Obituary.

The Council regret that they have to record the loss by death of the following Fellows and Associates during the past year:—

Fellows:—Rev. Edward Allen.
Robert Grant.
John Hartnup.
Thomas Archer Hirst.
Joseph Kleiber.
Benjamin Loewy.
W. Edwards Michell.
Rev. I. Vale Mummery.
A. V. Nursing Row.
Captain G. C. Parker.
Charles Greville Prideaux.
Rev. Joseph Spear.
Thomas Taylor.
George Turnbull.
W. Mattieu Williams.

Associates:—Annibale de Gasparis.
Admiral E. Mouchez.
Lewis M. Rutherfurd.

John Couch Adams was born on June 5, 1819, at the farm-house of Lidcot, seven miles from Launceston, in Cornwall. His father was a tenant farmer, and his mother possessed a small estate of land of her own. She had also inherited her uncle's library, and these books, which included some on astronomy, were his early companions. At the village school at Laneast he made rapid progress, and was learning algebra before he was twelve years old. At this age he went to a private school at Devonport, kept by the Rev. John Couch Grylls, a first cousin of his mother.

He remained under Mr. Grylls's tuition for a good many years, first at Devonport and afterwards at Saltash and Landulph, and received the usual school training in classics and mathematics. Astronomy had been his passion from very early boyhood, and at fourteen years of age he made copious notes and drew tiny maps of the constellations. He read with avidity all the astronomical books to which he could obtain access, and in particular he studied the astronomical articles in Rees's

"Cyclopædia," which he met with in the library of the Devonport Mechanics' Institute, where he used to spend his spare time in reading astronomy and mathematics. In the same library he came across a copy of Vince's "Fluxions," which was his first introduction to the higher mathematics.

He showed such signs of mathematical power that in 1837 the idea of his going to Cambridge was entertained. He accordingly entered St. John's College, Cambridge, in October 1839. During his undergraduate career he was invariably the first man of his year in the college examinations, and in 1843 he graduated as Senior Wrangler, being also first Smith's Prizeman. In the same year he was elected Fellow of his college.

His attention was drawn to the irregularities in the motion of *Uranus* by reading Airy's report upon recent progress in astronomy in the British Association volume for 1831–32,* and on July 3, 1841, he made the following memorandum:—"Formed a design at the beginning of this week of investigating, as soon as possible after taking my degree, the irregularities in the motion of *Uranus* which are yet unaccounted for, in order to find whether they may be attributed to the action of an undiscovered planet beyond it; and, if possible, thence to determine the elements of its orbit &c. approximately, which would probably lead to its discovery." This memorandum was made at the beginning of his second long vacation, when he was in his twenty-third year.†

In 1843, the year in which he took his degree, he attempted a first rough solution of the problem on the assumption that the orbit was a circle with a radius equal to twice the mean distance of *Uranus* from the Sun. The result showed that a good general agreement between theory and observation might be obtained. In order to make the data employed more complete application was made through Professor Challis to the Astronomer Royal for the results of the Greenwich observations of *Uranus*. When these were obtained Adams undertook a new solution of the problem, taking into account the most important terms depending on the first power of the eccentricity of the orbit of the supposed disturbing planet, but retaining the same assumption as before with respect to the mean distance. In September, 1845, he communicated to Professor Challis the values which he had obtained for the mass, heliocentric longitude, and elements of the orbit of the assumed planet. The same results, slightly corrected, he took with him to the Royal Observatory, Greenwich, on October 21, 1845. The paper which he left at the Observatory on this occasion also contained a list

* This report does not contain any reference to the irregularities being possibly due to an exterior planet. It is merely mentioned that it seems impossible to unite all the observations in one elliptic orbit, and that Bouvard was therefore obliged to reject the ancient observations entirely (p. 154).

† The original memorandum, written by itself on a slip of paper, has been found among his papers since his death.

R

of the residual errors of the mean longitude of *Uranus*, after taking account of the disturbing effect of the new planet, at dates extending from 1690 to 1840.

On November 10, 1845, Le Verrier presented to the French Academy an elaborate investigation of the perturbations of *Uranus* produced by *Jupiter* and *Saturn*, in which he pointed out several small inequalities which had previously been neglected. After taking these into account, and correcting the elements of the orbit, he still found that the theory was quite incapable of explaining the observed irregularities in the motion of *Uranus*.

On June 1, 1846, Le Verrier presented to the French Academy his second memoir on the theory of *Uranus*, in which he concluded that the unexplained irregularities in the motion of *Uranus* were due to the action of an undiscovered planet exterior to *Uranus*. He investigated the elements of the orbit of such a planet, and, assuming its mean distance to be double that of *Uranus*, and its orbit to be in the plane of the ecliptic, he found that the most probable value of the true longitude of the disturbing body for the beginning of 1847 was about 325°, but he did not give the elements of the orbit or the mass of the planet.

The place thus assigned by Le Verrier to the disturbing planet differed by only 1° from that given by Adams in the paper which he had left at the Greenwich Observatory seven months earlier. Le Verrier's third memoir, in which he gave the elements of the orbit, was communicated to the French Academy on August 31, 1846.

Professor Challis commenced the search for the planet with the Northumberland telescope of the Cambridge Observatory on July 29, 1846, three weeks before the planet was in opposition, and the observations were continued for two months. His plan was to examine a zodiacal zone having its centre in the ecliptic at 325° of longitude, and extending 15° of longitude in each direction from the central point, and from 5° north latitude to 5° south latitude. He proposed to make two sweeps over each portion of the zone, so that, when the observations were compared, a planet could be at once detected by its motion in the interval. For the first few nights the telescope was directed to the part of the zone in the immediate neighbourhood of the place indicated by theory. Unfortunately the observations were not immediately compared with each other, or Professor Challis would have discovered, what he found afterwards to be the case, that he had actually observed the planet on August 4 and August 12, the third and fourth nights of observation. The star-map of the Berlin Academy for Hora xxi. of right ascension had lately been published, but the English astronomers were not aware of its existence. By the help of this map the search would have been extremely easy and rapid, as the observations could have been compared with the map as fast as they were made.

On September 3, 1846, Adams communicated to the Astronomer Royal a new solution of the problem, supposing the mean distance of the planet as originally assumed to be diminished by about the $\frac{1}{30}$th part. The result of this change was to produce a better agreement between the theory and the later observations, and to give a smaller and therefore a more probable value of the eccentricity. Meanwhile, on August 3, 1846, Le Verrier had presented to the French Academy his second paper upon the place of the disturbing planet, which, however, did not reach this country till the third or fourth week in September. In this elaborate paper Le Verrier obtained elements of the orbit of the disturbing planet, which are very similar to those obtained in Adams's second solution. Le Verrier communicated his principal conclusions to Dr. Galle, of the Berlin Observatory, in a letter received by him on September 23, 1846, and, by comparing his observations with the Berlin star-map, Dr. Galle found the planet on the same evening.

Adams's researches, therefore, preceded Le Verrier's by a considerable interval; and, in spite of the delay in commencing the search, it had been carried on at Cambridge for nearly two months before the planet was found at Berlin. Adams's investigation may be regarded as having been completed on October 21, 1845, when he left his paper at the Royal Observatory. This was three weeks before Le Verrier's memoir, showing that the irregularities could not be attributed to the known planets, was presented to the French Academy, and more than seven months before the date of presentation of his second memoir. As we know, Adams had resolved to undertake the work in 1841, and his first solution was effected, as soon as he had leisure, in 1843. We may presume that Le Verrier did not attempt the actual solution until after the completion of his memoir of November 10, 1845.

The discovery of the actual planet by Dr. Galle, in consequence of Le Verrier's prediction, was received with the most unbounded enthusiasm by astronomers of all countries, and the planet was at once called *Le Verrier's Planet.* Adams's work was only known to Airy, Challis, and a few other persons, chiefly private friends. The first public mention of his name occurred in a letter written by Sir J. Herschel on October 1, which appeared under the heading "Le Verrier's Planet" in the *Athenæum* for October 3, 1846. He refers to the address he had delivered on September 10, on the occasion of resigning the Presidential Chair of the British Association at Southampton, in which, after referring to the astronomical events of the year, among which was included the discovery of a new planet, he added the words—"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 that of ocular demonstration."

To justify the confidence which these words express, Herschel first describes a conversation with Bessel in 1842, in which the latter had said that it was highly probable that the deviations of *Uranus* might be due to an unknown planet (being systematic, and such as an exterior planet would produce), and then proceeds :—

"The remarkable calculations of M. Le Verrier, which have pointed out, as now appears, nearly the true situation of the new planet by resolving the inverse problem of the perturbations—if uncorroborated by repetition of the numerical calculations by another hand, or by independent investigation from another quarter—would hardly justify so strong an assurance as that conveyed by my expressions above alluded to. But it was known to me at that time (I will take the liberty to cite the Astronomer Royal as my authority) that a similar investigation had been independently entered into, and a conclusion as to the situation of the new planet very nearly coincident with M. Le Verrier's arrived at (in entire ignorance of his conclusions) by a young Cambridge mathematician, Mr. Adams, who will, I hope, pardon this mention of his name (the matter being one of great historical moment), and who will, doubtless in his own good time and manner, place his calculations before the public."

This passage seems to have passed almost unnoticed, however, in the outburst of enthusiasm with which Le Verrier's discovery was hailed, and it was not till October 17, when a letter from Challis appeared in the *Athenæum*, giving an account of the circumstances, that attention was directed to Adams's calculations. It was then for the first time known that his conclusions had been in the hands of the Astronomer Royal and Challis since 1845, and that the latter had actually been engaged in searching for the planet. There was naturally a disinclination to give full credit to facts thus suddenly brought to light at such a time. It was startling to realise that the Astronomer Royal had had in his possession the data which would have enabled the planet to have been discovered nearly a year before. On the other hand, it seemed extraordinary that a competent mathematician, who had determined the orbit of the disturbing planet, should have been content to refrain for so long from making public his results. No time was now lost in bringing the evidence before the world. On November 13, 1846, Airy communicated to the Society an "Account of some Circumstances historically connected with the Discovery of the Planet exterior to *Uranus*"; and Challis also described the observations which he had undertaken in search of the planet. At the same meeting Adams communicated a memoir containing an account of his mathematical investigations in connection with the determination of the mass, orbit, and position of the new planet, by which he had obtained the elements communicated to the Astronomer Royal on October 21, 1845, and September 3, 1846. All of these papers are published in vol. xvi. of the *Memoirs*; but as it was felt that the immediate publication and circulation of Adams's memoir was a matter of national interest, it was at once printed separately by Lieut. Stratford, superintendent of the *Nautical Almanac* Office, as a special appendix to the *Nautical Almanac* for 1851, and widely circulated at the beginning of 1847. This appendix was also circulated as a supplement to No. 593 (1847 March 2) of the *Astronomische Nachrichten*. It is not necessary here to discuss the question whether blame can fairly be laid to the charge of Airy or any of the other persons concerned. There is, fortunately, no dispute whatever about the facts, and in a letter to Sheepshanks, which is in the possession of the Society, Adams expressed himself as quite satisfied with Airy's statement of the case. It is perfectly true that Adams, as was said at the time, conducted himself more like a "bashful boy" than an investigator who had made a magnificent discovery. It is also true that he did not reply to an inquiry of Airy's, thinking it trivial. On the other hand, we are met by Airy's apparent indifference to Adams's results in comparison with the enthusiasm with which he greeted Le Verrier's results and prediction, although the honour of the actual discovery might have belonged to this country if he had shown more interest in the researches that had been carried out in his own university.

The French astronomers, especially Arago, were at first very unwilling to admit that Adams had any right whatever in connection with the planet, either as an independent discoverer or otherwise. Strange as it may seem, this view was also taken by some English astronomers, who, giving full credit to Le Verrier, denied all merit to Adams. It was contended that the fact that Adams's results had not been announced publicly, but had been merely communicated to the Astronomer Royal and a few others, deprived him of all claims in relation to the discovery. As has been stated, Adams himself, immediately after the finding of the planet, wrote out his investigations and submitted them to the Society; but neither then nor at any subsequent time did he take any part in the discussion himself, nor did he ever write or speak an unfriendly word with respect to anyone concerned in it. In France a very fair view of the matter was taken by Biot, and in England the speedy recognition of the merits of Adams's researches was mainly due to the warm and generous advocacy of two Cambridge men—Sedgwick and Sheepshanks, the latter of whom was at that time Secretary of the Society and Editor of the *Monthly Notices*.*

* In an interesting letter to Schumacher, now in the possession of the Society, Sheepshanks wrote as follows, under date April 7, 1847:—"You will be surprised when I tell you that the *strongest* opponents to Mr. Adams's claims to consideration are to be found in England, of course with the exception of France. All acknowledge M. LeVerrier's merits, and all admit his *undoubted* claim to *independent* discovery. All are agreed, too, that in *making public his results and investigations* in the masterly and confident way he did, he deserves the highest praise. As to *national* feeling (which, by the way, is too often national injustice) there is absolutely none whatever, so far as I know, or

The Royal Society at once awarded their highest honour, the Copley Medal, to Le Verrier (1846), and it was not till two years afterwards that it was awarded to Adams. Our Society was saved from expressing a similar preference by the bye-law requiring that the award of the medal should be confirmed by a majority of three-quarters of the Council. A sufficient minority were of opinion that "an award to M. Le Verrier, unaccompanied by another to Mr. Adams, would be drawing a greater distinction between the two than fairly represents the proper inference from facts, and would be an injustice to the latter." * It had been proposed that the General Meeting should be recommended to suspend the existing bye-law, so that more than one medal might be given, but the proposal was not carried on the Council. No medal of the Society has been awarded in connection with the discovery of *Neptune*, either at that time or subsequently.

Looking back now upon Adams's achievement, which, as has been truly said, belongs at once to the science and the romance of astronomy, there are several points that stand out as very remarkable: his extreme youth when he attacked, unaided, so difficult a problem, and steadily carried it through to success; his faith in the Newtonian law and in the results of his own mathematics; and his extreme modesty. As soon as he took his degree in 1843 he devoted his whole leisure, in term time at Cambridge, and in vacations in Cornwall, to the new planet's orbit, without assistance or encouragement from anyone. How quietly and unassumingly he pursued his investigations is shown by the fact that at the time of the finding of the planet his name was only known to Airy, Challis, Herschel,

among astronomers. In England at present the current runs the other way, and though I very much prefer this failing of the two, yet it is provoking too. I assure you that it was with difficulty that one could get a hearing, while pointing out the fact that Mr. Adams had deduced the elements and place of the planet in October 1845. I have been told repeatedly by those who should have known better that this was nothing at all, simply because the over-modest man communicated his results to Airy and Challis, that the planet might be looked for, instead of bringing his investigation before the world as he ought to have done. Surely it is a greater honour to science that two men should independently have come to the same conclusion from the same data than that one should have hit on it, as it were, accidentally. Thanks to Struve and Biot, &c. our anti-Adamites are calmer, and as there never was any opposition to Le Verrier, we are quite satisfied at present, and so I hope are the two discoverers. I think there is a hope that Mr. Adams will continue his astronomical researches. In any other country there could be no doubt of it, but in England there is no *carrière* for men of science. The Law or the Church seizes on all talent which is not independently rich or careless about wealth." The principal contemporary publications relating to the new planet are to be found in vol. xvi. of the *Memoirs*, in the *Comptes Rendus*, in the *Athenæum*, in the *Astronomische Nachrichten*, and in vol. vii. (1847) of the *North British Review*, which contains an article by Brewster. Challis's report of December 12, 1846, to the Observatory Syndicate at Cambridge, which contains an account of his own proceedings relating to the new planet, was published in the *Monthly Notices* for 1883 (vol. xliii. pp. 165–172).

* *Monthly Notices*, vol. vii. p. 216.

Earnshaw, and a few intimate university friends of his own standing. The implicit reliance that he placed in the sufficiency of the Newtonian law to explain all the phenomena of the heavens is also noteworthy, as it was then considered much more likely than it is now that the true law might not be exactly that of the inverse square, or might be subject to other modifications.* He was perfectly convinced of the reality of the planet and of the approximate accuracy of the place he had assigned to it; and in the paper which he placed in the hands of Challis in September, 1845, he used the words "the new planet." It is to be regretted that he should have refrained from publishing his results at the time (which he had been advised to do by some of his Cambridge friends), but he can scarcely be blamed for believing that the course he had taken would lead to a search being made for the planet at Greenwich or elsewhere.

A French translation of the memoir presented to the Society in 1846 was published in Liouville's *Journal de Mathématiques Pures et Appliquées* for 1875. The Editor, M. Résal, stated

* The words in the text were in type before the writer saw a private letter from Adams to Professor James Thomson, dated November 26, 1846, from which the following very interesting sentences relating to this subject are extracted:—"On considering the subject it appeared to me that by far the most probable hypothesis that could be formed to account for these irregularities was that of the existence of an exterior undiscovered planet whose action on *Uranus* produced the disturbances in question. None of the other hypotheses that had been thrown out seemed to possess the slightest claims to attention, as they were all improbable in themselves, and incapable of being tested by any exact calculation. Some had even supposed that, at the great distance of *Uranus* from the Sun, the law of attraction became different from that of the inverse square of the distance, but the law of gravitation was too firmly established for this to be admitted till every other hypothesis had failed to account for the observed irregularities; and I felt convinced that in this, as in all previous instances of the kind, the discrepancies which had for a time thrown doubts on the truth of the law, would eventually afford it the most striking confirmation. In contrast with all these vague hypotheses, the supposition that the irregularities were caused by the action of an unknown planet appeared to be thoroughly in accordance with the present state of our knowledge, could be tested by calculation, and would probably lead to important practical results—viz. the approximate determination of the position of the disturbing body." After quoting the memorandum of July 3, 1841, he proceeds:—"Accordingly, in 1843, I commenced my calculations, and in the course of that year I arrived at a first solution of the problem, which, though incomplete in itself, fully convinced me that the hypothesis which I had formed was quite adequate to account for the observed irregularities, and that the place of the disturbing body might be very approximately determined by a more extended investigation. Having received from the Astronomer Royal, in February 1844, the whole of the Greenwich observations of *Uranus*, I accordingly attacked the problem afresh, and in a much more complete manner than before, and, after obtaining several solutions, differing little from each other, by gradually taking into account more and more terms in the series expressing the perturbations, I communicated my final results to Professor Challis in September 1845, and the same, slightly corrected, to the Astronomer Royal in the following month. The near agreement of the several solutions which I had obtained gave me great confidence in my results, which included a determination of the mass, position, and elements of the orbit of the supposed planet."

that he had been led to undertake this republication by the pressing solicitations of several eminent mathematicians. In introducing the memoir he writes:—"Le problème fut résolu simultanément, en Angleterre par M. Adams, et en France par M. Leverrier, qui, ainsi que le reconnaît M. Adams, a publié le premier les résultats de ses recherches. . . . Il est impossible de rencontrer, dans l'histoire des sciences, une découverte qui fasse plus d'honneur au génie humain. Les lois de Newton recevaient ainsi la plus éclatante des confirmations, et l'Astronomie, désormais indiscutable dans ses principes, était arrivée à l'état de science parfaite. Le Mémoire de M. Adams a valu, à juste titre, à son auteur la plus glorieuse célébrité: il est digne, en effet, de figurer à côté des plus beaux mémoires de Laplace et Lagrange." This republication of the memoir, after an interval of thirty years, in a purely mathematical journal, derives additional interest from the fact that the author himself has added a few notes at the end, some of which relate to the objections made by Professor Benjamin Peirce to the legitimacy of the methods pursued by both Adams and Le Verrier. In Peirce's paper, which was published in 1847, he contended that the period of *Neptune* differed so considerably from that of the hypothetical planets that the modes of procedure adopted were unreliable, so that the finding of the planet was partly due to a happy accident. In reply to this, Adams points out that the objection would be valid if the object in view had been to represent the perturbations of *Uranus* during two or three synodic periods, but that the case is different when, as in this instance, it is only required to represent the perturbations for a fraction of a synodic period.

The honours so freely and deservedly bestowed upon Le Verrier in France and other countries form a striking contrast to the general want of appreciation with which Adams's work was at first received. But there were conspicuous exceptions. In 1847, on the occasion of the Queen's visit to Cambridge, the honour of knighthood was offered to Adams, but this offer he felt obliged to decline. The members of St. John's College, also, were not slow in showing their sense of the honour he had conferred on his college and the University, for in a very short time a fund, producing about 80*l.* per annum, was raised for establishing a prize to be connected with his name. This fund was offered to the University, and accepted on April 7, 1848. The Adams Prize, which is biennial, is awarded for the best essay on some subject of pure mathematics, astronomy, or other branch of natural philosophy. It was awarded in 1857 to Clerk Maxwell for his well-known essay on the stability of *Saturn's* rings.

Before leaving the subject of *Neptune*, it should be stated that Adams always expressed the warmest appreciation of Le Verrier's work. It was a great pleasure to him when they met at Oxford in 1847. In the same year Le Verrier visited Adams at

Cambridge. The honorary degree of LL.D. was conferred upon Le Verrier in 1874 by the University of Cambridge, and it cannot be doubted that this was owing to the action of Adams.

In 1847 he communicated to the Society a paper on an important error in Bouvard's tables of *Saturn*. Having been engaged upon a comparison of the theory of *Saturn* with the Greenwich observations, he was struck with the magnitude of the tabular errors in heliocentric latitude, which could not be attributed to imperfections in the theory. He found that the error was one of computation, two terms of different arguments having been, in effect, united into one.

In 1848 he was occupied with the determination of the constants in Gauss's theory of terrestrial magnetism. This investigation he afterwards resumed, and the calculations connected with it, with which he was occupied in the later years of his life, were left unfinished at the time of death. When failing health prevented him from any longer giving his personal attention to the work, he placed the manuscripts in the hands of his brother, Professor W. G. Adams, for completion.

In 1851 he was elected President of our Society, and held the office for the usual term of two years, during which he delivered the addresses on the presentation of the medal to Peters and to Hind. In 1852 he communicated to the Society new tables of the Moon's parallax, to be substituted for those of Burckhardt. Henderson had compared the parallaxes deduced from observation with those derived by calculation from the tables both of Damoiseau and of Burckhardt, finding a difference of no less than $1''\cdot3$, according as one set of tables or the other was employed. The parallax in Damoiseau's tables is given at once in the form in which it is furnished by theory, but that in Burckhardt's tables is adapted to his peculiar form of the arguments, and requires transformation in order to be compared with the former. When this was done, Adams found that several of the minor equations of parallax deduced from Burckhardt differed completely from their theoretical values as given by Damoiseau. He discovered that these errors were due to Burckhardt's transformations of Laplace's formula, and he succeeded in tracing them to their sources. He also examined carefully the theories of Damoiseau, Plana, and Pontécoulant, with respect to the same subject, and supplied a number of defects and omissions. Burckhardt's value of the parallax having been employed in the *Nautical Almanac*, Adams gave, in addition to the new tables, a table of corrections to be applied to the values in the *Nautical Almanac* for every day of the year from 1840 to 1855 inclusive. This contribution to astronomy is very characteristic of its author. It contains the results of an immense amount of intricate and elaborate mathematical investigation, carried out with great skill and accuracy in all its details, both analytical and numerical, but no part of the work itself is given. The method of procedure is briefly sketched, and the final conclusions are stated in the fewest words and simplest manner possible. No one unacquainted with the subject would imagine how much difficult and laborious research was represented by these few pages of results. The tables were printed as a supplement to the *Nautical Almanac* for 1856.

As Adams had not taken holy orders, his Fellowship at St. John's College came to an end in 1852, but he continued to reside in the college until February 1853, when he was elected to a Fellowship at Pembroke College, which he retained till his death. In the autumn of 1858 he was appointed Professor of Mathematics in the University of St. Andrews, and shortly afterwards, in the same year, he was elected Lowndean Professor of Astronomy and Geometry at Cambridge, in succession to Peacock. He continued his lectures at St. Andrews, however, until the end of the session in May 1859. In 1861 he succeeded Challis as Director of the Cambridge Observatory.

In 1853 Adams communicated to the Royal Society his celebrated memoir on the secular acceleration of the Moon's mean motion. Halley was the first to detect this acceleration by comparing the Babylonian observations of eclipses with those of Albategnius and of modern times, and Newton referred to his discovery in the second edition of the "Principia." The first numerical determination of the value of the acceleration is due to Dunthorne, who found it to be about 10″ in a century. Tobias Mayer obtained the value 6″·7, which he afterwards increased to 9″. Lalande's value was nearly 10″. The discrepancies were due to the eclipses selected, the results derived from the different eclipses being inconsistent with one another. The history of the theoretical investigations relating to the acceleration may be summed up as follows:—In 1762 the French Academy proposed as the subject of their prize the influence of a resisting medium upon the movements of the planets. The prize was won by Bossut, who showed that the principal effect of such a medium would be an acceleration in their motions, which would be much more sensible in the case of the Moon than in that of the planets. In 1770 the question proposed was whether the theory of gravitation could alone explain the acceleration. Euler obtained the prize, but he was unable to discover any term of a secular character, and concluded that the force of gravitation would not account for this inequality. The subject was proposed again in 1772, Euler and Lagrange sharing the prize between them. The former came to the same conclusion as before, attributing the acceleration to a resisting medium; the latter did not carry the application of his formulæ so far as to complete the investigation. The prize was again offered for the same subject in 1774, the competitors being required to examine whether the fact that the Moon appeared to have a secular acceleration, while there was no sensible effect of this kind in the case of the Earth, could be explained by the theory of gravitation alone, taking into account not only the action of the Sun and the Earth upon the Moon, but also the action of the other planets, and even the non-spherical figure of the Moon and Earth. The prize was awarded to Lagrange, who, after showing that none of the causes proposed would suffice to explain the secular variation of the Moon, concluded that, *if this variation is real*, it must be produced in some other manner, such as by a resisting medium. But as the existence of such a medium was not confirmed by the motions of the other planets, and was even contradicted by the motion of *Saturn*, which seemed to show a retardation, Lagrange expressed doubts with respect to the reality of the lunar acceleration, resting as it does on observations of eclipses in very remote ages. The next investigation relating to the subject is by Laplace, who showed that the acceleration could be accounted for by supposing that the transmission of the force of gravitation was not instantaneous, but that the rate of propagation was about eight million times that of light. Some years later, however, Laplace unexpectedly discovered the true gravitational cause of the acceleration. While working at the theory of *Jupiter's* satellites, he remarked that the secular variation of the eccentricity of *Jupiter's* orbit produced secular terms in their mean motions. Applying this result to the Moon, he found that the secular variation of the eccentricity of the Earth's orbit produced on the Moon's motion a secular term which agreed very well with the value assigned to it by observation; he found also that the same cause produced secular terms in the motion of the Moon's node and perigee. This result was communicated to the French Academy in November, 1787, and the memoir containing the details of the calculation was published in the following year. The Stockholm Academy of Sciences had already proposed in 1787 the secular variations of the Moon, *Jupiter*, and *Saturn* as the prize subject for 1791, but no essays being sent in, the prize was adjudged to Laplace for his memoir published in 1788.

Laplace's discovery was received with great applause, and the satisfactory explanation of so intractable a variation by means of the Newtonian principles, after so many years of fruitless attempt, was an important event in the history of astronomy. The honour of the discovery might very easily have belonged to Lagrange, for the formulæ given by him in a memoir published in 1783 would at once, if applied to the Moon, have produced Laplace's result. But Lagrange had found that, in the case of *Jupiter* and *Saturn*, these formulæ gave nearly insensible results, so that he did not extend the investigation to the other planets, or to the Moon, although the latter application would only have involved easy numerical substitutions, much simpler than those required for the principal planets.

In 1820, at the instigation of Laplace, the lunar theory was taken in hand afresh by Plana and Damoiseau, the approximations being carried to an immense extent, especially by the former. Damoiseau calculated the acceleration numerically, and found it to be $10''\cdot72$. Plana's process was algebraical, and he carried the series, of which Laplace had only calculated the first term, as far as quantities of the seventh order. By reducing to numbers the twenty-eight terms of this series he found $10''\cdot58$ as the complete value of the acceleration, for which the first term, which alone had been included by Laplace, gave $10''\cdot18$. Subsequently Hansen gave the values $11''\cdot93$ (1842), $11''\cdot47$ (1847), and in his tables published in 1857 the value $12''\cdot18$ was used. It does not seem clear, however, to what extent these values are to be regarded as theoretical determinations.*

Thus when Adams published his memoir in the *Phil. Trans.* for 1853 no suspicion had arisen that Laplace's discovery was not absolutely complete, and that the question of the acceleration had not been finally set at rest. In his short paper of only ten pages Adams shows that the condition of variability of the solar eccentricity introduces into the solution of the differential equations a system of additional terms which affect the value of the acceleration. He found that the second term of the series on which the acceleration depends was really equal to $\frac{3771}{64}m^4$, instead of $\frac{2187}{128}m^4$, as found by Plana. The former is more than three times as great as the latter, and the amount of the acceleration is greatly decreased by the correction of the error discovered by Adams. For some time the paper seems to have attracted no attention, but it then became the object of a long and bitter controversy. Plana, who was the person most concerned in the matter, published, in 1856, a memoir in which he admitted that his own theory was wrong upon this point, and he deduced Adams's result from his own equations. But shortly afterwards he retracted his admission, and, rejecting some of the new terms which he had obtained, arrived at a result which differed both from his original value and from Adams's. The question was in this state when Delaunay, by employing his own special method of treating the Lunar Theory and extending the investigation only to the fourth order, had the satisfaction of obtaining Adams's coefficient $\frac{3771}{64}$, a result which he brought before the French Academy in January, 1859. This caused Adams to communicate to the Academy, in the same month, the values which

* Hansen stated in 1866 (*Monthly Notices*, xxvi. p. 187) that he had never disputed the correctness of Adams's theory, but that he was not satisfied with "the development of the divisors into series." If this refers to the expansion of the acceleration-coefficient in powers of *m*, it should be noticed that Adams stated (vol. xxi. p. 15) that he had calculated the value of the acceleration by a method that did not require any expansion in powers of *m*, and found the result to be $5''\cdot70$. Hansen says that Adams's theory appeared too late to permit of his using it; "and it was well that it so happened, for I had already found by my own theory a coefficient which represents ancient eclipses as well as could be desired." It seems impossible to resist the conclusion that in this theory the new terms must have been omitted by Hansen, as they had been by Plana and Damoiseau.

he had obtained some time before for the terms in m^5, m^6, and m^7; and he pointed out at the same time that, when these terms were included, the value of the acceleration was reduced to $5''\cdot78$, and, inferring that the remainder of the series would be nearly equal to $0''\cdot08$, he concluded that the total value of the acceleration was about $5''\cdot70$. Soon afterwards Delaunay carried his approximation as far as terms of the eighth order, and by reducing the forty-two terms in the analytical expression to numbers he obtained the value $6''\cdot11$. Delaunay's result, which was communicated to the Academy in April, 1859, confirmed the accuracy of Adams's values of the terms in m^5, m^6, and m^7, and also those of $m^2 e^2$, and $m^2 \gamma^2$, which Adams had communicated to him privately. A month after the publication of this paper Pontécoulant made a vigorous attack on the new terms introduced by Adams, which he said had been rightly ignored by Laplace, Damoiseau, Plana, and himself, as they had no real existence. He also objected that if the result of Adams were admitted, it would "call in question what was regarded as settled, and would throw doubt on the merit of one of the most beautiful discoveries of the illustrious author of the 'Mécanique Céleste.'" Shortly afterwards he communicated a paper to the *Monthly Notices* on "the new terms introduced by Mr. Adams into the expression for the coefficient of the secular equation of the Moon," in which he characterised the mathematical process by which these terms had been obtained as 'une véritable supercherie analytique.'" It would appear that Le Verrier did not accept Adams's value, for in presenting a note by Hansen to the Academy in 1860 he states that Hansen's tables afford an irrefragable proof of the accuracy of the value $12''$ which is there attributed to the acceleration. Referring then to the fact that according to Delaunay the secular acceleration should be reduced to $6''$, he proceeds: "Pour un astronome, la première condition est que ses théories satisfassent aux observations. Or la théorie de M. Hansen les représente toutes, et l'on prouve à M. Delaunay qu'avec ses formules on ne saurait y parvenir. Nous conservons donc des doutes et plus que des doutes sur les formules de M. Delaunay. Très certainement la vérité est du côté de M. Hansen."

In the *Monthly Notices* for April, 1860, Adams replied to his objectors, pointing out simply and clearly the errors into which they had fallen. He mentions that before publishing his memoir of 1853 he had obtained his result by two different methods, and that he had subsequently confirmed and extended it by a third. In a series of letters addressed to Lubbock in June, 1860, Plana began by objecting to Adams's value of the term in m^4, but he soon admitted its accuracy. Lubbock also was led to apply his own formulæ to the question, and he too arrived at Adams's result. Another calculation was made by Cayley, who, by an entirely different method, also obtained the same result. As Pontécoulant still continued his reiterated attacks upon the accuracy of the new terms, Cayley's calculation was printed *in extenso* in the *Monthly Notices*, where it occupies fifty-six pages. Delaunay had also made another calculation, in which, by following the method indicated by Poisson in 1833, he was led to the same value. The coefficient of m^4 had also been verified in 1861 by Donkin, who used Delaunay's method of the variation of the elements. Thus Adams's value of the term in m^4 was obtained by himself in three ways, by Delaunay in two ways, and by Lubbock, Plana, Donkin, and Cayley. Pontécoulant continued his attacks with no abatement of violence in the *Comptes Rendus*. Ultimately he abandoned Plana's value and obtained one of his own, which differed both from Adams's and Plana's.

The whole controversy forms a very extraordinary episode in the history of physical astronomy; the indifference with which the memoir of 1853 was at first received, in spite of the interest and importance of the subject, being followed by the violent controversy which resulted in so many independent investigations by which Adams's result was confirmed. It is not known why Laplace did not carry the calculation beyond the term in m^2; but it may be supposed that he regarded the subsequent terms as not likely to modify the value of the first term to any considerable extent. Damoiseau's and Plana's theories passed under the review of Laplace, and may be regarded as having received his sanction. Thus Adams's result not only unsettled a matter which after years of difficulty and struggling had apparently received its full and final explanation, but it detracted from the completeness of a discovery which had long been regarded as one of the greatest triumphs of Laplace's genius. Although the point in dispute was one entirely relating to the mathematical solution of differential equations, in which observation in no way entered, there can be no doubt that the fact that Plana's result agreed with observation, while Adams's did not, created in the minds of many a presumption against the accuracy of the latter. This view was certainly taken by Le Verrier in the passage quoted above, and it seems also to have influenced Hansen. It is curious that it should have been possible for so much difference of opinion to exist upon a matter relating only to pure mathematics, and with which all the combatants were fully qualified to deal, as is clearly shown by their previous publications. The whole controversy illustrates the peculiar nature of the lunar problem, and of the analysis by means of which the results are reached. The complete solution being unattainable by any of the methods which have as yet been applied, the skill of the mathematician is shown in selecting from a vast number of terms those which will produce a sensible influence in that particular portion of the complete solution which is under consideration.

A most admirable account of the whole discussion was given by Delaunay in the Additions to the *Conn. des Temps* for 1864, in which the place occupied by Adams's memoir in the history of gravitational astronomy is so well summed up that it may be permissible to quote the passage in its entirety :—

" L'apparition du mémoire de M. Adams a été un véritable événement : c'était toute une révolution qu'il opérait dans cette partie de l'astronomie théorique. Aussi le résultat qu'il renfermait fut-il vivement attaqué ; on ne voulait pas l'admettre, et on ne manquait pas de raisons à donner pour cela. Il est, disait-on, en désaccord complet avec les observations ; il ne tend à rien moins qu'à enlever à Laplace l'honneur d'une de ses plus belles découvertes ; il est basé d'ailleurs sur une analyse fautive et erronée. Mais parmi toutes ces raisons il n'y en avait pas une bonne ; et la persistance avec laquelle elles ont été présentées et soutenues a produit un effet diamétralement opposé à celui qu'on en attendait : les confirmations de ce résultat tant contesté se sont accumulées à un tel point, qu'il serait difficile de trouver dans les sciences une vérité mieux établie que ne l'est maintenant celle que M. Adams a mise en avant le premier dans son mémoire de 1853. Toutes les objections qui avaient été formulées sont tombées d'elles-mêmes. L'analyse déclarée *fautive et erronée* a été reconnue exacte. L'accord ou le désaccord du résultat théorique avec les indications fournies par les observations n'a plus été regardé comme un moyen de contrôler l'exactitude de ce résultat théorique. Si le désaccord annoncé existe bien réellement, on en conclut simplement que la cause assignée par Laplace à l'accélération séculaire du moyen mouvement de la Lune ne produit pas seule la totalité du phénomène et on ne trouve dans ce désaccord rien qui soit de nature à amoindrir la découverte de l'illustre géomètre français."

These sentences derive additional interest from the fact that they were written by one who was himself the author of the most comprehensive and elegant method by which the lunar problem has ever been treated, and who was the first to recognise the accuracy of Adams's result. In 1866 the Gold Medal of the Society was awarded to Adams for his contributions to the development of the Lunar Theory, the address on the occasion being delivered by De la Rue. In the preparation of this very able address, which contains an excellent history of the problem of the secular acceleration, De la Rue had the invaluable assistance of Delaunay. To complete the account of Adams's connection with the secular acceleration, it should be stated that in 1880, thirty-seven years after Adams's memoir, Airy communicated to the Society a paper on the theoretical value of the acceleration (*Monthly Notices*, vol. xl. p. 368), in which he obtained the value of $10''\cdot1477$. At the next meeting of the Society Adams pointed out that in Airy's method of treatment certain terms were omitted, the effect being that the expression for the coefficient was reduced to its first term, so that the result necessarily agreed with Laplace's. Subsequently, taking into account these terms, Airy obtained the value $5''\cdot4773$. Adams took the occasion of the matter being thus raised again to communicate to the Society the investigation of the acceleration which he had been in the habit of giving in his lectures.

In the *Monthly Notices* for April 1867 he published an account of the results he had obtained with respect to the orbit of the November meteors. Professor H. A. Newton had concluded that these meteors belong to a system of small bodies describing an elliptic orbit about the Sun, and extending in the form of a stream along an arc of that orbit, which is of such a length that the whole stream occupies about one-tenth or one-fifteenth of the periodic time in passing any particular point. He showed that the periodic time of this group must be either 180·0 days, 185·4 days, 354·6 days, 376·6 days, or 33·25 years, and that the node of the orbit must have a mean motion of 52″·4 with respect to the fixed stars. Soon after the remarkable display of the November meteors in 1866 Adams undertook the examination of this question. From the position of the radiant-point observed by himself he calculated the elements of the orbit of the meteors, starting with the supposition that the periodic time was 354·6 days, the value which Professor Newton considered to be the most probable one. The orbit which corresponds to this period is very nearly circular, and he found that the action of *Venus* would produce an annual increase of about 5″ in the longitude of the node, that of *Jupiter* about 6″, and that of the Earth about 10″. Thus the three planets, which alone could sensibly affect the motion of the node would produce about 12′ in 33·25 years. The observed motion of the node is about 29′ in 33·25 years, which is therefore inconsistent with a periodic time of the meteors about the Sun of 354·6 days. If the periodic time were supposed to be about 377 days, the calculated motion of the node would differ very little from that in the case already considered, while if the periodic time were a little greater or a little less than half a year, the calculated motion of the node would be still smaller. Hence, of the five possible periods indicated by Professor Newton, four were incompatible with the observed motion of the node, and it only remained to examine whether the fifth period of 33·25 years would give a motion in accordance with observation. In order to determine the secular motion of the node in this orbit the method given by Gauss in his memoir "Determinatio Attractionis &c." was employed. By dividing the orbit of the meteors into a number of small portions, and summing up the changes corresponding to these portions, the total secular changes of the elements produced in a complete period of the meteors was determined, the result being that during a period of 33·25 years, the longitude of the node is increased by 20′ by the action of *Jupiter*, nearly 7′ by the action of *Saturn*, and about 1′ by that of *Uranus*. The other planets were found to produce scarcely any sensible effects, so that the entire calculated increase of the longitude of the node is about 28′, according very closely with the observed amount of

39′, and leaving no doubt as to the correctness of the period of 23·25 years. In order to obtain a sufficient degree of approximation it was requisite to break up the orbit of the meteors into a considerable number of portions, for each of which the attractions of the elliptic rings corresponding to the several disturbing planets had to be determined. These calculations were therefore of necessity very long, although a modification of Gauss's formula was devised which greatly facilitated its application to the actual problem. Subsequently certain parts of the orbit of the meteors were subdivided into still smaller portions, with the view of obtaining a closer approximation. Unfortunately the mathematical investigations which Adams carried out on this subject have not been published. They exist among his papers, together with a very great amount of numerical work connected with the calculations, and seem to be in a fairly complete state. It is hoped, therefore, that there will not be much difficulty in preparing them for press.

In 1877 Mr. G. W. Hill published a memoir on the motion of the Moon's perigee, in which he calculated that part of c which depends only upon m to fifteen places of decimals by a new method in which the expansion in powers of m was avoided, the numerical value of c being obtained by means of an infinite determinant. The publication of this memoir led Adams to communicate to the Society in November 1877 a brief notice of his own work in the same field, in which, after congratulating Mr. Hill upon his investigation, he mentions that his own researches had followed in some respects a parallel course. In particular he remarks that the differential equation for z, the Moon's coordinate perpendicular to the ecliptic, presents itself naturally in the same form as that to which Mr. Hill had so skilfully reduced his differential equations. In solving this equation, which was therefore of Mr. Hill's standard form, he fell upon the same infinite determinant as that considered by Mr. Hill, and developed it in a similar manner in a series of powers and products of small quantities, the coefficient of each such term being given in a finite form. This development was continued as far as the terms of the fourth order in 1868; and in 1875, when he resumed the subject, the approximation was extended to terms of the twelfth order, which is the same degree of accuracy as that to which Mr. Hill had carried his researches. On making the reductions requisite to render the two results comparable, he found that they were in agreement with the exception of one of the terms of the twelfth order, and that this discrepancy was due to a simple error of transcription. He states that the calculations by which he had found the value of the determinant were very different in detail from those required by Mr. Hill's method, but that he had not had time to copy them out from his old papers and put them in order. In this communication, therefore, he confined himself to making known the result which he had obtained for the motion of the Moon's node. After giving an outline of the method pursued, including the equation derived from the infinite determinant, he arrives at the formulæ by means of which the value of g, as dependent only upon m, was obtained to fifteen places of decimals.

It is difficult to appreciate too highly the mathematical ability shown by Adams and Hill in devising methods which did not require expansion in powers of m, and which yielded with such wonderful accuracy these values of g and c. Apart, however, from the mathematical and astronomical interest of the researches themselves, the coincidence of methods and ideas is very striking. But for the publication of Hill's memoir it is probable that no account of these results of Adams's would have been published in his lifetime, and it is not unlikely that he would never have put into writing his views on the mathematical treatment of the lunar problem which give additional interest to this short paper. As far back as 1853, in his memoir upon the secular acceleration, he mentioned that the new terms in the expression of the Moon's coordinates occurred to him some time before, when he was engaged in thinking over a new method of treating the lunar theory, and it is well known that the theory itself, or problems connected with it, constantly occupied his attention. In this paper of 1877 he states that he had long been convinced that the most advantageous mode of treatment is by first determining with all possible accuracy the inequalities which are independent of e, e', and γ, and then in succession finding the inequalities which are of one dimension, two dimensions, and so on with respect to these quantities. Thus, the coefficient of any inequality in the Moon's coordinates would be represented by a series arranged in powers and products of e, e', and γ; and each term in this series would involve a numerical coefficient which is a function of m alone, and which admits of calculation for any given value of m without the necessity of developing it in powers of m. This method is particularly advantageous when the results are to be compared with those of an analytical lunar theory such as Delaunay's, in which the eccentricities and the inclination are left indeterminate, since each numerical coefficient admits of a separate comparison with its analytical development in powers of m. He mentions also that, many years before, he had obtained the values of the inequalities independent of the eccentricities and inclination to a great degree of approximation, the coefficients of the longitude expressed in circular measure, and those of the reciprocal of the radius vector, or of the logarithm of the radius vector, being found to ten or eleven places of decimals.

We thus see that Adams preferred to treat the lunar theory as far as possible by means of its special problems; and this was also the method which he followed in his Cambridge lectures. Mr. R. A. Sampson, Fellow of St. John's College, Cambridge, who has been making a careful examination of Adams's manuscripts, states that he believes that the investigations he has left will suffice to form what will practically be a treatise on the lunar theory intermediate to the existing text-books and such complete theories as those of Plana and Delaunay. The manuscripts already put in order by Mr. Sampson contain the development of the infinite determinant and the other researches referred to in this paper.

The *Monthly Notices* for June, 1878, contains a short paper by Adams on a property of the analytical expression for the constant term in the reciprocal of the Moon's radius vector. Plana had found that the coefficients of e^2 and γ^2 in this term vanished when account was taken of terms involving m^2 and m^3, and Pontécoulant, who carried the development further, had found that this destruction of the terms in the coefficients still continued when the terms involving m^4 and m^5 were included. Thinking it probable that these cases in which the coefficient had been observed to vanish were merely particular cases of some more general property, Adams was led to consider the subject from a new point of view, and, so far back as 1859, he succeeded in proving that not only did these coefficients necessarily vanish identically, but that the same held good also for coefficients which were much more general, so that the coefficients of $e^2e'^2$, $e^2e'^4$, &c. $\gamma^2e'^2$, $\gamma^2e'^4$, &c. were also identically equal to zero. Further reflection on the subject led him in 1868 to obtain a simpler and more elegant proof of the property in question. He also found subsequently, in 1877, some remarkable relations connecting the coefficients of e^4, $e^2\gamma^2$, and γ^4. Adams himself remarks that the theorem "is remarkable for a degree of simplicity and generality of which the lunar theory affords very few examples." We thus see that a striking result—and one moreover which admitted of being isolated from the rest of the lunar theory—was obtained in 1859, but was not published till nearly twenty years afterwards, although in the meantime he had obtained another and more satisfactory proof. This illustrates the disinclination that Adams seems always to have felt to prepare his work for publication; a disinclination which was mainly due to his desire to obtain a still higher degree of simplification or perfection. The discovery of the additional relations in 1877 shows that his attention was at that time still occupied with the theorem of 1859.

Space does not permit of any more extended account being given of Adams's published astronomical writings, and it would be of little use to mention titles unaccompanied by some description of the contents of the papers. The shorter papers deserve much more detailed notice than might at first sight seem necessary, not only because they frequently consist wholly of results derived from laborious researches, but also because they afford glimpses of the nature and extent of the work with which he was occupied. For forty-five years his mind was constantly directed to mathematical research, relating principally to astronomy; and it is evident that what he had accomplished is very inadequately represented by what has been published. It is also noticeable that so few of his papers should have appeared quite spontaneously: it frequently happened that he was incited to give an account of something which he had done himself—probably years before—by the publication of a paper in which the same ground was partially covered by some other investigator; in other cases he was called upon to correct some misapprehension which was leading others astray.

As already stated, there can be no doubt that he constantly allowed himself to postpone the immediate publication of his researches, with the intention of effecting improvements in the processes and mode of representing the subject, or of attaining to an even more accurate result. A striking instance of this innate craving for perfection is afforded, even as early as 1845, by his calculation of the second orbit of the new planet. No able mathematician who is engaged upon a fruitful research can continually defer publication with impunity: the subject opens before him; his views expand; the earlier results, so interesting at the moment of discovery, lose their charm in comparison with the problems still unsolved and the novel vistas of thought opened out by them; and the rearrangement and rewriting of the old work—always an irksome task—become intolerable when later and still unfinished developments on the same subject are exciting the mind. In Adams's case the difficulty of satisfying himself, and reaching his own standard of completeness, also contributed to his apparent reluctance to publish investigations to which he was known to have devoted much time, and which were anxiously awaited; such, for example, as his work upon *Jupiter's* satellites. Those who knew him will remember his words when pressed, "I have still some finishing touches to put to it." It does not appear that he ever made any serious attempt to put his longer investigations in order for press, though it is known that occasionally, as his manuscripts on the different subjects increased in bulk, the feeling would come over him that it was time for him to do so. Although there is no similarity between the simple and easy style of Adams's writings and the cold severity of Gauss's, there is a certain resemblance in their mode of work. Each had the same dislike to early or incomplete publication, and "Pauca sed matura" might have been the motto of both. In beginning a new research, Adams rarely put pen to paper until he had carefully thought out the subject, and when he proceeded to write out the investigation he developed it rapidly and without interruption. His accuracy and power of mind enabled him to map out the course of the work beforehand in his head, and his mathematical instinct, combined with perfect familiarity with astronomical ideas and methods, guided him with perfect safety through the intricacies and dangers of the analytical treatment.*

* This method of working characterised him from the first, for in his Tripos Examination it was noticed that "in the problem papers, when every-

He scarcely ever destroyed anything he wrote, or performed rough calculations; and the manuscripts which he has left are written so carefully and clearly that it is difficult to believe that they are not finished work that has been copied out fairly. The sheets are generally dated, and during many years he kept a diary of the work he had done each day, which will be of the greatest use in the arrangement of his papers for publication.

His contributions to pure mathematics show the same power and excellence, and, as the subject affords greater opportunities for the display of elegance and style, they indicate even more plainly the attention he bestowed upon the form of his results, as well as upon the substance. A paper communicated to the Royal Society in 1878 may be specially noticed, in which an expression is given for the product of two Legendrian coefficients, and for the integral of the product of three. The extent of his mathematical interests is perhaps best seen by looking over the series of papers which he set in the Smith Prize Examination. These questions, which cover a wide field of mathematics, clearly indicate the bent of his mind and his favourite subjects of study: they are also noticeable for a high degree of finish, which is very uncommon in examination questions.

Like Euler and Gauss, he took very great pleasure in the numerical calculation of exact mathematical constants. We owe to him the calculation of thirty-one Bernoullian numbers, in addition to the first thirty-one which were previously known. The first fifteen are due to Euler, the next sixteen were calculated by Rothe, the whole thirty-one being given in vol. xx. of Crelle's Journal. Making use of Staudt's very curious theorem with respect to the fractional part of a Bernoullian number, Adams calculated all the numbers from B_{32} to B_{62}. The results were communicated to the British Association at the Plymouth meeting in 1877, and were also published in vol. lxxxv. of Crelle's Journal. A much fuller account of the work, which was very considerable in amount, appeared in an appendix to vol. xxii. of the "Cambridge Observations," where the process of calculation of the first, B_{32}, and of the last, B_{62}, is given in detail. Adams proved that if n be a prime number other than 2 or 3, then the numerator of the nth Bernoullian number is divisible by n. This afforded a good test of the accuracy of the work.

Having thus at his command the values of sixty-two Bernoullian numbers, he was tempted to apply them to the calculation of Euler's constant. For this purpose, not only the Bernoullian

one was writing hard, Adams spent the first hour in looking over the questions, scarcely putting pen to paper the while. After that he wrote out rapidly the problems he had already solved 'in his head.'" It may be here mentioned that in this examination he received more than double the marks of the Second Wrangler. This affords striking evidence of Adams's mental powers, for he was not a rapid writer.

numbers, but also the values of certain logarithms and sums of reciprocals were required. He accordingly calculated the values of the logarithms of 2, 3, 5, and 7 to 263 (afterwards extended to 273) decimal places, and by their means obtained the value of Euler's constant to 263 places. He also calculated the value of the modulus of the common logarithms to 273 places. The papers containing these results appeared in the "Proceedings" of the Royal Society for 1878 and 1887. Anyone who has had experience of calculations extending to a great many decimal places is aware of the difficulty of manipulating with absolute accuracy the long lines of figures; but this was an enjoyment to Adams, and the work, as carried out with consummate care and neatness, in his beautiful figures, is an interesting memorial of the patience and skill that he devoted to any work upon which he was engaged.

Some may think that the portion of his own time occupied by these calculations might have been more advantageously spent; but there is a charm of its own in carrying still further the determination of the historic constants of mathematics, which has exercised its attraction over the greatest minds. Those who feel the least possible interest in calculation for its own sake, and even dislike ordinary arithmetical computations, have been unable to resist the fascination of doing their share towards the calculation of the absolute numerical magnitudes which are so intimately connected with the foundations of the sciences dealing with abstract quantity. There is a special pleasure also in applying the resources of modern mathematics to obtain the values of the incommensurable constants to such an incredible degree of accuracy, and in verifying the distant figures by methods depending upon subtile principles and complicated symbolic processes of the absolute truth of which we thus obtain so striking an assurance.

Adams had the greatest possible admiration for Newton, and perhaps no one has ever devoted more careful and critical attention to his mathematical works, especially the "Principia." When Lord Portsmouth presented to the University, in 1872, the large mass of scientific papers which Newton left at his death, the arrangement and cataloguing of the mathematical portion of the collection were willingly undertaken by Adams. It was a difficult and laborious task, extending over years, but one which intensely interested him, and upon which he spared no pains. He found that these papers threw light upon the remarkable extent to which Newton had carried the lunar theory, the method by which he had obtained his table of refractions (showing that the formula known as Bradley's was really due to Newton), and the manner in which he had determined the form of the solid of least resistance. In several instances he succeeded in tracing the methods that Newton must have used in order to obtain the numerical results which occurred in the papers. The solution of the enigmas presented by these numbers written on stray papers, without any clue to the source from which they were derived, was the kind of work in which all Adams's skill, patience, and industry found full scope, and his enthusiasm for Newton was so great that he had no thought of time when so employed. His mind bore naturally a great resemblance to Newton's in many marked respects, and he was so penetrated with Newton's style of thought that he was peculiarly fitted to be his interpreter. Only a few intimate friends were aware of the immense amount of time he devoted to these manuscripts or the pleasure he derived from them. In 1887, on the occasion of the bicentenary of the publication of the "Principia," he was asked by Trinity College to deliver a commemorative address. Unfortunately the state of his health prevented him from undertaking a task which he alone could have adequately performed; but, with the kindness which all who sought his help invariably received, he most freely placed all the stores of his knowledge at the disposal of the present writer, who was appointed in his stead.

He was frequently asked to undertake calculations in connection with eclipses or other astronomical phenomena, and he never hesitated to lay aside his own work in order to comply with such requests. Mr. Downing has written: "His readiness to help, and his magnificent ability to help, will long be remembered at the Nautical Almanac Office," and similar words might be used with reference to the invaluable assistance which he so generously gave in other quarters. Many generations of officers of our Society can testify to his constant willingness to serve the Society, both as a referee and as a contributor to the annual reports. These reports and notices often cost him much time and thought.

He was President of the Society for the second time in 1874–76, when the medal was awarded to D'Arrest and to Le Verrier. In 1870, as Vice-President, he delivered the address on the presentation of the medal to Delaunay, of whose general method of treating the lunar theory he had the greatest possible admiration. In 1881 he was offered the position of Astronomer Royal, which he declined. In 1884 he was one of the delegates for Great Britain to the International Prime Meridian Conference at Washington. This visit to America afforded him great enjoyment and gratification.

He received the honorary degree of D.C.L. from Oxford, of LL.D. from Dublin and Edinburgh, and of Doctor in Science from Bologna and from his own university. He was a correspondent of the French Academy, of the Academy of Sciences of St. Petersburg, and of numerous other societies.

As Lowndean Professor he lectured during one term in each year, generally on the lunar theory, but sometimes on the theory of *Jupiter's* satellites, or the figure of the Earth. During his tenure of the directorship of the Cambridge Observatory in 1870 a fine transit circle by Simms was added to its equipment. This instrument has been employed in observing one of the zones of the "Astronomische Gesellschaft" programme. The zone assigned to the Observatory was that lying between 25° and 30° of north declination.

In 1863 he married Eliza, daughter of Haliday Bruce, Esq., of Dublin, who survives him.

The publication of his complete works has been undertaken by the Cambridge University Press. The first volume, which will be edited by his brother, Professor W. G. Adams, will contain the papers which have been already published, as well as the magnetic work. A memoir of Adams will also appear in this volume. The second volume will contain the lectures and such of the unpublished work as can be prepared for press.

Adams was a man of learning as well as a man of science, and his thoughts and interests were far from being restricted to astronomy and mathematics. He was an omnivorous reader, and, his memory being exact and retentive, there were few subjects upon which he was not possessed of accurate information. Botany, geology, history, and divinity, all had their share of his eager attention. He derived great enjoyment also from novels, and when engaged in severe mental work always had one on hand. Among his more marked tastes may be mentioned his love of early printed books. His collection, containing about eight hundred volumes, eighty of which belong to the fifteenth century, was bequeathed by him to the University Library at Cambridge. The works relate principally to mathematics or astronomy, theology, medicine, and the occult sciences; but he seems always to have bought any fine old book that took his fancy. He was so little given to talk about himself or his pursuits that probably but few of his friends were aware of his passion for black-letter books.

No one who knew him superficially, or who only judged by his quiet manner, could have imagined how deeply he was affected by great political questions or passing events. In times of public excitement (such as during the Franco-German War) his interest was so intense that he could scarcely work or sleep. His love of nature in all its forms was a source of never-failing delight to him, and he was never happier than when wandering over the cliffs and moors of his native county. Strangers who first met him were invariably struck by his simple and unaffected manner. He was a delightful companion, always cheerful and genial, showing in society but few traces of his really shy and retiring disposition. His nature was sympathetic and generous, and in few men have the moral and intellectual qualities been more perfectly balanced. An attempt to sketch his character cannot be more fitly closed than in the words of Dr. Donald McAlister, who knew him well, and attended him in his last illness:— "His earnest devotion to duty, his simplicity, his perfect selflessness, were to all who know his life at Cambridge a perpetual lesson, more eloquent than speech. From the time of his first great discovery scientific honours were showered upon him, but they left him as they found him—modest, gentle, and sincere. Controversies raged for a time around his name, national and scientific rivalries were stirred up concerning his work and its reception, but he took no part in them, and would generously have yielded to others' claims more than his greatest contemporaries would allow to be just. With a single mind for pure knowledge he pursued his studies, here bringing a whole chaos into cosmic order, there vindicating the supremacy of a natural law beyond the imagined limits of its operation; now tracing and abolishing errors that had crept into the calculations of the acknowledged masters of his craft, and now giving time and strength to resolving the self-made difficulties of a mere beginner, and all the time with so little thought of winning recognition or applause that much of his most perfect work remained for long, or still remains, unpublished."

He was suddenly attacked by severe illness at the end of October 1889, but he recovered sufficiently to resume his mathematical work in the usual way for several months. In June of the following year he was again attacked by an illness from which he never completely recovered, and he passed away on the early morning of January 21, 1892, after being confined to his bed for ten weeks. The funeral service took place in Pembroke College Chapel, and he was interred in St. Giles's Cemetery, on the Huntingdon Road. There were many who thought that his resting-place should have been in Westminster Abbey, and a royal wish was expressed to this effect; but it is perhaps more fitting that he should lie in this quiet graveyard close to the Observatory where he passed so many happy and peaceful years.

On February 20 a public meeting was held at St. John's College, Cambridge, with the view of taking steps to place a bust or other memorial of him in Westminster Abbey. The proceedings on this representative occasion bore eloquent testimony to the admiration and affection in which he was held by his friends, and to the widespread wish throughout the country for such a memorial to one who was not only a great but a good man. No suitable site for a bust could be found in the Abbey, but a medallion will be placed in an excellent position close to the grave of Newton. This medallion is being executed by Mr. Bruce Joy, who is also engaged upon a bust for presentation to St. John's College. It may be added that five years ago an excellent portrait was painted by Herkomer, which is now in the Combination Room of Pembroke College. J. W. L. G.

Charles Orchard Dayman was born 1803 July 9. In 1820 he went as a pensioner to St. John's College, Cambridge, of which he became a scholar, and in 1824 took his B.A. degree as third Senior Optime. He won in 1826, but did not accept, a