the first to have this new format.
39. *Astronomical observations made at the Royal Greenwich Observatory* (published yearly by the Board of Admiralty).
40. They also used the same 17 pre-discovery observations of Uranus given by Bouvard's *Tables* (ref. 37), p. xiii, from that of Flamsteed in 1690 to Lemonnier in 1771. See ref. 75 *re* accuracy.
41. "Lettre de M. Wartmann, de Genève, à M. Arago, sur un astre ayant l'aspect d'une étoile et qui cependant était doué d'un mouvement propre", *CR,* ii, séance of 28 March 1836, 307–11.
42. Baum and Sheehan, *Search* (ref. 37), 120.
43. Bode's law uses the formulation $0.4 + 0.3 \times 2^n$, giving 19.6 AU for Uranus ($n = 8$) and 38.8 AU for Neptune ($n = 9$).
44. J. Hubbell and R. Smith, "Neptune in America: Negotiating a discovery", *Journal for the history of astronomy,* xxiii (1992), 261–91, p. 266.
45. B. Peirce, [untitled note], *Proceedings of the American Academy of Arts and Sciences,* i (1846), 41–42.
46. J. Hind, "On Wartmann's supposed planet", *MNRAS,* vii (1847), 274.
47. T. Oppolzer, "Ueber den von Wartmann im Jahre 1831 Gesehen Planeten", *Astronomische Nachrichten,* xcvii (1880), cols 253–4; Baum and Sheehan, *Search* (ref. 37), 276–7, n.15.
48. The computations were done yearly for the instant of solar opposition, the point at which heliocentric and geocentric positions coincided. Adams averred in his 13 November 1846 RAS presentation that Uranus's "error at the present time exceeds two minutes of space, and is still rapidly increasing": "An explanation of the observed irregularities in the motion of Uranus, on the hypothesis of disturbances caused by a more distant planet", *MRAS,* xvi (1847), 427–59, p. 427. H. M. Lai *et al*., "Perturbation of Uranus by Neptune: A modern perspective", *American journal of physics*, lviii (1990), 946–53. See our Appendix III.
49. U. J. Le Verrier, "Note sur les inégalités introduites dans la longitude des planètes par les variations à longue période de leurs éléments", *CR*, xiv/13 (28 March 1842), 487–508; "Seconde note sur les perturbations de la planète Uranus", *CR,* xiv/18 (5 May 1842), 660–3.
50. U. J. Le Verrier, "Sur les variations séculaire des éléments des orbites, pour les sept planètes principales, Mercure, Venus, le Terre, Mars, Jupiter, Saturne et Uranus", *Connaissance des temps pour 1844* (Paris, 1841), 28–110.
51. U. J. Le Verrier, "Premier mémoire sur la théorie d'Uranus", *CR*, xxi/19 (10 November 1845),

1050–5; "Recherches sur les mouvements d'Uranus", *CR*, xx/22 (1 June 1846), 907–18; and "Recherches sur les mouvements d'Uranus", *Astronomische Nachrichten*, no. 580 (12 October 1846), cols 53–64; no. 581 (22 October 1846), cols 65–80.

52. The Astronomer Royal's journal 1836–1847, RGO 6.24; Airy, "Account…" (ref. 25, 1847), 395; letter, Airy to Arago, 29 September 1845, alluding to "M. Le Verrier's undertaking"; and also D. Rawlins, "My calculations of some of Bouvard's terms", *Dio,* ix/1 (1999), 18, fn. 69. Not in RGON. See Bouvard, *Tables* (ref. 37) *re* E. Bouvard.
53. G. B. Airy, "Report on the progress of astronomy during the present century", in *Report of the first and second meetings of the British Association for the Advancement of Science; at York in 1831, and at Oxford in 1832*, 2nd edn (London, 1835), 125–89, p. 154.
54. Letter, Airy to Schumacher, 24 February 1838 (ref. 36).
55. B. Peirce, untitled paper, *Proceedings of the American Academy of Arts and Sciences,* i (1846–48), 286–95.
56. Smart, "Adams" (ref. 16), 82.
57. E. W. Brown, "On a criterion for the prediction of an unknown planet", *MNRAS,* xcii (1932), 80–101; D. Rawlins, "Some simple results regarding gravitational disturbance by exterior planets — with historical applications", *MNRAS,* cxlvii (1970), 177–86.
58. M. Baghdady, "The discovery of Neptune: A critical examination of the theory of Leverrier", Ph.D. dissertation, University of Aston, Birmingham, 1980.
59. For how Le Verrier arrived at his predictions, see Robert Grant, *History of physical astronomy, from the earliest ages to the middle of the nineteenth century* (London, 1852), 175–90.
60. "This distance of 35.3 [AU] then, is a complete barrier to any logical deduction …", B. Peirce, in "The new planet Neptune", *American journal of arts & science,* ix (1847), 441–3, p. 443, owing to a 2:5 resonance periodicity with Uranus. For the hypothetical planet, "There are two different mean distances of least possible error — one of which is 36, and the other 30 [AU]": Benjamin Gould, *Report on the history of the discovery of Neptune* (Washington, DC, 1850; written in Cambridge), 5. Gould reviewed Peirce's views. Hubbell and Smith, *op. cit.* (ref. 44), 271.
61. U. J. Le Verrier, "Sur la planète qui produit les anomalies observées dans le mouvements d'Uranus — Détermination de sa masse, de son orbite et de sa position actuelle", *CR,* xxiii/9 (31 August 1846), 428–38; a supplement to this paper was published as "Sur la planète qui produit les anomalies observées dans le mouvements d'Uranus. Cinquième et dernière partie, relative à la détermination de la position du plan de l'orbite", *CR*, xxiii/14 (5 October 1846), 657–9.
62. Letter, Le Verrier to Airy, 28 June 1846, RGON.
63. U. J. Le Verrier, "Recherches sur les mouvements d'Uranus", *Astronomische Nachrichten,* no. 580 (12 October 1846), cols 53–64; no. 581 (22 October 1846), cols 65–80; no. 582 (5 November 1846), cols 85–92. This paper contained four sections: Part 1 ("Calcul des perturbations produites par Jupiter et Saturne") in cols 55–60; Part 2 ("Comparaison de la théorie avec les observations") in cols 61–68; Part 3 (untitled) in cols 69–78; and Part 4 ("Détermination plus précise de l'orbite de la planète troublante, et de sa position actuelle") in cols 77–80, 85–92. The cover letter from Le Verrier to Schumacher, dated 8 September 1846, was published as "Schreiben des Herrn Le Verrier an den Herausgeber", *Astronomische Nachrichten,* no. 580 (12 October 1846), cols 53–54. An English translation of the letter and of extracts of the first three parts of the text appeared, respectively, as "The new planet Le Verrier" and "Researches on the motions of Uranus", *The sidereal messenger*, i/8 (Cincinatti, December 1846), 57 and 57–59. The translated text was later reprinted in Harlow Shapley and Helen E. Howarth (eds), *A source book in astronomy* (New York and London, 1929), 249–50.
64. Editorial of *The sidereal messenger*, i/8 (December 1846), 57.
65. Letter, Le Verrier to Galle, 18 September 1846; reprinted in H. Turner, "Obituary notice of Johann

Gottfried Galle”, *MNRAS,* lxxi (1911), 275–81, pp. 278–9; Grosser, *op. cit.* (ref. 20), 115–16.

66. There was only a degree or so difference between Neptune’s heliocentric longitude, used for the computations, and the geocentric longitude required for finding it with a telescope (these converged annually at its solar opposition, in August):

Date	1 Oct. 1845	29 Aug. 1846	23 Sept. 1846	1 Jan. 1847
Neptune long. heliocentric	324°.7	326°.8	326°.9	327°.5
“ “ geocentric	323°.6	326°.5	325°.9	326°.2

67. Letter, Challis to Airy, 22 September 1845, RGON.
68. Letter, Airy to Challis, 29 September 1845, RGON; COA, V.
69. Wilfrid Airy (ed.), *Autobiography of Sir George Biddell Airy* (Cambridge, 1896), 172.
70. Letter, Richarda Airy to Sedgwick, 5 December 1846, Richarda Townsend collection (see also her letter of 9 December to Sedgwick).
71. Letter, Adams to parents, 23 October 1845, McA 25:2.3; JCL 16.2.3.
72. Letter, Airy to Adams, 5 November 1845, JCL, 2.1.1; RGON.
73. Adams to Airy, unsent draft, 13 November 1845, Cornwall Record Office, Adams family papers, AM 330 (found by Craig Waff in 2004, as mis-dated in the archive to 1846).
74. Smart, “Adams” (ref. 16), 57: to Glaisher in 1883.
75. The column of thirty ‘error-values’ in this note were mostly given to 1/100th of an arcsecond. Le Verrier’s computations as given in *Comptes rendues* were presented to one-twentieth of an arcsecond. J. E. Littlewood, re-checking Adams computation, found “It is worth while to work to work to 0.1″” (*A mathematician’s miscellany* (London, 1957), 121).
76. Much later, in December 1846, Challis correctly described these arcsecond values, as “a list of the residual errors” of the mean longitude of Uranus: J. Challis, “A report to the Cambridge Observatory Syndicate”, 12 December 1846, published as “Report of proceedings in the Cambridge Observatory relative to the new planet”, *The London, Edinburgh, and Dublin philosophical magazine and journal of science,* ser. 3, no. 198 (January 1847), 33–41, p. 36; “First report to the Cambridge Observatory Syndicate upon the new planet”, *SP,* pp. xlix–liv.
77. Adams obtained his solution of –50º 34′ on 18 September, as the angular distance in longitude between the Uranus and “hypothetical planet” mean motions, for the solar-opposition epoch of 3 May 1810: R. A. Sampson, “A description of Adams’s manuscripts on the perturbations of Uranus” (1901), in the St John’s College Library Adams MS EIII, 6–13; Sampson, “A description …”, *MRAS*, liv (1904), 143–70, p. 166. Rawlins has argued (*Dio*, ii/3 (1992)) that Adams’s mss showed that he was setting up the formulae for perturbations on 28 November and 6 and 24 December 1845 (Sampson, *op. cit.*, 1904, 158, 168), and that the 16 December 1845 work rightly should have occurred at the beginning of the calculation of what he later called his ‘Hyp 1’ (which indeed is where Adams places it in his published 13 November 1846 presentation to the RAS), and not after its alleged submission to Airy in October 1845.
78. “Sampson’s conjecture” is pertinent to what, if anything, Challis received in September 1845: in 1901 Professor Sampson found an undated and unaddressed piece of paper amongst Adams’s notes and surmised: “It is very likely the above sheet is a copy of the communication Adams sent to Challis” (*op. cit.* (ref. 77, 1904), 166). The document exists both as EIII p. 14 in Adams’s notes (see ref. 77, 1901) and as COA 32. Challis never made public *any* values from it.
79. www.ucl.ac.uk/sts/nk/neptune/unseen.htm
80. Sampson, “A description” (ref. 77, 1904), 167, Adams MS EVII, p.15: eccentricity, apse position and mean longitude were here cited for the hypothetical planet for the first time, enabling the solution to be sent to Airy on 2 September.
81. Turner, *Astronomical discovery* (ref. 27), 70–71.
82. R. A. Sampson, “The decade 1830–1840”, in J. L. E. Dreyer and H. H. Turner (eds), *History of the*

Royal Astronomical Society, 1820–1920 (London 1923), 82–109, p. 93.

83. Le Verrier, "Premier mémoire" (ref. 51).
84. Airy, "Account" (ref. 25, 1846), 131.
85. Smart, "Adams" (ref. 16), 66 fn. has Adams, Challis and Airy then meeting; Airy's journal (ref. 52) records a visit to Cambridge, 4–6 December.
86. A. Warwick, *Masters of theory: Cambridge and the rise of mathematical physics* (Chicago, 2004).
87. Sampson, "A description" (ref. 77, 1904), 168.
88. Letter, Sedgwick to Mrs Airy, 6 December 1846, RGON; McA 33.10.
89. D. Rawlins, "The Neptune conspiracy", *Dio*, ii/3 (1992), 115–42, p. 137; Sampson, "A description" (ref. 77, 1904), 167, 168.
90. Letter, Sedgwick to Airy, 3 December 1846, RGON; McA 33:9.
91. JCL Box 10: recollections to Agnes Clerke, "forty years later"; Smith, "The Cambridge network" (ref. 8), 401.
92. McA Box 39: Donald MacAlister states, "I cannot find any diaries between 1843 & 1847".
93. Diary fragment from 9 to 28 March 1846; JCL 20.22.3.
94. John Herschel, *Outlines of astronomy* (London, 1849), 359.
95. Patrick Moore, *History of astronomy* (London, 1961, 1983), 117: this was a short-period comet that had been seen only once before. It returned as a double-comet in 1852, and thereafter gave rise to a meteor-shower, the Andromedids. For a description of the various apparitions of Biela's comet, see Gary W. Kronk, *Comets: A descriptive catalog* (Hillside, N.J., and Aldershot, Hants, 1984).
96. A. Rupert Hall, *The Cambridge Philosophical Society: A history 1819–1969* (Cambridge, 1969), 14.
97. Letter, Richarda Airy to Sedgwick, 9 December 1846, private collection of Richarda Townsend (great-granddaughter of Airy), present location unknown.
98. Harrison, *Voyager* (ref. 28), 78.
99. J. W. L. Glaisher, "Preface" to *SP*.
100. Letter from Hind to Adams, 28 February 1846, JCL 9:23 (alluded to a letter from Adams as "received yesterday").
101. Letter from Challis on Biela's Comet, MNRAS, vii/5 (1846), 73–74; J. C. Adams, "Duplicity of Biela's comet", *MNRAS*, vii/6 (1846), 83; J. Hind, "Biela's comet", *MNRAS*, vii/1 (1846), 2–3.
102. Glaisher, "Preface" (ref. 99), p. xxxi; Standage, *The Neptune file* (ref. 9), 79.
103. Letter, Herschel to Adams, 23 January 1846, CU AM 9:15. This letter has not hitherto been noticed (compare, Adams to his brother George, 10 July 1845, McA 35:12). My colleague Dr Craig Waff doubted this date, on the grounds that Adams's expertise in lunar theory only developed years later, and, in response, archivists at St John's College have confirmed that the date here given is correct.
104. Challis proposed Adams as Fellow in letter to RAS, 6 May, and he became one on 14 November 1845: MNRAS, vii/1 (1845), 1.
105. Letter, Sedgwick to Airy, 3 December 1846, RGON; McA 33:9.
106. Smith, "The Cambridge network" (ref. 8), 408.
107. *Ibid.,* 418, on the extent to which such a term could be warranted.
108. Letter, Airy to Le Verrier, 26 June 1846, Obs. de Paris Ms 1072.4; RGON.
109. Letter, Airy to Whewell, 25 June 1846, Trinity College 0.15.48,5; found by Smith, "The Cambridge network" (ref. 8), 402. Whewell averred that he had put in press by August 1846 the note in the second edition of *History of the inductive sciences* (1847), 306, concerning Adams's work, based on this latter from Airy.

110. Letter, Le Verrier to Airy, 28 June 1846, RGON; an extract of this letter was published as document no. 14 in Airy's "Account": Moore, *Neptune* (ref. 10), 97–99; Bouvard, "Tables" (ref. 37).
111. Airy, "Account" (ref. 25, 1846), 135.
112. The annual Visitation of the Observatory fell on 6 June, a fact not discernable (I believe) in *any* telling of the Neptune story, and Airy then delivered his 11-page report. The same group reassembled on 10 June for a Board of Admiralty meeting and heard the previous minutes. Owing to a letter which Airy had sent to the first Lord of the Treasury, an "adjoined meeting" was then held on 29 June at the Admiralty, with copies of the letter circulated. Babbage put a motion concerning Airy's status, in the light of the proposed improvement in international collaboration between observatories. Airy's diary used the phrase "Board of Directors" for that meeting: Astronomer Royal's journal (ref. 52).
113. Visitors of the Royal Observatory at Greenwich minute book, Public Records Office, Kew, London ADM.190/4, 213–19, 220–4; "Report of the Astronomer Royal to the Board of Visitors", 6 June 1848, 1–11, in: *Royal Observatory Greenwich Report of the Astronomers Royal 1836–68*, RGO publication; see also CUL RGO archives, Board of Visitors, add. ms. for 29 June 1846; Smith, "The Cambridge network" (ref. 8), 412.
114. No letters from Hansen about Neptune are known, except for one published by Benjamin Gould, who was in Europe in 1847: *Report* (ref. 60), 11–12.
115. Sampson, "Description" (ref. 77), 156.
116. Letter, Airy to Sedgwick, 4 December 1846, RGON; McA, 34.17.1.
117. David Brewster, "Mr Adams' and M Le Verrrier's researches respecting the new planet Neptune", *North British review*, May 1847, 207–46, p. 221.
118. Challis, "Account" (ref. 32), 415, 416.
119. The document can be viewed at www.ucl.ac.uk/sts/nk/neptune/july.htm. It is mis-filed in the Cambridge Observatory manuscripts now in the CUL, with pages belonging to January 1847 (COA, 41).
120. See Rawlins, *Dio,* ix/1 (1999), 13–14 *re* circularity of orbit in Adams's July ephemeris.
121. Francis Baily, "The British catalogue of stars", in *An account of the Revd. John Flamsteed* (London, 1835), 439. Baily noted *re* this phantom star, seen by Flamsteed as 7th magnitude in March 1695: "I cannot find any star that will correspond with the position here given", 550.
122. Sampson, "Description" (ref. 77), 154.
123. Challis, "Report" (ref. 76), esp. p. liii.
124. Letter, Challis to Main, 7 August 1846, RGON.
125. Letter, Adams to Airy, 2 September 1846, RGON; Airy's "Account" (ref. 25, 1847); Moore, *Neptune* (ref. 10), 100–2.
126. C. J. Brookes, "On the prediction of Neptune", *Celestial mechanics,* iii/1 (December 1970), 67–80, esp. p. 71: "the first hypothesis was the more accurate of the two." Brookes reconstructed Adams's equations for his "The discovery of Neptune: A critical examination of the theory of Adams", Ph.D. dissertation, University of Aston, Birmingham, 1969. Smart (ref. 16, p. 55) argued *au contraire* that Adams's second hypothesis had "a small advantage over the first".
127. Grant, *History* (ref. 59), 185.
128. With hindsight one can appreciate the quicksand into which the argument was sinking, with the radius near to the 5:2 resonance position between the planetary periods causing the perturbation-equations to go awry. This large error results from a discontinuity at 35.3 AU, as was argued by Benjamin Peirce (*op. cit*., ref. 60). Brookes ("Prediction" (ref. 126), 78) showed how solutions derived from Adams's equations varied with orbit-radius, and his simulation obtained a value of 317° at 34 AU; see also D. Rawlins, "Some simple results regarding gravitational disturbances by exterior planets — with historical applications", *MNRAS,* cxlvii (1970), 177–86.

129. Letter, Adams to Airy, 15 October 1846, RGON; McA, 33.3. ff.16–17.

130. D. Rawlins, “British Neptune-disaster file recovered”, *Dio,* ix/1 (June 1999), 17.

131. Adams’s error was 2–3 times that of Le Verrier: “Le Verrier was less than 1° out (Adams between 2° and 3°)”, Littlewood, *Mathematician’s miscellany* (ref. 75), 128.

132. Letter, Adams to Main, 7 September 1846, RGON; McA 33:1.

133. Sampson, “Description” (ref. 77), 146, described Adams’s endeavour during this period with the node equations as “unsuccessful”.

134. Letter, Adams to Main (ref. 132).

135. Letter, Hind to Challis, 16 September 1846, COA no. 10.

136. A report on the conference was published as “Sixteenth meeting of the British Association for the Advancement of Science”, *The Athenaeum,* no. 986 (19 September 1846), 961–73; no. 987 (26 September 1846), 992–1006; no. 988 (3 October 1846), 1024–8; no. 989 (10 October 1846), 1048–54. *Report of the 16th meeting of the British Association for the Advancement of Science* (London, 1847).

137. Letter, Adams to Airy, 18 November 1846, RGON; McA, 33.8. ff. 48–50.

138. Grosser, *Neptune* (ref. 20), 114; *The Cambridge Advertiser*, 30 September 1846, “Report on BAAS meeting”.

139. Herschel averred that he had spoken these words, in a letter to *The Athenaeum* of 3 October 1846, “Le Verrier’s Planet”, 1019. A letter from John Stavelly of Belfast to Herschel corroborates this: “I remember the words most distinctly”, letter to Herschel, 8 October 1846, RS:HS 17.233.

140. M. J. Crowe *et al*., *A calendar of the correspondence of Sir John Herschel* (Cambridge 1998), 329: letter of Charles Piazzi Smyth to Herschel, 5 September 1846, RS:HS 16:227.

141. Letter, Herschel to Sheepshanks, 17 December 1846, RS:HS 25.B2.26c, 16.49.

142. Whether Challis was present remains unclear from *The Athenaeum’*s account (ref. 136); he had been invited to give the yearly “Report on the advance of astronomy” but had been too busy to do so.

143. Letter, Galle to Le Verrier, 25 September 1846, Institut de France, *Centenaire de la naissance de U. J. J. Leverrier* (Paris, 1911), 19.

144. The CR announced the discovery in its 5 October issue.

145. In the *Journal des débats*, 30 September 1846, a report by Leon Foucault [William Tobin, *The life and science of Leon Foucault: The man who proved the Earth rotates* (Cambridge, 2003), esp. chap. 6, “Order, precision and clarity: Reporter for the *Journal des débats*”] stated that news of the new planet had arrived just too late for that week’s séance (Monday, 28 September); the 30 September issue of *Le National* also reported it.

146. Only Hind in England had heard the news, by a letter from Dr F.Brünnow that morning (cited in his *Times* letter).

147. Letters, Le Verrier to Galle, 1 October 1846, and to Airy 1 October 1848, RGON.

148. Letter, Hind to Adams, 30 September 1846, JCL 9:23.2; McA 38.27.2.

149. Letter, John Hind to *The Times,* “Le Verrier’s planet found”, 1 October 1846.

150. Letters, J. Glaisher to *The Illustrated London News*, dated 1 and 3 October 1846, “Le Verrier’s new planet”: 10 October, 230; some copies of the 3 October issue carried the 1 October letter.

151. Letter, Challis to *Cambridge Chronicle,* dated 1 October 1846 and published two days later, “Discovery of a new planet beyond Uranus”. An undated letter from Glaisher was also published in the same issue. These letters also appeared in *The Cambridge Advertiser,* 7 October 1846.

152. Herschel, *Outlines* (ref. 94), 508.

153. Letter dated 16 October, Challis to *Cambridge Chronicle,* 17 October 1846, “The new planet”.

154. Chapman, “Private research” (ref. 15), 133.

155. Letter, Challis to Arago, 5 October 1846, "Planète Le Verrier", *CR,* xxiii/15 (12 October 1846), 715–16 (only the *CR* transcription of this letter remains).
156. Letter, Le Verrier to Airy, 19 October 1846, RGON, McA 33:5.
157. Letter, Le Verrier to Airy, 23 October 1846, RGON, McA 33:6.
158. The Astronomer Royal's journal (ref. 52); see also his letter to Sheepshanks, 13 October 1846, RAS Ms Sh. 3.65.
159. Le Verrier, "Recherches" (ref. 51).
160. Letter, Airy to Challis, 14 October 1846, COA 14, McA 33:2.
161. Letter, Airy to Adams, 14 October 1846, JCL 2:2.2.
162. Letter, Airy to Le Verrier, 14 October 1846, Observatoire de Paris 1072.5, McA 33:3.
163. Letter, Airy to Le Verrier, 21 October 1846, Bibl. Institut de France, 3710/Ai/14; RGON; McA, 33.6.ff. 31–32.
164. Brewster, "Researches" (ref. 117), esp. p. 226.
165. Letter, Airy to Le Verrier, 14 October 1846, Observatoire de Paris 1072.5; McA 33.3; published in *Comptes rendus*, xxiii/16 (9 October 1846), 748–9.
166. Letter, Le Verrier to Airy, RGON; McA 33:4.
167. Letters, Challis to *The Athenaeum*, 3 October 1846, 1019; to *Cambridge Chronicle*, 16 October 1846.
168. Letter, Challis to *The Athenaeum* on "The new planet", written on 15 October 1846 and published in the issue of 17 October, p. 1069.
169. Letter, Challis to *The Athenaeum* (ref. 168).
170. F. Arago, "Examen des remarques critiques et des questions de priorité que la découverte de M. Le Verrier a soulevées", *CR*, xxiii/16 (19 October 1846), 741–54, esp. p. 751.
171. D & T, "Le vol à la planète", *Le National,* 21 October 1846.[WHAT IS D & T?]
172. Letter, Le Verrier to *The London Guardian,* dated 16 October: "Le Verrier's planet", 21 October 1846, 404.
173. Diary of John Herschel, Royal Society Herschel papers, MS 584.
174. Letter, Le Verrier to *The Guardian* (ref. 172).
175. Letter, Herschel to *The Guardian*, 25 October 1846, published in *The Guardian*, 28 October 1846, 421.
176. Crowe, *Calendar* (ref. 140), 331.
177 [De Morgan], "The new planet and the French astronomers", *The Athenaeum,* no. 992 (31 October 1846), 1117, cols 2–3. Augustus de Morgan wrote these editorials, a fact revealed posthumously by his wife Sophie: *Memoir of Augustus de Morgan by his wife Sophia Elizabeth de Morgan* (London, 1882), 130.
178. Letter, Challis to *The Athenaeum*, 17 October 1846, 1069.
179. Letters, Hind to Adams: 18 February 1846, JCL 9:23; 28 February 1846 (ref. 100); 30 September 1846, JCL 9:23, McA 28:27.
180. Letter, Hind to Challis, 16 September 1846 (transcribes Faye's letter to him, arrived that morning), COA, no. 10; Smith, "The Cambridge network" (ref. 8), 407.
181. Letter, Hind to Sheepshanks, 12 November 1846, RAS Sh. Ms.15.2; McA 34:15.
182. Challis, "Account" (ref. 32), 421.
183. [De Morgan], "The new planet," *The Athenaeum,* no. 997 (5 December 1846), 1245–6.
184. Letter, Airy to Challis, 12 July 1846, COA 4 (attachment p. 4)
185. Letter, Challis to Airy, 3 November 1846, RGON, McA 33:8.
186. Sampson, *op. cit.* (ref. 82), 96.

187. Airy, "Account" (ref. 25, 1847), 398.
188. J. Glaisher, "Biographical notice", preface to SP, p. xviii.
189. Adams's text was twice published in 1847, first as *The nautical almanac and astronomical ephemeris for the year 1851* (London, 1847), appendix: "On the perturbations of Uranus", 265–93; then, Adams, "An explanation" (ref. 48), reprinted in *SP*, chap. 2. In December 1846 a brief report appeared: "An explanation of the observed irregularities in the motion of Uranus", *MNRAS,* vii/9 (1846), 149–52.
190. Challis, "First report" (ref. 76), SP.
191. COA 32, plus a copy of these results exists in AM as EIII 14 (found by Sampson in 1901).
192. Craig Waff and William Sheehan, summer 2004.
193. Rawlins, "Conspiracy" (ref. 89), 132.
194. Turner, *Astronomical discovery* (ref. 27), 65.
195. Dreyer and Turner, *History* (ref. 82), 96.
196. Challis, "A report" (ref. 76, 1846), p. li.
197. Le Verrier's comments on Uranus's radius vector: *op. cit.* (ref. 61), 438.
198. Letter, Airy to Sedgwick, 8 December 1846, RGON; McA, 34.17.3.
199. From an unsigned review of *Dio* (which had published the text of Airy's 8 December letter in its June 1999 issue) in the *British Society for History of Mathematics newsletter,* Spring 2000, 30–31, presumably by the late John Fauvel, its editor.
200. Grosser's book cited the first letter of this correspondence on 3 December, from Sedgwick about how "I must myself chime in with the pack of grumblers", and then blithely affirmed that such criticism "had almost no effect on Airy": Grosser, *Discovery* (ref. 20), 137. Grosser ignored all of the irate letters sent by Airy responding to Sedgwick's complaints: on 4 (two sent), 8 and 10 December! Published in 1962, Grosser's research was conducted prior to the Neptune file's disappearance (mid-1960s), but he shows no sign of having perused it. It was not even cited in his manuscript sources. He did use the St John's College archives at Cambridge, however, and transcripts of five out of these seven Airy–Sedgwick letters were contained in the MacAlister collection there, including Airy's 8 December letter. Scholars have, for whatever reason, found these manuscripts less than fully accessible, prior to their computer indexing, which was completed in 1999. (The present writer obtained Adams's March 1846 diary-fragment, alluded to earlier, merely by asking for it, though it had never previously been cited.) Airy's 8 December letter returned from Chile in the same year as its copy became accessible in St John's College archive retrieval system.
201. Chapman, "Private research" (ref. 15), 126; Chapman, "The story" (ref. 12), 44. The Astronomer Royal's journal (ref. 52), 27 October 1845, "very serious charge of incest"; 14 February 1846, "presumed wilful murder"; 13 May 1846, "acquittal" (!).
202. Airy, *Autobiography* (ref. 69), 181.
203. Letter, Sheepshanks to Challis, 20 November 1846, COA 24; I am grateful to the archivist, Godfrey Waller, for assistance in reading this and other letters.
204. Sophie de Morgan, *Memoir* (ref. 177), 134.
205. Letter, Herschel to Sheepshanks, 17 December 1846, RS:HS 25.B2.26c, 16.49.
206. Editorial, "The new planet", *The Athenaeum,* 997 (5 December 1846), 1245.
207. Letter, William Airy to Airy, 9 December 1846, RGON. Airy's reply alluded to "the sort of connivance amongst educated persons which produces rank fibs". Airy to W. Airy, 11 December 1846, RGON.
208. Letter, Herschel to Sheepshanks, 25 December 1846, marked "confidential", RS:HS 16:52,25. B2.29c.

209. Letter, C. Babbage to RAS Council: RAS Letters, 12 March 1847.
210. Letter, C. Babbage to *The Times,* 12 March 1846.
211. They were somewhat prevented by the inclusion of this "missing star" in "Harding's Maps" and also the Berlin Star-Map Hour 14: Adams to Airy, 28 April 1847, RGON, McA 34.2.
212. Letter, Maury to Mason, 8 February 1847, National Archives, Washington, RG78:1, 2, 198–201; "The new planet", *The sidereal messenger*, xiv/**??** (June 1847), 107–8.
213. Preface to the *Histoire celèste,* p. 6, quoted in Sears Walker's letter of 20 May 1847 to *The National Intelligencer,* 22 May 1848, 3.
214. Rawlins, *op. cit.* (ref. 89), 130.
215. J. C. Adams, "Corrected elements of Neptune", *MNRAS,* vii/13 (March 1847), 244–5.
216. Letter, Adams to Airy, 28 April 1847, RGON, McA 34:2.
217. Letter, Sheepshanks to Adams, 13 March 1847, JCL 13.38.3: "Airy [at the RAS's March meeting] will have it that the observations are not sufficient to determine the orbit."
218. This perplexing issue was finally resolved in the 1990 paper by Lai *et al.* (ref. 48), see our Appendix III.
219. Hubbell and Smith, "Neptune in America" (ref. 44), 269.
220. Sears Walker, "To the Editors", letter dated 1 June 1847 to *The National Intelligencer*, 5 June 1847, 1, col. 6.
221. Letters, Herschel to Adams, 18 January 1847, JCL 9:15.3; Herschel to Fitten, 20 February 1847, RS:HS 25.7.5.
222. "It may seem strange", pondered Sears Walker (ref. 220), "that Lassell should have seen with his Newtonian reflector of only two feet aperture a satellite which has eluded the searching eyes of the astronomers of Pulkova, Cambridge, England, Cambridge, New England, and Cincinnati, with their great refractors". This was due, he explained, to Lassell's use in his telescope of a Fraunhofer prism instead of a plane mirror.
223. A series of letters by Lassell to *The Times* published on 9 July, 4 August and 24 September 1847 concerned the existence and period of Neptune's satellite.
224. Letter, Le Verrier to Herschel, 17 July 1848, RS:HS 11.200.
225. Also in the *Weekly Supplement to the Liverpool Mercury,* 11 December 1846.
226. *Cf.* Rawlins, "Adams' role in the discovery was actually nil" (ref. 89), 115.
227. Letter, Challis to *The Guardian*, 4 November 1846, 437, RGON.
228. Letter, Struve to Challis, 23 January 1847, COA, V.
229. Brewster, *Researches* (ref. 117), 237 and 244.
230. *Ibid*., 224.
231. *Ibid*., 233.
232. Letter, Charles Babbage to *The Times*, "The planet Neptune and the Royal Astronomical Society's medal", 15 March 1847, 5.
233. Editorial, *The Athenaeum,* 1014 (3 April 1847), 390 (quoting from Biot's paper "Sur la planète nouvelle découverte …", in *Journal des savants,* February 1847, 71–85, p. 84).
234. Letter, G. B. Airy to *The Athenaeum*, 1012 (20 March 1847), 309.
235. Letter, J. C. Adams to parents, 29 May 1847: AM/332, McA 35.3.2.
236. D. Rawlins, *Dio,* ii/3 (1992), 125.
237. Airy, "Account" (ref. 25); Moore, *Neptune* (ref. 10), 94.
238. An apocryphal story appeared, to the effect that in 1845 that Airy had shown the vital document to the amateur astronomer William Dawes, who was impressed enough to write about it to William Lassell, who then intended to search for Neptune but unfortunately lost the letter. The story has been dismissed by Moore (Moore, *Neptune* (ref. 10), 25; the incident is placed in August 1846

by Baum and Sheehan, *Search* (ref. 37), 279, n. 64); however, it may serve to show the public need for some corroboration of the British tale, which cannot in fact be found.

239. Moore, *Neptune* (ref. 10), 104.
240. William Ellis, "The discovery of Neptune", *The observatory,* xxviii (1905), 181–5, p. 184.
241. Smart, "Adams" (ref. 16), 68.
242. RGO Archives, RGO 6/40; Robert Smith, "Greenwich in the nineteenth century", *Journal for the history of astronomy*, xxii (1991), 5–20, p. 16.
243. Herschel, *Outlines* (ref. 94), 512, used the correct value of 19.18 AU; Adams's manuscripts give no value for this parameter.
244. Herschel, *Outlines* (ref. 94), 512; Grant, *History* (ref. 59), 617.
245. Dennis Rawlins, *Dio,* ii/3 (1992), 142.
246. Herschel, *Outlines* (ref. 94), 508.
247. J. B. Biot, "Sur la planète nouvellement découverte par M.Le Verrier", *Journal des savants*, February 1847, 83, gave the same values.
248. Adams, "Explanation" (ref. 48), 432.
249. See, e.g., E. W. Brown, "On a criterion for the prediction of an unknown planet", *MNRAS,* xcii (1932), 80–97, fitting a curve that ignored the pre-1750 data.
250. Lai *et al*., "Perturbation" (ref. 48), 950, figs 5 and 7.
251. VSOP98 planetary ephemeris from the Bureau des Longitudes, obtained by David Harper.
252. Grant, *History* (ref. 59), 598.
253. See, e.g., Littlewood, *Mathematician's miscellany* (ref. 75).