magnetic changes observed at the Greenwich Observatory. He was elected a Fellow of the Society on May 8, 1874.

James Challis was the fourth son of Mr. John Challis, of Braintree, Essex, where he was born on December 12, 1803. He first went to Braintree school, where, as he used often to say himself, he soon learned all that they could teach him. He then went for a short time to a small school kept in Braintree by the Rev. Daniel Copsey, who was afterwards author of *Essays on Moral and Religious Subjects* (1821), *Studies in Religion* (1826), and other works. Seeing his talent Mr. Copsey, in conjunction with Mr. Matthews, vicar of Coggeshall, sent him to try for a presentation to Mill Hill School, near London, which he succeeded in obtaining by examination. Before proceeding to the University he read for a time with Mr. Matthews.

In October 1821 he entered Trinity College, Cambridge, as a sizar. He was elected a scholar in 1824, and in 1825 he graduated as senior wrangler, being also first Smith's prizeman. The same tripos list contains the name of Sir J. W. Lubbock, whose researches in the Lunar Theory are well known. In the following year Challis was elected Fellow of Trinity, and he resided in the college until he was ordained in 1830, when he was presented to the college living of Papworth Everard, which he held until 1852. He held no college office except during the last two years of his residence, when he took part in the college examinations. The vacations he spent with pupils in the Isle of Wight, Wales, and the English Lakes, once also visiting France. In 1831 he vacated his Fellowship by marriage with the widow of Mr. Daniel Copsey, second daughter of Mr. Samuel Chandler, of Tyringham in Buckinghamshire. He was re-elected Fellow of Trinity in 1870, and was a Fellow at the time of his death.

On February 2, 1836 he was elected Plumian Professor of Astronomy and Experimental Philosophy in the University, in succession to Professor Airy, who had been appointed Astronomer Royal. Mr. Challis was also at the same time made Director of the Cambridge Observatory, where he resided for the next five-and-twenty years, diligently engaged in making and reducing astronomical observations, and where he dispensed, in conjunction with Mrs. Challis, a kindly hospitality that is well remembered by Cambridge men of that time. He resigned the directorship of the Observatory in 1861, when he was succeeded by Professor Adams; but he retained the Plumian Professorship and resided in Cambridge till his death. From 1843 until within the last three or four years he always lectured on Practical Astronomy and the Use of Astronomical Instruments, and when his health became impaired and he was no longer able to lecture himself, he appointed as his deputy Mr. Freeman, late Fellow of St. John's College, who lectured for him on these subjects. Professor Challis was a man of kindly disposition and of simple and courteous manners. His strength gradually declined in the last few years of his life, and during the last year he was very weak, and it seemed as if death might come at any moment. He quietly passed away on Sunday, December 3, 1882, when within a few days of completing his seventy-ninth year. He was buried on Friday, December 8, at the Mill Road Cemetery, Cambridge, in the same grave with his wife.

Professor Challis was one of those who played a conspicuous part in what is not only the most important episode in the short history of the Cambridge Observatory, but perhaps the most striking event in the long records of Astronomy itself—the discovery of the planet *Neptune*. It is true that the planet was actually discovered at Berlin through Leverrier's predictions, quite independently of what had taken place at Cambridge, but it is true also that Adams had predicted the planet's place, and that Challis, looking in the place predicted, had actually twice seen the planet through the Northumberland Equatorial at Cambridge six weeks before the Berlin telescope was ever directed to the sky to look for it. The accidental possession of one of Bremiker's star-maps enabled Dr. Galle to detect the planet on the night on which he began the search; but the systematic and excellent method followed by Challis must soon have led to its discovery. It is difficult, perhaps, not to feel some regret that one who was so nearly successful, and who so well deserved success, should not have been enabled to announce to the world the actual discovery, and that the greatest of the triumphs of the Newtonian principles should not have been absolutely completed in the University where they had their birth; but so far from attributing any trace of blame to Professor Challis, one can scarcely admire too highly the zeal, industry, and conscientiousness which he brought to bear upon a research quite without precedent in the history of astronomy. He fully recognised the importance of the question, and showed no want of faith in the results obtained by refined and laborious analytical processes. On the contrary, he took every measure to secure the success of his undertaking, and success must have rewarded his efforts, had not their continuance been suddenly rendered unnecessary. The history of the discovery of the planet *Neptune* is given in three papers, all read before the Society on November 13, 1846, and printed in Vol. XVI. of the *Memoirs*. The first by the Astronomer Royal, which is entitled "Account of some Circumstances historically Connected with the Discovery of the Planet exterior to *Uranus*," appeared also in vol. vii. of the *Monthly Notices*. In the second Professor Challis gives an account of his observations at the Cambridge Observatory for the purpose of detecting the planet, and the third is Professor Adams's own paper, containing his mathematical investigations.*

* To secure more speedy publication, this paper was also issued with the *Nautical Almanac* for 1851, and copies of it were circulated with the number of the *Astronomische Nachrichten* for March 27, 1847.

The following *résumé* of the events relating to the investigations that preceded the discovery of the planet has been principally derived from these papers. It is well known that there were matters which gave rise to controversy at the time, but these are not referred to in what follows, only the essential facts being given. It is scarcely necessary to state that there is absolutely no doubt that the investigations of Adams and Leverrier were quite independent.

In 1841, Adams, then an undergraduate at St. John's College in his second year, formed the design of investigating the inequalities in the motion of *Uranus* that were still unaccounted for, as soon as he should have taken his degree. He graduated as senior wrangler in 1843, and at once attacked the problem. In February 1844 he asked Professor Challis to obtain from Mr. Airy, the Astronomer Royal, the errors in the tabular geocentric longitudes of *Uranus* for 1818–1826, with the factors for reducing them to errors of heliocentric longitude. These Professor Challis applied for, and the Astronomer Royal forwarded to him all the heliocentric errors of *Uranus* in longitude and latitude from 1754 to 1830. In September 1845, Adams called upon Professor Challis, and gave him a paper containing numerical values of the mean longitude at a given epoch, longitude of perihelion, eccentricity, mass and geocentric longitude of the new planet. On September 22, 1845, Professor Challis wrote a letter of introduction to the Astronomer Royal, beginning: "My friend Mr. Adams, who will probably deliver this note to you, has completed his calculations respecting the perturbation of the orbit of *Uranus* by a supposed ulterior planet, and has arrived at results which he would be glad to communicate to you, if you could spare him but a few moments of your valuable time." Adams called at Greenwich in September and October, but on neither occasion was he successful in seeing Mr. Airy, who at the time of the first visit was absent in France. At his second visit he left a paper, giving the following values of the mass and the orbit of the new planet:—

Mean Distance (assumed nearly in accordance with Bode's law)	38·4
Mean Sidereal Motion in 365·25 days	1° 30′·9
Mean Longitude, 1845, Oct. 1	323 34
Longitude of Perihelion	315 55
Eccentricity	0·1610
Mass (that of the Sun being unity)	0·0001656

This was accompanied by the list of the residual errors from 1690 to 1840, when the disturbance of the new planet was taken account of, the errors being very small, except in the case of Flamsteed's observation of 1690.

Some months later, in the number of the *Comptes Rendus* for June 1, 1846, Leverrier gave reductions of the existing observations of *Uranus*, and concluded that the observations were irreconcilable with theory, and that there was no other possible explanation of the discrepancy except that of a disturbing planet exterior to *Uranus*. He investigated the elements of the orbit of such a planet, its mean distance being assumed to be double that of *Uranus*, and its orbit being in the plane of the ecliptic. The value of the mean distance was suggested by Bode's law. Leverrier gave, as the most probable result of his investigations, that the true longitude of the disturbing planet for the beginning of 1847 must be about 325°, and that an error of 10° in this place was not probable. No elements of the orbit or mass of the planet were given. On July 9, 1846, Mr. Airy wrote to Professor Challis from the Deanery, Ely, suggesting that search should be made for the planet with the Northumberland Equatorial, and offering the services of an assistant; and on July 13 he transmitted to him certain suggestions with regard to the proposed sweeps for the planet. On July 18, Professor Challis wrote to the Astronomer Royal: "I have only just returned from my excursion. . . . I have determined on sweeping for the hypothetical planet. . . . With respect to your proposal of supplying an assistant I need not say anything, as I understand it to be made on the supposition that I decline making the search myself. . . . I purpose to carry the sweep to the extent you recommend." On August 7 Professor Challis wrote to Mr. Main, in the supposed absence of the Astronomer Royal, saying that he had undertaken the search for the new planet, and that he had made trial of two methods of observing. In the one recommended by Mr. Airy he had met with a difficulty, as he had anticipated, and he had therefore adopted another method.

On September 2 Professor Challis wrote to Mr. Airy: "I have lost no opportunity of searching for the planet; and, the nights having been generally pretty good, I have taken a considerable number of observations: but I get over the ground very slowly, thinking it right to include all stars to 10–11 magnitude; and I find that to scrutinise thoroughly in this way the proposed portion of the heavens will require many more observations than I can take this year." On the same day (September 2) Adams wrote to the Astronomer Royal a letter, the opening paragraphs of which are as follows: "In the investigation the results of which I communicated to you last October, the mean distance of the supposed disturbing planet is assumed to be twice that of *Uranus*. Some assumption is necessary in the first instance, and Bode's law renders it probable that the above distance is not very remote from the truth: but the investigation could scarcely be considered satisfactory while based on anything arbitrary; and I therefore determined to repeat the calculation, making a different hypothesis as to the mean distance. The eccentricity also resulting from my former calculations was far too large to be probable; and I found that, although the agreement between theory and observation continued very satisfactory down to 1840, the difference in subsequent years was becoming very sensible, and I hoped that these errors as well as the eccentricity might be diminished by taking a different mean distance. Not to make too violent a change, I assumed this distance to be less than the former value by about $\frac{1}{30}$th part of the whole. The result is very satisfactory, and appears to show that, by still further diminishing the distance, the agreement between the theory and the later observations may be rendered complete, and the eccentricity reduced at the same time to a very small quantity. The mass and the elements of the orbit of the supposed planet, which result from the two hypotheses, are as follows:—

	Hypothesis I. $\left(\frac{a}{a'}=0{\cdot}5\right)$	Hypothesis II. $\left(\frac{a}{a'}=0{\cdot}515\right)$
Mean Longitude of Planet, 1846, Oct. 1 ...	325° 8′	323° 2′
Longitude of Perihelion	315° 57′	299° 11′
Eccentricity	0·16103	0·12062
Mass (that of Sun being 1)	0·00016563	0·00015003

He also adds the errors of mean longitude, exhibiting the difference between theory and observation on the two hypotheses, and after pointing out that the errors given by the Greenwich Observations of 1843 are very sensible on both hypotheses, he proceeds: "By comparing these errors it may be inferred that the agreement of theory and observation would be rendered very close by assuming $\frac{a}{a'}=0{\cdot}57$, and the corresponding mean longitude on October 1, 1846, would be about 315° 20′, which I am inclined to think is not far from the truth. It is plain, also, that the eccentricity corresponding to this value of $\frac{a}{a'}$ would be very small." In consequence of the divergence of the results Adams asked for two normal places near the oppositions of 1844 and 1845. In the Astronomer Royal's absence these were sent by Mr. Main; and on September 7 Adams wrote: "I hope by to-morrow to have obtained approximate values of the inclination and longitude of the node."

But on August 31 Leverrier's second paper on the place of the disturbing planet had been communicated to the French Academy. The number of the *Comptes Rendus* containing this paper could not reach this country until the third or fourth week in September, and it does not appear that any earlier notice of its contents was received in England.

The elements given by Leverrier are—

Semi-axis Major	$36{\cdot}154 \left(\text{or } \frac{a}{a'} = 0{\cdot}531\right)$
Periodic Time	$217{\cdot}387$
Eccentricity	$0{\cdot}10761$
Longitude of Perihelion	$284^\circ\ 45'$
Mean Longitude, 1847, Jan. 1	$318^\circ\ 47'$
Mass	$=\frac{1}{9300}=0{\cdot}0001075$
True Heliocentric Longitude, 1847, Jan. 1	$326^\circ\ 32'$
Distance from the Sun	$33{\cdot}06$

Leverrier gave also comparisons between theory and observation, and he concluded that the planet would have a visible disk and sufficient light to make it conspicuous in ordinary telescopes.

In a letter received at Berlin on September 23, Leverrier invited Dr. Galle to search for the planet, suggesting that it might be recognised by its disk. The same evening Dr. Galle examined the heavens, comparing the stars with Dr. Bremiker's map (Hora xxi. of the Berlin Academy's Star Maps). He soon found a star of about the eighth magnitude, nearly in the place pointed out by Leverrier, which did not exist in the map. There could be little doubt that this was the new planet, and the observations of the two days following showed that its motion was nearly the same as that of the planet predicted. The finding of the planet was due to Dr. Bremiker's map: the disk could not easily be recognised before its existence was known.

It seems but just to Professor Challis that the following report, which he drew up for the Cambridge Observatory Syndicate, and which was printed at the time by them, should now be placed permanently on record as giving his own account of the circumstances attending the search for the planet at Cambridge. The report is dated December 12, 1846, and the preamble, which is signed by the syndics, runs: "The syndicate appointed to visit the Observatory, conceiving the subject at the present time to possess peculiar interest, beg leave to submit to the Senate the following statement of Professor Challis, describing the course of observations, founded on the theoretical calculations of Mr. Adams, of St. John's College, and made at the Observatory with a view to the discovery of the new planet." Professor Challis's report is as follows:—

"At a meeting of the Observatory Syndicate, held at the Observatory on December 4, for the despatch of ordinary business, a strong desire having been expressed by the Vice-Chancellor and the members of the Syndicate generally, to receive from me a Special Report of Observatory proceedings relating to the newly-discovered Planet, drawn up in such a manner, and in such detail, as would enable them to lay complete information on the subject before the members of the Senate, I considered it to be my duty at once to comply with this request. A new body of the solar system has been discovered, by means depending on the farthest advances hitherto made in theoretical and practical astronomy, and confirming, in a most remarkable manner, the theory of universal gravitation. It is, therefore, on every account desirable that the members of the Senate should be made fully acquainted with the part which has been taken by the Cambridge Observatory, relatively to this important extension of astronomical science. The observations I shall have to speak of, and the reasons for undertaking them, are so closely connected with theoretical calculations performed by a member of this University, to account for anomalies in the motion of the planet *Uranus*, that the history of the former necessarily involves that of the latter. I hope that for this reason, and because of the peculiar nature of the circumstances, I may be allowed to make a communication less formal and restricted in its character, than a mere Report of Observatory proceedings.

"The tables with which the observations of the planet *Uranus* have been uniformly compared, were published by A. Bouvard in 1821. They are founded on a continued series of observations extending from 1781, the year of its discovery, to 1821. Previous to 1781, it had been accidentally observed seventeen times as a fixed star, the earliest observation of this kind being one by Flamsteed in 1690. Bouvard met with a difficulty in forming his Tables. On an attempt to found them upon the ancient, as well as the modern, observations, it appeared that the theoretical did not agree with the observed course of the planet. He thought this might be attributed to the imperfection of the ancient observations, and consequently rejected all previous to 1781, in the formation of the Tables finally published. These Tables represent well enough the observations in the forty years from 1781 to 1821; but very soon after the latter year, new errors began to show themselves, which have gone on increasing to the present time. It was now evident that the ancient observations had been rejected on insufficient grounds, and that from some unknown cause the theory was in fault. Were the Tables calculated inaccurately? The difference between observation and theory (amounting in 1841 to 96″ of geocentric longitude) was too great, and Bouvard's calculations were made with too much care to allow of this explanation. The effect of small terms neglected in the calculation of the perturbations caused by *Jupiter* and *Saturn*, could not be supposed to bear any considerable proportion to the observed amount of error. This state of the theory suggested to several astronomers the idea of disturbances, caused by an undiscovered planet more distant than *Uranus*. But there is no evidence of this hypothesis having been put to the test of calculation previous to 1843. The usual problem of perturbations is to find the disturbing action of one body on another, by knowing the positions of both. Here an inverse problem, hitherto untried, was to be solved; viz. from known disturbances of a planet in known positions, to find the place of the disturbing body at a given time. Mr. Adams, Fellow of St. John's College, showed me a memorandum made in 1841, recording his intention of attempting to solve this problem as soon as he had taken his degree of B.A. Accordingly, after graduating in January 1843, he obtained an approximate solution by supposing the disturbing body to move in a circle at twice the distance of *Uranus* from the Sun. The result so far satisfied the observed anomalies in the motion of *Uranus*, as to induce him to enter upon an exact solution. For this purpose he required reduced observations made in the years 1818–1826, and requested my intervention to obtain them from Greenwich. The Astronomer Royal, on my application, immediately supplied (February 15, 1844), all the heliocentric errors of *Uranus* in longitude and latitude, from 1754 to 1830, completely reduced. Mr. Adams was now furnished with ample data from observation, and his next care was to ascertain whether Bouvard's theoretical calculations were correct enough for his purpose. He tested the accuracy of the principal terms of the perturbations caused by *Jupiter* and *Saturn*, and concluded that the small terms which Bouvard had not taken into account would not sensibly affect the final results, the chief of them being either of long period or of a period nearly equal to that of *Uranus*. Besides which he introduced into the theory several corrections which had been derived from observation and calculation by different astronomers since 1821. The calculations were completed in 1845. In September of that year, Mr. Adams placed in my hands a paper containing numerical values of the mean longitude at a given epoch, longitude of perihelion, eccentricity of orbit, mass, and geocentric longitude, September 30, of the supposed disturbing planet, which he calls by anticipation 'The New Planet,' evidently showing the conviction in his own mind of the reality of its existence. Towards the end of the next month, a communication of results slightly different was made to the Astronomer Royal, with the addition of what was far more important, viz. a list of the residual errors of the mean longitude of *Uranus*, for a period extending from 1690 to 1840, after taking account of the disturbing effect of the supposed planet. This comparison of observation with the theory implied the determination of *all* the unknown quantities of the problem, both the corrections of the elements of *Uranus* and the elements of the disturbing body. The smallness of the residual errors proved that the new theory was adequate to the explanation of the observed anomalies in the motion of *Uranus*, and that as the error of longitude was corrected for a period of at least 130 years, the error of radius vector was also corrected. As the calculations rested on an assumption, made according to Bode's law, that the mean distance of the disturbing planet was double that of *Uranus*, without the above-mentioned numerical verification, no proof was given that the problem was solved or that the elements of the supposed planet were not mere speculative results. The earliest evidence of the complete solution of an inverse problem of perturbations is to be dated from October 1845.

"Although the comparison of the theory with observation proved synthetically that the assumed mean distance was not very far from the truth, it was yet desirable to try the effect of an alteration of the mean distance. Mr. Adams accordingly went through the same calculations as before, assuming a mean distance something less than the double of that of *Uranus*, and obtained results which indicated a better accordance of the theory with observation, and led him to the conclusion, which has since been confirmed by observation, that the mean distance should be still farther diminished. This second solution taken in conjunction with the first may be considered to relieve the question of every kind of assumption. The new elements of the disturbing body, and the results of comparing the observed with the theoretical mean longitudes of *Uranus*, were communicated to the Astronomer Royal at the beginning of September 1846. These were accompanied by numerical values of errors of the radius vector, the Astronomer Royal having inquired, after the reception of the first solution, whether the error of radius vector, known to exist from observation, was explained by this theory. It would be wrong to infer that Mr. Adams was not prepared to answer this question till he had gone through the second solution. Errors of radius vector were as readily deducible from the first solution as from the other.

"The preceding details are intended to point out the circumstances which led astronomers to suspect the existence of an additional body of the solar system, and the theoretical reasons there were for undertaking to search for it. No one could have anticipated that the place of the unknown body was indicated with any degree of exactness by a theory of this kind. It might reasonably be supposed, without at all mistrusting the evidence which the theory gave of the *existence* of the planet, that its position was determined but roughly, and that a search for it must necessarily be long and laborious. This was the view I took, and consequently I had no thought of commencing the search in 1845, the planet being considerably past opposition at the time Mr. Adams completed his calculations. The succeeding interval to midsummer of 1846 was a period of great astronomical activity, the planet *Astræa*, Biela's double comet, and several other comets, successively demanding attention. During this time I had little communication with Mr. Adams respecting the new planet. Attention was again called to the subject by the publication of M. Leverrier's first researches in the *Comptes Rendus* for June 1, 1846. At a meeting of the Greenwich Board of Visitors held on June 29, at which I was present, Mr. Airy announced that M. Leverrier had obtained very nearly the same longitude of the supposed planet as that given by Mr. Adams. On July 9 I received a letter from Mr. Airy, in which he suggested employing the Northumberland Telescope in a systematic search for the planet, offering at the same time to send an assistant from Greenwich, in case I declined undertaking the observations. This letter was followed by another dated July 13, containing suggestions respecting the mode of conducting the observations, and an estimation of the amount of work they might be expected to require. In my answer, dated July 18, I signified the determination I had come to of undertaking the search. Various reasons led me to this conclusion. I had already, as Mr. Adams can testify, entertained the idea of making these observations; the most convenient time for commencing them was now approaching; and the confirmation of Mr. Adams's theoretical position by the calculations of M. Leverrier appeared to add very greatly to the probability of success. I had no answer to make to Mr. Airy's offer of sending an assistant, as I understood the acceptance of it to imply the relinquishing on my part of the undertaking.

"I have now to speak of the observations. The plan of operations was formed mainly on the suggestions contained in Mr. Airy's note of July 13. It was recommended to sweep over, three times at least, a zodiacal belt 30° long and 10° broad, having the theoretical place of the planet at its centre; to complete one sweep before commencing the next; and to map the positions of the stars. The three sweeps, it was calculated, would take 300 hours of observing. This extent of work, which will serve to show the idea entertained of the difficulty of the undertaking before the planet was discovered, did not appear to me greater than the case required. It will be seen that the plan did not contemplate the use of hour XXI. of the Berlin Star Maps, the publication of which was equally unknown at that time to Mr. Airy and myself. It may be proper here to explain that the construction of a good star-map requires a great amount of time and labour both in observing and calculating, and that precisely this sort of labour must be gone through to conduct a search of the kind I had undertaken. The stars must first be mapped before the search can properly be said to begin. With a map ready made, the detection of a moving body, as it happened in this instance, might be effected on a comparison of the heavens with the map by mere inspection. Not having the advantage of such a map, I proceeded as follows. I noted down very approximately the positions of all the stars to the 11th magnitude that could be conveniently taken as they passed through the field of view of the telescope, the breadth of the field with a magnifying power of 166 being 9′, and the telescope being in a fixed position. When the stars came thickly, some were necessarily allowed to pass without recording their places. Wishing to include *all* stars of the 11th magnitude, I proposed, in going over the same region a second time, to avail myself of an arrangement peculiar to the Northumberland Equatorial, the merit of inventing which is due to Mr. Airy. The Hour-circle, Telescope, and Polar Frame are movable by clockwork, which may be regulated to sidereal time nearly. While this motion is going on, the Telescope and Polar Frame are movable *relatively to the Hour-circle*, by a tangent-screw apparatus, and a handle extending to the observer's seat. This contrivance enables the observer to measure at his leisure differences of Right Ascension however small, and therefore meets the case of stars coming in groups. The observations made by this method might include all the stars it was thought desirable to take, and therefore might include *all* the stars taken in the first sweep. The discovery of the planet would result from finding that any star in the first sweep was not in its position in the second sweep. If two sweeps failed in detecting the planet among the stars of the first sweep, it might be among the stars of the second, which would be decided by taking a third sweep of the same kind as the second. It will appear that this plan carried out would not only detect the planet if it were in the region explored, but would also, in case of failure, enable the observer to pronounce that it was not in that region. The second mode of observing required the aid of my two assistants, Mr. Morgan and Mr. Breen, in reading off and recording the observations.

"I commenced observing July 29, employing on that day the first method, with telescope fixed. The next day I observed according to the second method, with telescope moving. On August 4, the telescope was fixed as to Right Ascension, but was moved in Declination in a zone of about 70′ breadth, the intention of the observations of that day being to record points of reference for the zones of 9′ breadth. On August 12, the fourth day of observing, I went over the same zone, telescope fixed, as on July 30 with telescope moving. Soon after August 12, I compared, to a certain extent, the observations of that day, with the observations of July 30, taken with telescope moving; and finding, as far as I carried the comparison, that the positions of July 30 included *all* those of August 12, I felt convinced of the adequacy of the method of search I had adopted. The observations were continued with diligence to September 29, chiefly with telescope fixed, and were made early in Right Ascension for the purpose of exploring as large a space as possible before I should be compelled to desist by the approach of daylight. On October 1 I heard that the planet was discovered by Dr. Galle, at Berlin, on September 23. I had then recorded 3150 positions of stars, and was making preparations for mapping them. The following results were obtained by a discussion of the observations after the announcement of the discovery.

"On continuing the comparison of the observations of July 30 and August 12, I found that No. 49, a star of the 8th magnitude in the series of August 12, *was wanting in the series of July* 30. According to the principle of the search, this was the planet. It had wandered into the zone in the interval between July 30 and August 12. I had not continued the former comparison beyond No. 39, probably from the accidental circumstance that a line was there drawn in the memorandum-book in consequence of the interruption of the observations by a cloud. After ascertaining the place of the planet on August 12, I readily inferred that it was also among the reference stars taken on August 4. Thus, after four days of observing, two positions of the planet were obtained. This is entirely to be attributed to my having, on those days, directed the telescope towards the planet's theoretical place, according to instructions given in a paper Mr. Adams had the kindness to draw up for me. I would also beg to call attention to the fact that, after August 12, the planet was discoverable by a closet-comparison of the observations, a method of observing, depending on novel and ingenious mechanism, having been adopted, by which I could say of each star, to No. 48, 'This is not a planet,' and of No. 49, 'This *is* a planet.' I lost the opportunity of announcing the discovery by deferring the discussion of the observations, being much occupied with reductions of comet observations, and little suspecting that the indications of theory were accurate enough to give a chance of discovery in so short a time. On September 29 I saw, for the first time, the communication presented by M. Leverrier to the Paris Academy on August 31. I was much struck with the manner in which the author limits the field of observation; and with his recommending the endeavour to detect the planet by its disk. Mr. Adams had already told me that, according to his estimation, the planet would not be less bright than a star of the ninth magnitude. On the same evening I swept a considerable breadth in Declination, between the limits of Right Ascension marked out by M. Leverrier, and I paid particular attention to the physical appearance of the brighter stars. Out of 300 stars, whose positions I recorded that night, I fixed on one which appeared to have a disk, and which proved to be the planet. This was the third time it was observed before the announcement of the discovery reached me. This last observation may be regarded as a discovery of the planet, due to the good definition of the noble instrument which we owe to the munificence of our Chancellor.

"From the reduced places of the planet, on August 4 and August 12, and from observations since its discovery extending to October 13, Mr. Adams calculated, at my request, values of its heliocentric longitude at a given epoch, its actual distance from the Sun, longitude of the node, and inclination of the orbit, which were published as early as October 17. I am now diligently observing the planet with the meridian instruments, and when daylight prevents its being seen on the meridian, I propose carrying on the observations as long as possible with the

Northumberland Equatorial, for the purpose of obtaining data for a further approximation to the elements of the orbit.

"My report of proceedings relating to the planet here terminates. I beg permission to add a few remarks, which the facts I have stated seem to call for. It will appear by the above account, that my success might have been complete, if I had trusted more implicitly to the indications of the theory. It must, however, be remembered, that I was in quite a novel position: the history of astronomy does not afford a parallel instance of observations undertaken entirely in reliance upon deductions from theoretical calculations, and those too of a kind before untried. As the case stands, a very prominent part has been taken in the University of Cambridge, with reference to this extension of the boundaries of astronomical science. We may certainly assert to be facts, for which there is documentary evidence, that the problem of determining, from perturbations, the unknown place of the disturbing body, was first solved here; that the planet was here first sought for; that places of it were here first recorded; and that approximate elements of its orbit were here first deduced from observation. And that all this may be said, is entirely due to the talents and labours of one individual among us, who has at once done honour to the University, and maintained the scientific reputation of the country. It is to be regretted that Mr. Adams was more intent upon bringing his calculations to perfection, than on establishing his claims to priority by early publication. Some may be of opinion, that in placing before the first astronomer of the kingdom results which showed that he had completed the solution of the problem, and by which he was, in a manner, pledged to the production of his calculations, there was as much publication as was justifiable on the part of a mathematician whose name was not yet before the world, the theory being one by which it was possible the practical astronomer might be misled. Now that success has attended a different course, this will probably not be the general opinion. I should consider myself to be hardly doing justice to Mr. Adams, if I did not take this opportunity of stating, from the means I have had of judging, that it was impossible for any one to have comprehended more fully and clearly all the parts of this intricate problem; that he carefully considered all that was necessary for its exact solution; and that he had a firm conviction, from the results of his calculations, that a planet was to be found."

With regard to the disk of the planet, Encke, in his account of the discovery by Dr. Galle in Vol. xxv. (col. 52) of the *Astronomische Nachrichten*, writes: "Erlauben Sie mir nur hinzuzufügen, dass die Auffindung so schnell bloss durch die vortreffliche akademische Sternkarte von Bremiker möglich war. Eine Scheibe lässt sich erst erkennen, wenn man weiss dass es seyn wird." Bremiker's star-map, Hora xxi., was communicated to the Berlin Academy on December 9, 1844, and it was lying for correction at the Berlin Observatory when Leverrier's letter was received (*Monthly Notices*, vol. xxxviii. p. 151).

Professor Challis published a second report to the Syndicate, dated March 22, 1847, relating to the subsequent observations of the new planet, but this need not be further referred to here, as it was reprinted in the *Astronomische Nachrichten* (vol. xxv., col. 309). The more Professor Challis's part in the history of the planet is examined, the more highly one appreciates his assiduity and zeal. He seems to have throughout done all in his power to encourage and assist Adams in his investigations, and it was through no fault of his that the honour of discovering a planet whose existence had been thus predicted does not belong to this country.

At the second return of Biela's comet since it was discovered to be periodic, in 1826, and the eleventh of its returns since it was first observed in 1772, it was found to have divided into two. It was observed by Encke on December 21, 1845, at Berlin, and by Valz, on December 25, at Marseilles; but no trace of separation was then noticed. In Europe the existence of two separate nuclei was first observed and announced by Professor Challis. In a letter to the President of the Society, printed in vol. vii. pp. 73, 74, of the *Monthly Notices*, he wrote:—"On the evening of January 15, when I first sat down to observe it, I said to my assistant, 'I see *two* comets.' However, on altering the focus of the eye-glass and letting in a little illumination, the smaller of the two comets appeared to resolve itself into a minute star, with some haze about it. I observed the comet that evening but a short time, being in a hurry to proceed to observations of the new planet [*Astræa*]. On first catching sight of it on this evening (Jan. 23), I again saw two comets. Clouds immediately after obscured the comet for half an hour. On resuming my observations I suspected at first that both comets had moved. This suspicion was afterwards confirmed: the two comets have moved in equal degree, retaining their relative positions. . . . . What can be the meaning of this? Are they two independent comets? or is it a binary comet? or does my glass tell a false story? I incline to the opinion that this is a binary or double comet, on account of my suspicion on Jan. 15. But I never heard of such a thing. I am anxious to know whether other observers have seen the same thing. . . . In the meanwhile I thought, with the evidence I have, I had better not delay giving you this information." In a subsequent letter he wrote:—"There are certainly two comets. . . . . I think it can scarcely be doubted, from the above observations, that the two comets are not only apparently, but really, near each other, and that they are physically connected. When I first saw the smaller on Jan. 15, it was faint, and might easily have been overlooked. *Now* it is a very conspicuous object, and a telescope of moderate power will readily exhibit the most singular phenomenon that has occurred for many years—a double comet!" It appears that M. Wichmann, at Königsberg, observed the comet on the 14th, but saw nothing of the companion: there was, however, some vapour in the air. On January 15, the same night as that on which Professor Challis saw the two comets, the air being purer and the moon not risen, he saw the companion comet immediately with a power of 45. The duplication of the comet had, however, been previously observed at Washington by Lieutenant Maury, Director of the Naval Observatory, who "discovered during his observations on Jan. 13th a nebulous-looking object altogether cometary in its appearance, preceding Biela's comet by nine or ten seconds in the lower part of the field of view." On the 14th "both objects had increased about three minutes in Right Ascension since the night before" (*Monthly Notices*, vii. pp. 74, 90).

Professor Challis communicated his observations of the two heads of the comet to Adams, who calculated their orbits. The relative positions of the two heads formed the subject of Adams's first communication to the Society (March 13, 1846).

The comet at its return in 1852, when the distance between the nuclei was about eight times as much as before, was again observed by Professor Challis. Neither comet was seen in 1859 or 1866, and the remarkable circumstances relating to their supposed connection with the meteor shower of November 27, 1872, are too well known and too recent to need notice here (see *Monthly Notices*, vol. xxxiii.). Remarkable as the Cambridge observations of the Comet in January 1846 seemed to be at the time, its subsequent history has given even additional interest to them.

During the twenty-five years in which Professor Challis directed the Cambridge Observatory he was a very accurate and assiduous observer, making great use of the Northumberland Equatorial, and his contributions to the publications of the Society and to the *Astronomische Nachrichten* are very numerous. He also paid great attention to instrumental improvements, and to him is due the introduction in its present form of the collimating eyepiece, an instrument now so generally used that it is worth while to reproduce here the account he gives of it in his *Lectures on Astronomy* (p. 69):—

"This important auxiliary instrument, which enables the observer to obtain instrumental corrections exclusively by optical means, was the invention of Bohnenberger, of Tübingen, who has given a description of it in the *Astronomische Nachrichten* (Band iv., 1826, col. 327–336). My attention was first called to it by Henderson, late Astronomer Royal at the Cape of Good Hope, who brought me a specimen (made apparently according to the above-mentioned description), having a metallic reflector with a hole at the centre, through which the wires and their reflected images were looked at with a Ramsden Eyepiece. On trial I found this construction to be extremely inconvenient, on account of the limited field of view and the small interval between the eye-glass and the wires, rendering it difficult to hold a lamp for throwing light upon the reflector. On mentioning these circumstances to the late William Simms, he constructed for me the instrument represented by Figs. 19 and 20, in which a three-glass eyepiece is substituted for the Ramsden Eyepiece, and for the metallic reflector a piece of plate-glass, the reflection from which, as will presently be explained, gives the means of seeing the wires, together with their reflected images, with quite sufficient distinctness. By these changes the above-stated inconveniences were entirely removed. As far as I am aware, the collimating eyepiece has since been uniformly made according to this pattern. I brought it into use in the Cambridge Observatory in the year 1850; the next year it was adopted at Greenwich when the new Transit Circle was first made use of. It had already attracted the attention of Bessel, Gauss, and Lamont, but had not, I believe, been definitively employed for exact determinations relating to meridian observations with the Transit instrument and Mural Circle before I made such application of it at the Cambridge Observatory."

He also invented the Transit-Reducer, a machine for calculating the formula

$$(a + b \cos z + \sin z)\frac{\operatorname{cosec} \delta}{15},$$

the total value of which is given by a single operation. The instrument which he used in the Cambridge Observatory was shown in the Great Exhibition of 1851, and received the award of a bronze medal. The machine is described in vol. x. of the *Monthly Notices*, and also on pp. 387–390 of his *Lectures*.

Another mechanical contrivance to which he devoted much attention was connected with his method of correcting the errors due to the forms of the pivots of a Transit instrument. The method which involved the use of the collimating eyepiece is described in vol. xix. of the *Memoirs*. Mention should also be made of the "Meteoroscope," an instrument invented by him for the purpose of rapidly determining the altitude and azimuth of any point of the heavens at which a meteor appeared. This instrument was a good deal used at Cambridge.

In the twenty-five years, 1836–61, during which Professor Challis was director of the Observatory, he published vols. ix.–xix. of the *Cambridge Observations*; vol. xx., which contained the observations for the years 1855–1860, was published by him in 1864, in accordance with the arrangement made when he retired from the directorship in 1861, by which he undertook the superintendence of the reduction and publication of the remainder of the observations made prior to 1861. On the Introductions to the different volumes of the *Cambridge Observations* he bestowed great pains and attention; the Introductions to those for 1836 and 1837 contain a detailed description of the methods of observing with the meridian instruments.

In the first years of his Professorship he lectured upon Hydrodynamics, Pneumatics, and Optics with special reference to the mathematical theories of Light and Sound; the leading facts were exhibited experimentally, and explanations were given of the principles employed in the mathematical reasoning. He published a Syllabus of these lectures in 1838. In 1843, when he had been director of the Observatory for seven years, he began a course of lectures on Astronomy and Astronomical Instruments, and this course he continued to give regularly, without interruption, until, as has already been stated, within the last few years. The Syllabus of these lectures, which he published in 1843, bears the title "A Syllabus of Lectures on Practical Astronomy and Astronomical Instruments: to which is added a list of Formulæ used in the Reduction of Astronomical Observations." Towards the close of his life he arranged his lectures in a form suitable for publication, and they were issued from the University Press at Cambridge in 1879, under the title "Lectures on Practical Astronomy and Astronomical Instruments." The volume contains 400 pages, and on every page of it there is evidence of the author's efforts to attain accuracy and his careful attention to *minutiæ* in all that concerns the instruments of an Observatory. It has special reference to the Cambridge instruments, and was intended mainly for use in the University; but he writes in the preface: "Although the instruments of the Cambridge Observatory and processes of observation I adopted in the use of them, have been more especially described, and the treatise consequently partakes somewhat of a local and personal character, I may venture, I think, to say that as having been written after twenty-five years of continuous labour in astronomical observations and calculations, and containing what may have occurred to me in the course of that experience as contributory to the advancement or improvement of practical astronomy, it will be found of some general utility as respects the work carried on in an Astronomical Observatory." All who attended Professor Challis's lectures will feel satisfaction that they are now placed on record. For nearly fifty years no one could have been more faithful than he was to the study of practical astronomy in the University.

Professor Challis wrote several papers on points connected with the integration of the equations in the Lunar Theory, which appeared in the *Philosophical Magazine* for 1854 and 1855, and a memoir on the Problem of Three Bodies, which was printed in the *Philosophical Transactions* for 1856. The first of the papers in the *Philosophical Magazine* (April 1854) was originally communicated to the Cambridge Philosophical Society, and was reported upon unfavourably by Professor Adams. In the number of the *Philosophical Magazine* for June 1854 Professor Challis invited Professor Adams to discuss with him its merits, and accordingly in the July number Professor Adams gave in detail the reasons for his disapproval of the new theorems contained in the paper. It is only fair to Professor Challis to mention the handsome manner in which, fifteen years afterwards, in the introduction to his "Notes on the Principles of Pure and Applied Calculation," he acknowledges the justice of this criticism. He admits that the unfavourable report of the paper was made to the Council "not without reason; for it was a premature production, and had in it much that was insufficiently developed, or entirely erroneous. . . . . Theorem II. was wholly erroneous;" and he proceeds: "In my reply in the August number I said much in the heat of controversy that had better not have been said, and some things, also, that were untrue." He states further that when he found the discussion had not settled the matter, he pursued the inquiry in a series of communications, "which will at least attest the diligence with which I laboured to get at the truth of the question."

Before leaving the astronomical writings of Professor Challis, it is interesting to notice that the earliest of all his papers was astronomical, its object being to investigate an extension of Bode's law to the case of the satellites of the planets. It was read before the Cambridge Philosophical Society so long ago as December 8, 1828, and is printed in vol. iii. of their *Transactions*.

Professor Challis was the author of numerous papers on Hydrodynamics, Heat, Light, the Theory of Colours, &c. His Report on the State of Hydrodynamics—perhaps the best known of his mathematical papers—appeared in the British Association volume for 1833. It was in order to be enabled to devote more time to the development of his theories of mathematical physics that he resigned his charge of the Observatory. His "Notes on the Principles of Pure and Applied Calculation, and Applications of Mathematical Principles to Physics," is a large volume of 700 pages, which was published in 1869. He states that 112 pages were printed in 1859, when he was compelled to desist from it by the pressure of his occupations at the Observatory. After remarking that he holds it to be indisputable that physical science is incomplete till experimental inductions have been accounted for theoretically, and that the completion of a physical theory demands mathematical reasoning, he proceeds, "When according to the best judgment I could form respecting the applications which the results of my hydrodynamical researches were capable of, I seemed to see that no one was as well able as myself to undertake this necessary part in science, I gave up (in 1861) my position at the Observatory, under the conviction, which I expressed at the time, that I could do more for the honour of my University and the advancement of science by devoting myself to theoretical investigations than by continuing to take and reduce astronomical observations after having been thus occupied during twenty-five years. The publication of this work will enable the cultivators of science to judge whether in coming to this determination I acted wisely. Personally I have not for a moment regretted the course I took; for although it has been attended with inconveniences arising from the sacrifice of income, I felt that what I could best do, and no one else seemed capable of undertaking, it was my duty to do. It should, farther, be stated that after quitting the Observatory, and before I entered upon my theoretical labours, I considered that I was under the obligation to complete the publication of the meridian observations taken during my superintendence of that institution. This work occupied me till the end of 1864, and thus it is only since the beginning of 1865 I have been able to give undivided attention to the composition of the present volume."

He subsequently published "An Essay on the Mathematical Principles of Physics with reference to the study of physical science by candidates for mathematical honours in the University of Cambridge" (108 pp., 1873), and "Remarks on the Cambridge Mathematical Studies and their relation to modern physical science" (93 pp., 1875).

Much that he wrote, especially on Hydrodynamics, did not receive acceptance from other mathematical physicists. He devoted his life with great assiduity and constancy to the search for philosophical truth, endeavouring to carry out Newton's principles. Although personally he was modest in the extreme, yet he was so earnest in his views and held such strong convictions as to the mode in which philosophical inquiries should be carried out that his language sometimes became almost self-assertive. In a letter to Whewell (1863) he wrote: "It has been the business of my life to endeavour to reach 'the second main series of physical discovery' in the direction that Newton indicated. Accordingly, I have adopted implicitly his 'foundation of all philosophy,' including therein his views expressed at the end of the *Principia* respecting the action of a 'very subtle spirit' (the ether) which 'pervades dense bodies,' and to the agency of which he attributes the phenomena of light, heat, electricity, &c. In conjunction with the Newtonian ideas I have taken advantage of the modern advancement in pure analysis, and in particular have applied partial differential equations in determining the motions and dynamical action of the supposed ether. It is marvellous how readily the results so obtained, taken in connection with the Newtonian properties of matter, adapt themselves to the solution of the great problems of Natural Philosophy. And yet none of my mathematical contemporaries have taken the same course, and I seem to remain the sole representative of the spirit of the Newtonian philosophy." He was so gentle in his character and his life was so simply and unselfishly devoted to the search after truth, that it is all the more matter for regret that the exceptional character of some of his views rendered part of his work of doubtful scientific value.

As Plumian Professor he was examiner for the Smith's Prizes, and he examined without intermission from 1836 to 1878. He set long papers, and he took great trouble over them. They form a very remarkable series, and afford a perfect record of the matters that occupied his attention in all these years. Not only were the papers well suited to their purpose, but they possessed considerable interest of their own; and this is especially true, perhaps, of some of the earlier and simpler questions in each paper. The papers are to be found in the University Calendars for the different years; he had thoughts of reprinting them, in which case they would have formed an interesting and remarkable volume, but he abandoned the idea.

He was also author of the following works:—"Creation in Plan and in Progress: being an essay on the first chapter of Genesis" (1861); "A Translation of the Epistle of the Apostle Paul to the Romans, with an Introduction and Critical Notes" (1871); "An Essay on the Scriptural Doctrine of Immortality" (1880); "The Counting and Interpretation of the Apocalyptic 'number of the Beast'" (1881).

He was elected a Fellow of this Society on April 8, 1836, and of the Royal Society on June 9, 1848.

He leaves one son and one daughter. His son, Mr. James Law Challis, was appointed in 1860 to the Rectory of Papworth Everard, which had been held by his father from 1831 to 1852, and in 1878 he was presented by the Council of this Society to the Vicarage of Stone in Buckinghamshire, of which they were then the patrons. J. W. L. G.

Henry Dodgson, M.D., of Derwent House, Cockermouth, was born at Mockerkin, in Cumberland, on March 27, 1833, and was the youngest son of Isaac Dodgson, Esq. He chose the medical profession, and studied at the Universities of Edinburgh and Paris, graduating M.D. at Edinburgh in 1856. Since then he has practised in Cockermouth, and ultimately succeeded to one of the most extensive practices in the neighbourhood, where he was widely respected and esteemed. In 1866 he was proposed by the late Isaac Fletcher, F.R.S., M.P., and elected a Fellow of this Society. At that time he gave much of his time to observations in astronomy, and had an Observatory with a good telescope erected at considerable cost. But latterly, owing to the death of his partner and the calls of a large practice, he was obliged to relinquish a study he had a great love for. He, however, found time to be interested in the great educational movements of the day, and some years ago was elected Chairman of the School Board, which office he held up to the time of his death. He was a Fellow of the Meteorological Society, and took regular meteorological observations, which were published in the Registrar-General's reports. He died, after a fortnight's illness, of pneumonia, followed by typhoid fever. In 1866 he married his partner's niece (daughter of the late Edward Hughes, Esq., F.R.G.S., Head Master of the Royal Naval School, Greenwich) by whom he had nine children, who, with his widow, survive him.

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