

## JOHN HERSCHEL ON THE DISCOVERY OF NEPTUNE

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**Abstract:** The letters of John Herschel that concern the discovery of the planet Neptune have not been greatly discussed by historians of science. I have transcribed these in the course of archiving the British Neptune-discovery documents. Herschel tends to be depicted as a background figure in narrations of the story of Neptune's discovery, whereas the present account focuses upon his evolving view of the topic: the rival merits of the two main protagonists, and the startling manner in which an obscure branch of mathematics (perturbation theory) was able to pinpoint the position of a new sphere in the sky. As the son of the man who found Uranus, his views have a special relevance. Also, I suggest that his eloquent prose style may still be enjoyed today.

**Keywords:** Neptune, Sir John Herschel, U. Le Verrier, J.C. Adams, G.B. Airy

Celebrating the 160<sup>th</sup> anniversary of the Discovery of Neptune on 23 September 1846

### 1 INTRODUCTION

Sir John Herschel (Figure 1) played a key role in the turbulent post-discovery Neptune debates of the 1840s. In the process of archiving the British Neptune-discovery papers I have transcribed quite a few of his letters on this topic.<sup>1</sup> These letters remain of interest because of Herschel's eloquent command of the English language, of a quite different order from the other persons concerned with Neptune's discovery, which make his letters a delight to read; but also, because he moved at the centre of the British debate, being President of the British Association for the Advancement of Science, on the Council of the Royal Society and becoming in 1847 President, for the third time, of the Royal Astronomical Society. The letters are mainly stored in Britain's Royal Society Herschel Collection, as well as other libraries: at St John's College, Cambridge, which had the John Couch Adams correspondence; at the former Royal Greenwich Observatory now kept at the Cambridge University library, collated by George Airy the Astronomer Royal over this period, which in 1999 returned from its eventful antipodean journey (see [www.ucl.ac.uk/sts/nk/neptune/takes.htm](http://www.ucl.ac.uk/sts/nk/neptune/takes.htm)); and at the Paris Observatory, which preserves letters sent by Herschel to Urbain Le Verrier.

Sir John Herschel here appears as a chief philosopher in the discussions, consulted by all parties. His view concerning the significance of the near-synchronous discoveries by John Couch Adams and Le Verrier, has clearly been little appreciated (e.g. see Ronan, 1992, which contains almost nothing on the subject). His best-selling *Outlines of Astronomy* appeared in 1849, and its view on the joint discovery was not its least point of interest. This was the last great, classic, English-language astronomical textbook, and it rolled through twelve editions, as well as being translated into many languages, including Chinese and Arabic. At the period which concerns us, Herschel was no longer making scientific discoveries of his own, his last having been the ascertaining in 1840 of the variability of Betelgeuse. In 1847 his observations on southern-hemisphere stars were published, with his theorising about the structure of the Milky Way. This book brought him the Royal Society's Copley Medal. The reader may wish to consult the author's website concerning the discovery of Neptune

([www.ucl.ac.uk/sts/nk/neptune/](http://www.ucl.ac.uk/sts/nk/neptune/)), or his recent paper on the subject (Kollerstrom, 2006), as a background for appreciating Sir John's remarks. The occasional question-mark in the text indicates that I could not fully read Herschel's handwriting.

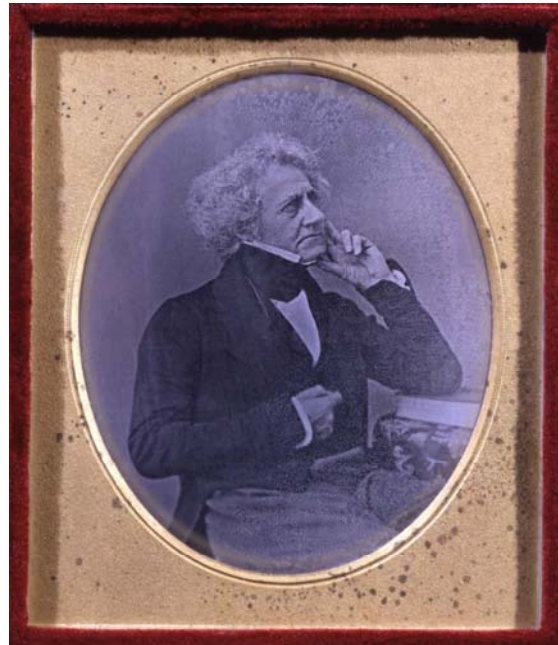


Figure 1: Restored version of a Daguerrotype of Sir John Frederick William Herschel, ca. 1848, taken by J.E. Mayall, © National Portrait Gallery, London (P660).

The letters we here peruse are from a time when, in the words of U.S. astronomer Benjamin Gould (1850: 21), "The remembrance of the enthusiasm excited by this discovery, of the amazement with which the tidings were received, not only by astronomers, but by almost all classes of the community, and of the homage paid to the genius of Le Verrier, is still fresh in the memory of all. Nations vied with one another in expressions of their admiration." The discovery was made on 23 September, 1846. The Neptune-debate was one which, as Sophie de Morgan (1882: 134) commented in her biography of her husband Augustus, threatened the RAS with mere dissolution through the turbulence of the passions

which it aroused, and it climaxed around the turn of 1846/1847.

Upon reading news of the planet's discovery, in the form of Hind's letter to *The Times* on 30 September, Herschel (Figure 2) swiftly composed a letter to *The Athenaeum*, the British weekly that carried the best coverage of the debate, sending it off the next day:

In my address to the British Association assembled at Southampton, on the occasion of my resigning the chair to Sir R. Murchison, I stated, among the remarkable astronomical events of the last twelve months, that it had added a new planet to our list, - adding, "it has done more, - it has given us the probable prospect of the discovery of another. We see it 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." (Herschel 1846c).

The 'new planet' here alluded to was the asteroid Astraea. Widespread correspondence amongst European astronomers from December 1845 onwards concerning a 'new planet' alluded to this—and not anything else! Herschel's prophetic words here remembered the great discovery of his father William, in finding Uranus. This BAAS meeting had been a mere week or so prior to the moment of discovery, yet had no mention or discussion of the expected new planet, on which Le Verrier had by then twice gone into print, except only for these allegedly-spoken words of Herschel. Did they comprise the first British allusion to Adams, the 28-year old Cambridge mathematician, in this context? Herschel was here claiming so. By way of confirming these words, I found a letter by an Irish correspondent (Stevelly, 1846) who states that Herschel had indeed spoken them on the occasion of his valedictory speech to the BAAS in Southampton.



Figure 2: Sketch of John Herschel presiding over an 1846 meeting of the British Association for the Advancement of Science, as depicted in an issue of the *Illustrated London News* (courtesy RAS Archives).

## 2 THE ENGLISH AND FRENCH CLAIMS

Herschel averred, in his *Athenaeum* letter, that this expression of confidence would hardly have been warranted merely from Le Verrier's calculations, and

that it was their corroboration by Adams which "... justified so strong an assurance." This provoked an angry rebuke from Le Verrier, writing to the *London Guardian*, concerning Herschel's want of faith in his predictions: "When he scrupled not to put into print that my calculations were not sufficient to command his confidence, did he not perceive that he was bringing discredit on his own scientific penetration, when he attacked a calculation ...", etc, and then Le Verrier added the commendable sentiment: "Among men of science of different countries, there ought to remain only that friendly rivalry, which, as leading to the benefit of science, so far from hindering, does but cement, the frank and brotherly friendship of those who cultivate it." (Le Verrier, 1846a). In his reply, Herschel (1846d) assured *Guardian* readers that "The prize is his [Le Verrier's] by all the rules of fair adjudication, and there is not a man in England who will grudge him its possession ...", and then suggested that the synchrony of this discovery was *beneficial for science*:

The history of this grand discovery is that of thought in one of its highest manifestations, of science in one of its most refined applications. So viewed, it offers a deeper interest than any personal question. In proportion to the importance of this step, it is surely interesting to know that more than one mathematician has been found capable of taking it. The fact, thus stated, becomes, so to speak, a measure of the maturity of our science; nor can I conceive anything better calculated to impress the general mind with a respect for the mass of accumulated facts, laws, and methods, as they exist at present, and the reality and efficiency of the forms into which they have been moulded, than such a circumstance. We need some reminder of this kind in England, where a want of faith in the higher theories is still to a certain degree our besetting weakness. (ibid.)

His diary for that day, however, says: "Wrote to the editor of *The Guardian* in reply to M. LeVerrier's savage letter. These Frenchmen fly at one like wildcats." (Herschel, 1846a).

Astronomer James Challis (Director of the Cambridge Observatory) had failed, after a strenuous six-week search, to find a planet which Galle and d'Arrest at Berlin spotted in half an hour. Herschel conveyed his regret at this outcome, to his old friend the British philosopher William Whewell:

I mourn over the loss to England and to Cambridge of a discovery which ought to have been theirs every inch of it, but I have said enough about it to get heartily abused in France, and I don't want to get hated in England for saying more. Only if you have any influence with Challis for heavens sake exert it to prevent him saying more about it in the papers - or elsewhere. (Herschel, 1846e; in pencil he added to his copy of the letter: "After all it is now quite clear Adams was the prior discoverer.").

After being chastised by a *London Guardian* editorial (21 October, p.404), Challis responded by agreeing that he and Adams had no claim over the discovery of the new planet:

I beg distinctly to say that I had no intention of putting in 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 ... I certainly was desirous of

proving, for the credit of English science, that Mr Adams's researches were spontaneous and independent: but I am unable to see that the fact of their being so at all diminishes M. Le Verrier's merits, or that the making of the fact public implies an intention of taking in any degree from the honour of the discovery. The very natural wish to show that the University of Cambridge could produce a mathematician capable of handling a problem of so high an order ... (Challis, 1846).

That was far from being the consensus British view, and it provoked Herschel's above-quoted response, as well as a stiff rebuke from Airy.

Concerning the relative merits of the two claims, British and French, Sir John opined to R. Jones:

It is a shame to make rivals and competitors of two men who ought to be sworn brothers. Adams has the acknowledged priority in point of time that nothing can shake but till the Planet was found it was only a physical hypothesis upon trial, and no one can truly deny also that LeVerrier *shot fair*, and *brought down the bird*. Now my view of the matter is that there is quite enough for both ... (Herschel, 1846f).

Both mathematicians, Adams and LeVerrier, had used the perturbation-theory of Lagrange and Pontecoulant, a French creation:

Barring Newton's law of gravity (who never meddled with the planetary perturbations), what Englishman ever furnished the smallest tittle of a tool towards rigging out a man for such a struggle? It is all French du fond en comble [?] Clairaut, D'Alembert, Laplace, Lagrange, and more recently Poisson and Pontecoulant for the analysis and Bouvard for the tables, which though not *quite* correct were yet correct enough to raise the hue and cry. - The New Planet is as much Laplace's as it is either Leverrier's or Adams's. (ibid.).

A postscript added: "Who made one and all of the formulae by which both have grappled the planet but Frenchmen?" (ibid.). We may note that both Adams and LeVerrier used the same textbook, Pontécoulant's *Théorie Analytique du Systeme du Monde*.

The new planet's discovery had been "... in every way a most spirit-stirring event ..." Herschel (1846n) found, writing to Otto Struve at the Pulkova Observatory. He had nearly found it himself, he realized, during a sky-sweep in 1830 (see Buttman, 1974: 162), however it was better that it had not been found by mere accident (Herschel, 1846n). A couple of days later, he wrote to William Whewell, again weighing up the priority claims:

... Galle looked for it and found it on the *sole* ground of Leverrier's place, while Challis cannot shew that he looked for it (when at last he did so) purely and simply by Adams's. When he began to look he had already a knowledge of Leverrier's results, and he did not *find* it till after Galle had done so - for I do not call finding an individual object merely including it in a crowd of others (without knowing that it is there, and rather suspecting it *not* to be) with an *intention* of examining them at leisure to ascertain if it be among them or not - Nobody but Sheepshanks will ever say that Challis *found* it before Galle.

Until the planet was actually seen and shewn to be a planet - there was no discovery. (Herschel, 1846p).

(Challis had observed Neptune, i.e. recorded its transit in his log-book, on both the 4 and 12 of August, amongst the three thousand stars he also noted; but, failed to recognize it.) On the back of his copy of the letter, Herschel had pencilled in anguish:

God forgive me for writing in this way - The truth lies on the other side & Adams is the 1st theoretical discoverer of Neptune. The whole thing was parried [?] and perverted by Airy's indefensible reticence. On him be the responsibility of the (temporary) transfer of one of the brightest stars in Britain's Scientific fame to France. (ibid.).

Fortunately, he never published this somewhat unbalanced view.

### 3 NO RAS MEDAL

In December 1846 the full text of Adams' 13 November RAS presentation was published in the *Nautical Almanac*, and this publication of his case awakened great sympathy and appreciation for the strength of his argument. The Royal Society had earlier awarded Le Verrier their prestigious Copley Medal, a relatively unproblematic decision (which Herschel had received on Le Verrier's behalf). December 1846 was stressful for the British astronomical community, because the RAS had its annual Gold Medal to award, which its bye-laws stipulated could only be done in January. They further stipulated that a 3:1 vote was necessary for awarding this medal, and that only one such could be awarded each year. Passions were running high, and there was simply not enough time for the RAS Council to sort out a realistic course of action.

Herschel's first letter on the subject was written on 3 December to the RAS's Secretary, the Reverend Richard Sheepshanks, and it seems to imply that the RAS's medal should be awarded to Le Verrier: "My own opinion is that Adams stands in quite as good perhaps a better position without a medal as with - that if he be medallised it should be most cautiously worded so as not to bear the least allusion to that ugly word priority - and that to medallise Galle and Challis (or even Bremiker) would be decidedly wrong." (Herschel, 1846g). Alas, this advice was not taken, and the RAS medal decision sank into the quicksand of these multiple proposals.

Two weeks later Herschel (1846h) proposed three Gold Medals:

If the council resolve on medallising Mr Adams, I would by no means *object* or oppose it - but I conceive the way of stating the grounds of proceeding in that case, both in reference to him and to M. Leverrier ought to be more carefully considered so as in the first place neither to state nor to imply anything that all the world will not admit to be true in the most ordinary acceptance of the words (already the word "discovery" begins to break down under the weight of meaning laid upon it) - and 2ndly not to assume to the Astronomical Society as a body a dictatorial power of deciding points of such a nature, which the public mind would rebel against as it tends to do against all decisions *ex cathedra*.

He suggested that three medals be awarded, to Adams, Le Verrier and Hencke. This turned out not to be a very helpful idea (Hencke, at Frankfurt/Oder, had

discovered the asteroid Astraea). Still wrestling with the matter, later that same day he penned a second letter to Sheepshanks:

I really am desirous to say as little as I can about this matter of the Planet. But I must most urgently protest against any official assertion of priority - against any bringing into competition of dates and claims by the wording of our resolutions. What our worthy President may say in his address is his own affair, but I should advise him to keep clear of anything which may tend to stir up a national controversy in the matter as that will be sure to do, and a bitter one. Heaven knows I would not depreciate this if I thought our case *as a whole* were tenable. But though Neptune ought to have been born an Englishman and a Cambridge man every inch of him - *Diis aliter visum* - you will never make "an English discovery" of it no matter what you will. I assure you seriously that the conviction that such is the case has given me more pain and grief than any national event since the expedition to New Orleans or such other *coup manqué* as your military imagination may suggest. It has really made me ill. (Herschel, 1846i; William Smyth was the RAS's President at the time).

A week later Herschel (1846j) wrote again to Sheepshanks, fearful that the impending decision "... may prove a more fatal apple of discord than any that has been thrown down among us for years."

On Christmas Day he wrote a letter marked 'confidential' to Sheepshanks, concluding 'burn this.' By that time six candidates had been advanced for the Gold Medal:

I see Airy proposes LeVerrier, Adams, Challis and Argelander - Bishop, Hencke - and Johnson, Galle ... I must very candidly tell you that I think this one of the most disastrous combinations of circumstances the A.S. has ever had before it, and that it comes in a most portentous form for the peace of the scientific community of England ... I know you have much influence with Airy, and I am convinced that no other man than yourself has any chance of inducing him to reconsider his judgement in the form he has cast it - and if you can get Challis and Galle left out, all will be well. Probably if he would withdraw Challis and Argelander, Johnson would withdraw Galle - and trusting that this may be the upshot, I remain ... (Herschel 1846m; Bishop and Johnson were RAS Council members).

If the number of candidates could be reduced to merely two, Herschel hoped that there might be a slim chance of the Council deciding to award one extra Gold Medal that year - a view championed by Charles Babbage. Sheepshanks, however, may not have had quite so much influence as Herschel here credits him with. He replied by return concerning "... our good friend Smyth ... [who] had this bitter cup impending over him ... the whole evidence as to Leverrier was out, understood and believed, before anything was known of Adams ... LeV's merits too are of such an order that every one feels anxious to shew his liberality in a case so clear and free from danger." However, "I scarcely expect that half will agree to apply for a suspension of the Bye Laws to present the additional medals." (Sheepshanks, 1846). This was, it turned out, a correct apprehension.

But could a vote for Le Verrier reach the necessary 3:1 majority to award the medal? "Le Verrier's medal will be voted unanimously unless, perhaps, Airy may object to it without some condition, this I think however he will waive ..." Sheepshanks' letter continued with this dire, futile logic: after describing the various voting postures of key Council members, he stated: "Some (I for one) think that in granting a medal to LeVerrier alone we do in fact & to all the world deny any merit in Adams & even the necessity of Airy's memoir. I am certain that in France (where fairness seems not understood) it would be impossible by any language in our Report to prevent this conclusion. I believe moreover that in England the same conclusion would be generally drawn." He concluded by saying that he wished the Society did not have to award Gold Medals.

On 8 January, a motion proposed by Augustus de Morgan prohibited any alteration in the Society's by-laws for the vote (RAS, 1847),<sup>2</sup> and this motion was carried. Then the six separate candidates were voted for, one by one, and, inevitably, no single name received the necessary 3:1 majority. Airy voted *against* awarding a medal to Le Verrier, and it is not unfair to say that he thereby exerted the casting vote in preventing any such medal being awarded. After this debacle, Herschel wrote next month to Sheepshanks about the course adopted by the Council, "... for I think it a wrong one - or rather a *sheer mistake & nobody's doing*." (Herschel, 1847d). It was De Morgan's doing! Could Council members find some way of extricating themselves, Herschel wondered, from "... the hard knot in which they have got themselves tied up?" (ibid.). From more than one correspondent, he had gathered that the no-medal resolution "... is productive of very great dissatisfaction among the body of the Society & indeed generally among the scientific world." (ibid.). He was perplexed over "... what reasons influenced the rejection of the proposal to admit more than one medal...":

The actual state of the subject is therefore an un contemplated result & the work of nobody; & probably *as a result* and as the *final* and *only* result of the discussion, disapproved by all present. (Herschel, 1847e).

No RAS medals were awarded that year.

#### 4 PUBLIC ESTEEM FOR SCIENCE

The year 1847 began with the arrival of Le Verrier's Memoir on the new planet, and Herschel (1847c) enthused to Airy about it:

I have within these 2 days got Le Verrier's Book - and I must say my impression is one of unbounded admiration. There is no part of the subject shied or slurred over - a *tabula rasa* - and a total reconstruction with a view from the beginning to the crowning pinnacle of the whole edifice. It is an Epic Poem complete in beginning middle & end with a catastrophe<sup>3</sup> such as *could not possibly be heightened* by any additional circumstances. I am sorry for Adams & for England, but it would really have been a *pity* that so superb a struggle should not have been crowned with victory as a spectacle for Gods & men. (Herschel, 1847c).

Within a day or so he also received Adams's tract, *Explanation of the observed irregularities of the motion of Uranus*,<sup>4</sup> and wrote to the latter:

Though it is now long since I entered at all into the Planetary theory and can do little more than seize the spirit of the methods & practices and yet I see enough in both to excite my unbounded admiration of the skill and power displayed in grappling with so difficult a problem, and I cannot say that the triple coincidence of your results *with each other* and *with the fact*, considering the minute amount of the quantities to be dealt with seems to me by far the most wonderful gave [?] of the whole affair and gives an idea of the firmness of grasp which theory has obtained of the Planetary perturbations infinitely beyond what the most sanguine could have dared to hope would *ever* be obtained. In this point of view (and setting aside all question of rivalry and competition between two men whose names will go down indisputably linked together to the latest posterity and between whom, if even, there ought to be a brotherhood of mutual admiration and regard) I cannot help considering it as fortunate for science that this should have happened. All idea of a lucky guess - a mutual destruction of conflicting errors - of a right result got at by wrong means is precluded - and the most reluctant to accord any merit to theories must be bound to admit that in this matter at least theories are facts. (Herschel, 1847b).

These two documents confirmed his view that this synchronous discovery had been beneficial to the public's appreciation of science. Writing to Fitten, after admitting that he had an unanswerably large pile of letters from the RAS's no-medal debacle, he admitted rather too late in the day that

... it will be the *right* course to give two medals, making however such a distinction in the tenor of the award as shall secede to Leverrier the intact possession of the first honours of the achievement - upon the grounds that he *shot fair* and *brought down the bird* - while at the same time every possible justice shall be done that words can do to Adams' merit. (Herschel, 1847f).

Concerning Adams's claim:

It is the correctness of the mathematical conduct, & the perfect independence of Adams' researches, and not their priority, which in my opinion constitutes his claim to a reward & a proposing of our gratitude as astronomers. As a competition to Leverrier I never will consider to regard him. But I think it is precisely one of the finest, most interesting & most admirable points in this discovery that it can be satisfactorily shown by evidence that whether published or not, the same result has been arrived at independently by two different Geometers both starting from the ordinary recognised formulae of the planetary perturbations. It is an infinitely greater part - infinitely more creditable to the state of Science, infinitely more illustrative of the reality of its grasp in the planetary theory that two shared have done this than one only. I am not aware that this view of the subject has been taken, but I pray you to give it your serious consideration. (ibid.).

## 5 THE NAMING OF URANUS AND NEPTUNE

Concerning the name of the planet, François Arago before the Paris Academy had impetuously pledged himself not to call it anything other than 'planete Le Verrier,' a mere week after its discovery—possibly not

realizing that Le Verrier had already written to various European observatories suggesting the name 'Neptune.' Subsequently Le Verrier came to adopt Arago's suggestion, leaving European astronomers in perplexity. There turned out to be an implication to Arago's proposal, a kind of corollary, namely that the planet Uranus had to be called 'Herschel.' When Le Verrier (1846b) wrote to Herschel on 28 November 1846 and sent him a copy of his Memoir, he pointed out that he had altered its title, 'Researches on the Movements of Uranus', by changing 'Uranus' to 'Herschel'. However, he had not altered it within the text, which produced some confusion. This Memoir did not arrive until the beginning of January, when Herschel politely declined the nomenclatural dedication to the memory of his father, explaining: "I have personally committed myself to a mythological name, a few years ago ..." (Herschel, 1847a).

European astronomers became immersed in a debate over what should be the name of the new sphere, and gradually came to agree that national sentiment should be excluded from heavenly nomenclature, whereby Uranus received that name and not *Georgium Sidus* or Herschel, and at around the same time Neptune ceased to be called Planète Le Verrier. In May 1847 Herschel (1847g) wrote:

As regards *Uranus* I have for a long time used that name and intend to do so. Of course I cannot possibly object to its being used in the N.A. or in any other publication. I thought I had expressed as much at the time of the "Reform" of the Nautical Almanac.<sup>5</sup>

My full impression is that the name Uranus has taken too deep a root to be displaced.

As to the name of the new Planet - As Adams acquiesces in Neptune - As Neptune is a name of French origin (which I think *very* important) and as it is a mythological name, I give my adhesion to it as an admirable *mezzo-termine* to avoid bringing its two discoverers into needless opposition.

I say I consider it as very important that the name Neptune is of French origin (and also that it had at one time the acquiescence if not the implied sanction of Leverrier himself). I regard the discovery whether made by Leverrier or Adams or both as in the main of French origin. The analytical theory of the Planetary Perturbation which alone render it *possible* is almost exclusively French. Clairaut, Laplace, Lagrange Pontecoulant and Poisson are the authors of those formulae which, used as tools or as *telescopes of the intellect* have done the thing and we owe them this national recognition.

He would personally have preferred "1<sup>st</sup>. Minerva as having sprung fully armed from the head of Jupiter - or 2<sup>nd</sup> Hyperion (the transcender) the offspring of Uranus and Terra." (ibid.). As regards Arago's pledge, he diplomatically suggested that 'Le Verrier's planet' was more of a description than a name: "Those who think it 'LeVs Planet' may yet *call* it Neptune without compromise and may also if they like *speak of it* as LeV's P[lanet]." (ibid.).

## 6 AMERICAN SCEPTICISM

American astronomers emphasized how different was the actual planet's orbit compared to the two models of Adams and Le Verrier, because the latter's orbit radii, eccentricities and apse positions had all been so wrong.

Benjamin Peirce was using the phrase ‘happy accident’ to describe its discovery (Hubbell and Smith, 1992: 269), a view also associated with the American astronomer Sears Cook Walker. Edward Everett, the President of Harvard University, wrote to Herschel in some concern upon this matter, saying he wished for a confidential opinion. He first thanked Herschel for an early, pre-publication copy of the *Outlines*: “I should regard the volume – however it had come into my possession – as one of the most valuable in my library. The letter of the duke accompanying it, with the inscription on the blank-page, makes it truly inestimable.” (Everett, 1847). He then described the controversy stirred up by “... Prof. Peirce of this University ...” (ibid.). At the American Academy of Arts and Sciences, “... he holds that the real elements of Neptune as observed, are so different from the predicted elements of Adams and Le Verrier, that the discovery must be considered as accidental.” (ibid.). Everett distrusted these views, as having an “... extravagant and improbable cast ...”, yet had to admit that Peirce was regarded as “... one of our very first mathematicians ...” and seemed confident enough when propounding his views. Everett feared that they might bring discredit to the University, and asked: “I wish you would impart to me your view of the subject, as freely & candidly as if we were talking over the matter quietly at Trinity Lodge, with no-one but Dr Whewell to listen.” (ibid.). Sir John’s reply is, alas, lost.

In the summer of 1848, the Paris Archive librarian, Jacques Babinet, began advancing the argument associated with Peirce whereby the new sphere’s discovery was a mere ‘happy accident.’ The two Neptunes, as predicted, had radii far too large (38-35 AU as compared to 30 AU), and their masses were also too large to compensate for this. Here is Sir John explaining the matter, to his old friend, William Whewell:

By the way what a fuss is raising about the identity of Neptune - The case is as clear as daylight - Neptune (the real Nep.) - comported himself all the time he was within pull of U. very nearly indeed as the hypothetical N of Leverrier and Adams would do. - Their Nep. was a respectable counterfeit - he put on a mass to hide the excess of his distance - an eccentricity to get him within reach in spite of his huge axis - and a place of perihelion near conjunction to spur up his sluggish angular motion and enable him to keep tolerably in the right direction. But what can have set Babinet (who is a good mathematician) at sea about it? (Herschel 1848b).

The hypothetical planet had been placed by both parties near to an imagined perihelion of an orbit with hugely exaggerated eccentricity, so that—over the time of its discovery and for some decades earlier (i.e. the period containing the most accurate observations)—it could be seen as keeping “... tolerably in the right direction.” (ibid.).

Le Verrier had been obliged to defend his case against Babinet. The latter’s view he summarized as: “That Galle’s planet had nothing to do with the one which Adams and I had searched for; and that the coincidence was fortuitous.” (Le Verrier, 1848). Babinet argued that the predicted planet still awaited discovery—and he named it Hyperion! Even Le

Verrier found himself coming round to accept Herschel’s argument, that the synchrony and concordance of the two predictions was the best argument against Peirce’s ‘happy accident’ thesis. Herschel (1848a) reassured him: “My faith in Neptune being the real planet which has perturbed Uranus has never for an instant been disturbed ...”, and he entered into a discussion of the perturbation-theory involved, so as to reassure Le Verrier. When Uranus and Neptune became conjunct in 1820, the two imaginary orbits were then, he noted, both near their perihelia. On his somewhat simplified version of how-to-find-the-planet he wrote:

The perturbation of an interior by an exterior planet in the longer planetary orbits becomes large only when the bodies approach conjunction. The disturbing force of N. on U. in conjunction is 10 or 12 times greater than in opposition or in quadratures. - Now, the first and only conjunction of N & U which has taken place since 1690 has been that of 1820, and the period of disturbance may, I suppose be taken at about 20 years on either side. (ibid.).

The perturbation of Uranus increased until somewhere around 1817 and then started to decrease. The perturbations should be centred around conjunction, as Airy had explained in his book *Gravitation, an Elementary Explanation of the Principal Perturbations in the Solar System* (1834). This gives a general indication of when the meeting with the unseen new sphere must have been, from which its present position could be roughly inferred, and “This in great measure indicates the direction in which the new planet must lie.” (Herschel, 1849b: 513).<sup>7</sup>

In his *Outlines of Astronomy*, Herschel (ibid.) added that he had described the new planet’s discovery, “... and I hope also to put the salient points of the present discussion in a light intelligible to all the world.” One must surely agree that he did so. Herschel (1848a) also wrote Le Verrier an encouraging letter regarding the validity of the calculation he had performed:

The actual longitude of your and Mr Adams’s perihelia of N. is nearly that of the two planets in conjunction - hence the angular motion of the hypothetical planet being, by reason of the large eccentricity, much greater at perihelion than at its mean distance, would approach nearly to the angular motion of the true Neptune - and in fact the hypothetical Neptune appears to have been a *very fair imitation* of the real one at that epoch.

Replying to RAS Secretary, Richard Sheepshanks, by way of excusing himself from contributing anything to the *Monthly Notices* immediately prior to the publication of his *Outlines of Astronomy*, Sir John explained: “... what little I have to say on the subject of Neptune will be said *very quietly and guardedly* in Chap. 14 of the ‘Outlines’ (whenever they shall appear) for to say the truth there are points in the matter of the perturbations which I do not quite see my way through by the light of common sense and dynamics.” (Herschel, 1849a).

On 1 October 1846, the same day that Herschel penned his decisive letter to *The Athenaeum*, he had also written a letter to William Lassell (near Liverpool), whose large equatorially-mounted reflecting telescope could easily track the stars. “Look out for

satellites with all possible expedition!" was the President's injunction (Herschel 1846b). Lassell did so, becoming the first to spy Triton, the large moon of Neptune,<sup>8</sup> and he announced the existence of this satellite to *The Times* in several letters between July and September of 1847. Lassell achieved priority over several other European and American astronomers with large telescopes who were likewise searching for any such moon. From its orbit Neptune's mass was found, and thereby key questions concerning the manner of its prediction could be resolved. Herschel had written to the right person.

On 10 July, 1847 Le Verrier and John Couch Adams finally met, at Herschel's home 'Collingwood', in Kent, on the occasion of an Oxford meeting of the British Association for the Advancement of Science. Despite a language barrier to their communication, the two were reported to have got on together. Later that year, Le Verrier (1847) asked for an extra copy of Sir John's new book, *Results of Astronomical Observations at the Cape of Good Hope*, so that he could present it to the King of France. Herschel was a representative figure of British science to the extent that, indeed, "In his own day, the name 'Herschel' meant 'science' ..." (Ruskin, 2004: 202), having become "... England's most influential philosopher of science in the 1830s ..." (Buttmann, 1974: 162) following the publication of his *Preliminary Discourse on the Study of Natural Philosophy* in 1831. His contributions helped to guide British science through the stormy drama of Neptune's discovery, and elucidated the key scientific concepts of prediction, discovery and priority.

## 7 NOTES

1. Thirty letters are cited here, both to and from Herschel, of which only twenty-two are archived within Crowe's collection of 14,815 Herschel letters (see Crowe *et al.*, 1998); six of those cited here (i.e. Herschel, 1846f, 1847a, 1847b, 1847c, 1848a, and Le Verrier, 1848, in Section 8) were, for whatever reason, omitted. My Neptune-discovery archive ([www.ucl.ac.uk/sts/nk/neptune-corr.htm](http://www.ucl.ac.uk/sts/nk/neptune-corr.htm)) has forty-eight letters from Herschel, including twenty-eight in the Royal Society Library; seven in John's College, Cambridge; three in the RAS's 'Neptune file' in the Cambridge University Library; two in Trinity College Library, Cambridge; and two in the Observatoire de Paris Archives.
2. The Minutes of the RAS Council (1847) record Augustus de Morgan's motion: "It is not expedient to recommend a General Meeting to depart from the course laid down in the bye laws as to the award of the medal." Sheepshanks and Main proposed a motion to omit 'not' from this text, but it was refused. Herschel was not present at the meeting but Airy was.
3. This is a technical literary term used to describe that moment in Greek tragedy *katastrophe*, from *katastrephain* to overturn, in which the final event of the dramatic action of a tragedy occurs (my thanks to W. Sheehan).
4. Adams' manuscript, 'On the Perturbations of Uranus,' was published as an appendix to *The Nautical Almanac and Astronomical Ephemeris for the Year, 1851* (see Adams, 1847).

5. *The Nautical Almanac* only changed this planet's name from 'The Georgian' to 'Uranus' in 1851.

6. On 24 December, Herschel (1846k) had written to Augustus de Morgan, advocating these two names. In this letter he states that Heinrich Schumacher, Editor of the *Astronomische Nachrichten*, had written asking his view concerning a name for the new sphere, and that he had advocated these two names, but these letters are lost.

7. See Herschel's (1849) *Outlines of Astronomy*, page 513, section 773. Sampson (1904: 149) argued against this attractively simple view: "The conclusion is drawn [by Herschel] that Uranus arrived at conjunction with the disturbing planet about 1822; and this was the case. Plausible as this argument seems, it is entirely baseless." For more recent comments on this theme, see Kollerstrom, 2006 (Appendix III).

8. A response to Herschel's letter came from the astronomer William Dawes who was staying with Lassell. On 6 October 1846 he wrote to Herschel that, "Lassell has described its [Neptune's] appearance as 'a neat pale small bluish disc' and believes he may have detected a ring around it." (Dawes, 1846). This is probably the first astronomical allusion to Neptune's colour.

9. After Herschel's death his son, Colonel John Herschel, collected and copied his father's correspondence, and these copies, and the originals, are now in the Royal Society's library in London. After John Couch Adams' death, Douglas McAlister transcribed many letters relevant to his life, and these included some Herschel letters. The McAlister Collection is now stored in the St John's College library in Cambridge. Transcription of the Herschel letters has been done primarily using copies of the letters, as being more legible, while any originals have been used for checking the text. Letters here cited are originals unless listed as copies.

## 8 REFERENCES<sup>9</sup>

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RS:HS = The Royal Society's Herschel Collection

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