

## OBITUARY NOTICES OF FELLOWS DECEASED.

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SIR GEORGE AIRY was born on July 27, 1801, and died on January 2, 1892. It is not a possible task to compress into a few pages the ninety years' work of a great man; all that can be done is to indicate a few of his many achievements. When his life is written it will be a book and not a pamphlet, and only then shall we understand how much of our scientific knowledge we owe to him. The number of articles and memoirs which he has communicated to the various Societies and journals in which he was interested are over five hundred in number. The first of these is a paper read to the Cambridge Philosophical Society, on November 25, 1822, on the use of silvered glass for the mirrors of reflecting telescopes, and the last is his Numerical Lunar Theory. The date of the first paper is particularly interesting, for it is the year *before* he took his degree. The last paper is also remarkable, for, remembering that the theory of the Moon is one to which some mathematicians have devoted nearly their whole lives, it shows the old man attacking a laborious problem with the energy of youth.

Sir George was educated at private schools at Hereford and Colchester. His vacations from school were spent at the Hill Farm, near Playford, with the uncle by whose assistance he was enabled to go to Cambridge. He never appears to have played cricket, or football, or rowed, but he delighted in pedestrianism. We are told that, with a companion, he once attempted to walk from Playford to Bury St. Edmunds and back in the same day. They reached Rushmere Church on their way home, but could not do the last mile and a half, and the journey had to be finished in a cart sent to meet them.

In 1819 he entered Trinity College, as a sizar, but it does not appear that he was elected a scholar until he was in his third year. In 1823 he became Senior Wrangler and First Smith's Prizeman. The writer can remember that when, thirty years after that date, he entered Cambridge, the story was still told amongst the undergraduates of how wonderfully Airy had answered in the examination. Possibly nothing had been lost in the telling, but there must have been some extraordinary excellence to have attracted such long continued attention.

As soon as he had taken his degree he began a life of ceaseless scientific activity. Memoirs followed each other with ever increasing rapidity, each bearing evidence of much thought and of considerable

work. The subjects also were of the most varied kinds and on all parts of mathematical philosophy. At first he wrote chiefly for the Cambridge Philosophical Society; thus in March, 1824, he calculates the effect which the ring of Saturn produces on his figure. He tries to verify the curious observations of Herschel that this planet is protuberant between the poles and the equator, but he finds that theory leads to an exactly opposite result. The observations of Herschel were, however, considerably modified by those of Bessel some years after. In another paper in the same year he discusses achromatic eye-pieces; for this and his other papers on optical subjects, the Copley Medal of the Royal Society was adjudged to him in 1831. Soon after, he writes on the proper forms of the teeth of wheels, though, owing to the extensive use of iron where wood was formerly used, this subject has no longer the interest it once had.

One of the most interesting papers written by him, about the year 1825, is on a peculiar defect of his own eye and the mode of correcting it. He discovered that in reading he did not use his left eye. Supposing this to be due to habit, he endeavoured to read with the right eye shaded, but found he could not distinguish a letter at whatever distance from the eye the characters were placed. Some time after he made a further discovery, viz., that the image of a point formed by that eye was not circular but elliptical. From this and other appearances he inferred that the refraction of the eye was greater in the vertical plane than in that at right angles. To correct this it would be necessary to use a lens which would refract more powerfully the rays in one plane than those in the perpendicular plane. His idea was that the lens should have one surface cylindrical and the other spherical, and he describes at length his experiments to determine the proper radii. The result was so successful that he was able to read the smallest print at a considerable distance with the left eye as well as with the right. In his subsequent papers he frequently returns to this subject and makes several reports to the Society on the changes produced in his eye by lapse of time.

In 1826, when Ivory criticised Laplace, Mr. Airy, in a paper contributed to the Cambridge Philosophical Society, was not afraid to intervene between two such distinguished analysts when he thought that both had gone wrong. Mr. Airy was not indisposed to controversy; possibly it added a touch of life to his science. We find him afterwards engaged in many disputes, in all of which he was able to prove that he was a tough adversary. In this year, three years after his degree and ten years before he became F.R.S., he read his first paper before the Royal Society. The subject was the much debated question of the figure of the Earth. Alluding in it to the peculiar views of Ivory of fluid equilibrium, he was attacked by that mathematician in a somewhat arrogant manner. This called forth from Mr. Airy a

crushing reply, which he published in the 'Philosophical Magazine' in 1827.

It is, however, impossible even to mention the names of the many papers of which he was the author. The few which have been mentioned above prove how soon after his degree he took a leading part in the scientific work of the day. They show also how, from the very beginning, his mind was turned to practical applications, leaving aside any pure theorems of which he did not see the immediate use.

In 1826 Mr. Airy published his mathematical tracts, which almost immediately became the standard text-book for students in the University. In the first edition we find only the lunar theory, the figure of the Earth, precession and the calculus of variations, the tract on the planetary theory and that on the undulatory theory being added in the second edition, 1831. As his object was to give a clear statement of first principles, he put into the book just what was wanted at the time he wrote, making his judgment with admirable skill. The student world has now outgrown the book, but this is in part due to the excellence of the teaching of the book itself. The first tract, that on lunar theory, is interesting to Cambridge students for another reason. The attention of the University had been so long confined to the works of Newton that the analytical mode of treatment had been almost entirely neglected. The methods of Newton are, Mr. Airy remarks, beautiful, but they have all the imperfections which necessarily accompany first attempts; for the explanation of some of the lunar inequalities they are hardly sufficient, and for the calculation of most they are quite inadequate. For other branches of physical astronomy, such as the planetary theory, their inadequacy has never been questioned. In this tract he endeavours to lay before the student an analytical view of the lunar theory, giving references to the 'Principia' to show the connexion between the different systems. The tract on the calculus of variations is the only one which is purely mathematical. Though it does not go very far into the subject, yet the author must have had a deep sense of the power of this calculus, for he has used it in his physical papers, even in places where simpler methods might more naturally have suggested themselves to his mind. In the preface he speaks of this calculus as the most beautiful of all the branches of the differential calculus. The excellence of the tract on the undulatory theory is evident when we remember the length of time in which it was regarded as the standard text-book of the University. When the other tracts, after a long life, became antiquated, this one retained its popularity, and has been reprinted several times by itself, and is even yet in use.

Mr. Airy was elected a Fellow of Trinity College the year after his degree, and later on in life he was chosen one of the three first Honorary Fellows of the College, the others being Thirlwall and

Tennyson. In 1826 he was appointed Lucasian Professor of Mathematics, but this professorship he soon exchanged for the Plumian, to which he was appointed in 1828. According to the Calendar of that date, his predecessor in office merely gave lectures in the first half of the midsummer term, while those of the former Lucasian Professor are only vaguely referred to. But these were greatly enlarged by Mr. Airy, whose syllabus extends over forty-eight pages of print. They comprise statics, dynamics, hydrostatics, and geometrical optics, but their chief character seems to have been the theory of undulations. Many of the experiments on polarised light whose mathematical theory is given in his tract on the undulatory theory were exhibited here. He appears to have been the first to introduce into Cambridge studies the beautiful theories of Fresnel. With these as subjects, treated in his own skilful manner, we need not wonder at the popularity of his lectures. Even after he had become Astronomer Royal, we learn from his first report to the Board of Visitors, that application to the Admiralty had been made by several members of the University and by the Plumian Professor to allow him to give another course of lectures at Cambridge.

Along with the Plumian Professorship Mr. Airy undertook the duties of the Director of the Observatory. He at once entered on these arduous duties with his usual energy. His efforts were well seconded by the University, who at once raised the slender income of the professorship to an amount nearly double its former value. In the first volume of the 'Astronomical Observations' he tells us that he was induced to fix on a plan of publication very different from that of the 'Greenwich Observations.' He remarks that the value of unreduced observations is so small that to most persons they are absolutely useless. Few, who have not made observations, understand how much time and calculation must be employed before they can be applied to any useful purpose. On the average, the preparatory steps and the observation of a transit occupy from five to ten minutes, while the complete reduction and discussion of the observations employ full half an hour. The professor even said that if an offer was made of a mass of regular meridional observations unreduced, he would not think it worth acceptance. In giving, therefore, the results, he was giving the produce of four or five times as much labour, necessarily irksome, as if he gave merely the unreduced observations. The report for the year 1828 covered the interval of five months' residence at the Observatory; he had no assistant, and every step from making the observations to revising the proof-sheets had to be done by himself alone. Yet in April of the following year the report was published with all the necessary reductions. This promptness is maintained in the succeeding years, and excited the admiration of M. Quetelet, the Director of the Obser-

vatory at Brussels, who says, "Nous sommes à peine au milieu de 1832, et déjà nous possédons les observations de M. Airy, pour toute l'année 1831: et ce qui peut paraître plus étonnant encore, toutes ces observations sont calculées et discutées avec soin."

It is interesting to observe the care with which he chose the objects to which he should turn his attention as an astronomer, and the constancy with which he stuck to his choice when once made. The chief object, he says, must be such that it could be accomplished by a single unassisted observer, and yet be so important as to be of public use. After consideration he decided that the observations of planets had at that time been so neglected, that one who wished to revise the planetary tables would find himself destitute of the necessary data on which to found his investigation. As soon, therefore, as the Cambridge Observatory was placed under his direction, he made the observation of planets the leading object of his labours. He says in one of his reports that "hardly a single observation of a planet has been lost when the transit was at such an hour that in the regular routine of observations it was practicable to observe it." The wisdom of his choice is shown by the fact that his successor followed closely the same objects. Other pressing wants in astronomy were also present in his mind, and others again rose unexpectedly in the course of his work. In reading his yearly volumes of observations, one notices among other things the care which is taken to secure accuracy. No labour is spared, no calculation is allowed to pass without repeated examination. "To observe all night and to calculate all day" is the description of an astronomer's duties given by an astronomer. In the arrangement of his results, we notice, also, how everything is subordinated to increasing their immediate utility as well as securing accuracy in their details.

When Professor Airy first went to the Observatory the only large instrument was a transit, though this was one of the best of its kind. So energetic an astronomer was not likely to be satisfied with this; accordingly in 1834 he obtained a large mural circle. In the report for that year he describes the unexpected and annoying difficulties which arose in connexion with that instrument. In the next report we find that these difficulties have been overcome by considering that the effects of the discordance of zenith points on direct and reflexion observations are equal. Later on the great Northumberland equatoreal was added. The establishment to work these was also necessarily increased, and two assistants were given to him.

Perhaps one of the most remarkable examples of Mr. Airy's insight into astronomical questions is his discovery of a new inequality in the motions of Venus and of the Earth. The attention of the Board of Longitude having been directed to the state of the solar tables used in the construction of the 'Nautical Almanac,' he was desired to

examine the discrepancies between the computed right ascensions of the Sun and those observed at Greenwich. On making a comparison between the discrepancies in the position of the Sun's perigee as given by late observations with those given by the observations of the last century, he concludes there must be some yet undiscovered inequality which has been omitted from the calculations. He soon discovered that this originated in the fact that thirteen times the periodic time of Venus is so nearly equal to eight years that the term depending on this phase received a multiplier of more than two millions in integrating the differential equations. On the other hand, the coefficient is of the fifth order with regard to the eccentricities and inclinations of the orbits. In the report on this paper drawn up by Whewell and Lubbock for the Royal Society, it is pointed out that no numerical calculation of a perturbation of the fifth order had been executed, except in the case of Jupiter and Saturn, where, as Laplace states, this labour, "*pénible par son excessive longueur,*" had been performed by Burckhardt; and no calculations of a new inequality of a high order, requiring to be placed in the planetary tables, with a new argument, had been published since that of the great inequality by Laplace in 1784. They conclude by remarking that this is the first specific improvement in the solar tables made by an Englishman since the time of Halley. For this brilliant investigation the Astronomical Society in 1833 awarded to its author their gold medal. The whole of Professor Airy's process was afterwards verified, first by Pontécoulant, and secondly by Leverrier, and found to be correct.

In the years 1831-32, Professor Airy, though so fully employed at the Observatory, was yet able to make some important investigations in the theory of light. Thus he communicates to the Cambridge Philosophical Society a paper to show that the two rays produced by the double refraction of quartz are elliptically polarised. This is soon followed by two or three papers on some phenomena connected with Newton's rings. Just as Sir W. Hamilton afterwards predicted internal and external conical refraction after studying the analytical properties of the wave surface, so Professor Airy discovered these phenomena by using Fresnel's general formula for the intensity of reflected light. When Newton's rings are formed by light polarised in a plane perpendicular to that of incidence between two substances of different refractive indices, and the angle of incidence lies between the polarising angles, the rings should appear white centred, instead of having a central dark spot. Here was a recondite phenomenon which could only be seen when several special conditions were satisfied. Would it be confirmed by experiment? He describes the difficulties of the experiment and its final success. As we read the paper, we perceive how he is led on by slight unexpected discrepancies to

improve the theory. He remarks that there must be a gradual, though rapid, change of phase, instead of the sudden one given by Fresnel's formula, thus seeing faintly a result clearly explained five years after by the theoretical investigations of Green.

At this period of his life Professor Airy's labours are evidently divided between astronomy and the theory of light. The first was connected with his work at the Observatory, the second with his lectures as Professor. Thus, in 1833, he writes in the 'Cambridge Transactions' on Newton's experiments in diffraction; in 1835, on the diffraction of an object glass with a circular aperture; in 1838, on the intensity of light in the neighbourhood of a caustic. In 1840 he chose as the subject of the Bakerian Lecture the theoretical explanation of an apparent new polarity in light.

There is an equally important list of papers on astronomy. In 1832 he communicates to the British Association a report on the progress of astronomy during the present century. This was translated into German, three years after, by C. L. Littrow, of the Royal Observatory, Vienna. The Viennese astronomer thinks that Professor Airy has treated German astronomy like a step-mother, but, nevertheless, he says there is no other work in which the progress of astronomy is so briefly and so accurately given. In 1834 he writes for the 'Nautical Almanac,' on the perturbations of small planets and comets of short period. There is more than one paper on the mass of Jupiter. In 1834 he writes a paper, for the Astronomical Society, on the solar eclipse of July 16, 1833, which was seen extremely well at Cambridge. On this occasion he adopted a new plan of observation; instead of noting the times of the beginning or the end, he so chose the quantities to be measured that any errors in the elements would be observed after they had been largely multiplied. For example, at the beginning of the eclipse, when the discs of the Sun and Moon only slightly overlap, it is obvious that the length of the straight line joining the cusps is much more affected than the versine by any small error in the angular distance of the centres of the discs. To detect such errors, the attention of the observer should be directed to the length of this line. In like manner, the whole duration of the eclipse was divided into periods, for each of which he arranged appropriate measures.

These papers, too numerous to catalogue in this place, did not exhaust the energy of the Professor, for he found time to publish treatises on Trigonometry, the Figure of the Earth, and one on Gravitation. The latter was written for the 'Penny Cyclopædia,' but previously published, in 1834, for the use of students in the University of Cambridge. It was an attempt to explain the perturbations of the solar system without introducing an algebraic symbol. Having thus denied himself the use of the most powerful

engine of mathematics, he expresses his surprise at finding that a satisfactory explanation could be offered for almost every inequality recognised as sensible in works on physical astronomy. The book, though well received, was, for many reasons, not so popular as his tracts. In 1884, however, it received the honour of a second edition.

In 1836-37 he was President of the Astronomical Society. In the first of these years, when presenting the medal of the Society to Herschel for his catalogue of nebulae and clusters of stars, he gave an interesting account of the history and of the then state of our knowledge of these curious bodies. The next year the address was on the perturbations of comets.

The year 1835 was a great epoch in Mr. Airy's life, for he was then appointed Astronomer Royal. How thoroughly he intended to work the National Observatory is evident from his very first report, for here we find traces of the reforms he intended to introduce; the arrangement of the volumes of observation was to be remodelled; the library improved; a new equatoreal was suggested; a magnetic apparatus had already been acquired, and the site of a magnetic observatory chosen.

Remembering the views he had expressed on unreduced observations when he began work at the Cambridge Observatory, we naturally inquire what he did with the vast mass of ancient observations which he found unreduced when he arrived at Greenwich. This we learn gradually as we read his reports to the Board of Visitors. In 1841 the observations of planets from 1750 to 1830 had already been reduced to longitude and latitude, and every one had been compared with the place computed from the best modern tables. Sufficient time had not yet elapsed to allow of the reduction of the lunar observations, for here 8000 places of the Moon had to be deduced from observation, and 8000 places had to be computed in duplicate from tables exhibiting the complicated results of the most advanced modern theory. In extent and in importance this work may be considered comparable to any that has yet been undertaken in astronomy. In 1846 these lunar reductions were entirely completed. One immediate result was that Hansen discovered two inequalities of long period in the Moon's motion, produced by the attraction of Venus, though some doubts were afterwards thrown on one of these by Delaunay and Newcomb. For these reductions he received in 1846 a gold medal, and in 1848 a testimonial, from the Astronomical Society. Sir John Herschel, in the latter of these years, after noting that this work will remain to the latest posterity a monument of national glory, remarked that we owe to other nations, and especially to the French, the filling up of the great outline struck by Newton with the analytical expressions of the laws of lunar and planetary motion. This glory, he says, they have fairly won, and it is theirs.

“But the broad basis of observations upon which this magnificent superstructure has been reared is British; in the National Observatory it was created. Such has been the mission of that establishment, and such Mr. Airy has wisely judged it must continue to be, to furnish now, and in all future time, in an unbroken series, the best and most perfect data by which the laws of the lunar and planetary movements, as developed by theory, can be compared with observation.”

In the report for 1841 he also describes how the Magnetic and Meteorological Department had grown into an important branch of the Observatory. He tells us that the regular work of the establishment is to observe the meridional, bifilar, and horizontal needles, the barometer and thermometers, besides several other instruments, every two hours night and day, except on Sundays; to pursue incessantly the magnetic observations whenever anything unusual occurs; to observe some of the instruments every five minutes during twenty-four hours on a fixed day every month. As the observations, when made, had all to be reduced and tabulated in proper shape, it is clear that the amount of work done must have been very great. Some of these troublesome observations were afterwards abbreviated by a system of self-registration by photography. The description of this new system is given in the volume of ‘Greenwich Observations’ for 1847.

In April, 1839, Mr. Airy read to the Royal Society his first paper on the correction of compasses. Captain Flinders, in his famous voyage to Australia, had observed that the north end of his compass appeared to be drawn towards the bows of his ship; and he, and others after him, had suggested methods of compensating the cause of the disturbance. The Astronomer Royal was, however, the first to make a thorough investigation of the laws of magnetic disturbance. The iron ship “Rainbow” was placed at his disposal with a view of discovering by experiment some method of controlling the strange deflections of the compass. The use that he made of this vessel will make its name as famous in the history of the mariner’s compass as Stephenson’s “Rocket” is in the history of locomotives. Assuming that every particle of iron in the ship is, by the action of terrestrial magnetism, converted into a magnet, he calculated the resolved forces on one end of a compass needle whose centre holds a fixed position in the ship. He found that these forces contained two sets of terms, which he called the semi-circular and quadrantal variations, their phases being respectively the azimuth and twice the azimuth of the ship. The latter of these he found to be due to the induced magnetism; while in the former the permanent, the sub-permanent, and the induced magnetism had shares. Having determined the coefficients of these variations by observing the times of vibration of a delicate needle placed first on shore and then on the ship, he was

able to compare the actual and calculated deviations of the compass from the north for all azimuths of the ship. He soon found that almost the whole deviation of the compass was accounted for by the permanent magnetism, and that the residual part followed nearly the quadrantal law. He thence deduced a simple rule by which the compasses could be practically corrected to a first approximation. Putting the ship's length (1) north and south, and (2) east and west, he showed how to place two permanent magnets near the compass so that it indicated true magnetic north in each position. Placing next the ship's length north-east and south-west, the effect of the quadrantal deviation became prominent, and this he corrected by a mass of soft iron, whose own induced magnetism, when properly placed, counterbalanced that of the ship. These corrections being disturbed when the ship heeled over, another magnet was added. On applying this method to the "Rainbow," and trying the compass with the ship's head in different positions through the circumference, it was sensibly perfect; the deviation, which at first had exceeded  $50^\circ$ , sometimes on one side and sometimes on the other, was at once reduced to half a degree. The great development of iron-built ships soon rendered some modification in these corrections necessary; improvements were made by their author, and other mathematicians also made a special study of the deviations of the compass. Mr. Airy wrote several other papers in connexion with this subject, such as those in 1840, 1856, 1860, and 1862, and in 1865 he delivered a course of three lectures at the School of Naval Architecture and Marine Engineering at South Kensington. The principle that the compass ought to be corrected by magnets or otherwise has not been universally received; it was contended that it was better to use a table of errors. The Astronomer Royal maintained that the former course was the proper one, while Mr. Archibald Smith has been the champion of the latter. The question has been much discussed, but cannot be entered into here.

Mr. Airy formed one of an important Commission for the restoration of the standards of weights and measures which had been injured by the fire at the House of Commons. Contrary to the opinion prevalent in France, the Commission recommended that the standard measure should be defined by the length of a certain rod preserved in some place of safety, and not by any natural standard, such as the length of the seconds pendulum. Contrary, also, to the method adopted by Bessel for the Prussian standard, the yard is defined by the distance between two points marked on the bar and not by the length of the bar. The history of standards is given by Mr. Airy in a long paper in the 'Phil. Trans.' for 1857.

About 1841 Mr. Airy turned his attention to the theory of tides. He wrote several papers on this subject, discussing separately the

tides in the Thames, at Ipswich, Southampton, the coast of Ireland, and, later on, the tides at Malta. A Royal Medal was adjudged to him by the Royal Society in 1845 for his inquiry into the laws of the tides on the coast of Ireland. His chief work on this subject is his essay on "Tides and Waves," printed in the 'Encyclopædia Metropolitana.' Taking the usual division of the theory into three parts, viz., the equilibrium theory, that of ocean tides, and that of river tides, we may ascribe the initial steps in the first to Newton and Bernoulli, those in the second to Laplace, while the last may be said to have begun with Airy. This important paper, perhaps because it had not been published by any learned society, did not attract the attention it deserved on the Continent, but its merits could not remain unnoticed, and in 1875 the section on river tides was translated and printed in 'Liouville's Journal.' In this section he discusses the broken water seen on the edge of a shoal, why the rise of tide occupies less time than the fall, the solitary wave, the breaking of waves, the effect of the wind, tidal waves, the effect of friction, the form of the wave in broad channels with shallow sides, and other interesting questions. When M. Delaunay, in 1866, suggested that a portion of the apparent acceleration of the Moon was due to a real retardation of the rotation of the Earth caused by tidal friction, Mr. Airy gave a general explanation, founded on the theory of river tides. He discovered two terms of the second order in his equations whose general effect was to produce a constant acceleration of the waters in the direction of the Moon's apparent diurnal course. He therefore gave his entire assent to the views of Delaunay on the existence of one real cause for the retardation of the Earth's rotation. Other causes of retardation have been discovered by mathematicians since then, but these, of course, lie outside the scope of the present sketch.

In the summer of 1844 the arc of longitude between Greenwich and the island of Valentia was measured. As an intermediate station, the longitude of Kingstown was also determined. The difference of longitude was found by making thirty pocket chronometers travel from Greenwich to Kingstown and back again several times; the difference between Kingstown and Valentia being found in a similar manner. The differences in longitude having been found, the next step was to compare the results with the data of the trigonometrical survey, and to see how far they agreed with the best existing determination of the figure of the Earth. The triangulation was then only partially completed, but enough had been done to enable Mr. Airy to arrive at the result that in the latitude of  $51^{\circ} 40'$  the length of  $1''$  in an arc perpendicular to the meridian is 101.6499 feet in terms of the standard bar of the Ordnance Survey. In the same year Struve determined the difference of the longitudes of Altona and Greenwich by the transmission of forty-two chronometers across the German Sea

sixteen times. In the summer of 1862 the longitude of Valentia was again determined, this time by the use of the electric telegraph. The electric telegraph was also used in 1853 to determine the differences between the longitudes of Greenwich, Cambridge, and Edinburgh, and, when the submarine telegraph was laid to Ostend, the difference of longitude between Greenwich and Brussels. These differences of longitude have been combined with other Continental determinations, and, by the use of the whole series, the longitudes of places in the extreme east of Europe may be compared with places in America.

In the year 1844 Mr. Airy also assisted in tracing the Canadian boundary under the Treaty of Washington. The corps of Royal Engineers who were to mark the boundary were placed at the Observatory for instruction and practice in the use of instruments under his eye. The most difficult part of the boundary was a straight line of nearly seventy miles in length, passing through a country of impervious forests, steep ravines, and dismal swamps. He arranged a plan of operations founded on a determination of the absolute latitudes and the differences of longitudes of the two extremities. The azimuths of the line for the two ends were then computed, and marks laid off for starting from each end. One party of engineers, after cutting more than forty-two miles through the woods were surprised, on the brow of a hill, at seeing a gap in the woods, on the next line of hill; this turned out to be the line of the opposite party. On continuing the lines until they passed abreast of each other, their distance was found to be 341 feet. This corresponds to an error of only a quarter of a second of time in the difference of longitudes, and is about one-third of the error which would have been committed if the spheroidal form of the earth had been neglected. This is a striking testimony both to the accuracy of Mr. Airy's method and to the skill of the engineers.

At a special meeting of the Board of Visitors in 1843, Mr. Airy proposed to construct the first of the important instruments he has added to the Observatory. He points out that the Royal Observatory was instituted chiefly to observe the Moon, and that this object had been so continuously kept in view ever since its foundation, that the existing theories and tables of the Moon are founded entirely on Greenwich observations. The unavoidable interruptions to the regularity of the series were, however, so numerous, that the number of complete observations then made was under a hundred a year. He proposed to supply this deficiency by erecting an altitude and azimuth instrument, by which the Moon could be observed in any part of the sky. He assures the Visitors that its cost ought not to be an objection when it is remembered that each complete lunar observation was then worth ten pounds. In the report of 1847 we read that the new instrument had been completed, and was in working

order. In 1848 he proposed to erect a transit circle. Other great instruments followed, and in 1855, when the want of an equatoreal was pointed out, the Board of Visitors warmly supported his views. In the summer of 1860 the instrument was in a state fit for use. Enough has now been said to show that by his energy and perseverance the Royal Observatory has been equipped with the most admirable instruments which could be obtained. As early as December, 1849, the Astronomer Royal made an oral statement to the Astronomical Society on the method of observing and recording transits lately introduced in America. He explains how a measure of its accuracy, as compared with Greenwich observations, had been made, and, after pointing out some defects, he thought the possible advantages so great that he contemplated adopting it at the Royal Observatory. In this report for 1854 he tells us that the new barrel-apparatus had been practically brought into use, not, however, without a succession of difficulties, whose causes it was sometimes very hard to discover. He concludes by remarking that the method is troublesome in use, and consumes much time in preparation, but that, amongst the observers who use it, there is only one opinion on its astronomical merits, viz., that, in freedom from personal equation and in general accuracy, it is very far superior to the observations by eye and ear.

In 1846 Mr. Airy was one of three Commissioners appointed by the Queen to report on the proper gauge for railways. The Commissioners considered that the chief advantage of the broad gauge lay in the speed of the trains, while in the conveyance of goods and the cost of outlay the narrow gauge was the superior. For these and other reasons they recommended that the present narrow gauge should by statute be that of all railways to be constructed in future. After examining and rejecting several ingenious inventions by which the same carriage could be made to run on both gauges, they also recommended that some equitable means should be found by which the railways then on the broad gauge could be reduced to the narrow. This second recommendation was not, however, adopted by the legislature. In 1858 Professor Airy was one of the Commission on the Ordnance Survey appointed to consider, amongst other questions, the scales on which the maps were to be constructed.

In the 'Monthly Notices' for November, 1846, there is a memoir by the Astronomer Royal, giving his own, Professor Challis', and Mr. Adams' separate accounts of the discovery of Neptune. Mr. Airy remarks that in the whole history of astronomy there is nothing comparable to this discovery; Uranus, Ceres, Pallas, and other planets were discovered by observation, but, in the case of Neptune, mathematicians stated beforehand that a planet would be found exactly in a certain place and presenting exactly a certain appearance. In that

place and with that appearance the planet was found. The predictions of Adams and Leverrier differed by only one degree of longitude. The controversy which has arisen on this discovery cannot be briefly discussed, and must be omitted in a sketch as short as the present one.

In the years 1850, 1851, Mr. Airy turned his attention to antiquarian researches. There are several papers in the 'Athenæum' on the Exodus of the Israelites, and some more on the place of the landing of Cæsar. The first of these lines of enquiry led gradually to the "Notes on the earlier Hebrew Scriptures" (1876), and the latter to the "Treatise on the Roman Invasion of Britain" (1865). Halley, reasoning on the phenomena of the tides as described by Cæsar, and comparing these with the Channel tides as then known, had concluded that Deal was the landing place. Mr. Airy, however, showed that, with fuller knowledge of the local tides, this line of reasoning would prove that Pevensey was the actual landing place. He also contended that this result was confirmed by a study of Cæsar's movements in Gaul before the crossing, and his transactions in the interior of Britain after the passage of the straits. Mr. Airy was also interested in several other antiquarian questions. Thus in the 'Phil. Trans.' for 1853 there is a paper on the eclipses of Agathocles, Thales, and Xerxes, in which he arrived at some new dates for these events. After the publication of Professor Adams' theory of a diminished value of the acceleration of the Moon's mean motion, Mr. Airy repeated his calculations, and somewhat modified his results. He also compared the dates of thirty-six eclipses given in a Chinese historical work called 'Chun Tsew' with those calculated by theory by a French writer, and points out how generally accurate the Chinese records are on these points.

Mr. Airy had the pleasure of viewing three total eclipses of the Sun, the first from the Superga, near Turin, in 1842, the second at Göthenburg, in Sweden, in 1851, and the third at Hereña, in Spain, in 1860. In the many accounts which he has given of these eclipses, he continually dwells on the impressiveness and awfulness of the scene, pointing out that no degree of partial eclipse gave the least idea of a total eclipse. He mentions, on the authority of Arago, that the officers of a French corvette who had been trained to observe the eclipse of 1842 lost their discipline when the darkness came on, and the observations were not made. The most remarkable of all the appearances at the first eclipse were the red mountains or flames seen round the Moon. It was afterwards discovered that these had been seen in 1733 by a Swedish astronomer, but all the observers at Turin were taken by surprise. It was difficult then to decide what they were, or even whether they belonged to the Sun or to the Moon. In the eclipse of 1851 special attention was given to these flames, and,

in his report to the Astronomical Society, Mr. Airy said it was impossible to see the changes in their aspect without feeling the conviction that they belonged to the Sun and not to the Moon. Still many doubted, but in 1860 the observed angular displacements and velocities of displacements of these red appearances as the Moon's disc passed over the Sun proved decisively that they belonged to the latter body. On the expedition to Spain, the Astronomer Royal was accompanied by a large body of observers; some were trained astronomers, the others amateurs. All did good service, there was much work to be done and it was well done. The Government placed the large steamer "Himalaya" at the disposal of the party to carry them to and from their destination, and in conferences on the deck of that ship the different classes of observation were divided amongst those present. All the details of the organisation of the expedition rested in great measure on the Astronomer Royal, and in his report to the Board of Visitors in 1861 he pronounced the enterprise to have been very successful. In his lecture to the Astronomical Society he remarked that it would be advantageous to collect from the various accounts, first, all the facts which relate to one part of the phenomena, secondly, those which relate to another, and so on; and, finally, to arrange these in separate chapters in order that a systematic comparison might be made. In the forty-first volume of the *Memoirs of the Astronomical Society*, edited by Mr. Ranyard, these comparisons are published, and occupy 792 pages. The mere titles of the chapters are sufficient to show the importance and interest of the work.

At the meeting of the British Association at Manchester in 1860, Mr. Airy delivered a lecture on the solar eclipse of that year to an assembly of perhaps 3000 persons. The writer of this sketch remembers the great Free Trade Hall crowded to excess with an immense audience whose attention and interest, notwithstanding a weak voice, he was able to retain to the very end of the lecture. This lecture he repeated at Cambridge, as the Rede Lecture, in 1864, where it was again well received. It was afterwards translated into Dutch by D. Bierens de Haan about 1877. The charm of Professor Airy's lectures lay in the clearness of his explanations. The subjects also of his lectures were generally those to which his attention had been turned by other causes, so that he had much that was new to tell. His manner was slightly hesitating, and he used frequent repetitions, which, perhaps, were necessary from the newness of the ideas. As the lecturer proceeded, his hearers forgot these imperfections and found their whole attention rivetted to the subject matter. On many occasions Mr. Airy has given successful lectures. In March, 1848, he delivered six lectures at Ipswich on Astronomy, which were first taken down by shorthand writers and, after correc-

tion by their author, published in a collected form. This treatise has been very popular and has gone through several editions. He also lectured in the Town Hall at Neath; in 1851 at the Royal Institution, on the total solar eclipse of that year; and in 1853 on the eclipse of Thales. In 1878 he lectured at Cokermouth on the probable condition of the interior of the earth. Besides these there were many other lectures, some of which will be mentioned further on.

At his Rede Lecture at Cambridge in 1864 Mr. Airy took occasion to point out what appeared to be defects in the system of education as connected with mathematical physics, and he followed up these remarks by a letter to the Vice-Chancellor. To assist in remedying these deficiencies he had already written, in 1861, his now well-known treatise on the theory of Errors of Observation. With the same object he published, in 1866, his "Partial Differential Equations," in which he introduced the novelty of giving stereoscopic views to illustrate the surfaces under consideration. These were followed by a treatise on Sound in 1868. In order to direct the attention of the University to the subject of magnetism, he gave a course of lectures in the Easter term of 1869, at Cambridge. These being attended by crowded audiences, were developed into the treatise on Magnetism which appeared in the following year.

One of the most remarkable of Mr. Airy's investigations is that in which he determined the mean density of the Earth. He had always been much interested in this subject, and in the fourteenth volume of the Memoirs of the Astronomical Society we find that he assisted Mr. Baily in his important repetition of the Cavendish experiment by contributing the theory and the formulæ. In 1826 another method had occurred to him which promised to give a more accurate result than either Maskelyne's method of measuring the attraction of a mountain or the Cavendish experiment. His immediate object was to compare the force of gravity at the surface and at the bottom of a deep mine, using the pendulum as the means of observation. In the 'Phil. Trans.' of 1856 he describes the attempts he had made at Dolcoath in 1826 and in 1828, and their failure on both occasions in consequence of accidents, once by fire and once by water. Twenty-six years passed before he was in a position to repeat the experiment for the third time. In 1854, however, a new power was, he tells us, placed in his hands. The galvanic system had been established at the Royal Observatory, and, in the familiarity which he now possessed with telegraphic applications, he perceived that the difficulty of comparing the upper and lower clocks would be entirely removed. The experiment was conducted at the Harton Pit, a mine about 1260 feet deep. The result was that gravity below was greater than that above by  $\frac{1}{19286}$ th part, so that the mean density of the earth was 2.7 times the surface density and 6.6 times that of water.

The value thus obtained is larger than that given by the Schehallien method, and considerably larger than that deduced by Baily from the torsion rod experiments. After the experiments at Harton Pit were concluded, he gave a lecture at the Central Hall, South Shields, on the pendulum experiments, and, in the next year, a Friday Evening Lecture at the Royal Institution, in London, on the same subject. These experiments were also noticed by "Mr. Punch" in a copy of verses on "Airy and the Coal Hole," written by Shirley Brooks.

In 1867 Professor Airy read a paper at the Institution of Civil Engineers on the use of the suspension bridge with stiffened roadway for railway and other bridges of large span. This paper was the result of a discussion with Mr. R. Stephenson on the method to be adopted for the Britannia bridge, and the author thought there were better methods available for wide crossings than simple tubular bridges. Besides writing this paper, for which he received the medal of the Society, he often attended the meetings and joined in the discussions.

As the management of the expeditions to observe the transits of Venus in 1874 and 1882 would necessarily fall on the Astronomer Royal, Mr. Airy began his preparations as early as 1857. In that year, he called the attention of the Astronomical Society to the means available for correcting the measure of the Sun's distance in the next twenty-five years. On this occasion he pointed out the peculiar advantages of observing Mars for that purpose, showing that at the opposition of 1860 that planet would make a near approach to the Earth. The preparations for the transit were continued during the following years; the proper places to which the expeditions should be sent were discussed at great length and finally chosen. The northern stations having been occupied by Continental observers, the southern ones were, by the advice of the Board of Visitors, divided amongst the English parties. It was also decided that photographic should be combined with eye observations, and an extra grant was obtained from the Treasury for that purpose. By 1873 preparations had so far advanced that an efficient body of observers from all classes, naval, military, and civilian, were collected at the Royal Observatory, and were being instructed and practised in all the practical details of observation with the transit, the altazimuth, the equatoreal, and especially with the working model of the transit. At this time the received measure of the Sun's distance depended on the transits of 1761 and 1769, but mainly on the latter. Though there was a close accordance in the results obtained from the different transits, yet all investigators expressed doubts on their correctness. In the transit of 1761 the results depended almost entirely on an accurate knowledge of the differences of the longitudes of very distant stations. In that of 1769 the result rested in great measure on the observations of a single person, viz., those of Father Hell at

Wardhoe. Another difficulty had also presented itself; it was found that after the planet appeared to have fairly entered upon the Sun's disk it was for some time connected with the Sun's limb by a black ligament. Even after the planet had separated itself from the edge, it did not immediately assume a circular appearance. It is clear that the determination of the precise moment of contact is more difficult than it was expected to be at the time of the invention of this method by Halley. For these reasons the results of the transit of 1874 were looked forward to with the greatest interest. The observations were on the whole successful, though some perplexing difficulties arose. At several places the black ligament was not seen, but a faint thread of light of sensible width, due to the atmosphere of Venus, presented itself. This unexpected appearance introduced an element of uncertainty as to the exact moment of contact. Two hundred and sixteen photographs were taken, but their examination did not realise all that had been hoped for from them. On the return of the several expeditions, the reduction of the observations was proceeded with under the scrutiny of Captain Tupman. The immense labour of these calculations delayed the publication of the complete results until June, 1881, nearly seven years after the event, though a preliminary report was made to the House of Commons in 1877.

In the 'Phil. Trans.' for 1872 there is a curious determination of the magnetic state of the iron in the Britannia and Conway tubular bridges. This investigation was suggested by the peculiar tremor of the iron felt by the hand when a train was passing. Both Sir George Airy and Mr. Stephenson expressed this by saying that the metal seemed to be in a state of "molecular shiver." As all experiments show that iron in this state of tremor is peculiarly subject to the inductive action of external magnetic force, Sir George thought that observations made along the axis of the tube might lead to some important conclusions. One general result was that "in the axis of a rectangular tube, at all points except very near the ends, the action of external magnetic forces in planes normal to the axis is absolutely destroyed." Some strange anomalies were observed in one tube which could not be explained until it was remembered that that tube had had a fall while being raised into its place. Thus the effects of an accident were discovered a quarter of a century after it had occurred by a magnetic experiment.

The system of time signals by which Greenwich time is spread over the country by means of the electric telegraph is in great part due to Sir G. Airy. It should be noticed that the whole system is automatic; the Greenwich clock being once set each morning to the exact time, the signals are distributed without any person having to touch the apparatus.

It is impossible to consider in detail the numerous additions which Mr. Airy has made to our knowledge at various times. It is necessary to pass over with merely a mention such an important memoir as that in which he discusses theoretically the stresses in the interior of a rectangular beam. In another paper he mentions a method of correcting the chromatic dispersion of the atmosphere in observing transits of Venus. He has also written several papers on the comparison of Earth currents and magnetic disturbances, on the diurnal and annual inequalities of terrestrial magnetism, and on some lunar magnetic inequalities. We must hasten on to his last work.

In 1874 the Astronomer Royal brought before the notice of the Astronomical Society a new mode of treating the lunar theory. After giving a rapid survey of the methods hitherto employed, he remarks that the nature of the steps has compelled the investigators to decide the succession of their terms, not by numerical magnitude, but by algebraical order, and that this has produced great inequality of convergence. The mental labour cannot be alleviated by an amanuensis, and he quotes a remark of M. Plana, "Quelquefois ces calculs me font presque perdre la tête." He proposes, as a new method, to begin with Delaunay's final numerical expressions for the longitude, latitude, and parallax with a symbolical term attached to every number for contingent correction. These corrections are so small that it is sufficient to retain only their first power. The expressions are then substituted in the equations of motion with the time for independent variable, and the result of the substitution is a great number of equations for determining the numerical values of a great many small quantities. In this theory the orders of the terms are numerical and equally accurate throughout; the details are so easy, that a great part can be intrusted to a mere computer. Though he was then seventy-three years of age, he had already begun the work. He says that, though it is sufficiently possible that he may not be able to complete it, he desires to have it in such a state that a successor may be able to take it up. For this reason in each of the following years he gives further details as to the theory, and describes how far he had advanced in the approximations. Finally, in 1886 the numerical lunar theory was given to the world in the form in which it was left by the author; a wonderful monument of what a man can do at the age of eighty-five. He explains in the preface how the work was delayed by the heavy pressure of business, not only in the ordinary conduct of the Observatory, but also in completing the calculations for the transit of Venus in 1874, and in preparing for that in 1882. He then remarks on some serious discordances which remain to be accounted for; "I cannot conjecture," he adds, "whether I may be able to examine sufficiently into this matter." He never was able.

A long catalogue of the honours and titles which Sir G. Airy

received from the universities and other scientific bodies is given after his name in the List of Fellows of this Society. He was D.C.L. of Oxford, LL.D. of Cambridge and Edinburgh, one of the first members of the Senate in the University of London. He was one of the eight foreign Associates of the Institute of France, and received the Lalande Medal. He was made K.C.B. in 1872. In 1875 he received the freedom of the City of London, enclosed in a gold casket. He was a Fellow of the Royal Society for fifty-five years, received both the Copley and Royal Medals, and was made President in 1871. He was on the Council of the Astronomical Society for more than fifty years, was five times President, and received two gold medals. He was President of the British Association at the meeting at Ipswich in 1851. He was an honorary member of the Institution of Civil Engineers, and received the Telford Medal. He was elected a foreign Associate of the Dutch Academy of Sciences in 1878, and held honorary titles from many Continental and American Societies. He also received the Albert Medal of the Society of Arts, which was presented to him by the Prince of Wales. He was a member of the Royal Society of Edinburgh and of the Royal Irish Academy. He received a diamond snuff-box from the Emperor Nicholas of Russia, and decorations from the Emperor of Brazil. He was Chevalier of the order "Pour le mérite" of Prussia; he belonged to the Legion of Honour of France, and had decorations from Sweden. A gold snuff-box was given to him by the Steam Navigation Company, a silver-gilt inkstand by the River Dee Company, and he held a French Sèvres vase as a Commissioner on Standards.

When Sir George Airy retired from the Observatory at the age of eighty, the Board of Visitors recorded in their Proceedings a resolution expressing, in an emphatic manner, their sense of his eminent services throughout the long period of forty-five years during which he had presided over the Observatory. Among his many services to science they especially mentioned the following:—(a) The reorganisation of the Observatory; (b) the designing of instruments of exceptional stability and delicacy; (c) the extension of the means of making observations on the Moon in such parts of her orbit as are not accessible to the transit circle; (d) the investigation of the effects of iron ships on compasses; (e) the establishment of time signals. Turning next to his labours in departments of science not directly connected with the Royal Observatory, they called attention to the high estimation in which his contributions to the theory of the tides, to the undulatory theory of light, and to various abstract branches of mathematics are held by men of science throughout the world.

During his residence at Cambridge Sir George married Richarda, the eldest daughter of the Rev. Richard Smith, of Edensor, in Derbyshire. Lady Airy died in 1875, after a long illness. After his

retirement Sir George received a pension from the Government, and resided close to Greenwich Park, and not far from the scene of his former labours. Here, on his ninetieth birthday, he held his last reception. It was attended by a numerous and distinguished company, among whom was one old friend even older than himself.

Sir George owned a cottage at Playford, near Ipswich, to which he often retired for rest and recreation. Here he had spent his boyhood, and here at last he was laid in the grave by the side of his wife. Visiting the village every year, he could remember five generations of more than one family, and could give the early history of most of the others. The last scene of his life may be said to have begun and ended here. On his last visit he had a fall, which in his enfeebled state proved more serious than might have been expected. He never properly rallied, but gradually sank and passed away at his residence in Greenwich. His funeral at Playford Church was quiet and simple, fitting the noble simplicity of his life.

E. J. R.

EDMOND BECQUEREL.—In the long and brilliant roll of physicists of whom France may justly boast, few names deserve a more prominent position than that of Becquerel. The Becquerel family constitutes a true dynasty of *savants*, and affords a twice repeated instance of father and son engaged in the self-same studies, and seated side by side in the same identical section of the Academy of Sciences of Paris.

The second of the family, Edmond Becquerel was born in 1820, and grew up, as remarked in the official *éloge* delivered by M. Duchatre on the occasion of his death, in a scientific atmosphere. From his illustrious father, Antoine César Becquerel, he evidently inherited his acute power of observation, and that "infinite capacity for taking pains," which seems to be the essential characteristic of the Newtons, the Faradays, the Darwins, and, in short, of all the great leaders of science.

While thus born a *savant*, the cause which determined Edmond Becquerel to become a physicist must be sought in the example and the society of his father Antoine, first as a pupil, then as assistant. Under different circumstances he might have become equally eminent as a chemist or as a student of the organic sciences.

As he approached manhood, the discovery of photography burst upon the world, and naturally drew general attention to the chemical action of light. Edmond Becquerel found here his opportunity, and studied with zeal and success the conditions and laws of the novel phenomena. His papers on the chemical radiations accompanying solar and electric light, and on their effects, were of distinguished value. He even succeeded in obtaining a photographic repro-

a twice