image and the plate were in the same direction, so that the relative speed of the image across the plate was less at these positions, and thus the exposure was effectively longer than for the less dense broad maxima where the motions of the meteor image and the plate were in opposite directions.

(5) For several cycles near the middle of the trail where the meteor was the brightest, the density of the trail fades off gradually in the direction from which the meteor came. This appearance is attributed to the train of hot luminous gases in the wake of the meteor. Because of the component of vibrational motion at right angles to the direction of motion of the meteor the images at the nodes of the sinuous curve are effectively an instantaneous photograph of the meteor and the luminous gases in its wake. This appearance is not compatible with a real sinuous motion of the meteor, for in this case the luminous gases would be left along the path that the particle traversed. This appearance emphasizes that we should not consider a meteor as a moving point source of light but that we must take into account the light from the luminous gases in its wake. This phenomenon is an important limiting factor in the design of rotating shutters for meteor cameras.

I wish to express my appreciation to Mr. E. L. Rowland and the Jacksonville Amateur Astronomers Club for the loan of their negative and to Dr. Fred L. Whipple for valuable discussions of this problem.

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John Couch Adams and the Discovery of Neptune*

By W. M. SMART†

The discovery of the planet Neptune, one hundred years ago, was the result of very unusual and dramatic circumstances. Since the discovery practically every writer on astronomy has set forth the principal facts in connection with it. However, the recognition of the centennial last year brought out some material not generally known before. Three papers bearing on the question were published in NATURE. *We are privileged to reprint these papers here for the benefit of those who may not have access to the copies of* NATURE. *We do so with great satisfaction.* EDITOR.

Until 1781, the planet Saturn represented the outermost boundary

*Summary of addresses to the Royal Astronomical Society on October 8 on the occasion of the centenary celebrations of the discovery of Neptune, published in *Nature*, November 9, 1946.

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of the solar system; on March 13 of that year the planet Uranus was discovered by Sir William Herschel, and by the beginning of 1846 (the year of the discovery of Neptune) five minor planets had been found. In all these instances, the discovery was made at the telescope, in one or two cases purely by accident. The discovery of Neptune was on a far different level of human achievement; the discrepancies between the predicted and observed positions of Uranus since its discovery furnished the means whereby two mathematicians, Adams and Le Verrier, applied their unrivalled skill to deduce independently the position of a new planet the gravitational attraction of which on Uranus, they confidently believed, was responsible for the discrepancies referred to.

When, shortly after 1781, an approximate orbit had been calculated for Uranus, it was suggested by Bode that perhaps the planet had been observed previously as a 'star'; the search of catalogues proved surprisingly successful, for no fewer than nineteen authentic observations of Uranus had been recorded, the earliest in 1690 by Flamsteed (the first Astronomer Royal), who designated it 34 Tauri. In the nomenclature of the time, these pre-discovery observations of Uranus are known as the 'ancient' observations, those after discovery as the 'modern' observations. In the second decade of last century the accurate establishment of the planet's orbit was undertaken by Bouvard, who was soon faced by a peculiar difficulty. If he used the 'ancient' observations alone, he obtained an orbit differing unmistakably from the orbit derived from the 'modern' observations alone, these covering nearly forty years. In this dilemma he rejected the 'ancient' observations entirely, on the plea that they carried very much greater observational errors than the 'modern' observations, and his tables of Uranus, published in 1821, were based entirely on the latter. But soon Uranus was seen to be falling behind its predicted position; by 1832 the error in longitude was ½′, and in 1837 Airy (the Astronomer Royal) reported that the errors were "increasing with fearful rapidity"; the anomalous behaviour of the planet had now become the most puzzling problem in contemporary astronomy.

Several suggestions were offered to account for the phenomenon; the law of gravitation might not be exactly according to the inverse square of the distance (a suggestion regarded by Airy as possible even so late as 1844); the existence of a resisting medium—an ever-popular hypothesis—was put forward; perhaps the errors in the positions of Uranus were due to a massive satellite (how this could have escaped observation was not stated); perhaps about the time of discovery in 1781 Uranus had been hit by a comet, this suggestion being made, of course, to explain the difference in the orbits derived separately from the 'ancient' and 'modern' observations; and finally it was hazarded that the discrepancies—or, technically, the perturbations—resulted from the attraction of an undiscovered planet far beyond the bounds of Uranus.

Airy himself had no doubts about the last hypothesis, for he wrote: "If it (the anomalous behaviour of Uranus) be the effect of any unseen body it will be very nearly impossible ever to find out its place." Fortunately, Adams and Le Verrier had a clearer perception of the problem than Airy, and they were little daunted by the difficulty and magnitude of the task to which in due course they applied their incomparable mathematical skill. From the beginning they were supremely confident of the existence of an unknown planet and of the power of analysis to ensure its optical discovery.

When Adams was still an undergraduate at St. John's College, Cambridge, he and a companion, Drew, were discussing their futures. When Drew asked him what he proposed to do, Adams replied deliberately: "You see, Uranus is a long way out of his course. I mean to find out why. I think I know." Drew said afterwards that this reply gave him a queer feeling, as if a young prophet were speaking. Adams was born on June 5, 1819, the eldest of the seven children of a Cornish farmer. In 1836 his mother inherited a small property, and it seems almost certain that but for this 'windfall' the family economy—always exiguous—would never have stood the strain of a university education for the future astronomer. In January, 1843, Adams was Senior Wrangler, and within a few months he had won the First Smith's Prize and had been elected to a College fellowship. Earlier, on July 3, 1841, Adams wrote his celebrated memorandum—now preserved in St. John's College library—in which he expressed his determination to start operations, as soon as he had taken his degree, on the mathematical discovery of a trans-Uranian planet.

The problem to which Adams devoted his energies during 1843-46 was one of considerable complexity. On the hypothesis of an unseen planet, the orbital elements of Uranus as deduced by Bouvard must be somewhat erroneous, for the observed positions of Uranus which he used must be affected by perturbations of which he was unaware; the corrections to Bouvard's orbital elements of Uranus constituted the first group of the unknowns in the mathematical formulation of the problem; to these must be added the mass of the hypothetical planet and the elements of its orbit. Owing to the way in which the mean distance of the new planet entered into the equations of condition it was necessary, if the problem were to be made practicable, to assume some value for this mean distance.

Adams, and afterwards Le Verrier, started with the value suggested by Bode's Rule as applied to the known planets. Before the end of 1843, Adams—he was then only twenty-four—had arived at a preliminary solution which convinced him that the hypothesis of an unknown planet was adequate to explain the anomalous behaviour of Uranus. He then proceeded to introduce some necessary refinements into his mathematical investigations. By September, 1845, he had made such progress that he was advised by Challis—then Plumian professor and director of the Cambridge Observatory—to place his results before Airy. Accordingly, when on his way to Cornwall for a holiday in September, Adams, armed with an introduction from Challis, called at the Royal Observatory, Greenwich, only to discover that Airy was in France. On his return from Cornwall, Adams again called on Airy (October 21, 1845) ; Airy was out at the time, but Adams left his card and a message to say that he would call in about an hour; he did so, but was informed that Airy was at dinner. Adams had perforce to depart, leaving, however, for the Astronomer Royal a "short statement" of the results of his researches, which, as we now know, were adequate to ensure the optical discovery of the planet at that time. Airy wrote to Adams fifteen days later putting his famous question as to whether Adams' theory could also explain the discrepancies between the values of the radius vector, as computed on Bouvard's theory, and the values which Airy had derived from observation. Adams did not bother to reply; he was not prepared to regard the question as other than trivial (although at the time he was only twenty-six, he was a master of planetary theory), and further he was disappointed that his efforts to make personal contact with Airy had proved fruitless. Adams' "short statement" remained in Airy's pocket for eight months, and probably would never have seen the light of day if events in France had not rescued it from oblivion.

In November, 1845, Le Verrier read his first memoir on Uranus; this can be described simply as "Bouvard" amended and brought up to date; there was no mention of a hypothetical planet. In June, 1846, Le Verrier read his second memoir, in which, after discussing the reasons for the necessity to assume the existence of an extraneous planet, he announced the position of a hypothetical body as deduced from his mathematical investigation; the mass and the elements of the orbit were not stated. The position obtained by Le Verrier was within a degree of the position found by Adams.

Towards the end of June, 1846, Airy put the same query about the radius vector to Le Verrier as he had put to Adams eight months earlier. Le Verrier replied without delay, assuring the Astronomer Royal that his theory accounted automatically for the errors in the radius vector; further, he applied to Airy for assistance in the search for the planet, promising to send him at once fuller details of his work. This request for practical aid and the offer of more precise information passed unheeded; nor did Airy inform Le Verrier that mathematical investigations of a similar character had been in progress at Cambridge for nearly three years previously. A day before Le Verrier's letter reached Airy, the latter announced to the Board of Visitors of the Royal Observatory the almost identical results—as regards the longitude of the new planet—obtained by Adams and Le Verrier, and on July 9, realizing that the situation was indeed becoming "desperate"—as he described it—he wrote to Challis, the director of the Cambridge Observatory, asking him to undertake the search for the new planet with the Northumberland Telescope, at that time one of the biggest instruments in the world. Challis agreed, and the search began on July 29. In the absence of a stellar chart of that part of the sky in which the planet was believed to be situated, Challis had perforce to undertake a laborious program of observations, determining the positions of all the stars within the suspected zone. Up to the end of September, when the news of the telescopic discovery of Neptune at Berlin reached Cambridge, Challis had made altogether 3,150 observations of stars and, as it transpired afterwards, had actually observed the planet on four occasions.

On August 31, 1846, Le Verrier's third paper was presented to the Academy at Paris; in this paper he gave the mass and the orbital elements of the planet and also stated that the planet should show a disk of about 3″ in diameter, which observations in due course confirmed almost exactly. Two days later Adams wrote to the Astronomer Royal giving him the results of a new solution of the problem, and, remembering Airy's former query about the radius vector, he indicated how reasonably well his theory fitted in numerically with the established errors of radius vector. There can be no doubt that, at this time, Adams was entirely ignorant that Le Verrier was hard on his heels; it is also certain that Le Verrier had no inkling of Adams' investigations.

On September 18, Le Verrier wrote to Galle, the assistant at the Berlin Observatory, requesting the latter to undertake the search for the planet; the letter was received on September 23, and Galle decided to start operations at once. A young student-observer, d'Arrest, suggested that the first thing to do would be to find out if Bremiker's star-chart (Hora XXI)—which included the zone in which the planet might be expected to be found—had been finished. A search in the director's house proved successful. There they found the edition of the relevant chart which had been engraved at the beginning of 1846 and which was being held back from distribution until another chart could keep it company in the post. Galle took charge of the telescope and described the configurations and magnitudes of the stars in the field of view, with d'Arrest checking Galle's observations on the chart. Soon Galle described the position of an eighth-magnitude star; d'Arrest immediately exclaimed: "That star is not on the chart." Subsequent observations confirmed its planetary character; the hypothetical planet had become a reality.

Naturally, there was great enthusiasm in France; Arago (director of Paris Observatory), referring to Le Verrier's achievement, declared that the discovery of the new planet "would remain one of the most magnificent discoveries of astronomical theory, one of the glories of the French Academy and one of the noblest titles of his country to the gratitude and admiration of posterity."

Into this atmosphere of rejoicing came immediately the first rude shock in the form of a letter from Sir John Herschel to the Athenoeum, making the first public reference to Adams and to his investigations; Herschel's knowledge of these was limited to the information briefly given by Airy at the meeting of the Board of Visitors three months previously. A second shock was provided by Challis' announcement that he had been engaged at Cambridge in the search for the hypothetical planet since the end of July, and that since its optical discovery at Berlin a scrutiny of his observations had revealed the fact that he had actually observed it on four occasions. Except for a comparison of his observations on July 30 and August 12, Challis made no attempt to discuss the fruits of his toil, despite the sense of urgency, which Airy's importunity and his own knowledge of Le Verrier's June paper would have made imperative to one who had even a modicum of faith in the results of mathematical analysis. Comparing his observations on August 12 with those on July 30, Challis noted that the first thirty-nine stars on the former date agreed with the observations on July 30; if he had gone on to star number forty-nine he would have seen that this star was absent from the records of July 30; this star was the planet. On September 29, before the news of the discovery at Berlin reached Cambridge, Challis, impressed with Le Verrier's insistence that the planet would show an unmistakable disk, noted against a star: "It seems to have a disc"; this again was the planet. The fourth observation had been made on August 4.

As might have been expected, consternation reigned in Paris at what appeared to be an impudent claim to priority of discovery made on behalf of Adams. No wonder that Arago announced pontifically that Adams had "no right to figure in the history of the new planet, neither by a detailed citation, nor even by the slightest allusion." The defense of Le Verrier was promptly undertaken by Airy who, in a letter to the former, declared: "You are to be recognized beyond doubt as the real predictor of the planet's place." A little later he wrote: "No one will dispute the completeness of your investigations and the fairness of your moral convictions as to the accuracy and certainty of the results. With these things, the produce not only of a mathematical but also of a philosophical mind we have nothing which we can put in competition. My acknowledgment of this will never be wanting." It is to be remembered that Airy's knowledge of Le Verrier's work was confined to the three abstracts printed in *Comptes Rendus,* for the full mathematical investigation was published only towards the end of 1846. Later, Airy's opinion was less dogmatic. Writing to Biot in June, 1847, he says: "I assure you that I have a very high opinion of Mr. Adams and that upon the whole I think his mathematical investigations superior to M. Le Verrier's. However, both are so admirable that it is difficult to say."

In the weeks following the discovery of Neptune, the French press was exceedingly bitter in its attacks on Airy, Challis, and Herschel. English men of science were dumbfounded at the revelations of Airy's and Challis' shares in the transaction. Considering the latter first, we have his own word that he had very little faith in the outcome of theoretical investigations for detecting a new planet; he seemed to undertake the laborious series of observations merely because Airy was firm on the matter; and when he had embarked on the observational program it never occurred to him to discuss his observations as they proceeded—except for the instance recorded earlier, and the comparison in this case was merely a test of the adequacy of the two separate observational methods he had adopted. Challis comes out of the Neptune episode as a skeptic and procrastinator, perhaps not earning, however, the almost brutal judgment passed on him by the historian of the Royal Astronomical Society.

It was Airy, however, on whom the greatest weight of criticism fell. His long silence as to Adams' investigations, his alleged 'snubbing' of Adams, but above all his fulsome praise of Le Verrier without any accompanying reference to Adams were the main points of accusation. Le Verrier, indeed, deserved every eulogy from whatever quarter it came; but the apparently pointed neglect of a young Cambridge graduate by the acknowledged head of British astronomy was something that no fair-minded person could understand. After reading the private papers of Adams and the contemporary literature, I am convinced that criticism of Airy was on some points unfair and unjustifiable; but I am equally convinced that his treatment of Adams in general was unbecoming to the leading astronomer of his generation. In any event some kind of action was called for. At the famous meeting of the Royal Astronomical Society on November 13, 1846, Airy read his "Account of some Circumstances Historically connected with the Discovery of the Planet exterior to Uranus"; he was followed by Challis, who described his observations at Cambridge, and finally by Adams, who outlined his theoretical investigations. In his "Account," Airy claimed to know the history of the whole business; but it is significant that of Adams he scarcely knew anything. In asking Adams for permission to insert in his "Account" such corespondence as had passed between them (this was Airy's second letter to Adams, the first being that containing the radius vector query) he addressed him as "The Rev. W. J. Adams"! Moreover, until then Airy, on his own confession, had met Adams only twice; on the first occasion he had forgotten where; on the second, in company with Hansen, on St. John's Bridge on July 2, 1846; each interview lasted no more than a couple of minutes. It seems extraordinary that on the second occasion—Le Verrier's second paper was by then known to both—two of the world's most eminent astronomers should meet the young Johnian without making some reference to his share in disentangling the most baffling problem in contemporary astronomy. Airy's "Account" contained several extraordinary passages, full of the liveliest eulogies of Le Verrier, but almost destitute of the deserved recognition of Adams' achievements. At the conclusion of his "Account," which in some measure must be reckoned a defense of his own conduct, Airy made one remarkable statement (its significance seems to have been overlooked by all previous commentators) to the effect that if Adams and Le Verrier had not adopted Bode's rule of distances they would never have arrived at the elements of the orbit. It is legitimate to ask if Airy really understood the problem of inverse perturbations so confidently and successfully tackled by Adams and Le Verrier, for unless some value of the semi-major axis, a, of the unknown planet is assumed, the problem becomes intractable owing to the complicated way a enters into the expression of the disturbing function. It was obvious to Adams and Le Verrier who, it must be remembered, were supremely confident of the existence of an exterior planet, that a 'trial and error method' was the only one to be adopted. They both soon found that the value of a must be considerably reduced—in other words that Neptune provided an exception to Bode's rule. If the rule had never been heard of, they must of necessity have adopted some value for a and proceeded on the lines of their respective investigations.

Challis had a most unenviable task at the meeting. A few days before, he had written to Airy: "I am in difficulties about this report (for the meeting) and should be glad to see some means of getting out of it." His 'report' was a confession of skepticism and procrastination. Adams' share in the proceedings took the form of a masterly account of his own investigations, concluding with a generous tribute to Le Verrier. It should be stated that he never took any part in the controversy that raged so long around his name, nor did he ever utter a harsh word about those to whom an inexperienced youth might have expected to look for guidance, advice, and encouragement.

Perhaps the greatest slight to which Adams was subjected was the award of the Copley Medal of the Royal Society to Le Verrier on November 30, 1846. In this award the discovery was attributed to Le Verrier alone without any refernce to Adams, despite the fact that those responsible for the award must have known about the proceedings at the Royal Astronomical Society meeting more than a fortnight before. The Royal Society was evidently of Arago's opinion that Adams had no right to figure in the history of the discovery of Neptune in any way. The Society, however, made amends by awarding to Adams the Copley Medal in 1848. The Royal Astronomical Society was saved by its by-laws from perpetrating a similar injustice. One medal and only one could be awarded; it was proposed, however, to waive the by-law protem, with the obvious intention of honoring both Le Verrier and Adams. This proposal in council was defeated. A resolution to award the Medal to Le Verrier alone was carried by 10 votes to 5, but as the by-laws stipulated a 3 to 1 majority the proposal was inoperative. Thus, there was no award by the leading astronomical society in the world for the most spectacular discovery in the history of astronomy.

Honors were immediately—and deservedly—showered on Le Verrier from all quarters. Recognition of Adams' achievements was much more tardy. It is worthy of mention that on the occasion of Queen Victoria's visit to Cambridge in the summer of 1847 the Vice-Chancellor was informed that "Her Majesty had commanded the honor of knighthood to be offered to Mr. Adams"; but Adams, against the advice of Prof. Adam Sedgewick, whom he consulted, modestly prayed to be allowed to decline the honor. About the same time he also declined the chair of natural philosophy at St. Andrews.

A subsidiary controversy—intimately connected, however, with the French claim on behalf of Le Verrier for the undivided credit of discovery—raged around the name to be given to the planet. It is usually stated that the name of Neptune (with a trident as the astronomical sign) was at first mutually agreed upon by Le Verrier and the Bureau of Longitudes. M. Danjon, director of the Paris Observatory, has recently informed me that there is no record in the Bureau confirming this; the name was certainly suggested by Le Verrier himself a few days after the discovery of the planet. But a little later, Le Verrier persuaded Arago to accept the discoverer's privilege of naming the new planet. Arago immediately announced that he had decided to name the new planet "Le Verrier," adding that in consequence there must be a wholesale renaming of the planets hitherto discovered (Uranus and five minor planets) in accordance with this new principle of attaching the discoverer's name to the planet discovered by him; for example, the name "Uranus" must now be changed to "Herschel." Both he and Le Verrier vowed that the new planet would never be referred to by them except by the name of "Le Verrier." At this time Le Verrier's complete mathematical investigations were presented to the Academy with the title "Researches on the Motion of the Planet Herschel (formerly Uranus)"; in the body of this large memoir the planet is referred to as Uranus, and Le Verrier explained in the preface that owing to the advanced state of printing it was impossible to effect the change throughout. This clumsy device of associating the new planet with Le Verrier alone met, quite naturally, with the unanimous disapproval of astronomers in other countries; it is perhaps worthy of mention that the Royal Astronomical Society Club—a festive body not usually prone to the discussion of serious subjects—was quite prepared to accept any mythological name proposed by Le Verrier. Soon the French astronomers were constrained to fall into line, and the name of Neptune passed eventually into established nomenclature.

The question as to whether Adams or Le Verrier should be accorded priority of discovery agitated scientific circles for several months. There could be no doubt as to the relevant events and their sequence. But could Adams' communications to Airy and Challis be regarded as 'publications,' for no one disputed the fact that Le Verrier was the first to get into print? The fact that Adams was engaged in investigations of a trans-Uranian planet was known to various reputable astronomers in Britain and was the subject of general comment in Cambridge; it is true, of course, that Airy and Challis were alone familiar, at some time or other, with the main features of Adams' investigation and in possession of such information as to lead to the detection of Neptune in October, 1845. The doctrine of priority was stated unequivocally by Biot in terms of "the common and imprescriptible law without which no scientific title could be assured that a discovery belongs to him who proclaims and publishes it to all." It is to Airy's credit that he explicitly denied the existence of such a law. Scientific workers in the past had adopted various expedients to ensure their titles to a discovery —the anagrams of Galileo and Huygens relating to the peculiar appearance of Saturn and to the rings of the planet are well-known instances. In later times the device of the 'sealed packet' became almost universal; Faraday, Wheatstone, and Brewster adopted this expedient, which was even more popular with the Paris Academy of Sciences for, in 1846, no fewer than ninety were deposited and recorded in *Comptes Rendus*. There was thus some reason for the claim on behalf of Adams, for was not Airy the custodian of the young astronomer's results, and was he not responsible (in the last resort) for jogging the apathetic Challis to activity? The impartial verdict of the illustrious Struve may be quoted: "It cannot be denied that Mr. Adams has been the first theoretical discoverer of Neptune, though not so fortunate as to effect a direct result of his indications." In this centenary year there is no need for us to try to settle this vexed—and interesting—question of priority; rather do we hail Adams and Le Verrier as the co-equal sharers of one of the greatest triumphs of science.

But the element of dramatic surprise had not yet been exhausted. Using Challis' observations at Cambridge, Adams proceeded to calculate the elements of the new planet's orbit, as accurately as such observations permitted; the results proved to be in excellent agreement with results derived afterwards from more abundant observational material. It is interesting to note that Airy had little faith in such attempts. Writing to Adams near the beginning of 1847, he says: "I cannot conceive that you can obtain from the observations made at the now expiring appearance of the new planet any determination of its actual distance from the sun sufficiently accurate to be of the smallest service to you." In this opinion he showed a sad lack of appreciation of the possibilities of determining the orbital elements and, what was of the greatest importance, of using these for the search of 'ancient' observations, as was so successfully done in the case of Uranus. The surprising result of Adams' and other calculations was the comparatively small value of the semi-major axis of Neptune's orbit: this was thirty astronomical units as against about thirty-five used in Adams' and Le Verrier's solutions; no wonder that Peirce of Harvard was led to declare that Neptune was not the planet resulting from mathematical analysis and that its discovery must be accounted a happy accident. The arguments in refutation of such a suggestion are somewhat technical, and we must be satisfied on the present occasion with the mere statement that Neptune is indeed the fruit of Adams' and Le Verrier's genius.

Adams, now armed with satisfactory orbital elements, himself examined old catalogs in an attempt to discover 'ancient' observations of the planet, but his efforts were unsuccessful. However, Walker at Harvard and Peterson at Altona discovered an old observation of Neptune made by Lalande on May 10, 1795; this observation was marked 'doubtful,' and it seemed bad luck that, out of so many thousand observations of stars, the only one that mattered in this connection should be reckoned by Lalande to be unworthy of confidence. Subsequent reference to Lalande's manuscripts revealed the interesting fact that Lalande had observed the 'star'—as he believed it to be—on May 8, as well as on May 10; as the observations on the two nights were discordant he discarded the first and included only the second in his catalog, labelling this as 'doubtful.' Instead of one unsatisfactory position of the planet, astronomers were now provided with two satisfactory positions, and these contributed very substantially to the accurate determination of the planet's orbit.

The subsequent careers of Le Verrier and Adams may be briefly indicated. For the former, a professorship of celestial mechanics was especially created in Paris; later he became director of the Paris Observatory. Le Verrier received the Gold Medal of the Royal Astronomical Society on two occasions in recognition of his masterly investigations in planetary theory.

Adams occupied the chair of mathematics at St. Andrews for a year, returning to Cambridge in 1859 as Lowndean professor; in 1861 he succeeded Challis as director of the Cambridge Observatory, where he resided until his death in 1892. He was president of the Royal Astronomical Society during 1851-53 (perhaps the most youthful occupant of the chair in the history of the Society) and again during 1874-76. In 1881 he was offered by Gladstone, then Prime Minister, the post of Astronomer Royal in succession to Airy, but this he declined. Adams' contributions to celestial mechanics were outstanding—perhaps no one has ever possessed such a thorough grasp of this most intricate subject, in which he was the acknowledged master.