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OBITUARY NOTICES

OF

FELLOWS DECEASED.

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(Continued.)

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SIR DIETRICH BRANDIS, 1824—1907.

By the death of Sir Dietrich Brandis, which occurred at Bonn, on May 28, 1907, a man of world-wide renown has been removed. He was born on April 1, 1824, at Bonn, being the son of Dr. Christian Brandis, Professor of Philosophy in the University of that place. As a boy, he followed his father to Greece, where he spent several years, and thus came into touch at an early age with the life and customs of the nearer East. On his return from Greece he was educated at the Universities of Copenhagen, Göttingen, and Bonn. In 1849 he established himself as "Privatdocent" on Botany at Bonn. While he thus started life as a Botanist, during his Botanical excursions his attention was soon turned to questions connected with the management of forests.

After the occupation of the Province of Pegu, in Burma, Lord Dalhousie offered Brandis the appointment of Superintendent of the Pegu teak forests, an offer which he accepted. He landed in India in 1856, and a year afterwards the rest of the Burma forests was placed under his charge. Brandis proceeded from Calcutta, where he had an interview with Lord Dalhousie, to Rangoon in a separate vessel from that which conveyed his herbarium and botanical library. The latter was wrecked in the Rangoon River, and Brandis thus lost his herbarium and books. He looked upon this almost as a direction to his future line of action. While he never quite abandoned botanical studies, he devoted for years his main energies to mastering the science and practice of Forestry. It was only thirteen years later that he resumed earnest botanical studies for the space of three or four years, and he then again returned to Forestry for a further period of nine years. After his retirement he occupied himself once more chiefly with botanical work.

From 1856 to 1862 Brandis worked indefatigably to bring the forests of Burma under systematic management. During these years a great conflict raged between the merchants of Burma and the Government, the former maintaining that the supply of teak timber from the forests was inexhaustible, and that, therefore, State interference was unnecessary. Brandis, supported by the Commissioner of Pegu, Major (afterwards Sir Arthur) Phayre, held different views. After a long continued struggle the forests were placed under systematic management, and they, with those of Upper Burma, are now the chief supply of teak timber to the world. If Brandis had done nothing else than save the Burma teak forests from destruction, he would have deserved well of the British Empire and the world in general.

In 1862, he was called to Simla at the suggestion, it is believed, of Dr. Cleghorn, one of the principal pioneers of forest conservancy in India, to advise the Government on forest matters in other parts of the country, and in 1864, he was appointed the first Inspector-General of Forests to the

Government of India. He then set to work to introduce systematic forest management on scientific lines throughout India. A regular department was created, and a forest law was passed which provided for the demarcation and management of the State forests. Brandis travelled from one end of the Bengal Presidency to the other, advising and organising the department. He also visited the Bombay Presidency twice, and he spent two years (1881—83) in Madras on special duty.

When he first started operations, he had to do with what staff he could lay his hands on, but he determined to obtain one fit to deal with the requirements of the case. There were already a few military officers in the Department, some of them medical men, and he began by inducing others to join. Some of these did excellent service, and they gave tone to the new Department. In 1866, while on sick leave in England, he obtained the sanction of the late Marquis of Salisbury, the Secretary of State for India, to educate young Englishmen in Continental forest schools, partly in France and partly in Germany. Under this system of training, which lasted until 1886, a number of distinguished Forest officers were supplied to India.

About the year 1881, a movement was set on foot to arrange for the education of Indian forest officers in Britain, and this led, in 1885, to the establishment of a School of Forestry in connection with the Royal Indian Engineering College at Coopers Hill. Brandis, who had then retired, was, of course, consulted about this move, and he did not approve of it, considering it to be premature. After some years, however, he agreed to superintend the practical training of the students on the Continent, from 1888 to 1896, when his functions ceased. On the closure of Coopers Hill, the forest branch was, in 1905, transferred to the University of Oxford. But Brandis went a step further. In 1878 he started a forest school at Dehra Dun for the training of natives of India, which now sends annually from forty to fifty trained executive officers into the Service. By these combined means a trained staff of 200 Englishmen and more than 1,000 Indians has been obtained which, assisted by about 10,000 subordinates, manages the Indian State forests, which comprise an area of 239,000 square miles, equal to one-fourth of the area of British India.

The results of Brandis' work in India are very remarkable. The supply of timber, firewood, and a variety of other produce to the teeming millions of India has been placed on a satisfactory footing; the productiveness of the forests is increasing year by year; the more important areas are protected against jungle fires, and the net revenue from the forests has risen from £40,000 in 1864 to £660,000 in 1904, although produce valued at a similar sum is given annually free of charge to the people of the country.

Brandis was equally interested in the indirect effects of forest vegetation. He started experimental stations at Dehra Dun and in Central India, where observations were made to test the effects of forests on temperature, humidity of the air and the soil, and the preservation of mountain slopes. His interest in the subject is testified by the fact that he was the first to compile a rainfall map of India, in 1871. It has been improved by subsequent observations, but as regards the main points it holds good to this day. The map served to show clearly the relation between the rainfall and forest vegetation in the several parts of India.

Brandis practically relinquished the post of Inspector-General of Forests in India in 1881, when he proceeded on special duty to Madras. He retired finally in 1883. On that occasion the Government of India acknowledged his services in the most complimentary terms, granting him not only a special pension, but a substantial gratuity in recognition of his specially meritorious services. He had been created a Companion of the Indian Empire in 1878, and he was promoted to a Knight Commandership in 1887 for his services in India, as soon as that grade was added to the Order.

Apart from British India proper, Brandis did all he could to encourage better forest management in the Native States. He had a considerable share in the development of systematic forest management in many of the Colonies by advising the Government of India to lend competent officers for service in various parts of the Empire, and by advice. After his retirement from India he continued to show uninterrupted interest in his great work by articles published in the 'Indian Forester,' and by letters of advice to his numerous friends in India.

As already mentioned, Brandis supervised the practical instruction on the Continent of the Coopers Hill Forest Students from 1888 to 1896. During that time, and afterwards, he also guided the studies of a number of young Americans, who have since established a great Forest Department in the United States dealing with State forests covering more than one hundred millions of acres. His influence in this respect has been so great that President Roosevelt, about a year ago, sent his picture to him with the following inscription: "To Sir Dietrich Brandis, in high appreciation of his services to forestry in the United States. From Theodore Roosevelt." Thus Brandis has left his mark upon every continent of the earth. As regards this country, his name will go down to posterity as the founder of systematic forest management in the British Empire.

Brandis was not only a great administrator, but also a scientific man of a high order. During his career in India he wrote an endless number of reports and papers, and in 1872—74 he interrupted his forest work by writing the "Forest Flora of North-west and Central India," a work so highly thought of that he was elected a Fellow of the Royal Society in 1875.

The last eight years of his life were devoted by him to the writing of a general Indian forest flora, which he published in 1906 under the title of "Indian Trees," a monumental work which is likely to be the standard book of reference on the subject for another generation.

Of other publications, the following may be mentioned :—

1. "Vegetation and Country from Narkanda to Pangi." Simla, 1879.
2. "The Ringal of the North-western Himalaya." 1885.
3. "Die Nadelhölzer Indiens." 1886.

4. "Wall Pictures to Illustrate the Minute Structure of Plants." Simla, 1886 and 1887.

5. "Three Families of Plants in Engler and Prantl's 'Die natürlichen Pflanzenfamilien.'" 1894.

6. "Dipterocarpaceæ." Brandis and E. Gilg, 1894.

7. "Geographische Verbreitung der Dipterocarpaceen." 1897.

8. "Ueber die geographische Verbreitung der Bambusen in Ostindien." 1896 and 1897.

9. "Biological Notes on Indian Bamboos." 1899.

10. "Anbau der grossen Bambusen in Deutsch Ostafrika." 1899.

11. "On Some Martaban Bamboos." 1906.

12. "Remarks on the Structure of Bamboo Leaves," in the 'Transactions of the Linnean Society.' 1906.

13. "The Spruce of Sikkim and the Chumbi Valley." 1906.

Brandis was a Fellow of the Royal Society, the Linnean Society, and the Royal Geographical Society, and LL.D. of the University of Edinburgh; an Honorary Member of the Royal Scottish Arboricultural Society, the Society of American Foresters, and of the Pharmaceutical Society of Great Britain.

He had scarcely completed his great work, "Indian Trees," when he fell ill. After a painful illness of several months' duration he died at the age of 83 years, thus bringing to a close a most remarkable life filled with work from beginning to end, which only his iron constitution enabled him to achieve. He never spared himself, and he was always a warm friend of those associated with him in his work. For the natives of India he was full of sympathy, and he did all he could to advance their education and fit them to partake in the administration of the country, and more particularly of the Forest Department. He married, in 1854, Rachel Shepherd, a daughter of Dr. Marshman, of Bengal. She accompanied him to India, where she died in 1863. In 1867 he married Katherine, daughter of Dr. Rudolph Hasse, of Bonn, who survives him. He left three sons and one daughter.

W. S.

SIR WILLIAM HENRY BROADBENT, Bart., K.C.V.O., 1835—1907.

Sir William Henry Broadbent, born in 1835, was the son of John Broadbent, of Longwood Edge, Huddersfield. He married, in 1863, Eliza, daughter of Mr. John Harpin, of Birks House, Holmfirth, Yorkshire, by whom he had two sons and three daughters. His two sons, Dr. John Francis Harpin Broadbent, who succeeds his father in the baronetcy, and Dr. Walter Broadbent, are both members of the medical profession.

Broadbent had a distinguished career as a student of medicine and afterwards as a great physician. He took a distinguished place in the medical world not only as a practitioner, but as an original investigator of difficult medical questions, and of physiological subjects bearing on the science and practice of medicine. He had the originality of a man who thought for himself; originality shown in finding out new things and not merely in developing a little further the discoveries of others. His professional success was great and well deserved.

In 1892 he was appointed physician-in-ordinary to H.R.H. the Prince of Wales, now King Edward VII. Next year a baronetcy was given him, and in 1901 he was made a Knight Commander of the Victorian Order. Nor were honours of this kind limited to those conferred in this country; he was invested with the Grand Cross and Insignia of the Legion of Honour. In 1898 he was appointed physician-extraordinary to Queen Victoria, and, later, physician-in-ordinary to King Edward.

Besides these honours he received numerous academic distinctions, including the honorary LL.D. of the Universities of Edinburgh, St. Andrews, and Toronto, and the honorary D.Sc. of the University of Leeds. He was elected a Fellow of the Royal Society in the year 1897.

That Broadbent was a man of high intellectual power and of great nobility of character is well known to his friends, and is evidenced by his career and also by the nature of the original medical and physiological work he did. His contributions to medical literature cover a wide and varied field. He was deeply interested in the study of the problems of diseases of the nervous system. One of his earliest publications dealing with this subject was an important paper entitled "The Sensori-Motor Ganglia and Association of Nerve Nuclei," in which he enunciated his memorable "hypothesis" explaining the immunity from paralysis of bilaterally associated muscles in hemiplegia. In 1869 he published a paper on "The Structure of the Cerebral Hemisphere," in which he gave a description of the course of the various groups of nerve fibres in the cerebral hemisphere, based on a series of careful dissections which he had been carrying out for some years. In a lecture on "The Theory of Construction of the Nervous System," delivered at Wakefield in 1876, he referred to these dissections, and gave a lucid exposition of his views on the mechanism of speech and thought, and the problems of aphasia, a subject in which his interest was maintained throughout his life. To this his papers on "A Case of Amnesia," on "A Particular Form of Amnesia," "Loss of Nouns," read before the Royal Medical and Chirurgical Society in 1878 and 1884 respectively, and an article in the 'British Medical Journal' as late as June 15 this year, on "Some Affections of Speech," bear witness. Indeed, at the time of his death he was engaged in writing a treatise on "Aphasia." Amongst other publications dealing with diseases of the nervous system, were "Remarks on the Pathology of Chorea," published in the 'British Medical Journal' in 1869, the Lettsomian Lecture on "Syphilitic Affections of the Nervous System," delivered before the Medical Society of London in 1874, and papers on "Ingravescent Apoplexy" and on "Alcoholic Spinal Paralysis," read before the Royal Medical and Chirurgical Society in 1874 and 1884. In 1866, the year in which he published his "Hypothesis," he contributed an article on "Prognosis in Heart Disease" to the 'British Medical Journal.' In 1884, before the Harveian Society of London, he gave the Harveian Lectures on "Prognosis in Valvular Disease of the Heart." In 1887, at the Royal College of Physicians, he delivered the Croonian Lectures on the "Pulse," and in 1891 the Lumleian Lectures on "Structural Diseases of the Heart from the Point of View of Prognosis." These lectures constituted the foundation of a work on "Heart Disease," published in 1896, in the preparation of which he was assisted by his son, Dr. John Broadbent, and of which a revised and enlarged edition appeared in 1906.

His early interest in the scientific application of therapeutics was shown by a paper entitled, "An Attempt to Apply Chemical Principles in Explanation of the Action of Remedies and Poisons," published in 1869; and the line of thought in this was followed out in later years in an address on "The Remote Effects of Remedies," read at the annual meeting of the British Medical Association in 1886, and in his Presidential Address delivered before the Clinical Society on "The Relation of Pathology and Therapeutics to Clinical Medicine." Amongst other notable contributions to the literature on therapeutics is the Cavendish Lecture on "Some Points in the Treatment of Typhoid Fever," which he delivered before the West London Medico-Chirurgical Society in 1894.

Sir Thomas Barlow has observed that, in a remarkable paper submitted several years ago to the Royal Medical and Chirurgical Society, but not published in its 'Transactions,' Broadbent anticipated the development of pharmacology on the lines of chemical affinities of the elements.

Broadbent has left a record of splendid work done in the advancement of neurology. The value of what he did towards the elucidation of different problems presented by cases of aphasia is universally acknowledged. This is not the place to give details of any of his researches. I shall limit my remarks to the wide bearings of a great principle he established, to what is known as "Broadbent's hypothesis." This principle has brought method into the analysis of complex symptomatologies of some very different nervous maladies. More than forty years ago ('British and Foreign Med. Chir. Review,' April, 1866), he published an article on "the Bilateral Association of Nerve Nuclei to the Higher Centres." These words, his latest but not his best, deliverance on the subject, are from a lecture he delivered before the Neurological Society, published in 'Brain,' No. 103, p. 347, 1903. In that lecture he remarked that: "The principle is that when muscles on the two sides of the body always act together, their nuclei, situate in opposite sides of the cord, are so closely associated by commissural fibres as to be practically one nucleus." Whatever modifications and corrections of details have been required, what is essential in the principle which the hypothesis embodies has not been invalidated. Thus, in a case of ordinary hemiplegia—say right—owing to disease of the left half of the brain there is loss of power of the right limbs, that is, of those parts of the body which are most nearly unilateral, and, as we may say, "most voluntary," in their actions; but there is no, or very little, disability of the intercostal muscles of either side of the chest. Nevertheless, there is loss of many *movements* of the intercostal *muscles* of *both sides*, that is to say, there is paralysis *in the sense of loss of movements* of the intercostal muscles of both sides, but without disability of these muscles. The seeming paradox in this statement disappears when we reflect that, as Broadbent told us long ago, in such a case the intercostal muscles of both sides of the chest remain represented in another set of movements in, and are still empowered by, the undamaged right half of the brain; there is almost complete compensation by the right half for the effects of the destruction lesion in the left half. The truth of this hypothesis is demonstrated in two ways. In some cases of what may here be called Rolandic epilepsy (it is sometimes called "cortical epilepsy") there is from a local lesion of the motor region of the cortex cerebri of one half—say, left—of the brain occasionally convulsion of the limbs of the right side of the body and of both sides of the chest. In the facts of hemiplegia contrasted with those of the case of Rolandic epilepsy there is a striking verification of Broadbent's hypothesis. From a destruction lesion of part of one half of the brain there is no, or very little, obvious disability of the intercostal muscles of either side, whilst from a discharge lesion of a part of the cortex (a part belonging to the same anatomico-physiological system as that part which is the seat of a destruction lesion in hemiplegia) there is great spasm of the intercostal muscles of both sides. Another, a third, confirmation of the hypothesis is given by so-called pseudo-bulbar paralysis in cases of this malady there is a double cerebral destruction lesion, causing great disability of bilaterally acting muscles of a certain region of the body—of both sides of the tongue, lips, and palate. A destruction lesion of the left half of the brain only causes very slight, almost no, disability of the bilaterally acting parts mentioned, compensation being for the effects of that one-sided lesion practically complete. But when that compensation is lost from a lesion of the right half also, there is very great disability of the bilaterally acting parts mentioned. Speaking more at large, it follows from Broadbent's hypothesis that "double hemiplegia" is more than the double of hemiplegia.

So much for three applications of the hypothesis. There is another one of great importance to which allusion may be made. The hypothesis "leads ('Brain,' *op. cit.*) to the conclusion that words are represented in the right as well as in the left hemisphere." (Broadbent, 'Brain,' *loc. cit.*)

From what has here been said, it will be seen how fundamental and of what wide application Broadbent's hypothesis is. This basic contribution to neurology has lasted forty years, and is still not only valuable for the explanation of certain neural symptomatologies, but is also fruitful in its indications for further research in medical neurology. Moreover, the writer thinks that it and deductions or inferences from it, will be found of great value in the study of still larger problems than those here dealt with, such as investigations into the physiology of the organism, when that physiology is considered as especially corresponding to psychology, both to the psychology of the sane and of the insane. J. H. J.

NOTE.—Much of this Obituary is taken from an article, part of which was contributed by the author, in the 'British Medical Journal,' July 20, 1907.

WILLIAM TENNANT GAIRDNER, 1824—1907.

William Tennant Gairdner was born in Edinburgh on November 8, 1824 and he died in the same city on June 28, 1907. Descended from an Ayrshire stock, he was the son of Dr. John Gairdner, who was for many years a well known medical practitioner in Edinburgh. His mother was Susanna Tennant, a granddaughter of the Rev. Dr. Dalrymple, of Ayr, the "Dalrymple mild" of Robert Burns. Gairdner was educated at the University of Edinburgh, where he graduated in 1845. The teachers who appear to have influenced him most were William Alison, the physiologist, and Robert Christison, the most distinguished pharmacologist of his day. After a short sojourn in Italy, in the company of Lord and Lady Beverley (afterwards the Duke and Duchess of Northumberland), he returned to Edinburgh, was appointed one of the resident assistants in the Royal Infirmary, pathologist to the Infirmary in 1848, obtained wards in 1853, and became an extra-mural lecturer on Practice of Medicine in the same year. He was elected to the chair of Practice of Medicine in the University of Glasgow in 1862, and this office he held till his retirement in 1902. In 1863, Gairdner was appointed the first "Medical Officer of Health" for Glasgow, a position he held for nine years. He was President of the British Medical Association in 1888, became a Fellow of the Royal Society in 1893, and, in 1898, Queen Victoria honoured him by creating him a Knight Commander of the Bath. He also represented his University on the General Medical Council from 1893 to 1902. He was President of the College of Physicians of Edinburgh during the years 1893–5. In 1870 he married Miss Helen Bridget Wright, of Norwich, who, with several sons and daughters, survives her husband.

In his long and varied career Gairdner engaged in many kinds of work, and the record of his life must, therefore, present various aspects, according as we view him as pathologist, clinical physician, sanitarian, and man of letters. In the early Edinburgh days, while Pathologist to the Royal Infirmary, he devoted special attention to the pathology of the kidney, and, in particular, gave an early description of the waxy kidney. About 1850, he investigated the pathological changes in bronchitis, and more particularly diseases of the lung associated with bronchial obstruction. He enunciated a theory of emphysema, accounting for the changes in the air cells of the lung in that condition by the force of the inspiratory instead of the expiratory act. This theory has not been generally adopted. But Gairdner's contributions to pathology were not so much in the form of specific investigations as in the general trend of his clinical work. Every case that came before him was subjected to the most rigid scrutiny, not merely in its clinical aspects, but in the verification and correction of these by the facts of the *post-mortem* theatre. In the case books of the Royal Infirmary of

Edinburgh and of the Western Infirmary of Glasgow, there are painstaking records of the most valuable description, monuments of industry, insight, and method.

As a clinical physician he belonged to the first rank of workers. In the old days this part of investigation, especially as regards the teaching of students, was carried on in a very loose fashion. The physician often passed from bed to bed in the wards, followed by a crowd of students, who were left to gather wisdom and knowledge as they could from the oracular utterances of the wise man. There was no systematic investigation of cases nor any attempt at methodical teaching of the student. The first great improvements were no doubt initiated by John Hughes Bennett, who, fresh from the cliniques of the Parisian hospitals, was the first in this country to teach the student at the bed side, by causing him to observe the facts of the case, and to discuss, with the teacher, all its features. Gairdner readily took up the same course of procedure. More thorough and philosophical than Bennett, he went more deeply into the case, and worked out what may be termed its natural history. He disliked clinical pictures. He disliked the practice of drawing up an imaginary schedule for each disease with a space for each sign and symptom, which had to be filled in whether it happened to be in the case under discussion or not. His method was rather to study each individual case and to closely scrutinise each symptom, to note those belonging to the typical form of the disease that were absent in this particular case, and to draw inferences with care and precision. It was the scientific method that impressed the student and trained him to be an observer. Gairdner always attracted to him the better type of men, and many of his pupils became, in their turn, able pathologists and physicians. In practical therapeutics he also did valuable work. He was one of the first to check the over-stimulating mode of treatment in fevers and pneumonia, and when he did use medicinal substances they were employed in a simple form. He never wrote a "grape and canister" prescription containing half a dozen ingredients. He had no great faith in drugs, nor in specially vaunted remedies, or modes of treatment. There must be, in his view, a good reason for the employment of a particular remedy and he was not guided by empirical considerations, except to a very limited extent. As a consultant, therefore, he was often somewhat disappointing, as he was more interested apparently in the clinical history of the case and in its pathology than in the treatment. He had a strong view that in many cases natural processes would remove the disease, while, in others, so-called remedies were of little avail.

One of Gairdner's chief claims to distinction is the splendid work he did as the first medical officer of health of Glasgow. During the first half of the nineteenth century, and closely connected with the industrial revolution that followed the introduction of machinery and the factory system, many large towns rapidly increased in population and unsanitary conditions of life were met with everywhere. In no city was this more conspicuous than in

Glasgow. In 1801 the population was 83,805; in 1811, it had risen to 110,400; in 1821, to 147,043; in 1831, to 202,426; in 1841, to 280,692; and in 1851, to 347,001. In 1818, there was a severe epidemic of typhus; in 1832, this was repeated, with, in addition, an epidemic of cholera, and a death-rate of 46 per 1000. In 1837, there was another typhus epidemic; 1843 brought an epidemic of relapsing fever; 1847 had a typhus epidemic with a death-rate of 56 per 1000; 1848–9 was visited by a second cholera epidemic, in which 3772 deaths occurred; 1851–2 showed more typhus; and 1853–4 had a third cholera epidemic with 3885 deaths, and a death-rate of 42 per 1000. It is now difficult to realise exactly what those figures mean, and yet this must be attempted if we wish to understand the real state of matters. Thus, in 1837, the population of Glasgow was 253,000, the death rate was 41 per 1000, and the number of deaths from "fever" was 2180, or 8·6 per 1000. At least 21,800 persons suffered from the disease during that year. In 1847, the number of persons affected by fever must have been about 45,000. If we wish to contrast this with the state of matters in Glasgow at the beginning of the present century, we may take 1901, said to be the worst epidemic year of recent years.* In 1901, the population of Glasgow was 761,712; the general death-rate was 20·6; the number of deaths from fever was 220; the number of cases of fever notifiable, 1385; and the number of deaths from all infectious diseases 3416, or 4·4 per 1000 living. The total number of cases of infectious diseases registered during the year was 21,145, or less than the number believed to have suffered from "typhus" fever alone in 1837.

The cause of the terrible state of matters that prevailed up to the passing of an important municipal act in 1862 was undoubtedly due to overcrowding in insanitary dwellings, to the entire absence in the poorer parts of the city of even the most obvious sanitary appliances, and to the want of hospitals for the segregation of the sick during an epidemic. In January of 1862, Gairdner was chosen to be the first medical officer of health, and five district surgeons of police were appointed his assistants. At the same time, a "special non-medical inspector" represented the entire sanitary staff. The first sanitary office was a room measuring fifteen feet by ten feet. These rudiments of a sanitary department soon developed. Disinfecting and washing houses were established, nuisances were removed, careful inspection was made of specially insanitary districts, committees were formed for special purposes, intra-mural burial grounds were closed, and a hospital for fever was founded at Belvidere, and became in course of time one of the finest fever hospitals in the country. Year by year the sanitary machinery was improved and in particular the danger of overcrowding was combatted

* These figures have been obtained from a paper from the late Dr. J. B. Russell, entitled, 'The Evolution of Sanitary Administration in Glasgow.' The facts regarding 1901 are given by the present medical officer of health of Glasgow, Dr. A. K. Chalmers, in a footnote to Dr. Russell's paper, as it appears in a volume entitled, 'Public Health Administration in Glasgow,' a memorial volume of the writings of Dr. J. B. Russell, who succeeded Gairdner as medical officer of health. See pp. 1, 4, etc.

by means of "ticketing" houses, that is, specifying on the door of each room the number of inhabitants who were permitted to dwell in it. During this period Gairdner wrote many important papers, on sanitary questions, on hospital management, on the training of nurses, on dietetics both as regards the healthy and the sick, and on kindred subjects. In 1871, he resigned the office of medical officer of health into the able hands of James B. Russell, one of his own most distinguished pupils, who carried on for many years the work of the Sanitary Department in Glasgow, which is now one of the most complete in the world. Its initiation owed much to Gairdner and he had the satisfaction of watching its progress.

Gairdner in many directions showed marked literary gifts. He was a good classical scholar, of a type not common now-a-days in the medical profession, and he read Virgil and Horace and the New Testament in Greek, as one of his almost daily pastimes. Now and again he published, in addition to works bearing specially on medicine, isolated lectures and essays, such as 'The Physician as Naturalist,' a well-known volume which is a key to his character. Always animated by a thoroughly scientific spirit, he took a wide view of the functions of a physician, as one whose duty it was to investigate the natural phenomena of disease, and uphold the dignity of his calling. In his writings, as in his conversation, one always felt conscious of his lofty ideals and of his transparent sincerity and honesty of purpose. It would not be right not to advert to another marked characteristic. He was a man of sincere piety who ever lived in his daily life under the shadow of the Unseen. Broad and catholic in his religious views, there was always present the spirit of reverence, a generous appreciation of the views and claims of others, a desire to view every question fairly and impersonally, and a high ideal of the nobility of life.

J. G. McK.

ROBERT WARINGTON, 1838—1907.

THE name of Robert Warington will ever be associated with one of the most important advances in the agricultural chemistry of the latter half of the nineteenth century, though his classical work on nitrification, which may be regarded as his life-work, bears but a small proportion to the total of that accomplished by him. He, no doubt, owed his chemical proclivities to his father—a Robert Warington also—who was a prominent figure amongst the chemists of earlier days. The elder Warington was one of the first chemical assistants at University College, and was subsequently appointed chemical operator to the Society of Apothecaries. He, also, was a Fellow of the Royal Society, and published several papers on chemical subjects; yet chemistry is more indebted to him for the part which he took in founding the Chemical Society than for the extent of his own original work. It was through his zeal and powers of organisation that this Society was founded in 1841, and the work which he did for it as its secretary during the subsequent ten years helped in no small measure to launch it on its prosperous career.

Robert, his eldest son, was born on August 22, 1838, in the parish of Spitalfields. His mother was a daughter of George Jackson, M.R.C.S., to whom science is indebted for several improvements in microscopes which have not yet been superseded, as well as for the invaluable ruled glass micrometer. The original dividing machine made by him for ruling the lines was still being used by a well known optician in 1899, and is probably in use at the present time.

Very early in young Warington's life his parents took up their residence at the Apothecaries' Hall, and it was here, in the uncongenial atmosphere of the city, that he spent his childhood and youth. His constitution was naturally feeble, and a life in the heart of London, with but little exercise, and no companions of his own age to assort with, did not tend to strengthen it. All through life he had to contend with a lack of bodily vigour, which rendered his work doubly laborious to him. For his education he seems to have been chiefly indebted to his parents. While still quite young, he studied chemistry in his father's laboratory, and had the advantage of attending lectures by Faraday, Brande, and Hofmann.

In consequence of the unsatisfactory state of young Warington's health, his father sought to get him some employment in the country, and, with that object in view, applied to Mr. Lawes, with whom he was acquainted, and for whom he had done some professional work. The outcome of this was that in January, 1859, the youth went to work in the Rothamsted Laboratory as Lawes' unpaid assistant. Here he remained for one year, devoting all his time to ash analyses, of which he had had no previous experience, and examining various methods for obtaining the most satisfactory results. Dr. Pugh and Mr. F. R. Segeleke were also working in the laboratory at that time, and they gave Warington valuable assistance in his work. Of the two series of analyses eventually completed, the first comprised those of the ashes of grass grown under different manurial treatment, the results of which were published in Lawes' and Gilbert's "Report of Experiments with Different Manures on Permanent Meadow Land,"* the second series was that of the ash of grain from Broadbalk Field. These latter analyses were never published, their place having been taken by more complete work on the same subject by Richter.

Although Warington left the Rothamsted Laboratory in January, 1860, his interest in the work there never ceased, and, until he resumed his connection with Lawes a few years later, he devoted much of his time to studying the Rothamsted results, and was a frequent visitor to the laboratory.

His health having been somewhat re-established by his year's residence in the country, he returned to town, and continued to reside with his parents till 1862, spending his days at South Kensington, where he worked, under Dr. Frankland, as research assistant. But at the end of this period, a further breakdown in health forced him again to seek a country life, and he betook himself to the Royal Agricultural College at Cirencester. Here he remained for four and a-half years, the first nine months of which were spent in doing analyses for Dr. A. Voelcker, and the remainder of the time in fulfilling the duties of teaching assistant under Professor Church.

It was during his residence at Cirencester that Warington published the first papers on scientific subjects which appear under his name. These were printed in the 'Journal of the Chemical Society.' The earliest of them (1863) dealt with the quantitive determination of phosphoric acid. This was followed by two other short communications on kindred subjects, which preceded and prepared the way for his first work of importance—an investigation into the part played by ferric oxide and alumina in decomposing soluble phosphates and other salts, and retaining them in the soil. The results of this investigation are embodied in a series of four papers read before the Chemical Society, and are typical examples of the careful work and close reasoning which characterised all Warington's researches. That ferric oxide acted as a fixing agent for soluble substances applied to a soil, was already known, but the action was attributed to an indefinable physical attraction, which explained nothing. Warington proved, first by experiments with pure ferric oxide, and then with ordinary soil, that the action in the case of calcium phosphate was simply one of chemical decomposition, resulting in the formation of ferric phosphate, whilst, in the case of other salts, such as carbonates, sulphates, nitrates, etc., the chemical character of the action was indicated by the fact that the iron did not retain the salt as a whole, but partially decomposed it, retaining the basic portion in excess over the acid portion.

Warington did not allow his work at Cirencester to sever his connection

* 'Journ. Roy. Ag. Soc.,' vol. 20, 1859, p. 407.

with Rothamsted, and he offered to analyse three of the most important of the animal ashes which had been prepared there, on the condition that he might make use of the results thus obtained. He consequently received mixed ashes representing the whole bodies of a fat ox, a fat sheep, and a fat pig, and an abstract of the analysis made by him appeared in an article which he wrote for the second supplement to "Watts' Dictionary of Chemistry." The analysis, together with others by Richter, were also published by Lawes and Gilbert in the 'Phil. Trans.,' 1883.

In 1864 Warington commenced lecturing to the students at Cirencester on the Rothamsted experiments, and went systematically through all the work which had already been published, together with many additions of as yet unpublished results which had been communicated to him by Lawes and Gilbert. A desire was expressed at Cirencester that these lectures should be published, and negotiations to that end were consequently opened with Lawes and Gilbert. The outcome of these was that Warington was to write a book on the Rothamsted investigations, Lawes guaranteeing him from pecuniary loss, but offering no remuneration. Lawes also reserved to himself the right to supply a preface to the book, on the ground that there would be previously unpublished matter incorporated therein. The writing of this book involved a large amount of labour, especially as, in studying the effect of manures in different seasons, Warington was led to recognise the almost paramount influence of the rainfall on the results, and its action in washing the nitrates out of the soil, an action up to that time unrecognised. For the purpose of examining this action more closely, he compared the results from the plots at Rothamsted with the temperatures and rainfalls supplied to him by Glaisher; at the same time he applied to Gilbert to furnish him with unpublished data respecting the Rothamsted hay crops. Gilbert, however, objected to what now appeared to him in the light of a publication of Rothamsted results by others than Lawes and himself. Discussions ensued, the upshot of which was that the book remained in manuscript, and the seeds of an unfortunate dissension between Gilbert and Warington were sown. Some 120 pages of this book were written (and are still in existence), but Warington declined the pecuniary compensation which Lawes offered to him for his labour.

Leaving Cirencester in June, 1867, he became chemist to Lawes' manure and tartaric and citric acid factories at Millwall, where he remained till 1876. During these years he generally had a long conversation every week with Lawes on those problems in agricultural chemistry which happened to be under investigation at the time, and which were evidently more congenial subjects of discussion to both of them than the problems arising in the factory. Even these, however, were by no means lacking in interest, and at the conclusion of his engagement at Millwall in 1874, Warington remained in the laboratory there for two years longer, working on citric and tartaric acids, and ultimately publishing his results in a paper of 70 pages in the 'Journal of the Chemical Society.' This paper was published with Lawes' approval, and it is noteworthy for the opinion expressed therein, that "the large amount of information acquired in the laboratories of our great manufacturing concerns might well be published without any injury to the individual manufacturer." Eighteen years later, when Warington had for a second time gone to work in Lawes' tartaric and citric acid factory, he published another paper dealing with these acids, and with the detection of the presence of lead in them. With this solitary exception, all Warington's subsequent work was on agricultural chemistry, and all of it was done in the Rothamsted laboratory.

While still at Millwall, he had been writing a good deal on agricultural subjects—several articles for "Watts' Dictionary" and for the Agricultural and Horticultural Co-operation Association—and he had, moreover, as already mentioned, been in continual consultation with Lawes as to the Rothamsted results; he was naturally, therefore, prepared to receive Lawes' suggestion that he should go and work in the Rothamsted laboratory. The terms were all settled, and had readily been assented to by Warington; for, although they had involved a reduction of salary to two-thirds of that which he had been receiving at Millwall, he obtained a certain amount of freedom by way of compensation. He was to be at liberty to publish his own work in his own name, provided that it made its appearance as Rothamsted work; but in cases where the work dealt with subjects which had already occupied the Rothamsted investigators, it was to be published in the joint names of Lawes, Gilbert and Warington. This arrangement, however, owing to some unforeseen difficulties, was not carried out; and it was not till after a delay of two years that Warington went to Rothamsted (in 1876), under an agreement for a year only, to work simply as Lawes' private assistant. The engagement was subsequently extended, and all his results were published, either in his own name or in the names of Lawes, Gilbert and Warington.

Before removing to Harpenden, he went to work at the laboratory at South Kensington in order to learn water and gas analysis under Frankland's assistant, some of the Rothamsted soils being sent to him for practising determinations of nitrogen. While there he devised a method of extracting soils by the vacuum pump, which method has since been largely used at Rothamsted. In the autumn of the same year (1876) he made a short tour among the German experimental stations, and then took up his residence for good at Harpenden.

The construction of a gas analysis apparatus (under Frankland's direction) for the Rothamsted laboratory occupied a considerable time, and, pending its completion, Warington made a study of the indigo method of determining nitric acid. This method, as generally used, he found to be full of sources of error. The principal of these he succeeded in correcting, and, with the method of determination, thus rendered trustworthy, he proceeded to determine regularly the nitrates in the drainage-water from the various wheat plots in Broadbalk field. The chlorides were determined at the same time. No such systematic work had been previously done; whilst the methods of sampling, which had been adopted when any analysis had been made, had been faulty. Warington now altered these methods, so that the samples analysed should faithfully represent the average composition of the drainage-waters.

Having examined the indigo method for determining nitric acid, he next examined the Crum-Frankland method by agitation with mercury; and, subsequently, the method of Schlœsing, modified, however, in such a way that the nitric oxide produced was determined by gas analysis. The exhaustive examination of these methods of analysis are described in a series of papers published in his own name in the 'Journal of the Chemical Society' and elsewhere, extending down to 1882. The modified Schlœsing method was the one which he finally adopted, and with it he began a long series of determinations of nitrates in soils, and in mangels, swedes, and potatoes.

Having satisfied himself as to the methods of nitrogen determination, he next turned his attention to those for the estimation of carbon, and having examined the permanganate and the bichromate methods, and found them wanting, he finally adopted the combustion method, which proved to be thoroughly satisfactory, provided that carbonates were entirely removed by prolonged treatment with sulphurous acid. In this work he was assisted by Mr. W. A. Peake, and the results were brought before the Chemical Society in the names of Warington and Peake.

Warington's results from the examination of the rain and drainage water, together with results previously obtained at Rothamsted, formed the subject of a very long report published in the names of the three investigators in the 'Journal of the Royal Agricultural Society' for 1882. The subject, however, continued to occupy Warington's attention long after this date, and we find a report on the subject in the three joint names in 1883, and papers by Warington alone in 1889 and 1887. The last-mentioned paper is an important contribution* to the study of well waters, and deals with the wells in the chalk formation on which Harpenden is situated. In later years (1904) Warington was enabled to give these results a practical bearing on the supposed contamination of the Harpenden water supply, and he saved the community, at any rate, for a time, from adopting an expensive, and, apparently, quite unnecessary system of sewerage.

So far, Warington's work, as here described, consisted largely of examining and perfecting methods of analysis for use in agricultural research. For this work the precision of his nature, and the carefulness of his manipulation pre-eminently fitted him, and most of the methods of analysis which he elaborated have been accepted as standard methods, which promise to remain in use for many years to come. The remainder of his work, however, is that by which he made his name, and, if a strictly chronological sequence of events had been followed, it should have been mentioned earlier in this notice, for it was in 1877 that he began to study nitrification, and this subject occupied the foremost place in his mind till 1891, when his opportunities for pursuing the subject ceased. During this period he published about ten

* 'Journ. Chem. Soc.,' pp. 500—552.

papers on the subject, all in his own name, the principal of which were four communications to the Chemical Society, bearing the title "On Nitrification," Parts I to IV.

That the natural conversion of ammonia into nitric acid was the work of an organism, had been suggested by A. Müller as early as 1873, but it had been reserved for Schlœsing and Müntz to establish definitely that this was the case. In 1877 they showed that, when sewage was allowed to percolate through a column of sand and limestone, the nitrification which occurred during its passage could be prevented by the presence of a sterilising agent such as chloroform vapour, and, after such sterilisation, the activity of the sand could be resuscitated by inoculating it with a few particles of vegetable mould. Questions affecting the problems connected with nitrogen in the soil had naturally been amongst those to which the Rothamsted investigators had, from the first, devoted themselves, and, consequently, they at once set to work to examine such an important observation as that of Schlœsing and Müntz. A complete verification of it was obtained by Warington, operating with garden soil only, and a solution of ammonium chloride, instead of sewage; and he was enabled to add the additional information, that nitrification occurred only in the dark. This paper appeared within a year of that of Schlœsing and Müntz. Two and a-half years later he published a second paper which added considerably to the facts already established. He showed that the nitrifying organism, besides requiring darkness in order to do its work, must also be supplied with food for its growth—potash, lime and phosphorus—and moreover, that all liberation of free acid must be prevented by the presence of some salifiable base, such as calcium carbonate. He found, also, that after the introduction of a small quantity of active soil or solution into a liquid capable of nitrification, no action occurred till a certain time had elapsed, this period of incubation being probably due to the organisms having to multiply to a certain extent before they become sufficiently numerous to produce recognisable results. An increase of temperature was found to favour the action up to a certain point, and it was shown that various vegetable moulds and known bacteria were not the organisms to which nitrification could be attributed. Many difficulties, however, still remained to be cleared up, notably the want of uniformity of the action, which resulted in the production of nitrates in some instances, and nitrites in others. We now know that the process is performed by two quite distinct organisms, and that their nutrition is, in some respects, wholly different from that of any other organism hitherto studied; but until this knowledge was gained, work on the subject was singularly difficult, and the results were very perplexing.

Warington's third paper on nitrification added considerably to our knowledge of the circumstances attending the action, and established the fact that the organisms are almost entirely confined to the first nine inches of ordinary soil. The distribution of the organism in the soil was dealt with still more exhaustively in a subsequent communication in 1887.

The prize coveted by the workers on this subject was, however, the isolation of one organism itself; and to prepare himself for this task Warington went to London for a time, in 1886, to learn bacteriology under Dr. Klein at the Brown Institution. From Dr. Klein he obtained a large number of pure cultures of various bacteria, and all these, as well as others obtained from his own experiments with soils, he examined as to their behaviour towards ammonia and nitrates, and also as to their mode of growth on skim-milk. The results were brought before the Chemical Society, and proved that none of the bacteria, except the nitrifying organism itself, possessed any appreciable power of nitrification. The majority of the organisms examined, however, were active denitrifiers. Denitrification—whereby nitrates are converted into nitrites, oxides of nitrogen, or even nitrogen gas—was, at this time, a well recognised work of micro-organisms, but was one which, naturally, enhanced to a considerable extent the difficulties met in elucidating the reverse phenomenon of nitrification. Warington's work added a good deal to our knowledge of the subject, and showed that denitrification is a property actively exhibited by a large number, but by no means by all, micro-organisms, and that in a soil it becomes complete, before the nitrifying organisms begin their task of reversing the reaction. An excellent account of the denitrification of farmyard manure was subsequently written for the 'Journ. Roy. Ag. Soc.,' 1897, vol. 8, Part IV.

Warington's work on nitrification was amply sufficient to establish the fact that the oxidation of ammonia in the soil was the work of an organism, but that organism seems to have been isolated first by Schlœsing and Müntz in 1879, though the method which they adopted left, at the time, considerable doubt as to its real identity. But even the isolation of this organism did not solve the whole problem: there was still the independent formation of nitrites and nitrates to be accounted for; and it was here that Warington's work was most conducive to a solution of the difficulties, for he succeeded in proving that one organism alone could not be held accountable for the various phenomena observed, and that two different organisms must be concerned in the process of nitrification. His success all lay in the chemical aspects of the subject. He was the first to obtain (1879) liquid cultures which converted ammonia into a nitrite, and preserved this power in all sub-cultures, but which was incapable of producing any nitrate; and shortly afterwards (1881) he obtained cultures which were able to convert nitrites into nitrates, but were unable to oxidise ammonia. This was a practical separation of two distinct organisms, but at the time Warington did not grasp the true meaning of his results, and he associated the change from nitrites into nitrates with a white growth which appeared floating in the liquid, but which really had nothing to do with it.

In 1890, after the work of others had resulted in the isolation of the nitrous organism (that which converted ammonia into nitrites), Warington returned to the subject, and found that the white surface organism could not be held accountable for the conversion of the nitrites into nitrates. He eventually succeeded in isolating the organism which really produces this change, and obtained a nearly pure culture of the nitric organism. At the same time he showed that organic carbon is not necessary for the growth of these organisms, as he had previously imagined, but that they can obtain their carbon from carbonates. These results were published in his fourth paper on nitrification (1891), and were communicated to the Chemical Society only a few days before Winogradski made a similar communication to the French Academy. Winogradski, however, had pushed the matter somewhat further, having obtained the organisms in bodily form, and having shown how they could be cultivated on solid media, a problem which had baffled Warington and other investigators. Warington, therefore, had to share his final hard-won success with another.

The practical results of nitrification in the soil were being investigated while the search for the organism was still in progress, and Warington began a long series of determinations of nitrates in the Rothamsted soil, the first results of which were published as a lecture given before the Society of Arts, for which he was awarded a silver medal.

When, in 1889, Lawes resigned his active control to the present Committee of Management, it was arranged that Warington should leave in the following January. Having, however, in the meantime, reached a very interesting stage in his work on the nitrifying organism, he stayed on at the laboratory till 1891, and succeeded in bringing the work on hand to a successful termination.

Although Warington's original work in agricultural chemistry was brought to a close on his severance from Rothamsted, much useful work remained for him to do. The Committee of Management appointed him American lecturer under the Lawes Trust, and he, consequently, proceeded to the United States to perform his functions. The six lectures which he there delivered dealt chiefly with the subject of nitrification, illustrated by his own work at Rothamsted. They were published by the United States Department of Agriculture.

On his return to England, Sir John Lawes invited him to carry out an investigation at his tartaric and citric acid factory at Millwall on the contamination of these acids by the lead of the vessels used in their preparation. This Warington undertook, and he succeeded in finding a method for obviating the evil. He obtained, in addition, an excellent method for the accurate volumetric determination of lead in the acid. This formed the subject of a communication to the Society of Chemical Industry in 1893, the last communication of any investigation made by him.

In 1894, he was appointed one of the examiners in Agriculture under the Science and Art Department, and in the summer of the same year he was elected Sibthorpian Professor of Rural Economy at Oxford for three years.

The papers, other than those on original investigations, which Warington wrote, are numerous, and are all characterised by a lucidity of expression and precision of argument which renders them specially valuable. One of the most useful of his writings is, undoubtedly, a little volume entitled "The Chemistry of the Farm." The amount of appreciation with which it has been received, and the good which it has done, may be measured by the fact that it is now in its fifteenth edition, and is accepted as the text book on the subject throughout the world, and as a model of what a text book of that sort should be.

Warington continued to reside in Harpenden till the end. His habits and tastes did not predispose him to take any active part in village management, but whenever he thought that his knowledge might be of service to the community he did not hesitate to give what assistance he could.

Educational or charitable work, however, always enlisted his sympathies, and engaged his active support; whilst his strong religious convictions, guided by his clear judgment and absolute sincerity, rendered his church and philanthropic work peculiarly valuable. He certainly had an unusually high sense of public duty, and, persistently throughout life, did what he could to make his fellow-creatures better and happier. Missionary work always held a prominent place in his heart, as also did the training of the young, whether in religious or secular subjects; and, during the last few years of his life, much of his time and care was devoted to the Church day-schools. He was greatly interested in all work amongst the poor and needy, and was a liberal supporter of any organised charity which appealed to his judgment. Partly owing to his isolated boyhood and youth, and partly to his lack of robust health, life went harder with him than it otherwise would have done, for the characteristics thus developed stood in his way, and often prevented his gaining the sympathy and appreciation which he was so ready to give to others.

Warington was elected to the Chemical Society in 1863, and to the Royal Society in 1886. He served for two periods on the Council of the Chemical Society, and for one period as vice-president. For many years he was on the library committee of this Society, and did much useful work for the Fellows during the reorganisation and cataloguing of the books. For this, his extensive acquaintance with chemical literature rendered him specially fitted.

Warington was married twice. His first wife was a daughter of G. H. Makins, F.R.C.S., formerly chief Assayer to the Bank of England, and one of the Court of Assistants at the Society of Apothecaries. His second wife was a daughter of Dr. F. R. Spackman, who had for many years been medical practitioner at Harpenden. He has left five daughters by his first wife. In 1906, his health gave way, and he had a serious illness which necessitated a very difficult and dangerous operation. For this he prepared with singular equanimity and courage. The operation was successful; but though he nominally recovered from it, he never regained his strength, and, eleven months afterwards (March 20, 1907), he passed away.

With the death of Warington, the first generation of great Rothamsted workers is brought to an end. Their work, whether published in the form of independent communications, or as joint productions, constitutes one great whole, of which the various parts are beautifully correlated and interdependent. It has placed agricultural science on an altogether different basis from that which it previously occupied, and the institution which gave birth to it has served as the prototype for similar institutions throughout the whole civilised world. The three men to whom we owe these results—Lawes, Gilbert and Warington—devoted their whole lives and energies to the work, and only those who are acquainted with the difficulties attendant on co-operation in this case can appreciate the devotion to science which was required to master these difficulties. All three workers now lie at rest in the same quiet country churchyard, their combined work in the cause of scientific agriculture forming the most fitting and enduring monument of their labours, for its importance becomes every day more and more evident with the development of the superstructure which is being raised upon it.

P. S. U. P.

WALTER FRANK RAPHAEL WELDON. 1860—1906.*

Walter Frank Raphael Weldon, the elder son of the late Walter Weldon, F.R.S., was born at Suffolk Villa, Highgate. We have no record if he attended a school there. When his parents removed to Putney he had, as tutor, a neighbouring clergyman. In 1873 he was sent to a boarding-school at Caversham, where he remained not quite three years, and from which, after some months of private study he matriculated, in 1876, in the University of London. In October of that year we find him at University College taking classes in Greek, English, Latin, and French, with two courses of Pure Mathematics. In the summer term of 1877, physics and applied mechanics were studied. During this whole session he also attended Daniel Oliver's general lectures on botany and Ray Lankester's on zoology. Later in the Christmas vacation of 1879, after he had gone up to Cambridge, he was for some weeks under Ray Lankester, who set him to work out the structure of the gills of the mollusc "Trigonia."

In the autumn of 1877 he transferred himself to King's College, where he stayed for two terms, attending classes in chemistry, mathematics, physics, and mechanics, besides the zoology course of A. H. Garrod and the biology of G. F. Yeo. Divinity under Barry, at that time compulsory, was also taken. At this time Weldon had the medical profession in view. Though only entered on the Register of Medical Students on July 6, 1878, there can be no question that his course, on the whole, was directed towards the Preliminary Scientific Examination of the London M.B. This examination he took in December, 1878, after he had gone up to Cambridge; he was coached for it by T. W. Bridge, now Professor of Zoology in Birmingham, but he had already completed the bulk of the work in his London courses. With the Preliminary Scientific, Weldon's relation with London University ceased. In 1877 he attended the Plymouth Meeting of the British Association, and there he was generally to be found in Section D.

The presence of a life-long friend, who had already gone to Cambridge, was, at least, one of the causes which led to Weldon's entering himself as a bye-term student at Cambridge; and probably his choice of St. John's College was due to Garrod's influence. He was admitted on April 6, 1878, as a pupil of the Rev. S. Parkinson, D.D.

At Cambridge Weldon soon found his work more specialised, and he rapidly came under new and marked influences. Under the inspiration of Balfour, Weldon's thoughts turned more and more to zoology, and the medical profession became less and less attractive. During the years 1879 and 1880, he worked steadily for his Tripos; in the first year he was given

* This notice is abstracted (by A. E. S.) from the much longer biography in 'Biometrika,' written by Professor Karl Pearson, with some help from Mr. A. E. Shipley.

an exhibition at St. John's. In the second year a little original investigation on beetles was started; in May he took, for a month, Adam Sedgwick's place, and demonstrated for Balfour.

The Tripos work was continued, in spite of ill-health, till the Easter of 1881, when Weldon was unable to enter for the college scholarship examinations. By the influence of Francis Balfour, however, Weldon's real ability was recognised, and a scholarship was awarded to him. At the very start of his Tripos examination, his only brother, Dante Weldon, who had joined Peterhouse, died suddenly of apoplexy. It says much for Weldon's self-control that the terrible shock of his brother's death did not interfere with his place in the first class of the Natural Sciences Tripos. A few weeks later a second bereavement befell him, when his mother passed away. These trials, followed by Balfour's untimely death in the following year, and by the early death of his own father a few years later, left their indelible impresses upon him.

With the Tripos, Weldon's "Lehrjahre" closed, and, as his nature directed, the "Wanderjahre" began without any interval of rest. Immediately after his Tripos, Weldon started for Naples to work at the Zoological Station. The charm of Balfour's personality had aroused the affection of all who attended his classes, and had awakened a keen desire to follow in his footsteps. In those days the stimulus given by Darwin's writings to morphological and embryological researches was still the dominating factor amongst zoologists, and Weldon threw himself at first with ardour into the effort to advance our knowledge by morphological methods. In Naples he began his first published work, a "Note on the early Development of *Lacerta muralis*," and at the same time did much miscellaneous work on marine organisms.

In September he was back in England at the Southampton Meeting of the British Association. Adam Sedgwick, who had succeeded to the teaching work of Francis Balfour, now invited Weldon to demonstrate for him. Thus, the winter found Weldon in Cambridge again, and from Sedgwick's laboratory was issued the next piece of work: "On the Head-kidney of Bdellostoma, with a suggestion as to the Origin of the Suprarenal Bodies," and he followed the subject up in the next year by publishing his paper "On the Suprarenal Bodies of Vertebrates."

On March 14, 1883, the anniversary of his parents' wedding-day, Weldon married Miss Florence Tebb, the eldest daughter of William Tebb, now of Rede Hall, Burstow, Surrey.

After the death on January 14, 1883, of W. A. Forbes, a Fellow Johnian, Weldon for four months—June 15 to October 15—acted as *locum tenens* for the Prosector at the Zoological Gardens, London, and during that time he read the following papers before the Zoological Society:—"On some points in the Anatomy of Phœnicopterus and its Allies"; a "Note on the Placentation of *Tetraceros quadricornis*"; and "Notes on *Callithrix gigot*."

In the following year (1884) the paper above referred to on the development of the suprarenal bodies was published in the Royal Society 'Proceedings.' On November 3 of the same year, Weldon was elected to a Fellowship at St. John's College, and was shortly afterwards appointed University Lecturer in Invertebrate Morphology. About this time he took a permanent home at No. 14, Brookside, which soon became a centre for Cambridge workers on biology.

On his return to Cambridge in November, 1884, Weldon had taken up again his invertebrate work. His next Memoir, "On *Dinophilus gigas*," dealt with the anatomy and affinities of Dinophilus, at that time a very little known Annelid.

The next few years of Weldon's life were more active than ever. He had now given up coaching, and as he only needed to be in Cambridge two terms of the year, travel and research could occupy the time from the beginning of June to January. On May 8, 1885, he gave his first Friday evening lecture at the Royal Institution on "Adaptation to surroundings as a factor in Animal Development." No report of this lecture was published in the 'Proceedings,' but there are those who still remember the impression caused by the youthful lecturer of twenty-five years of age. Weldon was an adept in lecturing to classes of University students; it brought out all his force and enthusiasm as a teacher. As a writer in the 'Times' (April 18, 1906) says: "Seldom is it given to a man to teach as Weldon taught. He lectured almost as one inspired. His extreme earnestness was only equalled by his lucidity. He awoke enthusiasm even in the dullest, and he had the divine gift of compelling interest. In the University Lecture room he always impressed his hearers with the importance of his topic. You could not listen to him lecturing on a flame-cell or on the variations in the carapace of *Pandalus annulicornis* without sharing his intense conviction of the importance of the matter in hand. He aroused a consciousness in his students that things were worth studying for their own sake, apart from their examination value."

In July, 1886, Weldon crossed to America, and visited the Bahamas to collect. From his headquarters in the Bahamas, he went with two friends to North Bimini, in the Gulf Stream, and made considerable collections, but his published results were confined to "*Haplodiscus piger;* a new Pelagic Organism from the Bahamas," and a "Preliminary note on a *Balanoglossus* Larva from the Bahamas." Working at the *Balanoglossus* material in 1887, he found that his results differed from those reached by Professor Sprengel. He accordingly went to Giessen at Easter, and finally handed over to Professor Sprengel the whole of his *Balanoglossus* material. During the Lent and May terms he gave a course of lectures on Economic Entomology to the forestry students at the Royal Indian Engineering College, Coopers Hill.

In 1888, the buildings of the Marine Biological Laboratory in Plymouth were nearly completed, and to the Marine Biological Association Weldon gave both time and sympathy during the rest of his life. His annual visits of inspection to their second Laboratory at Lowestoft during the last few years were always a great pleasure to him. Lent and May terms, 1888, were spent as usual in Cambridge, but June to December were given up to Plymouth, with a brief Christmas holiday in Munich. And here we must note the beginning of a new phase in Weldon's ideas. His thoughts were distinctly turning from morphology to problems in variation and correlation. He has left on record the nature of the problems he was proposing to himself at this time, and they are summed up as follows:—

(1) The establishment of a new set of adult characters leading to the evolution of a new family has always been accompanied by the evolution of a new set of larval characters leading to the formation of a larval type peculiar to the newly established family; the two sets of characters having as yet no demonstrable connection one with the other.

(2) The evolution of the adult and that of the larval characters peculiar to a group advance *pari passu* one with the other, so that a given degree of a specialisation of adult characters on the parts of a given species implies the possession of a larva having a corresponding degree of specialisation and *vice versâ*.

The next year was to place in Weldon's hands a book—Francis Galton's "Natural Inheritance," by which one avenue to the solution of such problems, one quantitative method of attacking organic correlation, was opened out. From this book as their source sprang two notable friendships and the whole of the biometric movement, which so changed the course of his life and work. In 1889, also, another change came. Weldon found that his dredging and collecting work separated him from his books for half his time. Accordingly, he applied for a year's leave from Cambridge, and he and his wife settled down in a house of their own at Plymouth. This period of hard work lasted through 1890, and was broken only by flying visits to Dresden in September and at Christmas, 1889, and an autumn visit in 1890 to Chartres and Bourges. The intellectual development and the experience and knowledge gained in this period were far more important than the mere published work would indicate. In 1889, Weldon investigated the nature of the curious enlargement of the bladder associated with the green, or excretory, glands in certain Decapod Crustacea, and published in October of the same year his paper of "The Cœlom and Nephridia of *Palæmon servatus*." The result of his investigation was to confirm "the comparison so often made (by Claus, Grobben, and others) between the glomerulus of the vertebrate kidney and the end-sac of the Crustacean green gland." A little later, June, 1891, he published the results of more extended researches in this field in what proved to be his last strictly morphological paper. It was entitled: "The Renal Organs of certain Decapod Crustacea." In this he showed that in many Decapods spacious nephro-peritoneal sacs "should be regarded rather as enlarged portions of the tubular system . . . than as persistent remnants of a 'cœlomic' body cavity, into which the tubular nephridia open."

One further paper of a year later may be best referred to here, Weldon's only piece of work on invertebrate embryology, "The formation of the Germ Layers in *Crangon vulgaris*." This contains a clear account of the early stages of segmentation, and the building up of the layers of the shrimp, illustrated by excellent figures. And here it may be mentioned that his power with the pencil was not that of the mere draughtsman, accurate in detail but often lifeless; he was an artist by instinct, and he had the keenest pleasure in drawing for its own sake.

December, 1890, closed the Cambridge work; Weldon now succeeded Ray Lankester in the Jodrell Professorship at University College, London. In June he had been elected a Fellow of the Royal Society.

It has been seen that the years between Weldon's degree and his first professoriate were years of intense activity. He was teaching many things, studying many things, planning many things. His travels perfected his linguistic powers, and his fluency in French, Italian, and German was soon remarkable.

A word must here be said as to the transition which took place during the "Wanderjahre" in Weldon's ideas. He had started, as most of the younger men of that day, with an intense enthusiasm for the Darwinian theory of evolution, but he realised to the full that the great scheme of Darwin was only a working hypothesis, and that it was left to his disciples to complete the proofs, of which the master had only sketched the outline. Naturally he turned first to those methods of proof, morphological and embryological, which were being pursued by the biological leaders of the period, and it was only with time that he came to the conclusion that no great progress could be attained by the old methods. We have already seen that even before the appearance of "Natural Inheritance," his thoughts were turning on the distribution of variations and the correlation of organic characters. He was being led in the direction of statistical inquiry. The full expression of his ideas is well given in the first part of the "Editorial," with which 'Biometrika' started:—

> "The starting point of Darwin's theory of evolution is precisely the existence of those differences between individual members of a race or species which morphologists for the most part rightly neglect. The first condition necessary, in order that any processes of Natural Selection may begin among a race, or species, is the existence of differences among its members; and the first step in an inquiry into the possible effect of a selective process upon any character of a race must be an estimate of the frequency with which individuals, exhibiting any degree of abnormality with respect to that character, occur. The unit, with which such an enquiry must deal, is not an individual but a race, or a statistically representative sample of a race; and the result must take the form of a numerical statement, showing the relative frequency with which various kinds of individuals composing the race occur."

It was Francis Galton's "Natural Inheritance" that first indicated to Weldon the manner in which the frequency of deviations from the type could be measured.

In Plymouth, 1890, Weldon started his elaborate measurements on the Decapod Crustacea, and soon succeeded in showing that the distribution of variations was closely like that which Quetelet and Galton had found in the case of man.

His paper "The Variations occurring in certain Decapod Crustacea. I. *Crangon vulgaris*" was, as far as we know, the first to apply the method of Galton to other zoological types than man. In this paper the author showed that different measurements made on several local races of shrimps give frequency distributions closely following the normal or Gaussian law. In his next paper, "On certain correlated Variations in *Crangon vulgaris*," he calculated the first coefficients of organic correlation, *i.e.*, the numerical measures of the degree of interrelation between two organs or characters in the same individual. It is quite true that the complete modern methods were not adopted in either of these papers, but we have for the first time organic correlation coefficients—although not yet called by that name—tabled for four local races. These two papers are epoch-making in the history of the science, afterwards called Biometry.

It is right to state that Weldon's mathematical knowledge at this period was far more limited than it afterwards became. The first paper was sent to Francis Galton as referee, and was the commencement of a life-long friendship between the two men. With Galton's aid the statistical treatment was remodelled, and considerable modifications made in the conclusions.

The defect in mathematical grasp, which Weldon had realised in his first paper, led him at once to seek to eliminate it. He set about increasing his mathematical knowledge by a thorough study of the great French writers on the calculus of probability. He did not turn to elementary text-books, but with his characteristic thoroughness went to the fountain head, and he thus attained a great power of following mathematical reasoning, and this power developed with the years. He had, moreover, a touch with observation and experiment rare in mathematicians. In problems of probability he would start experimentally and often reach results of great complexity by induction. From 1890 onwards, his knowledge, theoretical and experimental, of the theory of chances increased by bounds.

Weldon's work at University College commenced in 1891. The house in Wimpole Street was taken and, if possible, life became more intense. In October came the college inaugural lecture for the session, on the subject of the statistical treatment of variation. This year and the next were strenuous years in calculating. The Weldons toiled away at masses of figures, doing all in duplicate. At Easter, 1892, they went to Malta and Naples, and the summer was spent over crab-measurements at the Zoological Station in the latter city, and the first biometric crab paper "On certain correlated Variations in *Carcinus mœnas*" was issued later in the year. This paper confirms on the shore crab the results already obtained on the common shrimp. The distributions of characters are closely Gaussian, with the exception of the relative frontal breadth, which the author considered dimorphic in Naples. He does not refer to this fact in his memoir. As for shrimps the correlations again came out closely alike for the Plymouth and Naples races. Weldon was not dogmatic on the point; he considered the constancy as at least an "empirical working rule."

To the biometrician, perhaps, the most interesting committee with which Weldon was associated in his later years was that which came to be called the Royal Society Evolution (Animals and Plants) Committee. His papers on variation and correlation in shrimps and crabs had brought him closely into touch with Francis Galton, and both were keenly interested in the discovery of further dimorphic forms such as had been suggested by the frontal breadths of the Naples crabs. Weldon was full also of other ideas ripe for investigation. He had started his great attempt at the measurement of a selective death-rate in the crabs of Plymouth Sound; experiments on repeated selection of infusoria were going on in his laboratory; he was gathering an ardent band of workers about him, and much seemed possible with proper assistance and that friendly sympathy which was ever essential to him. As a result of an informal conference held at the Savile Club towards the end of 1893, it was decided to ask the Royal Society to establish a Committee "for the purpose of Conducting Statistical Enquiry into the Variability of Organisms," and such a Committee was early in 1894 constituted by the Council, with Francis Galton as Chairman, and Weldon as Secretary, the Committee being entitled; "Committee for conducting Statistical Inquiries into the Measurable Characteristics of Plants and Animals." The use of the words statistical and measurable, somewhat narrowly, but accurately, defined the proposed researches of the Committee. It went on until 1897, with the same members, the same title and scope. Looking back on the matter now, one realises how much Weldon's work was hampered by the Committee. It is generally best that a man's work should be published on his own responsibility, and when he is a man of well-known ability and established reputation, grants in aid can always be procured. In this case Weldon had a sympathetic committee, but the members were naturally anxious on the one hand for the prestige of the Society with whose name they were associated, and secondly, they were desirous of showing that they were achieving something. Both conditions were incompatible with tentative researches such as Biometry then demanded. Trial and experiment were peculiarly needful in 1893; the statistical calculus itself was not then even partially completed; biometric computations were not reduced to routine methods, and the mere work of collecting, observing, experimenting, and measuring was more than enough for one man. Weldon with his "volume of life" was eager to do all these things, and run a laboratory with perhaps sixty students as well.

The "Attempt to Measure the Death-rate due to the Selective Destruction of *Carcinus mœnas*, with respect to a Particular Dimension," formed the first report of the Committee, and was presented to the Royal Society in November, 1894. Weldon's general project in this case was novel at the time, it consisted in determining whether the death-rate is correlated with measurable characters of the organism, or, as he himself puts it, "in comparing the frequency of abnormalities in young individuals at various stages of growth with the frequency of the same abnormalities in adult life, so as to determine whether any evidence of selective destruction during growth could be discovered or not."

Looking back now on Weldon's paper of 1894, one realises its great merits: it formulated the whole range of problems which must be dealt with biometrically before the principle of selection can be raised from hypothesis to law. Almost each step of it suggests a mathematical problem of vital importance in evolution, which has since been developed at length, or still awaits the labour of the ardent biometrician.

Unfortunately the paper, as well as the suggestive "Remarks on Variation in Animals and Plants" with its memorable words: "The questions raised by the Darwinian hypothesis are purely statistical, and the statistical method is the only one at present obvious by which that hypothesis can be experimentally checked," fell on barren soil. A further instructive report on the growth at two moults of a considerable number of crabs was made to the Committee in 1897, but appears never to have been published. Later, an account of work on Natural Selection in crabs was given by Weldon in his Presidential Address to the Zoological Section of the British Association, Bristol, 1898. In the paper just mentioned, after several years of discouragement and much hard labour, he succeeded in demonstrating that natural selection was really at work, and further that it was at work at a very sensible rate. The labour involved was excessive. One "crabbery" consisted of 500 wide-mouthed bottles, each with two syphons for a constant flow of sea water. Each crab had to be fed daily and its bottle cleaned. But in the autumn a rest came. The British Association Address was written and Weldon thoroughly enjoyed his presidency of Section D at Bristol.

It may not be out of place here to note the great aid Weldon's artistic instinct and literary training gave to his scientific expression. His papers are models of clear exposition, his facts are well marshalled, his phraseology apt, his arguments concise, and his conclusions tersely and definitely expressed. The result, however, was not reached without much labour. There was never any artificial brilliancy introduced in the process; rhetoric in the service of science was intolerable to Weldon. His was simply an attempt to choose the suitable form and the right words for a given purpose. It was comparable with his sense of sound, with his extraordinary gift of appreciating and reproducing the exact intonation of a foreign tongue.

Considerable changes were soon to take place in his environment and scheme of work. Lankester had been appointed Director to the British Museum (Natural History), and in February, 1899, Weldon succeeded him in the Linacre Professorship at Oxford. In the February of 1897 the Royal Society Evolution Committee received a large increase of membership; it ceased henceforth to "conduct statistical inquiries into the measurable characteristics of plants and animals," and became transformed into an Evolution (Plants and Animals) Committee and Weldon and the biometric members ultimately withdrew from it.

During the eight years of his London professoriate Weldon's development was great; he became step by step a sound mathematician, and gained largely in his power of clear and luminous exposition. His laboratory was always full of enthusiastic workers, and over forty memoirs were published by his students. His removal from the London field of work, while an incalculable loss to his colleagues, was not without its compensation to his nearest friends. They knew that the life of the last few years had been one of great tension, that Weldon's time had been too much encroached upon by committee work, that the separation between the locus of his teaching and of his research work was very undesirable; that even the social life of London involved too much expense of energy. He was a child of the open air and the breezes, and it was hoped that he might have more of them, if not in lowland Oxford, at least on the hills around. There was space and air too for the experimental work that had been so cramped in Gower Street. The *Daphnia* studies, which had occupied so much energy under unfavourable conditions in London, were at once resumed on broader lines in the ponds and ditches round Oxford. With a basket of bottles attached to his cycle handle, and a fishing creel, filled with more bottles, on his back, the Linacre Professor might be met even as far as the Chiltern Hills, collecting not only *Daphnia*, but samples of the water in which they lived. His University College work had shown him how widely *Daphnia* are modified by their chemical and physical environment, and how this modification is largely due to selection. There exist elaborate drawings of the *Daphnia* from the Oxfordshire ponds, indicating their differentiation into local races, with notes on the peculiarity of their habitat and the chemical constitution of the water.

A study of Kobelt's "Studien zur Zoogeographie," 1897–9, led him later to take up the same problem with regard to land-snails. What is the meaning of the slight but perfectly sensible differences in type to be found in shells from adjacent valleys or even from different heights of the same mountain? Weldon attacked the problem in his usual manner; he spent two Christmas vacations collecting Sicilian snails of the same species, from habitats extending over a wide area, the local environments were described, and the snails were often photographed with their immediate surroundings. Innumerable shells were brought back to Oxford, and the Professor delighted to discourse on the significant differences in local type, and yet the gradual change of type to type from one spot to another. No rapidly made measurement on the outside of the shell would satisfy him; the shell must be carefully ground down through the axis, and the measurements must be made on the section thus exposed. Perhaps four or five snail shells could be ground and measured in a day, and at the time of his death not more than a few hundred of the Sicilian thousands had even been ground.

But these attempts to get to the kernel of selection in its action on local races were far from occupying the whole of Weldon's thoughts in these early days. In conjunction with his assistant, Dr. E. Warren, he had commenced at University College his first big experimental investigation into heredity. The characters dealt with consisted of the number of scales in particular colour patches upon certain pedigree moths, and the work of counting these was very laborious. In the course of three years, many hundreds of pedigree moths were dealt with, and the observations were reduced. But no definite inheritance at all of the character selected for consideration was discovered. Weldon, apparently thought that there had been some fatal mistake in the selection of pairings, and undoubtedly, in some cases, parents of opposite deviation had been mated, so that a rather influential negative assortative mating resulted. But from other series of pedigree moth data, it seems probable that there is some special feature in heredity in moths, or possibly in those that breed twice in the year, and that the vast piece of work which Weldon and Warren undertook in 1898—1901 may still have its lesson to teach us.

In these three first years at Oxford, Weldon's intellectual activity was intense. To the pedigree moth experiments was added, in the summer of 1900, an elaborate series of Shirley Poppy growings, 1250 pedigree individuals being grown and tended in separate pots; Weldon's records were the most perfect of those of any of the co-operators, and his energy and suggestions gave a new impetus to the whole investigation. They were ultimately published in 'Biometrika' under the title, "Co-operative Investigations on Plants, I. On Inheritance in the Shirley Poppy." As Weldon himself expressed it, the moths and poppies meant "a solid eight hours daily of stable-boy work through the whole summer and through the Easter vacation, with decent statistical work between." After the Shirley Poppies were out of hand in the summer of 1900, the Weldons went to Hamburg and thence to Plön. The object of this visit was to collect *Clausilia* at Plön and Gremsmühlen for comparison with the race at Risborough. The same aim—the comparison of local races—led Weldon at Christmas to collect land-snails in Madeira. Thus he slowly built up a magnificent biometric collection of snail shells, *i.e.*, one sufficiently large to show, in the case of many local races of a number of species, the type and variability by statistically ample samples. Of this part of his work only two fragments have been published, "A First Study of Natural Selection in *Clausilia laminata*" (Montagu) and "Note on a Race of *Clausilia itala*" (von Martens). In the first of these memoirs he shows that two races of *C. laminata* exist, in localities so widely separated as Gremsmühlen and Risborough, with sensibly identical spirals, although no crossing between their ancestors can have existed for an immense period of time, and although there are comparatively few common environmental conditions. At the same time, while no differential secular selection of the spiral appears to have taken place during this period, there yet seems to be a periodic selection of the younger individuals in each generation, the variability of the spirals of the young cells being sensibly greater than of the corresponding whorls of adults. In other words, stability to the type is preserved by selection in each new generation. In the second memoir, Weldon sought for demonstration of a like periodic selection in the *C. itala* he had collected from the public walks round the Citadel of Brescia. He failed, however, to trace it, and was forced to conclude that *C. itala* is either not now subject to selective elimination for this character or is multiplying at present under specially favourable conditions at Brescia, or again, as both young and old were gathered in early spring, after their winter sleep, that elimination takes place largely during the winter, and "that individuals of the same length, collected in the autumn, at the close of their period of growth, might be more variable than those which survive the winter."

The problem of growth, to be studied only under conditions of captivity, possibly modifying the natural growth immensely, has made the crab investigation an extremely complex one. Weldon solved the difficulty by the brilliant idea that the snail carries with it practically a record of its youth. If the wear and tear of the outside of the shell to some extent confuses the record there, a carefully ground axial section will reveal by the lower whorls the infancy of the organism. Hence the days given to experimental grinding, the training in manipulation and the final success, and then the steady work, grinding and measuring a few specimens a day, till the necessary hundreds were put together; the laborious calculations not in the least indicated in the papers; and the illustration of how shells may be used—by those who will give the needful toil—to test the truth of the Darwinian theory.

On November 16, Weldon wrote :—

> "Do you think it would be too hopelessly expensive to start a journal of some kind ? . . . .
>
> "If one printed five hundred copies of a royal 8vo. once a quarter, sternly repressing anything by way of illustration except process drawings and curves, what would the annual loss be, taking any practical price per number ? . . . . If no English publisher would undertake it at a cheap rate, the cost of going to Fischer, of Jena, or even Engelmann would not be very great."

This was the first definite suggestion of the establishment of 'Biometrika.' On November 29, the draft circular, corresponding fairly closely to the first editorial of the first number, reached his co-editor from Oxford with the words: "Get a better title for this would-be journal than I can think of!" The circular went back to Oxford with the suggestion that the science in future should be called Biometry, and its official organ be 'Biometrika.' And on December 2, 1900, Weldon wrote ;—

> "I did not see your letter yesterday until it was too late for you to have an answer last night. I like 'Biometrika' and the sub-title."

Thus was 'Biometrika' born and christened. The reply to circulars issued during December was sufficiently favourable to warrant further proceedings. By June of 1901 its publication through the Cambridge University Press had been arranged for, and the sympathetic help of the Syndics and the care given by the University Printers enabled us to start well and surmount many difficulties peculiar to a new branch of science. During the years in which Weldon was co-editor with Karl Pearson he contributed much, directly and indirectly, to its pages. He was referee for all essentially biological papers; and his judgment in this matter was of the utmost value. He revised and almost re-wrote special articles. He was ever ready with encouragement and aid when real difficulties arose.

Starting on October 16, 1900, and extending throughout the early 'Biometrika' letters to his co-editor, runs a flood of information with regard to Mendel and his hypothesis.

> "About pleasanter things: I have heard of and read a paper by one Mendel, on the results of crossing peas, which I think you would like to read. It is in the 'Abhandlungen des naturforschenden Vereines in Brunn' for 1865. I have the R.S. copy here, but I will send it to you if you want it."—(October 16, 1900.)

Then follows a résumé of the first of Mendel's memoirs, and for months the letters—always treatises—are equally devoted to snails, Mendelism, and the basal things of life.

The earlier part of 1901 was chiefly occupied by snails, but a new factor had come into Weldon's many-sided occupations. It was settled that 'Biometrika' should have in an early number a critical bibliography of papers dealing with statistical biology. Weldon undertook the task of preparing it, as his study of Mendel had led him to a very great number of such papers dealing with inheritance, and the section on Heredity was to be published first. Like all his projects, it was to be done in so thorough and comprehensive a manner that years were required for its completion. A very full list of titles was formed, especially in the Inheritance section, and many of the papers therein were thoroughly studied and abstracted. But such study meant with him not only grasping the writer's conclusions, but testing his arithmetic and weighing his logic. Thus Weldon's note on "Change in Organic Correlation of *Ficaria ranunculoides* during the Flowering Season," arose from this bibliographical work and the erroneous manner in which he found Verschaffelt and MacLeod dealing with correlation. A further result of this work was that his confidence in the generality of the Mendelian hypotheses was much shaken. He found that Mendel's views were not consonant with the results formulated in a number of papers he had been led to abstract, and that the definite categories used by some Mendelian writers did not correspond to really well-defined classes in the characters themselves.

To those who accept the biometric standpoint, that, in the main, evolution has not taken place by leaps, but by continuous selection of the favourable variation from the distribution of the offspring round the ancestrally fixed type, each selection modifying *pro rata* that type, there must be a manifest want in Mendelian theories of inheritance. Reproduction from this standpoint can only shake the kaleidoscope of existing alternatives; it can bring nothing new into the field. To complete a Mendelian theory we must apparently associate it for the purposes of evolution with some hypothesis of "mutations." The chief upholder of such an hypothesis has been de Vries, and Weldon's article on "Professor de Vries on the Origin of Species" was the outcome of his consideration of this matter. During the years 1902 and 1903 an elaborate attempt was made to grow the numerous sub-races of *Draba verna*, with the idea that they might throw light on mutations. The project failed, largely owing to difficulties in the artificial cultivation of some of the species. But for a time all other interests paled before *Draba verna*.

A study of the work of von Guaita had convinced Weldon, early in 1901, that the cross between the European albino mouse and the Japanese waltzing mouse was not one which admitted of simple Mendelian description. In May, 1901, his letters contain inquiries as to Japanese mice dealers. During the summer and autumn the collection of Japanese mice was in progress. These mice were to be bred to test the purity of the stock; during December about forty does had litters, and pure breeding went on until the autumn of 1902, when hybridization commenced. The work on these mice was for two years entrusted to Mr. A. D. Darbishire, but the whole plan of the experiments, the preparation of the correlation tables, and the elaborate calculations were in the main due to Weldon. On Mr. Darbishire's leaving Oxford, Weldon again resumed personal control of the actual breeding arrangements, and from some second hybrid matings carried on the work to the sixth hybrids' offspring. The work was nearing completion at his death, and through the energy of Mr. Frank Sherlock, the skins of the 600 pedigree mice forming the stud at that time have been dressed and added to those of the earlier generations. Weldon had this work much at heart, and his letters during 1904 and 1905 give many indications of the points he considered demonstrated. The experimental part of the work would have been nearly completed had not his whole thought and energy been directed from November, 1905, into another channel.

In the summer 'Biometrika' was edited from Bainbridge in Wensleydale, and the co-editors cycled to the churchyards of the Yorkshire dales, collecting material for their joint paper "On Assortative Mating in Man" (34). From Bainbridge, the Weldons went to the British Association meeting at Belfast, where an evening lecture on Inheritance was given. At Christmas came one of the above-mentioned visits to Palermo to collect Sicilian snails.

In the spring of 1903, Weldon was busy, as were the whole available members of the biometric school, in studying the influence of environment and of period of season on the variation and correlation of the floral parts of Lesser Celandine.

> "Give my love to the Brethren who are co-operating in the matter of Celandines, and beseech them to make a better map of their country than the enclosed."—(Oxford, February 23, 1903.)

Weldon threw his whole energy and love of minute exactitude into the task, and his letters are filled with an account of the almost daily changes in the type and variability of the Celandine flowers from his selected stations. The result of this enquiry was the collection of an immense amount of data showing that environment and period in the flowering season affected the flower characters to an extent comparable with the differences attributed to local races. At Easter of 1903 a series of mishaps prevented the common holiday, but this was more than compensated for by the summer vacation. The Weldons started with a sea trip to Marseilles and back. They then returned to Oxford, in order that work on the article "Crustacea" for the Cambridge Natural History might be carried on, and that an eye might be kept on the mice. The data on assortative mating in man collected in the previous year were reduced and a joint paper sent to press; the immense amount of calculation and reduction involved in the mouse-paper was got through; a joint criticism of Johannsen's "Ueber Erblichkeit in Populationen und in reinen Linien" was written by the co-editors, under the title "Inheritance in *Phaseolus vulgaris*," and a joint study was made, at Weldon's suggestion, of the relationship between Mendelian formulæ and the theory of ancestral heredity. It was shown that there was no essential antagonism between the two methods of approaching the subject, and the results were published ultimately at Part XII of the "Mathematical Contributions to the Theory of Evolution," Weldon persistently declining to allow it to appear as a joint memoir, because he had taken no part in certain portions of the more complicated algebraic analysis. Christmas found the Weldons in Palermo on the snail quest. His letters thence to his co-editor teem with the freshness of the sky and the joy of open-air work.

> "Out between five and six, in the dark, without any breakfast, sunrise up in the hills, a day's tramp on a piece of bread and a handful of olives, and home at seven, laden with snails. Then after dinner to clean the beasts. That is not work, and it makes one very fit, but one gets tired enough to sleep when the snails are cleaned."

At the beginning of 1904 the work on the Brescia *Clausilia* was in progress, the mice were multiplying after their kind, and Weldon's thoughts were turning more and more to a determinal theory of inheritance, which should give a simple Mendelism at one end of the range and blended inheritance at the other. The summer found the Pearsons twelve miles from Oxford, at Cogges, near Witney, and the Galtons, twenty miles further, at Bibury; there was much cycling to and fro, and the plan of a new book by Weldon on Inheritance was drafted, and some of the early chapters were written.

The book on Inheritance occupied most of the remainder of the year, and to aid it forward and help those of us who were not biologists to clearer notions, Pearson suggested to Weldon a course of lectures in London to his own group of biometric workers. The project grew, other departments of the College desired to attend, and ultimately the lectures were thrown open to all members of the University and even to the outside public. The lecturer had a good audience of more than a hundred, and enjoyed the return to his old environment.

The letters of Weldon to both Francis Galton and Pearson during the years 1904 and 1905 are full of inheritance work, the details of the great mice-breeding experiment, the statement and the solution, or it might be the suggested solution, of nuclear problems leading to detrimental theories of inheritance. Occasionally, there would be a touch of conscience, and the drawings for the Crustacea would be pressed forward :—

> "I ought to give my whole time to the 'Cambridge Natural History' for a while. They had been very good to me, and I have treated them more than a little badly. I am rather anxious to get them off my conscience."—(Oxford, February 15, 1905.)

But only the chapter on "Phyllopods" was completed, figures and all, and was set up in type. Many figures were prepared for other parts; beautiful things, which gave Weldon not only scientific but artistic pleasure, he had made, but the text remains a mere fragment. In the same way but little was absolutely completed of the article on "Heliozoa" for Lankester's "Natural History." It was not Weldon's biometric friends who kept him from these tasks, but solely his own intense keenness in the pursuit of new knowledge.

The fascination of inheritance problems kept him, however, for months at a time at the Heredity book. At Exeter, 1905, he went to Ferrara because that place had a university, and as such must have a library, where work could be done. The contents of the library were perfectly mediæval, a characteristic appropriate in the castle, but hardly helpful in heredity. Still, portions of the manuscript came to England for comment and criticisms, and we were hopeful that the end of the year would see the book completed.

It must not be thought for a moment that Weldon was desultory in his work. As Sir Ray Lankester says in a letter: "His absolute thoroughness and unstinting devotion to any work he took up were leading features in his character." He pursued science, however, for sheer love of it, and he would have continued to do so had he been Alexander Selkirk on the island with no opportunity of publication and nobody to communicate his results to. He never slackened in the energy he gave to scientific work, but having satisfied himself in one quest he did not stay to fill in the page for others to read; his keen eye found a new problem where the ordinary man saw a cow-pasture, or a dusty hedgerow, and he started again with unremitted ardour to what had for himself the greater interest.

In the summer the Pearsons were at East Ilsley, some seventeen miles from Oxford, and there was cycling out several times a week; there was steady joint work on the determinantal theory of inheritance as outlined by Weldon, which, it is to be hoped, is sufficiently advanced to be completed and published. He had in August, 1905, given to the Summer Meeting of the University Extension in Oxford a lecture on "Inheritance in Animals and Plants," and this had taken up some of his energy during the summer vacation. On the whole, however, he worked persistently at the Inheritance book.

It cannot be denied that those who were often with Weldon during the last two years were occasionally anxious on his account. The pace at which he worked had been too great—but at no time was it definitely realised that there was cause for immediate alarm. His intellectual activity was never apparently diminished, and his long cycling rides were maintained to the end; though an occasional, but never long persistent, lack of the old joyousness of life was noticeable.

In November, 1905, Weldon was unfortunately taken off from the work on his Inheritance book by the presentation to the Royal Society of a paper by Captain C. C. Hurst, "On the Inheritance of Coat-Colour in Horses." He had had no proper summer holiday, but he threw himself nine hours a day into the study of "The General Studbook."

> "I can do nothing else until I have found out what it means . . . The question between Mendel and Galton's theory of Reversion ought to be answered out of these. Thank God, I have not finished that book. There must be a chapter on Race Horses!"

He promised to communicate a note to the Society involving details of his inquiry. This was done on January 18, 1906, in a "Note on the Offspring of Thoroughbred Chestnut Mares."

> "The object of the present note is partly to fulfil my promise and partly to call attention to certain facts which must be considered in the attempt to apply any Mendelian formula whatever to the inheritance of coat-colour in race horses."

Here it can only be said that he took up the subject with his usual vigour and thoroughness. But he was overworked and overwrought, and a holiday was absolutely needful. He went to Rome, but the volumes of the Studbook went with him. His letters are filled with Studbook detail till Easter, with hardly a reference to anything else. Re-reading them now, one sees how this drudgery, with no proper holiday, told on him. Hundreds of pedigrees were formed, and a vast amount of material was reduced. At Easter, he and his wife went to the little inn at Woolstone, at the foot of the White Horse

Hill, and his co-editor came down later to Longcot, a mile away, for the joint vacation. Weldon, still hard at work on the Studbooks, was intellectually as keenly active as of old; and was planning the lines of his big memoir on coat-colour in horses, and showing how they illustrated the points he had already found in the mice.

This extraordinary mental activity was now telling upon a constitution never very robust, but the end came with startling suddenness. A day or two of slight illness at Woolstone, which, as usual, he made nothing of, was followed by a visit alone to London on Wednesday, April 11. Here he was taken seriously ill, and within a few hours he died of pneumonia, on Good Friday, April 13, 1906.

So passed away, not unfitly—for it was without any long disabling illness and in full intellectual vigour—a man of unusual personality, one of the most inspiring and loveable of teachers, the least self-regarding and the most helpful of friends, and the most generous of opponents.

And lastly, as to Science, What will his place be? The time to judge is not yet. Much of his work has still to be published, and this is not the occasion to indicate what Biometry has already achieved. The movement he aided in starting is but in its infancy. It has to fight not for this theory or that, but for a new theory and a greater standard of logical exactness in the science of life. To those who condemn it out of hand, or emphasise its slightest slip, we can boldly reply, "You simply cannot judge, for you have not the requisite knowledge." To the biometrician, Weldon will remain as the first biologist who, able to make his name by following the old tracks, chose to strike out a new path—and one which carried him far away from his earlier colleagues. It is scarcely to be wondered at if those he joined should wish to see some monument to his memory; for he fell, the volume of life exhausted, fighting for the new learning.

HENRY BAKER TRISTRAM, 1822—1906.

The Rev. Henry Baker Tristram, long familiarly known to naturalists all over the world as Canon Tristram, was born on May 11, 1822, at Eglingham, near Alnwick, of which large parish his father was vicar. He received his early education at Durham School, and passed to Lincoln College, Oxford, where he graduated in 1844, taking a second class in classics. In the following year he was ordained Deacon, and shortly afterwards became Curate of Morton Bishop. He had not been long engaged in his clerical duties when he developed such signs of a weak chest that it was judged expedient to send him abroad. Accordingly, in 1847, he received the appointment of Acting Naval and Military Chaplain at Bermuda, and held it for two years. That he had been from early boyhood an ardent lover of nature and a keen collector of plants and animals cannot be doubted. But it was probably during his residence in Bermuda that his future career as a naturalist took a definite beginning. Among the officers of the 42nd Highland Regiment, quartered there at the time, was Henry Maurice Drummond (brother of Drummond of Megginch), who had been stimulated into active natural history pursuits by coming under the influence of Hugh Edwin Strickland, until he made himself more than a mere amateur ornithologist. Tristram caught from him the same spirit of scientific observation, and took up the study of shells and birds in the serious way which he never afterwards abandoned. On his return to England, in 1849, he was presented to the living of Castle Eden, in Durham, and in 1850 married a daughter of P. Bowlby, an officer who had served in the Peninsular and Waterloo campaigns. Eight children were born of this marriage, consisting of one son and seven daughters.

To the duties of a country clergyman he for some years added those of tutor, and took pupils (with whom he made occasional excursions to the Continent, travelling one year along the West Coast of Norway as far as the Arctic Circle). The lung affection, however, which had necessitated his seeking the warmer climate of Bermuda, again returned upon him. He was advised to spend a winter in Algeria. From this change, which he took in the winter of 1855—56, he received so much benefit that he repeated his visit next winter, and he used to refer to these two sojourns in Africa as the prime cause of his being able to throw off the ailment which had threatened him. Having formed a friendship with the French Governor-General, he was enabled to push his explorations to the furthest French outposts, and beyond these far into the desert, living almost all the time under canvas. In the year 1857 he was joined there by Mr. W. H. Hudleston and the late Mr. Osbert Salvin. This party succeeded in making large ornithological collections, which proved to be of great interest. During the following year (1858—59) Tristram travelled widely in the Eastern Mediterranean basin, including Palestine and Egypt.

In 1860 he became Master of Greatham Hospital and Rector of Greatham, and held these appointments until 1873 when, having obtained a Canonry in Durham Cathedral, he removed to Durham, which thereafter became his home until the close of his life. But his love of travel led him to return again and again to the East in order to gather fresh material illustrative of its geology and natural history. He renewed his acquaintance with Palestine in 1863—64, and again in 1872. In 1881 he travelled through Mesopotamia and Armenia. In 1891 he visited China, Japan, and the North-West of North America. In 1894 he was again in Palestine, and once more in 1897, at the age of seventy-five. On his last visit he had his leg broken by a kick from a horse when riding near Jerusalem, but such was his irrepressible vitality that, after a few weeks in hospital, he reappeared as hale and hearty as ever.

Throughout all these extensive wanderings Tristram showed the true instincts of a born naturalist, cultivated and enlarged by wide and constant experience. To him we are mainly indebted for our knowledge of the plants and animals of Palestine and the surrounding countries. His papers on the ornithology of Northern Africa, which appeared in the 'Ibis' for 1859 and following years, were important additions to what had previously been known on the subject. His frequent journeys through Palestine allowed him to acquire an unrivalled acquaintance with the geology, topography, and natural history of that country, and he gathered together an admirable account of his observations in his great work on the 'Fauna and Flora of Palestine,' which was published by the Palestine Exploration Fund in 1884. His scientific labours and his descriptive powers, however, were made more widely known by the separate volumes which appeared from his facile pen in successive years. The first of these, 'The Great Sahara,' published in 1860, at once established his place as an accomplished traveller and observant naturalist. It was followed by a series of attractive narratives of his wanderings through Palestine.

His friend, the late Professor Alfred Newton, remarked that "Tristram's study of the 'desert forms' of the birds induced him to declare in the 'Ibis' for 1859 (p. 429) his conviction 'of the truth of the views set forth by Messrs. Darwin and Wallace in their communication to the Linnæan Society,' adding that 'it is hardly possible, I should think, to illustrate this theory better than by the larks and chats of North Africa.' Three or four pages follow in which special examples are cited in illustration, and these were written, if not published, before the appearance of 'The Origin of Species,' so that Tristram appears to have been the first zoologist to accept publicly the principles of Darwinism." "He had to modify his expressions some time after, when the 'orthodox' tide was flowing, just as Galileo was obliged to do, but he held them all the same until the end, and great credit is due to him for this."*

* From MS. notes supplied to the writer by Professor Newton, who also, in 'Nature,' for March 16, 1906, called attention to Tristram's early Darwinian pronouncement.

"In all his voyages and journeys, ornithology received Tristram's chief attention. Among his discoveries may be especially mentioned that of a starling-like bird, named after him by Mr. Sclater, *Amydrus Tristrami*, peculiar to the gorge of the Kedron, and belonging to a genus previously thought to be purely Ethiopian. But his collection was not at all confined to specimens obtained by himself or his companions on his travels, extensive as these were; but comprehended the birds of the whole world, and formed one of the largest ever brought together by any private person. It was sold in his lifetime to the Free Public Museum of Liverpool."* It was described in the Report of the Committee of that institution for 1896 as containing "20,000 specimens, referable to 6000 species, of which 150 are types." Tristram likewise amassed a large and valuable collection of birds' eggs, which he sold to Mr. Philip Crowley, at whose death it passed by will into the Natural History Museum, South Kensington.

Canon Tristram endeared himself to a wide circle of friends by the singular modesty and geniality of his nature, by his keen sense of humour, the great range of his acquirements in natural history, and the delightful flow of his conversation, in which he would draw upon his wide and varied experience in so many different countries. He celebrated his golden wedding in the spring of the year 1901. Two years later his wife died, and he himself, retaining his faculties to the end, passed away on March 8, 1906, at the ripe age of eighty-four.

A. G.

* MS. of Professor Newton.

ALFRED NEWTON, 1829—1907.

By the death of Professor Alfred Newton, the ranks of British zoologists have lost one of their most venerable and distinguished ornaments, and Ornithology in particular has been deprived of its most learned and accomplished British representative. Born at Geneva on June 11, 1829, he spent his boyhood with his numerous brothers and sisters at Elveden, an estate on the borders of Suffolk and Norfolk, which belonged to his father. His undergraduate life, which began at Cambridge in 1848, does not appear to have been marked by any conspicuous success in the usual subjects of study, though he is said to have gained a considerable reputation in his college for his English essays. Certainly his literary style gave proof of his having cultivated the humanities. His natural bent, however, was already strongly pronounced towards natural history pursuits, which at that time met with but little encouragement at the university, nor were his tastes favoured by his own family, as they did not seem likely to lead to any kind of successful career.

In 1853, however, after having taken his B.A. degree, he was elected at Magdalene College to the Drury Travelling Fellowship, which is open to the sons of Norfolk gentlemen. He was thus enabled to throw himself heart and soul into the active prosecution of science. He went abroad during several years, and made various journeys through Arctic latitudes, studying the abundant bird-life of these regions. Lapland, Iceland, and Spitzbergen were successively visited by him in the course of these wanderings, and, not improbably, he then imbibed that affection for northern forms which distinguished him. He likewise took occasion to cross the Atlantic more than once. In 1857 he was in the West Indies, and went thence to confer with the naturalists of the United States in Philadelphia and Washington. In 1862 he spent some time in Madeira.

During those fruitful years of active experience, his ready pen was busy in the description of the facts which he had observed at home and abroad. He communicated his notes to the pages of the 'Zoologist' and 'Ibis,' of which latter journal he was one of the original founders. For a long succession of years his numerous papers in these publications, and in the 'Proceedings of the Zoological Society,' on the occurrence, distribution, structure, and habits of birds, formed notable contributions to Ornithology. They so fully established his reputation as an experienced naturalist that in 1866 he was appointed to the newly-created Chair of Zoology and Comparative Anatomy in his own university. Afterwards his college elected him to a Foundation Fellowship. For more than thirty years, up to the time of his death, he lived in the picturesque Old Lodge of Magdalene, surrounded with his books and papers, always busy with important and useful work, delighted to welcome his friends to his den, and constantly on the outlook for opportunities of doing a kindness to younger men, especially to those who had tastes akin to his own. His Sunday evening receptions were an important feature in the scientific life of the University. Not a few of the naturalists who have risen into prominent positions in this country can look back to the stimulus they received from those meetings, where the advantages to be derived from personal intercourse among the younger workers were enhanced by the ever ready sympathy and encouragement of the genial professor.

Newton was all his life a keen collector. His chief interests lay, of course, among birds, but he had the instincts of a true naturalist, and was always on the watch for specimens in all provinces of the animal kingdom which would help to enlarge and enrich the Museum at Cambridge. He was likewise a lover of books, and his rooms, with their well-filled shelves, showed the wide range of his literary tastes, and the success with which he had pursued the quest after rare and valuable works in natural history. He was, above all, a philosophical naturalist, intent rather on the higher and broader questions than on details of species or of structure. He was endowed, too, with a highly critical faculty, and could express his criticisms with pungent clearness. He could never be satisfied with anything less than the completest accuracy attainable, while his literary instinct led him to cultivate great simplicity of style, in which every word was well chosen, and none was redundant.

These characteristic features of the Cambridge professor are specially to be noted in the numerous essays which he wrote on questions of large biological interest, such, for example, as the series of articles from his pen which appeared in the ninth edition of the 'Encyclopædia Britannica,' and which formed the basis of his greatest work, the 'Dictionary of Birds.' This ornithological classic, issued in successive parts from 1893 to 1896, was prefixed with a Latin inscription to his youngest brother, Edward, who "for more than fifty years had been his most assiduous fellow-student in ornithological pursuits at home, abroad, under the open sky, and in caves." It shows his critical acumen alike in what he selected for treatment and in what he omitted. His habitual caution is well illustrated by his choice of an alphabetical rather than a taxonomic arrangement of his subject, while the occasionally caustic force of his language is displayed in his preface, where he denounces some attempts at systematic arrangement as "among the most fallacious, and a good deal worse than those they are intended to supersede." "I have no wish," he adds, "to mislead others by an assertion of knowledge which I know no one to possess."

Alfred Newton was one of the first naturalists in this country to give in his adhesion to the views propounded by Charles Darwin as to the origin of species. A few years after the publication of these views he contributed to the 'Proceedings of the Zoological Society' (1863) an interesting confirmation and illustration of Darwin's remarks on the way in which seeds may be dispersed by birds, describing the case of a partridge which had been found with its foot firmly imbedded in a lump of hardened earth. In the address which he gave to the Department of Botany and Zoology at the meeting of the British Association in 1876, while praising the then recently published volume by Alfred Russell Wallace on "The Geographical Distribution of Animals," he emphatically refers to the modern theory of evolution as worthy of "the chief glory in giving a real and lasting value to the interpretation of the facts of animal distribution."

The subject of the distribution of plants and animals over the surface of the globe was one to which Newton devoted much thought, and on which he wrote with his characteristic breadth, caution, and critical discernment. His treatment of the "Geographical Distribution of Birds" in the ninth edition of the 'Encyclopædia Britannica' may be referred to as an excellent example of the way in which he looked at such questions of wide biological bearing. He naturally took a deep interest in everything connected with the extinction of species. In the address to the British Association above referred to, he drew a vivid and humorous picture of the effects of human interference with the economy of nature, picturing the consequences of man's occupation of an island, as seen in the destruction of its indigenous fauna and flora, and their replacement by the animals and plants introduced by him—pigs, goats, rats, rabbits, ferrets, sparrows, and starlings. He entered an eloquent plea for an endeavour to protect and preserve the native forms, and he claimed that the naturalist alone had the knowledge that should guide the efforts to promote the use and prevent the abuse of the animal world. Unfortunately, though something has since been done in the direction pointed out by him, the indiscriminate slaughter, which he so feelingly deprecated, still goes on in various parts of the world. That this subject lay near to Newton's heart was shown by his returning to it in his admirable article on "Extermination" in the 'Dictionary of Birds.'

One who contemplated with such keen regret the approaching extermination of many remarkable forms of life could not but feel a saddened interest in those which have disappeared within the times of human experience. Newton was a diligent collector of all the information that could be obtained regarding the Dodo. He wrote a number of papers on this subject, and his article on it in his 'Dictionary' may be cited as an illustration of the learning and the exhaustive treatment with which he could discuss a matter that strongly appealed to him. In the same way he devoted himself to tracing out all that could be ascertained regarding the haunts of the Great Auk or Gare Fowl, so recently exterminated. He published several papers on the subject, and one of the objects of his last yachting cruise was to visit the ledge among the Orkney Islands, where the bird had its latest British home.

For many years during the later part of his life Newton had an annual opportunity of enjoying pleasant and easy travel, and of visiting some of the most crowded haunts of bird life in these islands. His friend, Henry Evans, of Derby, also an accomplished ornithologist, gladly welcomed him on board his steam yacht and directed her course to any coast or island that the

Professor wished to see. Year after year "Alfred the Great," as Evans used playfully to call him, was received with open arms not only by his host, but by every member of the crew. And no one could look forward with keener zest to these holidays than Newton, when for some weeks he could escape from the cares of University life to the firths and sounds of the west and north of Scotland, where no letters could reach him, even if he had left an address behind him, which he was generally careful not to do. Nowhere could he be seen to be more completely in his element than on board of the "Aster." He loved the sea and its associations with such a sturdy affection that inclemencies of weather, by no means infrequent in those regions, never drew from him the least sign of impatience, or seemed in any degree to disturb his habitual cheeriness and his enjoyment of the cruise. Clad in the light-grey tweed suit which did duty on these voyages, but without top-coat or waterproof, he would sit for hours on some exposed part of the vessel, smoking innumerable pipes and watching for every variety of sea-fowl that might show itself either in the air or on the water. In the course of a few days sun, wind, rain and salt spray told on his complexion, which then assumed a ruddiness that would have astonished the inmates of Magdalene College.

The sharpness of his eyesight in the detection of birds on the wing, even when he had nearly reached the age of seventy years, was always an astonishment to his companions. And the enthusiasm with which each fresh form was greeted by him as it flew overhead became infectious to all on board. Most of the crew reappeared year after year from their winter employments to take their places in the annual cruises, and some of them became almost as cunning in bird-life as their master. In successive seasons Newton was in this way enabled to visit almost every bay and sea-loch from the Mull of Cantyre to the furthest promontory of the Shetlands.

He repeatedly anchored at St. Kilda, and had excellent opportunities of seeing there at the height of the nesting season the most marvellous and varied crowds of sea-fowl anywhere to be found among the British Islands. Nor were the voyages confined to the Scottish coast. He one year sailed round the whole of Ireland, and was thus enabled to compare the bird-haunts of the Irish cliffs with those of Scotland. Twice the yacht carried him round the Faroe Islands, and afforded him a further display of that boreal bird-life which from his young days had such charms for him.

These cruises formed an important element in Newton's life during his later years. He looked forward to them with almost boyish exuberance and delighted afterwards to recount their varied incidents. They not only provided a healthful and delightful holiday, but kept him still in close personal touch with birds, which had been the main interest and study of his life. In spite of the lameness which was understood to have been the result of an accident during infancy, he was often the first to enter the boat which had been got ready for a landing on some surf-beaten rock, or for a closer inspection of the caves and stacks at the foot of a bird-haunted precipice.

On such occasions, so self-dependent was he, he would gently repel offers of the assistance which was always at his service. It was only when the increasing feebleness of his limbs would have made such assistance indispensable that he reluctantly gave up the annual cruise.

Continuing to hold the zoological professorship for the long space of forty-one years, taking also an active part in the conduct of general business, Alfred Newton became a distinct living force in the University. To him should be ascribed no small share in fostering the rise and progress of the natural sciences towards a recognised place in the scheme of studies of Cambridge. His scientific reputation in the world outside was sustained within the walls of the University by the stimulating and suggestive form of his teaching, by his enthusiastic devotion to the development of the Museum of Zoology, and by his untiring but not obtrusive advocacy of the claims of science. But his wide and beneficent influence in Cambridge sprang also in large measure from his strongly-marked personality, wherein kindness, courtesy, and fidelity, were combined with a fearless independence, an impatient antagonism to untruthfulness in every shape and degree, and a habit of frankly and forcibly expressing his convictions.

A. G.

SIR JOHN EVANS, K.C.B. 1823—1908.

THE death of Sir John Evans has removed from the Royal Society one who for forty years has been among its most conspicuous members, who for half of that long period filled the office of Treasurer, and who from first to last has taken an active and useful part in the general business of the Society. His eminent capacity in the conduct of affairs, the unremitting devotion with which he employed that talent in the Society's interest, and the genial courtesy which marked his intercourse with the Fellows have given him a strong claim on their grateful remembrance.

He came of a stock wherein both science and literature had been cultivated. His grandfather, Lewis Evans (1755—1827), the first mathematical master in the Royal Military Academy, Woolwich, studied astronomy and was elected into the Royal Society in 1823. His father, the Rev. Dr. Arthur Benoni Evans (1781—1854) was headmaster of the Grammar School at Market Bosworth and a prolific writer, who published many poems and theological works, together with a book on 'Leicestershire Words, Phrases, and Proverbs' (1848). His maternal grandmother belonged to a Huguenot branch of an old French family, and from her he perhaps inherited his lightness of heart. He was born on 17th November, 1823, at Britwell Court, Burnham, Buckinghamshire, and was educated under his father at Market Bosworth. Although entered for matriculation at Brasenose College, Oxford, he did not eventually proceed to the University. His education, however, under the paternal roof had been excellent. He had acquired such a knowledge of Latin and such an acquaintance with classic authors as remained a life-long possession to him. Every now and then, in the course of conversation, some happy phrase or line from a Roman poet would occur to him, with which he would light up and enforce the remarks he was making. His archæological writings indicate how diligently he sought in ancient literature such references as might illustrate the early history of mankind. His father's care in his upbringing was further shown by his being sent for a short time to Germany, in order to gain some facility in speaking the language.

Instead of entering the University, he, in 1840, at the age of seventeen, embarked on a commercial career. His maternal uncle, John Dickinson, the head and founder of the well-known firm of paper-makers of that name, and a man of scientific tastes, who became a Fellow of the Royal Society, invited him to join the staff at the paper-making works of Nash Mills, near Hemel Hempstead. John Evans found there the settled home in which he lived almost up to the end of his life and which became more widely known as the abode of the active and enthusiastic antiquary, numismatist, and geologist than as the headquarters of a commercial company. Having married his cousin, the daughter of the head of the firm, he was, in 1851, admitted as one of the partners, and in course of time he in turn became the senior member of the firm. To those who met him only in his business relations he was an active and enlightened paper-maker, keenly alive to every modern improvement in machinery and in the processes of manufacture, gifted with great clearness of judgment and remarkable capacity for mastering the most complicated details of business. His energy and initiative largely contributed to the success of the various enterprises of the firm. For many years he was President of the Paper Makers' Association, and took a leading part in the conduct of its affairs.

But while thus sedulously attentive to commerce he found leisure to gratify his strong bent towards the study and collection of antiquities and the prosecution of several branches of scientific enquiry. His taste for geology seems to have been developed even in boyhood, for he is said, when nine years old, to have hammered a collection of fossils out of the Wenlock limestone quarries at Dudley. But his geological proclivities were eventually drawn in two main directions, partly by the requirements of his business and partly by his love of antiquities. In paper-making an ample water supply is essential, and in Hertfordshire the subject of water-rights has long been keenly discussed. Evans, in the interest of his firm, studied the question of water-supply, both from the geological and the meteorological side, and he became on these matters a recognised authority, whose advice was often sought and always valued. No one stood up more stoutly and successfully than he for the conservation of the water-supply of his county, which was again and again threatened by the great metropolitan water companies. This important question being thus forced on his attention by pressing practical considerations, he devoted much time to its study. He explored the superficial deposits in all parts of his district as well as the water-bearing strata that lie deeper underground. In the course of these enquiries he was led to investigate the relations between rainfall and evaporation, and the percolation of rain through soil—subjects regarding which little information was available at the time when he began his researches. From the year 1853 he had under his own immediate care the rain-gauges and percolation-gauges which had been erected at Nash Mills in 1836 by his uncle.

He was drawn into the geological field by another and different pathway. In the first decade of the latter half of last century the discovery of what were alleged to be implements of human fabrication in the old river gravels of the north of France gave rise to a keen discussion among men of science. The conclusion, which some of the early observers drew from the evidence, that man must have lived on the earth for a far longer period than had generally been supposed, naturally aroused much interest among the general public. As far back as 1841 Boucher de Perthes had obtained from the old gravel terraces of the valley of the Somme, at Abbeville, numerous chipped flints which he recognised to be the handiwork of man. In 1847 he began to publish his observations, but they met with little or no support among his fellow countrymen. On the contrary, they were either ignored or denied and even derided, though one or two competent French geologists were convinced of their probable truth. It was not until the autumn of 1858 that Hugh Falconer, who then saw the collection made by Boucher de Perthes at Abbeville and was satisfied that the shaped flints were truly human implements, urged Joseph Prestwich to undertake an examination of the geology of the valley of the Somme, with the view of determining the precise position of these implements and of ascertaining whether or not there was evidence to prove their high antiquity. This task was accomplished in the spring of 1859 by Prestwich, who took Evans with him to assist in the investigation. The conjoint labours of these two observers, which completely demonstrated the accuracy of the French discoverer's observations and conclusions, formed the first important step in winning general acceptance to the opinion, which had been so stoutly contested, that the human race, together with various tribes of animals that have been long extinct, must have inhabited Western Europe for a long succession of ages, wherein the rivers cut their way deeply into the valleys which they traverse. Prestwich communicated his results to the Royal Society, while Evans submitted a statement on the subject to the Society of Antiquaries, which had elected him one of its number in 1852. This paper appeared in the 'Archæologia' (vol. 38, 1860, p. 280), under the title of "Flint Implements in the Drift; being an Account of their Discovery on the Continent and in England."

The journey with Prestwich formed the turning-point in Sir John's scientific career. From that time onwards he specially devoted himself to the investigation of the earliest traces of man which have been preserved in river-gravels, brick-earths, cavern deposits, or elsewhere. He became one of the most enthusiastic and successful collectors of flint implements. His singularly good powers of observation enabled him to detect them even on ground that had been already searched for them, and in any company of hunters for these objects he was generally the most fortunate. Even on a surface so long inhabited as that of Egypt his trained eyes enabled him to pick them up. Both abroad and at home he purchased freely every illustrative type which he could procure, until in the end he had amassed such a series of these objects as is probably possessed by no other private collector.

Throughout his life he continued to publish from time to time notices of the progress of discovery in regard to the occurrence and distribution of flint implements. So recently as December, 1907, he communicated to the Geological Society what proved to be his last paper, on "Some Recent Discoveries of Flint Implements," wherein he expressed his matured opinions regarding the probable origin of the high-level gravels in which these relics of primitive man have been found.

But besides writing these scattered papers, Evans rendered a great service to the progress of archæology by his published volumes, in which he gathered together all the evidence which had been accumulating in different countries as to the types and distribution of the various relics of early human workmanship. The first of these separate works appeared in the summer of

1872 with the title of 'The Ancient Stone Implements, Weapons, and Ornaments of Great Britain.' It at once took its place as the chief authority on the subjects of which it treats. The learning displayed in its earlier chapters, the careful arrangement of its material, its detailed yet interesting descriptions, and the importance attached throughout its pages to the stratigraphical position in which the relics had been found showed it to be no mere antiquarian enquiry but a treatise conceived and executed on thoroughly scientific lines. It had an important influence in connecting the pursuits of archæology and geology, by the way in which it marshalled the evidence for a chronological sequence in the relics of early man, and showed that the conclusions derived from a consideration of varieties in types of workmanship were supported by the geological evidence derivable from the positions in which these several types were found. In the midst of the numerous and multifarious duties which claimed his constant attention he brought out a second edition of the work in 1897, greatly revised, and incorporating a large amount of new material.

Although Sir John Evans chiefly occupied himself with the archæological side of geology, he occasionally ventured into other parts of the geological domain, and his incursions of this kind were always marked by the same quickness of insight and shrewdness of inference. It was he, for instance, who first detected that the toothed jaw which lay detached on the same slab of stone that contained the original specimen of *Archæopteryx* probably belonged to that ancient type of bird—a surmise which was completely confirmed twenty years later by the discovery of a second specimen wherein the jaws with pointed teeth lay in place in the skull. At another time he devised an ingenious piece of mechanism to illustrate how he supposed that great changes of climate might be brought about without any shifting of the earth's axis of rotation. By means of a moving wheel, on the rim of which he placed a weight between the pole and the equator, he showed that the centrifugal force gradually drew the weight towards the equator and he contended that on the supposition that the interior of our globe is a liquid mass enclosed within a shell of fairly uniform density and thickness, the effect of the elevation of a great mountain chain midway between the pole and the equator would be to draw the shell over the liquid nucleus until the original position of the pole might be moved as much as 45° to the south ('Roy. Soc. Proc.,' vol. 15, 1867, p. 46). He subsequently formulated his hypothesis as a definite mathematical problem ('Quart. Journ. Geol. Soc.,' vol. 32, 1876, p. 62). The mathematicians, however, who investigated it did not regard it as tenable, and there were formidable objections to his postulate as to the condition of the earth's interior. But Evans, though he bowed to the weight of authority, probably never wholly abandoned his view.

His more purely antiquarian work hardly comes within the purview of the Royal Society, but no notice of his life would be complete without some reference to that side of his activity. He began early in life to collect and study coins, and he became in the end one of the most accomplished numismatists of his day. His first independent volume, 'The Coins of the Ancient Britons,' published in 1864, possessed singular interest and value from the abundant evidence it supplied of the existence of a gold coinage in England before the coming of the Romans, and from its ingenious proofs (which had been first published by him as far back as April, 1848), that these British coins had originally been imitations of a stater of Philip of Macedon, but by successive copying of the imitation had become so rude that, but for the preservation of the intervening stages of debasement, the origin of their pattern would never have been surmised. Another of Evans' antiquarian writings which has taken its place as the standard treatise on the subject of which it treats is his 'Ancient Bronze Implements, Weapons and Ornaments of Great Britain and Ireland.' From the nature of these objects and the positions in which they have generally been found they do not furnish the same kind of geological evidence as to their relative dates, and the author discussed them mainly from the antiquarian side. As an instance of Sir John's watchful zeal and singular success as a collector of antiquities, the writer of this notice may allude to an incident which occurred a few years ago. On his way to Greece, Evans had picked up at a dealer's in Paris a well-preserved gold coin of one of the Roman Emperors and showed it to the friends whom he met in Rome. A few days after his arrival in the Italian capital he astonished these friends by producing another beautiful gold coin which he had bought from a dealer there—a coin of the wife of the same Emperor.

Sir John Evans was elected into the Royal Society in 1864. In the course of three years he was chosen to serve on the Council, a position which he again filled from 1873 to 1875. His business capacity on the Council was further recognised in 1878 when he was elected Treasurer of the Society. This distinguished and responsible office he continued to hold for the long term of twenty years. During that period he was unremitting in his care of the Society's finances, which he left in an orderly and sound condition.* At the same time he took an active part in the conduct of the general business, his practical knowledge of affairs and his experienced judgment always giving to his counsel an especial value. On the occasion, in 1884, when the President, Professor Huxley, was disabled by ill-health, Evans prepared and delivered the Anniversary Address. As, in the absence of the President, the Treasurer, who is usually also a Vice-President, takes the Chair, Sir John had frequent opportunities of presiding both at the Council and in the meetings of the Society. His conduct as Chairman was often singularly felicitous in the tact and humour of his remarks.

While his activity in the Royal Society was thus so marked, he had time and energy to spare for the claims of other societies. He had an especial affection for the Geological Society, which, as far back as 1857, had enrolled

* On his retirement from the Treasurership, Sir John gave an interesting account of the state of the Society's finances, and showed how considerably the funds had increased during the time in which he had held office.—'Year Book' for 1899, p. 160.

him in its ranks. For eight years, from 1866 onward, he was one of its secretaries until, in 1874, he was chosen to be its President. In 1880 he received its Lyell Medal "in recognition of his distinguished services to geological science, especially in the department of Post-Tertiary Geology." For the last thirteen years he has been its Foreign Secretary—an office which he only resigned last February, when he found that his increasing feebleness of health prevented him from regular attendance at the meetings of the Council. At the Society of Antiquaries, the Royal Numismatic Society, the Anthropological Institute, the British Association, and many other societies he has held the highest offices, and has for many years been a familiar and valued associate.

Those who met him only at scientific meetings in London might naturally take him to be a denizen of the capital, entirely engrossed in the work of the various societies in which he played so prominent a part. In reality, his headquarters were always at his home, in Hertfordshire. Not only was he fully immersed in the conduct of the paper-making works at Nash Mills, but at the same time for many years he stood out as the most active and prominent public man in the county. He was appointed High Sheriff of Hertfordshire in 1881, and for some years he filled the offices of Chairman of Quarter Sessions and Chairman of the County Council. The friends and neighbours who chose him for these responsible positions, whether or not they could appreciate his reputation in the scientific world outside, knew him at home as a worthy county gentleman, more capable than most of them of grasping and directing business matters. The universal testimony of the authorities in Hertfordshire at the time of his death was a touching tribute to the influence which he exerted among them, to their high personal esteem for him, and to the great value of the services which he had, in many varied ways, rendered to his county. It was a fitting recognition of these great public services, as well as of his reputation as an antiquary and man of science, when his neighbour, Lord Salisbury, in 1892, asked Queen Victoria to confer on him the honour of K.C.B.

Sir John Evans was married three times. His first wife, to whom allusion has already been made, left three sons and two daughters. One of these sons is the well-known explorer of Knossos, and now a Fellow of the Royal Society. The second wife, daughter of Mr. Joseph Phelps, died without children. Lady Evans, who survives her husband, is the daughter of Mr. Charles C. Lathbury, Wimbledon, and an accomplished classical scholar and antiquary. She has one daughter.

Those who were privileged to know Sir John Evans in the intimacy of private life mourn the loss of a true friend and a charming companion. His advice, so often asked and so freely and cordially given, has been a guide to many who survive him, for his long experience of men and things gave to his judgment a clearness and decision which were eminently helpful. He would spare himself no effort actively to serve one in whom he took interest. His invariable courtesy of manner seemed to belong rather to the quiet stateliness of a past generation than to the hurried intercourse of modern life. In a difficult situation, where tact as well as firmness was required, his qualities were altogether admirable. His conversation, always interesting, was often witty. He could rapidly throw off impromptu verses in which some passing incident was humorously depicted, and his memory, stored from a wide range of reading, enabled him often to interject a happy quotation. These characteristic features he retained almost unimpaired up to the last, even though the ailment which finally carried him off was gradually sapping his strength and causing him much suffering. He bore this burden bravely to the end, and died on 31st May, 1908, in the 85th year of his age.

A. G.

HENRY CLIFTON SORBY, 1826—1908.

THE ranks of British geologists have lost one of their most distinguished ornaments by the death of Dr. H. C. Sorby, who for more than half a century has been looked up to all over the world as the great master by whom modern Petrography has been regenerated. He came of a family that has been connected with the staple industry of Sheffield since the sixteenth century. One of his ancestors was the first Master Cutler of the Cutlers' Company, who died in 1628. His grandfather became, in turn, Master Cutler. His father was a partner in the well-known firm of John and Henry Sorby, edge-tool manufacturers. His mother, Amelia Lambert, of Queen's Square, London, appears to have been a somewhat remarkable woman, from whom he not improbably derived most of his versatile ability and powers of concentration.

He was born on May 10, 1826, at Woodbourne, near Sheffield, an estate which belonged to his father and which he inherited. His early education was obtained at the Sheffield Collegiate School. He used to tell that he there obtained, as a prize for arithmetic, a book entitled 'Readings in Science,' to which he ascribed no small influence in giving him his bent towards research and experiment. His tastes in that direction were further fostered, after he left school, by a mathematical tutor, the Rev. Walter Mitchell, who, having had a medical training, had become a fairly good anatomist and chemist, and who initiated his pupil into these subjects, besides superintending his mathematical studies. When this accomplished teacher left him, young Sorby, not being under the necessity of choosing a profession, determined to devote himself to a scientific life. He continued to study mathematics, optics, chemistry, and anatomy. He found time also for the prosecution of water-colour drawing. "I worked," he said, "not to pass an examination, but to qualify myself for a career of original investigation."* It must be acknowledged that this training was eminently successful in producing a man of science who, gifted with remarkable originality of mind, marvellous industry, unwearied perseverance, and singular mechanical ingenuity, attained distinction in various branches of science, and left his mark on every domain of research into which he entered.

Being in possession of ample means, he determined to remain at Sheffield, where opportunities for conducting experimental research offered themselves, and he made that town his home up to the end of his long life. He resided with his mother until her death in 1874. Thereafter, being free to take longer journeys, he bought a yacht, the "Glimpse," and for many years spent the summer months dredging and making biological and physical observations in the estuaries and inland waters of the east of England. The winters were spent in Sheffield, carrying on his experiments. For some years past he is known to have been engaged in working up the results of various researches made long ago, of which the details had never been published. Even when confined to bed, in the last months, from the effects of an accident, his mental activity continued as vigorous as ever. During that time he prepared and sent to the Geological Society a long and elaborate paper, and carried on his correspondence with his own hand. He continued to busy himself with his notes even up to the day before his death. On the night of Sunday, March 9, last, he lost consciousness, and lingered till the following evening, when he quietly passed away in the eighty-second year of his age.

In looking over Sorby's published papers, more than one hundred and fifty in number, one is first impressed by the extraordinary mental versatility which they display, and the uninterrupted continuity with which they came from his pen, from the time when he was one-and-twenty, up to within a few days of his death. His earliest published communication appears to have been one "On the Amount of Sulphur and Phosphorus in Various Agricultural Crops," which not only appeared in the 'Memoirs of the Chemical Society' and the 'Philosophical Magazine,' but was translated into 'Froriep's Notizen.' It was probably suggested by some of the chemical studies which he had carried on with his tutor. He soon struck out into a more original path. It was into the geological domain that, influenced by his environment, he was first attracted, and it was there that he found the widest field for his peculiar powers, and achieved his greatest success. The rivers Don and Rother, which flowed near his home, drew his attention by the evidence presented in their valleys that the streams had not always kept to their present channels, but had wandered to and fro across their alluvial plains. He was thus led to examine the internal arrangement of alluvial deposits,

* The writer of this obituary notice is indebted for information to an Address by Dr. Sorby on his 'Scientific Investigations during the last Fifty Years,' given to the Sheffield Literary and Philosophical Society, on February 2, 1897, and published in the 75th Annual Report of the Society in 1898. The occasional citations in this notice are taken from that Address.

and by the happy accident of a rain shower, which led him to shelter in a sandstone quarry, his eye caught the profile of the lines of current-bedding in the old Carboniferous alluvia. He at once perceived the interest and importance of these lines as indications of the direction and variation of the currents by which the sediment had been transported and laid down.

For more than ten years Sorby continued to devote much time to the study of this subject. He watched the action of streams and of sea-waves in the deposition of detritus, and contrived an ingenious piece of mechanism by which this action could be illustrated. Travelling over the country, he was always on the watch for rock-sections in which the history of sedimentation could be traced. By the evidence which these sections afforded he determined the direction of the currents that had carried the various layers of sand and gravel, and he speculated upon the probable site of the land from which this detritus had been derived. In this way he attempted to reconstruct the geography of the country at different geological periods. Thus the internal structures of the Oolitic rocks of the Yorkshire coast suggested to him the former existence of land between Norway and Scotland. Another series of observations furnished him with materials for discussing the probable physical geography of Central Scotland during the time of the Old Red Sandstone. He wrote a succession of papers in which he applied the same methods to the investigation of the former condition of the south and south-east of England during different times of geological history.

In these early researches, as we now learn, there were included many elaborate experiments, the detailed results of which were not published at the time. These results and a full discussion of their bearing on the history of sedimentation occupied the time of their author during the last months of his life. They were communicated by him to the Geological Society only a few weeks before his death, and they appeared after that sad event in the form of the paper already referred to, consisting of more than sixty pages with five plates, which has since been published in the 'Quarterly Journal' of the Society. This interesting memoir, his last legacy to the science he loved so well, supplies an excellent illustration of the manner in which he sought to apply experimental physics to the study of rocks. It is full of suggestion, and even where the results he obtained may seem too uncertain to warrant implicit confidence in them, they may serve to show the lines along which further research should be prosecuted. He was himself perfectly aware that some of them were probably only approximately correct, but he felt justified in giving them to the world. "It appears desirable," he said, "to do the best I can with the material at my disposal, hoping to lead others to do what I intended to do, and correct such errors as are now unavoidable."* The seed which he thus sowed may yet yield a further harvest of knowledge in this department of geological investigation.

At an early stage in his career Sorby vividly realised the fascination and

* 'Quart. Journ. Geol. Soc.,' vol. 64, p. 172 (May, 1908).

the potency of the microscope as an instrument of scientific research. His youthful studies in optics had, no doubt, made him familiar with all the details of its mechanism, and capable of adapting it to any line of investigation in which he might find it of service. It was natural that he should first apply this instrument to the solution of some of the problems which his work among sedimentary rocks suggested. As far back as the year 1831 a method had been devised and described by William Nicol of Edinburgh, whereby slices of fossil-wood could be mounted on glass and made so thin as to become transparent. In this form these sections were shown to reveal every detail of their internal organisation. Nearly twenty years passed before any geologist seems to have been induced to avail himself of this means of studying the minute structures of rocks. The first who did so was Sorby. He began by examining thin sections of the Calcareous Grit of the Yorkshire Coast, and he sent a communication on this subject to the Geological Society, which appeared in the 'Quarterly Journal' for 1851. As he advanced in his investigation of the microscopic characters of limestones and marls, he saw that the same method of study might be applied not only to sedimentary rocks, but to those of igneous origin, including even the most fine-grained and opaque. He thus entered a wholly new and untrodden field in geological enquiry.

The chemical composition of igneous rocks had long been investigated and was fairly well known. But though the component minerals of close-grained masses might be more or less probably surmised from the results of chemical analysis, no really accurate and precise information on this subject could thereby be obtained. In the early years of last century, indeed, Cordier had shown that by crushing fine-grained rocks and washing and separating the grains of their powder, their component minerals might be isolated and examined under the microscope. But this method threw little or no light on the relations of these minerals to each other in the genesis of the rocks composed of them. In 1858, however, Sorby's great paper "On the Microscopical Structure of Crystals indicating the origin of Minerals and Rocks" was published by the Geological Society. This masterly essay revealed that much of the origin and history of igneous rocks could be ascertained by means of the microscope. Not only was it shown to be now possible and easy to determine the several minerals, even in a close-grained rock, but to discover the order in which they had successively crystallised, the conditions of temperature and pressure in which their solidification had taken place, and the alterations which the rock composed of them had subsequently undergone. Sorby, besides making use of transmitted and reflected light, was familiar with the delicate applications of polarised light, and found with what great advantage they could be employed in the study of rocks.

It was from this memorable paper that the remarkable modern development of the petrographical side of geology took its rise. In every country where the study of rocks is pursued, the methods first indicated by Sorby have been followed. While many developments and improvements of his methods have been introduced, he is everywhere acknowledged to have been the "Father of Modern Petrography." To have entirely revolutionised this important branch of research and to have opened a new and boundless field of investigation into the past history of our globe will ever constitute his chief claim to a high place among his scientific contemporaries.

One of the geological problems which as far back as 1851 had interested Sorby was that of the origin of the slaty cleavage of rocks. This subject had been discussed by various observers, notably by Adam Sedgwick, who worked out with great skill the distribution of the chief lines of cleavage that have affected the rocks of Wales. He showed the intimate relation between the strike and the cleavage of large disturbed masses, and thus prepared the way for the true solution of the problem, although he himself favoured the notion that the structure was the result of the action of electric currents. Daniel Sharpe subsequently insisted that cleavage must be due to mechanical pressure, but as Sorby remarks, "little notice was taken of what he said, because he did not show that the ultimate structure of the rock was really such as would be produced by this cause." This relation of the effect to its producing cause was first experimentally demonstrated by Sorby. He satisfied himself that cleavage has no connection with electric currents, but is simply due to great mechanical pressure, whereby this structure has been superinduced in rocks along planes perpendicular to the direction of the pressure. He found the microscopic structure of cleaved rocks entirely to support this view, which he further illustrated and confirmed by ingenious experiments. Thus, by mixing scales of oxide of iron with pipeclay and subjecting the mass to strong lateral pressure he obtained a perfect cleavage structure. In his paper descriptive of these observations, published in 1853, he dwelt on the proofs which, as thus interpreted, the cleavage structure furnishes of the gigantic compression undergone by mountain masses during their elevation. His contention was soon afterwards supported by Tyndall and others, who showed that even in homogeneous substances like beeswax a cleaved structure could be induced by mechanical pressure. Hence, for more than half a century cleavage has now been recognised as one of the most convincing proofs of the enormous compression which the more disturbed parts of the earth's crust have undergone. For the establishment of this fact geologists are, in the first place, indebted to Sorby.

In the course of his investigation of the minute structures of rocks, and with the accumulating evidence before him of the enormous pressure to which many parts of the earth's crust have been subjected, it occurred to him that the mechanical force involved in great subterranean pressure may have been partly resolved into chemical action, as in other circumstances it might be resolved into heat, electricity, or other modification of force, and that in this way various puzzling appearances in rocks might receive an explanation. Accordingly, in his usual way, he set to work to test this hypothesis by direct experiment. After making a large series of investigations, he succeeded in proving that there undoubtedly is a direct correlation between mechanical pressure and certain kinds of chemical action. The results of this important research formed the subject of the Bakerian lecture which he gave to the Royal Society in 1863. The most striking geological illustration of the truth of his conclusion was furnished by him from various limestone conglomerates which have suffered severe compression and in which the pebbles have indented each other, the solution of their substance being greatest at the points of contact where the effects of pressure were most pronounced.

Sorby's interest in chemical questions, which began in his youthful days with his tutor, continued active all through his life, and gave a special character to many of his geological and mineralogical papers. One of his early observations, for example, related to the origin of magnesian limestone by the alteration of an ordinary calcareous deposit. In another enquiry, which involved an extensive series of experiments, on the production of artificial pseudomorphs, by the action of cold or highly-heated solutions, he showed that certain rocks, like the Cleveland Ironstone, were originally composed of carbonate of lime, which has been replaced by carbonate of iron derived from the associated strata. Again, he conducted a long investigation into the occurrence of the two forms of carbonate of lime—calcite and aragonite—in the shells of mollusca, and he was thereby led to some interesting and important conclusions. He found that some shells are composed of calcite, some of aragonite, and others partly of the one and partly of the other in distinct layers. He ascertained, further, that calcite, being in a state of stable equilibrium, could not be altered into aragonite; whereas aragonite, being unstable, could easily pass into calcite. Hence calcite shells may be preserved in a limestone and even retain their microscopic structure, while those made wholly or partly of aragonite may have lost their internal structure or may have been entirely effaced.

His microscopic studies of minerals had usually a chemical side. This feature of his work was especially illustrated by the numerous papers which he wrote in the year 1869. He then announced some new applications of the microscope to blow-pipe chemistry. He detected and described the minute crystals which he had detected in blow-pipe beads. He carried out an elaborate investigation into the nature of the liquids observable in various minerals, especially in sapphires, rubies, spinels, aquamarines and emeralds, and made many measurements of the rate of expansion of these liquids with increase of temperature. In the fluid cavities of sapphires he found that the volume of the liquid, when heated from 0° to 30° C., expands from 100 to 150, and he, consequently, inferred that it must be liquid carbonic acid. In the course of these researches, he had occasion to study the zircon, or so-called "jargon," of Ceylon, a mineral in which a remarkable assemblage of distinct elements had been shown by spectrum analysis to be present. At first he believed that some peculiar and characteristic spectra, which he obtained in the jargons, indicated the presence of a new element, to which he gave the name of Jargonium. Further examination, however, convinced him that this conclusion was erroneous, and that the peculiar spectra belonged to some compounds of the oxides of uranium with zirconia. With characteristic frankness he at once published an acknowledgment of the mistake.*

From the investigation of the microscopic structure of terrestrial rocks Sorby was naturally led to enquire into the structure and probable history of those masses of mineral matter which come to us from outer space in the shape of meteorites. He soon found that the olivine enclosed in these stones contains excellent "glass-cavities," proving that it was once in a state of igneous fusion, likewise "gas-cavities," like those so common in volcanic minerals and indicative of the presence of some gas or vapour. He ascertained that the minerals in meteorites, usually considered to be identical with those in terrestrial volcanic rocks, nevertheless present some characteristic differences in structure. When he turned to the siderites and siderolites or iron-meteorites, he soon saw that in order to gain an insight into their structure and probable origin it was desirable first to study various artificial irons. In this research he ascertained that certain microscopic structures very closely similar to those in some varieties of meteorites, could be artificially produced. He was thus enabled to indicate, as far back as 1864, how much may be learnt as to the structure and composition of different types of artificial iron by the aid of the microscope. Notwithstanding the obvious practical importance of his observations and conclusions in relation to the development of our iron industry, they attracted no attention. At last, after some twenty years, the matter was taken up seriously in 1887 by the Iron and Steel Institute. Sorby was then requested by that Society to consider, together with Dr. John Percy and Sir Henry Bessemer, the best way of illustrating a complete paper on the subject. "In those early days," he remarks, "if a railway accident had occurred, and I had suggested that the Company should take up a rail and have it examined with a microscope, I should have been looked upon as a fit man to send to an asylum. But that is what is now being done. What I really proved was that various kinds of iron and steel are varying mixtures of well-defined substances, and that their structure is in many respects analogous to that of igneous rock I also took specimens of iron and steel and acted upon them with acid, so that it was possible to print from them as from types, and show many interesting points connected with their structure." Sorby continued his investigation and published various papers on the subject during the following decade. He is now recognised as the great pioneer in micro-metallography, and his methods have proved of great practical use in the manufacture and testing of iron and steel. As a mark of this recognition one of the most important constituents of steel has been named after him, Sorbite.

One of Sorby's most useful inventions to which he was led in 1865 by his study of meteorites is the spectrum-microscope.† He applied this

* 'Roy. Soc. Proc.,' vol. 17, p. 511, and vol. 18, p. 197.

† It was fully described, together with its method of use and its application, in 'Roy. Soc. Proc.,' vol. 15 (1867), pp. 433–455.

instrument to a large number of different substances, and published some forty papers in different branches of scientific enquiry wherein colour plays a part, such as the pigments in human hair, birds' feathers, the shells of birds' eggs, and the colouring matter in almost every group of plants. One of the practical applications of this invention to which its author attached importance was the detection of blood-stains. He stated that "as small a quantity as a hundredth part of a grain may be detected under circumstances in which it would be utterly impossible to recognise it either microscopically or chemically even if present in much larger amount." It is interesting to note that many long years after these researches were made, when he lay on what proved to be his death-bed, he returned to this subject and collected some of his notes on experiments upon the colouring matters of plants, which he sent in the form of a communication to 'Nature'—the last paper which he published.*

After the year 1879 Sorby spent five months of every year on board his yacht chiefly among the waters that surround and penetrate the low coast line of the south and east of England. Having no rocks to notice in this region, he was led to enter new fields of enquiry wherein he manifested the same mental activity and ingenious mechanical resourcefulness. Meteorological changes engaged much of his attention. He wrote on the colour of the clouds, sky, and sea, and on forecasts of the weather as deduced from the rainfall and changes in the barometer. For many years he continued to take observations of the temperature of the estuaries and more open waters. In 1882 he spent seven hours a day for 240 days in studying the Thames in connection with the enquiries made by the Royal Commission on the Drainage of London. He applied the microscope to the detection of sewage-contamination and of the purifying influence of minute animals and plants. His numerous researches on these and other subjects enabled him to lay before the Commission a large mass of important evidence which he believed had considerable influence on the findings in their Report.

At frequent intervals he published accounts of the scientific results of his yachting cruises. These were usually communicated to the Sheffield Literary and Philosophical Society, sometimes to the 'Essex Naturalist.' More detailed observations on some of the groups of animals he collected at sea were now and then sent to the Linnæan Society. But perhaps the most interesting and memorable outcome of these cruises was the ingenious methods which he devised and perfected for preserving even the most perishable forms of marine life and exhibiting them as permanent preparations or as effective lantern-slides. At the soirées of the Royal Society in 1898 and 1899, he exhibited some of these preparations which attracted much attention from the perfection with which the internal structure of the animals was revealed by them. He had found by experiment that a great variety of modes of treatment for different animals was required to secure the best results. Excellent transparent lantern-slides of some forms were

* 'Nature,' January 16, 1908, p. 260.

obtained by mounting the objects on glass with Canada balsam. Actiniæ and other organisms were killed with menthol, the addition of a little of which to sea-water was found to cause the animals to expand very fully. In this distended condition they were preserved in formalin. It was further ascertained that excellent preparations of various marine animals could be made by placing them in strong glycerine, the index of refraction of which is so nearly that of the soft tissues of these organisms, that it makes them more or less transparent; they then look bright and life-like, and much of their internal structure is distinctly visible.

It has to some of Sorby's friends been matter of regret that he did not himself follow up the consequences of his own discoveries, but left this to be done by others, while he himself passed on into fresh fields of observation and experiment. But when his peculiar gifts are considered, it may be felt that he not improbably employed them better in the course which he adopted. He seemed to flit from subject to subject, but there was generally a well-connected mental chain in these transitions. When new ideas were suggested by the progress of one of his enquiries, he was apt to branch off into collateral investigations, which might soon become in his eyes more fascinating and important than that out of which they arose. He used to say of himself that his difficulty was to avoid discovering new things, but that for him, at least, it was "possibly better to invent new things than to work up old ones thoroughly." It must be admitted that there were few subjects to which he for a time devoted serious attention, which he did not enlarge and illumine.

This habit of divergence necessarily led to the accumulation of a vast mass of notes of the results of observations and experiments. In 1898, when receiving the presentation of his portrait, he said of himself: "The majority of my friends can have little idea of the amount of material I have collected in connection with a variety of subjects. The great difficulty I now have is to find time to work that material into shape and to publish it. I hope to be able to do so, but I am beginning to think it is a doubtful question."

How Sorby could be, as it were, enticed into one enquiry after another, until he had travelled far away from his starting-point, is well illustrated by his own account of what followed his prolonged investigation of the Thames. While engaged in studying the estuary of that river, he was naturally interested in the remarkable topographical changes which the surroundings of the Isle of Thanet have undergone within historic times. The evidence of these transformations, partly geological and partly antiquarian, suggested to him various enquiries, in the course of which he found it necessary to examine Roman, Saxon, and Norman buildings, to study their respective building materials and to carry out a great many experiments. In seeking further light on the subject from illuminated manuscripts he was struck by the variations in the unit of length employed by the scribes, and he drew some inferences therefrom as to the several countries where the MSS. had been written. Once embarked on the study of manuscripts he could not resist the temptation to enquire into ancient conceptions of cosmogony and geography, the archæology of natural history, and the origin of the ideas connected with the more or less mythological animals met with in ancient art. In pursuance of the quest he took up the study of Egyptian hieroglyphics, since he found so many of the natural history stories to be traceable back to Egypt and Babylonia. Addressing his friends in Sheffield on these matters he added, "with a view of carrying out this very extensive subject, I have collected most of the original works of importance from the earliest period down to mediæval times. This study will, I hope, ultimately lead to some important results in connection with the history of science and art." His vast preparations, however, remain behind him as a memorial of the indefatigable energy, the vivid interest in a wide range of enquiry, and the thoroughness of method which he retained unimpaired at the age of more than three score years and ten.

Dr. Sorby was elected a Fellow of the Royal Society in 1857. In 1863 he gave the Bakerian Lecture to which reference has above been made. He received a Royal Medal in 1874 "for his researches in Slaty Cleavage, and on the minute structure of Minerals and Rocks; for the construction of the Micro-spectroscope, and for his researches on Colouring Matters." He served on the Council in 1876—77.

His connection with the Geological Society was not less close. He became a Fellow of that body in 1850, and in 1869 received the Wollaston Medal, the highest distinction in the gift of the Society. He was elected President in 1878, and during the two years in which he held office he gave two memorable Addresses in which he discussed the minute structure and the origin of limestones and of non-calcareous sedimentary rocks. He communicated a number of masterly papers to the Society's Quarterly Journal, the last of the series, as already mentioned, having been his latest scientific achievement, written in bed shortly before his death.

Living all his life at Sheffield among his books and experiments, he was personally known to a comparative small circle of his contemporaries. Those who had the advantage of his acquaintance or his friendship were struck with his unassuming, childlike, trustful disposition, his kindly and helpful ways, and, above all, with the singularly absorbing ardour with which he would talk about what was engaging his attention. He would discourse with the same animation, and almost in the same language, to a child as to a master of a subject. One of his peculiarities was a scrupulous regard for his health. It is supposed that he never in the course of his life got thoroughly drenched with rain.

In his native town, where his fellow citizens were well aware of his high scientific reputation, his participation in municipal affairs would have been heartily welcomed. But he always shrank from mingling in public life. His name, indeed, was added to the Commission of the Peace, but he was hardly ever seen on the magisterial bench. He was keenly interested, however, in educational matters, especially in the provision of adequate technical instruction. Thus he was one of the active founders of the Firth College, out of which the Sheffield University has sprung, and the benefactions made by him during his life to that institution have been supplemented by a generous legacy in his will. It was in the rooms of the Literary and Philosophical Society, however, that he appears always to have found the most congenial company. For more than half a century he was the most active and useful member of that body. He was always pleased to communicate to it accounts of the progress or results of his researches, whether carried on in the laboratory or on board of the yacht. The jubilee of his connection with the Society was fitly celebrated in November 1898, by the presentation to him of his portrait as a token of personal esteem and a recognition of his world-wide fame as a man of science. This portrait, which now hangs on the Society's walls, has been well reproduced in autotype. It is an excellent likeness, which will serve to perpetuate the features of one of the most distinguished of the geologists of the Victorian era.

A. G.

SIR JOSEPH FAYRER, 1824—1907.

SIR JOSEPH FAYRER was born at Plymouth on December 6, 1824, and died at Falmouth on May 21, 1907. His father was a naval officer who served under Lord Cochrane, Marryat the novelist being one of his messmates. On his mother's side he was descended from John Copeland who took David, King of Scots, prisoner at the battle of Neville's Cross. His childhood was spent in the Lake District where he knew Wordsworth, Hartley Coleridge and John Wilson (better known as Christopher North), the editor of 'Blackwood's Magazine.' At the age of fifteen he began to study engineering, but he was very anxious to go to sea, and, as he was too old for the Navy, he made several voyages on a merchant vessel. On one of these he visited Bermuda during an epidemic of yellow fever, and became so much impressed with the importance of the medical profession that he determined to enter the medical service of the Navy, and accordingly commenced his studies at Charing Cross Hospital in 1844. One of his fellow students, with whom he contracted a warm friendship, was Thomas Henry Huxley, and this friendship, to a great extent, determined Huxley's career. In the chapter of autobiography prefixed to his Essays, Huxley says: "I was talking to a fellow-student (the present eminent physician, Sir Joseph Fayrer) and wondering what I should do to meet the imperative necessity of earning my own bread, when my friend suggested that I should write to Sir William Burnett, at that time Director-

General for the Medical Service of the Navy, for an appointment." This appointment he obtained, went for his famous cruise on the "Rattlesnake," and made the zoological observations which not only brought him fame, but settled his path in life.

After finishing his medical studies and becoming qualified to practise, Fayrer obtained a commission in the Navy, and was sent to the naval hospital at Haslar, where one of the assistant-surgeons was Andrew Clark, afterwards President of the Royal College of Physicians. Fayrer had only been a short time at the hospital when Lord Mount-Edgcumbe invited him to travel with him. They travelled together through Germany, Switzerland, and Italy. While they were at Palermo, fighting occurred between the Sicilians and Neapolitans. At Rome, he took his degree of M.D., being the first Protestant on whom it had been conferred. After his travels were over he did not return to the Navy, but obtained a commission in the Artillery and sailed for India on June 29, 1850, before his friend Huxley had returned from his voyage on the "Rattlesnake." In less than two years he was sent to Burmah, where he so distinguished himself that Lord Dalhousie appointed him to the post of Residency Surgeon at Lucknow, regarding which he says in an autograph letter: "I have purposely reserved it, that I might bestow it, as the best medical appointment in the gift of the Governor-General, upon the assistant-surgeon who should be found to have rendered the most approved services during the war with Burmah."

The extraordinary energy which had gained Fayrer distinction found full scope in the manifold duties of his new office, for, in addition to his work as Residency Surgeon, he had to superintend the hospital and other institutions, and to fill the office of postmaster. Shortly afterwards, he was appointed honorary Assistant-Resident, so that he was obliged to add political work and correspondence to his already onerous duties. When the King of Oude was deposed, the care of his horses, elephants, camels, wild animals, and artillery was thrown, in addition, upon Fayrer's shoulders. In spite of it all, he managed to reorganize the Post Office, and to extend its operations over the whole province. When the Indian Mutiny broke out, Fayrer's house at Lucknow was used both as a fortress and hospital, and during the famous siege he not only did his share of fighting, but had to prevent sickness from overcrowding, and to treat the wounded, among whom were Henry Lawrence, Outram, and Napier.

After the Mutiny was over, Fayrer returned to England, broken down in health, in March, 1858, but, instead of resting, as most men would have done, in order to recuperate, he entered at Edinburgh University as a medical student, rubbed up his classics so as to pass the preliminary examination, studied chemistry, botany, and anatomy, worked at the hospital, passed a special examination, and took his doctor's degree in March, 1859. A month later, in April, 1859, he began work as Professor of Surgery in the Medical College Hospital of Calcutta. Here, again, his wonderful energy enabled him to do the work of several ordinary men. Besides his lectures at the hospital, he had to give courses of operative surgery, perform numerous operations, and attend to private practice. In spite of all this, however, he managed to find time for scientific work, and made investigations into the pathology of various febrile and septic diseases of India which had previously received there little or no attention. He took up the hygiene of hospitals, and drew official attention to the defects in structure and sanitation which rendered the Indian hospitals unhealthy. But the research in which he took the greatest interest was his zoological work on the snakes of India, and his physiological investigation into the action of their venom. The difficulties under which his scientific work was carried on are shown by the fact that he had often to leave an experiment in order to attend to his hospital work, and while there amputating a limb, or performing some other operation, his mind would be disturbed by anxiety regarding the condition of his private patients who were anxiously waiting for him.

His scientific interests were very wide in character. It was in consequence of meteorological work that he had done that he was elected a Fellow of the Royal Society of Edinburgh in 1859. When on the Council of the Asiatic Society of Bengal, he proposed an ethnological investigation of the races of India. This proposal produced some useful reports, but was never fully carried out. Another proposal to form a Zoological Society and Gardens in Calcutta, which he made when President of the Asiatic Society in 1869, was more fortunate, and, though delayed for a time, it was ultimately carried into effect.

For a time Fayrer was surgeon to the Governor-General and also President of the Medical Faculty of the University. In 1870, he accompanied the Duke of Edinburgh on his travels through the North-West of India, and Lord Mayo in the Terai in the following year. In 1872, his health failed, and he returned to England, where he became President of the Medical Board at the India Office. His magnificent work on the Thanatophidia of India had been published by the Government, and, after his return to England, he resumed, in collaboration with Lauder Brunton, the researches he had begun in India on snake venom. Their researches on Cobra Venom were published in the 'Roy. Soc. Proc.' No. 145, 1873, and No. 149, 1874, and on Crotalus Venom in 'Roy. Soc. Proc.' No. 159, 1875. They examined the antidotal action of many substances, and found that permanganate of potash, which Fayrer had already tried, appeared to be most promising ('Roy. Soc. Proc.,' 1878, vol. 27, p. 465).

In 1875, Fayrer accompanied King Edward VII, who was then Prince of Wales, on his tour through India, and but for his extensive knowledge and firm decision in difficult circumstances, the Prince might have been induced by the earnest entreaties of various personages to visit infected places, with the probable result that cholera might have spread over large districts of India, and that our King might never have returned from his visit to that part of the Empire.

In 1876, he was elected a Fellow of the Royal Society, and was a Member of Council in 1895. As President of the Medical Board at the India Office, he had much to do with matters of public health, and in addition to his official work, he became President of the Epidemiological Society, in 1879, gave the Croonian Lectures on the "Climate and Fevers of India," at the Royal College of Physicians, in 1882, represented the Government of India at the International Cholera Conference, in Rome, in 1884, and was President of the Section of Preventive Medicine in the Hygienic Congress in London, in 1891.

He was a good linguist, and was obliged to acquire a fair knowledge of Hindostani and Persian, in order to conduct the correspondence necessitated by the offices he held at Lucknow. He knew sufficient Italian to be qualified to pass the examinations for M.D. at Rome, and to make a speech in Italian when he was representing the Royal College of Physicians at the Tercentenary of Galileo at Padua.

The law regarding experiments on animals prevented him and Brunton from continuing the researches on antidotes to snake venom they were making in 1875, but in 1903 Captain Leonard Rogers was able to continue their work by means of Professor A. D. Waller's method of keeping animals continuously under chloroform for thirty-six hours or more. In conjunction with him they published a joint paper in the 'Roy. Soc. Proc.,' 1904, vol. 73, p. 323, and the method there described has since been successful in saving several lives which would otherwise have been lost.

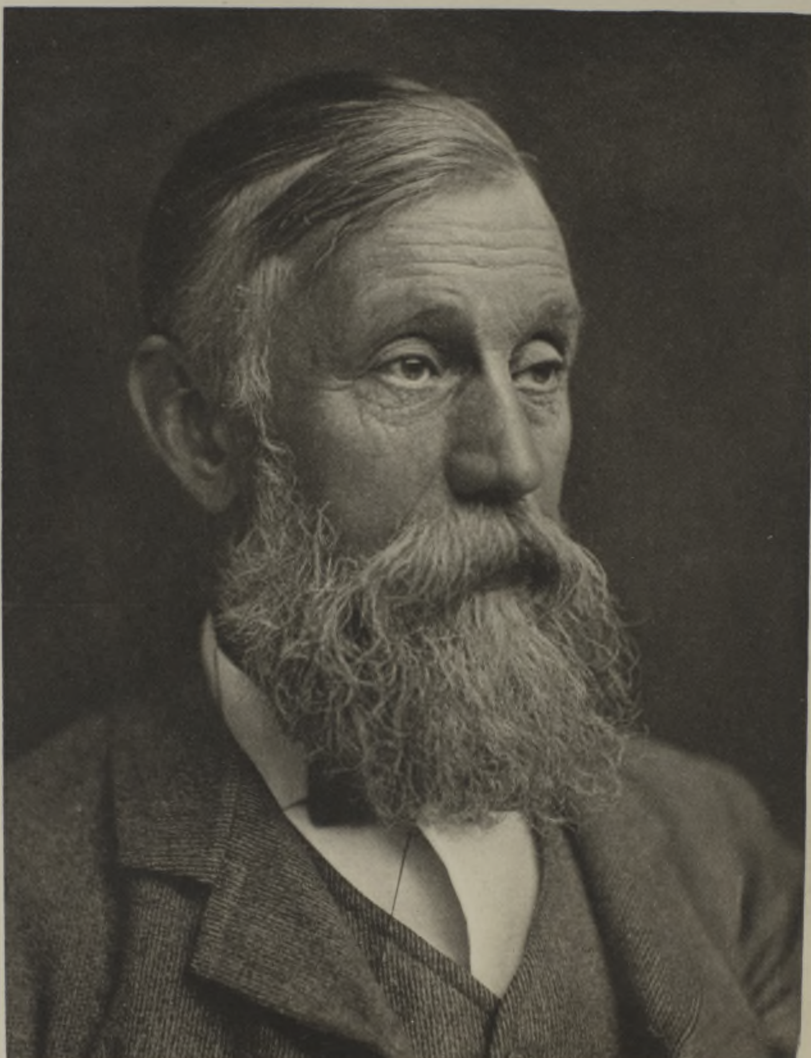
In trying to sum up Fayrer's work, one meets with the great difficulty that so much of it was official, and the credit for such work goes rather to the office than to the individual. Thus the enormous amount of good which he did in his official capacity cannot be estimated, except from the official recognition it received, not only during the Burmese War, but in every office which he filled.

His chief scientific work consisted in his early meteorological observations, his proposal of an ethnological investigation of the races of India, his foundation of a Zoological Society and Zoological Gardens at Calcutta, his contributions to sanitation and to the pathology of Indian diseases, and, most of all, in his work on venomous snakes. His monumental work on the Thanatophidia of India is the best and most comprehensive on the subject, and the researches which he began in India, and continued with the collaboration of others in England, have now led to a method of treating the bites of venomous snakes, which can be applied to bites of all kinds, and used everywhere.

There was a remarkable similarity, in many respects, between Fayrer and his friend and fellow-student Huxley, and this likeness was the attraction which drew them together, and led to their close friendship. It has been said that in every human face a resemblance may be traced to some animal, and this was markedly the case both in Fayrer and Huxley. Especially in his later years, Huxley's face and head suggested that of a lion, while Fayrer's large open forehead and calm expression reminded one of an elephant, and one could hardly look at him without thinking how rightly the Hindoos have chosen an elephant's head for their god of wisdom. Both men were alike in the extraordinary energy they possessed, in the stern uprightness of their characters, in the extent of their knowledge, and the wideness of their interests, in the clearness of their views, the correctness of their decisions, their absolute fearlessness, their prompt and energetic action, their firm determination to carry out whatever they thought right, in their tenacity of purpose, in a certain impatience of opposition, and in their great success in overcoming it. Associated with these qualities which compelled admiration were an extraordinary kindness and tenderness of heart, which gained the affection of all who knew them. In Fayrer, the writer of the Notice lost one of his best and truest friends, who could always be confidently relied upon in case of need. This feeling was shared by every one of Fayrer's friends, from the lowest to the highest. At his funeral, one of the wreaths bore the gracious inscription : "For auld lang syne, from Edward VII."

L. B.

[The Obituary Notice of Lord Kelvin has been issued as No. A 543 of 'Proceedings.']



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M. Foster

SIR MICHAEL FOSTER, 1836—1907.

Born at Huntingdon, March 8, 1836, eldest son of Michael Foster, F.R.C.S., of that town, he was educated at Huntingdon Grammar School until he was thirteen years of age, when he proceeded to University College School, in 1849.

Here, under the tuition of Dr. Key, the head master, he distinguished himself in classics, and in 1854 took his B.A. degree at the London University, on the Arts side, taking the first place and the University scholarship in classics. So great was his bent towards classics, and so highly did Dr. Key think of his abilities, that he would have undoubtedly tried for a classical scholarship at Cambridge, had the fellowships been at that time open to Nonconformists; but the Foster family is distinguished among Non-conformists in East Anglia. To this early classical training, and to his great friendship with Huxley, that master of lucid scientific writing, is undoubtedly due the wonderfully clear and fascinating style of all his writings.

At University College School he took great interest in cricket, and during his time there was captain of the eleven. His love of the game remained throughout his life. During his residence at "The Granhams," when his students were few in number, he inaugurated an annual cricket match beween the staff of the laboratory and the students, in which he always took part. This annual match was played on a field belonging to him, and was a great success; subsequently, when the class became too large and the physiological laboratory was but one of many others, this match became replaced by one between the teachers in the various laboratories and the assistants. Foster was captain of one side, and played in the match regularly up to 1895.

Cambridge and a classical career being closed to him, it was determined that he should follow his father's footsteps and enter the medical profession. Accordingly, in 1854, he entered the medical side of University College, London, and the practice of the hospital.

In 1856, he obtained gold medals in anatomy and physiology and in chemistry, and took his M.B. degree at London University in 1858 proceeding to the M.D. degree in the following year.

In the year 1859–1860 he went to Paris to continue his medical studies, and returned to England in 1860. At this time, signs of pulmonary disease appeared, and he therefore obtained an appointment as surgeon on the S.S. "Union," which went to the Red Sea to build a lighthouse on the Asaruf Rock, opposite Mount Sinai.

Throughout his life, or at all events through the earlier part of his career at Cambridge, the dread of consumption haunted him, and from the time of his first arrival, he made up his mind that he could not live in Cambridge itself, but that as far as possible he must live an open air life. For this reason, he took a house—"The Granhams"—at the foot of the chalk hills known as the Gogmagogs, nearly four miles from Cambridge, and later, in 1879, he built for himself a house on the summit of one of these hills on the same road as was his former house. In both houses he tried to make a rule of spending the afternoon in his garden, often working hard with the spade or pick. Excellent as this plan proved in staving off further tubercular troubles, it certainly prevented him from obtaining that full weight in the councils of the University to which his ability and the services he had rendered the University entitled him, for the morning hours are taken up in teaching and the administrative business of the University is of necessity undertaken in the afternoon.

In 1861, he commenced practice with his father at Huntingdon, and remained there until 1867, having, in 1863, married Miss Georgina Edmonds, daughter of Mr. Cyrus Edmonds. His married life with his first wife was short, for she died in 1869, leaving him two young children to look after.

For six years he remained in practice, but all the time his longing was for a scientific career. He had joined the British Association in 1859, and had attended the meetings in Oxford, Cambridge, and Dundee, so that when an invitation came from his old teacher, Sharpey, to give a course of practical physiology at University College, he accepted it, and relinquished medical practice for ever.

Sharpey's influence over Foster was very great; in after years he used always to say that in the dark ages of physiology in England, when physiology was a mere appendage to human anatomy, the only teaching being in most schools a course of lectures given by the professor of human anatomy and physiology, it was Sharpey alone who kept the lamp of research alight, he alone who recognised that advance in physiological knowledge could come only by experimentation, he alone who instilled into the minds of all his pupils that lectures by themselves were of little use, but that the student must see experiments for himself in order to obtain a real knowledge of the subject.

Besides Sharpey, Foster was much influenced by Claude Bernard, although when he was in Paris he does not appear to have attended any of his lectures. His appreciation and admiration for him appear in every page of that delightful memoir of Claude Bernard written by him for the 'Masters of Medicine Series,' a book, the dedication of which runs as follows:—"To the physiologists of France, both to those who had the happiness to know Claude Bernard in the flesh and to those who, like myself, never saw his face, this little sketch is dedicated in the hope that, as he has been to me a father in our common science, so may I be allowed to look upon them as brethren." A third great influence in the making of Foster was undoubtedly Huxley. He succeeded Huxley as Fullerian Professor of Physiology at the Royal Institution in 1869, and in 1870, when Huxley commenced his course of elementary biology at South Kensington, he was assisted by Foster, Ray Lankester, and Rutherford. Just as Sharpey and Claude Bernard had impressed upon his youthful mind the importance of experimental work in the study and teaching of physiology, so his intercourse and friendship with Huxley led him to take the broadest views of the meaning of physiology, strengthened his biological bias, and was, perhaps, the chief cause of his life-long effort to place physiology at the head of the group of biological sciences.

On the Continent at this time, experimental research in physiology was much more universal in the laboratories than in England, but there, too, the instruction was mainly given by lectures, with demonstrations during the lectures; there was no organised system of practical instruction.

The practical course of physiology and histology, inaugurated by Sharpey, which he gave over entirely into the hands of Foster, retaining for himself the theoretical part of the teaching, was unique in Great Britain at that time, and to it must be traced the emancipation of physiology from the bonds of human anatomy. The time had come when physiology was to take its true place in the annals of science and the experimental method, the only true way of advancing scientific knowledge, to attain by leaps and bounds its present high position in Great Britain; the man alone was required, and that man was Foster.

As is so frequently the case in scientific advance, the initiative came from the University of Cambridge, not indeed from the University itself, for that is a body ill provided with funds and slow to act, but from Trinity College. The original suggestion came apparently from George Eliot and George Henry Lewes, who were great friends of W. G. Clark. He and Coutts Trotter felt, and persuaded the College, that the time had come when it would be of advantage to the University for separate teaching in Physiology to be given. They therefore approached Huxley and asked him to help them; he replied without hesitation, "I know the very man for you, a young fellow at University College called Foster."

So Foster came to Cambridge as Trinity Prælector of Physiology, not belonging to the University but in it. As far as the University was concerned he had no status, no vote in the Senate, for though an Hon. M.A. degree was conferred upon him in 1871, it did not in those days carry with it any of the privileges of the ordinary M.A. degree. He was ineligible for election to any Board of Studies, could therefore only make his voice felt in the University through his friends.

The University granted one small room, now part of the Philosophical Library, to the Trinity Prælector of Physiology. Here Foster began his lectures, and here was at first his only laboratory for histology, chemical and experimental physiology. He brought with him, from London, H. Newell Martin as his demonstrator.

From this small beginning arose the whole Biological School of Cambridge.

Foster's teaching was a revelation: it was all new, not to be found in any English text-book, all so suggestive, opening out vistas of research, showing how little was known, how much remained to be found out.

Up to that time Humphry, who was Professor of Anatomy and Physiology, had given an annual course of physiology, interesting undoubtedly, but based on structure rather than on experiments on animals; the only practical work a few mounted specimens shown under a microscope. In those days there was no separate examination in physiology for the second M.B. examination. No wonder that Foster's lectures came as a revelation, and, in combination with the enthusiasm and sympathy of the man, caused many of the small band of his earliest students to decide there and then to take up a scientific career and follow him.

At the time when he came to Cambridge, biological sciences were represented by professors of the old school. Humphry was Professor of both Anatomy and Physiology, as was customary in medical schools of that time, and was well known as a surgeon and anatomist. Babington was Professor of Botany; his lectures consisted largely of the botany of the flowering plants, and he was distinguished as a systematist. Newton was Professor of Comparative Anatomy and Biology, and his attention was concentrated on the anatomy and distribution of the vertebrates; he was particularly distinguished for his knowledge of birds.

Foster, who, with Huxley, had initiated the teaching of elementary biology on evolutionary principles, and was thoroughly imbued with the great principle that physiology was one of the biological sciences, and must go hand in hand with botany and zoology, from the very first determined to form a biological school in Cambridge, which should be of the most advanced character and second to none.

For this purpose he carefully studied the bent of his various students, and picked out Balfour to study the new science of embryology and Vines to work at the new botany. In his own department of physiology he had besides H. N. Martin as workers and helpers Langley, Lea, Gaskell, and Dew Smith. By this means he gradually built up a school of biology of a newer type, running side by side with the University teaching, unpaid by the University, recognised only by the allocation of rooms. Here was Foster's strong point: rooms there must be for practical work and research. He would have nothing to do with the old system of teaching almost entirely by lectures. From the first the new zoology and the new botany must have rooms for practical work just as much as the new physiology.

Such a growth was only possible, owing to the endowments of the colleges; for these young and enthusiastic teachers who gave their whole time to Foster and his work, with neither appointment nor salary from the University, could not have done so but for the fellowship system and the recognition by their respective colleges that they were doing good work worthy of support.

This, however, would have availed but little but for the man himself. Not only would he point out the direction in which advance in any science was to be looked for, but by his earnestness, his lovable charm of persuasion, his entire freedom from any thought of monetary gain, or any kind of selfishness, the conviction was gradually borne in on his pupils that the particular line of research on which each was engaged was the one thing in life worth doing, and that the only place to do it was in Cambridge by Foster's side. As Foster used to say, the true man of science must feel with respect to his own research that "in this way only lies salvation." It was that feeling that he had so pre-eminently the power of raising in a man.

Soon the fame of the Cambridge Biological School began to spread over the country, and a very rapid increase in the number of medical and scientific students took place. The makeshift buildings in which Foster, Balfour, and Vines had up to this time taught their students were hopelessly inadequate, and new laboratories were a crying need. With the help of his friends, especially Coutts Trotter, Balfour, H. Sidgwick, J. W. Clark, and Newton, the University was persuaded to build a biological laboratory, which was completed in 1878. Subsequently this laboratory was extended so as to give more room for physiology and zoology, and quite recently a splendid botanical laboratory has been erected.

In 1883, consequent on the Report of the Royal Commission, a professorship of physiology was founded, and Foster was elected to the chair; the complete degree of M.A. was conferred upon him, and at last, after 13 years, he was able to speak for himself in the Administrative Boards of the University.

In 1872 he married Margaret, the daughter of Mr. Rust, Cromwell House, Huntingdon, who survives him.

Foster held very strong views as to the proper method of teaching physiology to students at the beginning of their medical study. He held it to be a mistake to demonstrate during the lecture, and insisted that practical work, carried on by the student himself, illustrative of the facts on which the lecture was based, must immediately follow the lecture. The physiology of each organ must be dealt with as a whole in lecture, and the practical work must be so arranged as to bring home to the student all the points of each lecture at the time, and not to be regardless of the lecture, as must be the case if the practical work is departmental while the lecture course is general. His ideal laboratory would be of sufficient size to provide every student with his own working place, both in the histological and in the chemical department at the same time. He also—and this was one of the great reasons of his success—encouraged his pupils at the very earliest moment to engage in some original research, and then persuaded them to give a few lectures of an advanced character upon the subject on which they were working; for, as he said, there is no way of discovering the gaps in your knowledge of a subject better than lecturing on it. In this way he associated with himself a band of younger workers engaged in research, who gave the advanced teaching to the students, thus allowing him to confine himself to the introductory course.

In the researches suggested to the students and in the minds of his students themselves, he always inculcated the close connection between the physiology of all living organisms, insisting upon physiology being a branch of zoology and of botany, to be studied as the greatest scientific biological subject, and no longer to be looked upon as an adjunct to anthropotomy.

With all these broad-minded philosophical ideas, he would never have succeeded to the extent that he did but for his personal influence not only in his own circle but in the University at large, which enabled him to establish respect for a new study in University circles naturally wedded to older studies. In about fifteen years, his and his group's influence succeeded in obtaining University and College recognition in fairly full measure for the subject as one on a par with the older studies of the place.

It is impossible to overestimate Foster's services to physiology. He was the prime mover in bringing together all English workers in physiology by the foundation of the Physiological Society. At its origin the Society was formed in consequence of the passing of the Vivisection Act in 1876, and was simply for the purpose of dining together at stated times with the object of interchanging views and keeping watch on the working of the Act; subsequently it was determined that a scientific meeting should precede the dinner, and Foster insisted from the very first that such meetings should, as far as possible, be demonstrational.

This gathering together of English physiologists into a society has been of enormous benefit to the progress of physiology. Still greater, perhaps, was the foundation of the 'Journal of Physiology.' Up to 1878 the only journal, apart from the 'Proceedings of the Royal Society,' in which physiological papers were published, was the 'Journal of Anatomy and Physiology,' edited by Humphry and Turner. The time had come, in Foster's and in Sanderson's opinion, for a journal devoted exclusively to physiology, and he determined to found one. From the very first the journal was run on dignified lines—paper, typography, figures, and plates being so good as constantly to excite admiration in the United States, in Germany, and other places—a policy which certainly for a long period of time involved financial loss.

The foundation of the 'Journal of Physiology' is yet another instance of the devotion which Foster inspired in his pupils, for it was the admiration which Dew Smith had for him which led him to insist that what Foster undertook should be of the best kind, regardless of expense; Dew Smith undertook to bear the financial loss, and he took the Journal under his special care as far as plates, illustrations, paper, and printing went. His affection for Foster, and his desire in every way to further his interests, led to the founding of yet another most valuable aid not only to physiology but to science in England generally, viz., the Cambridge Scientific Instrument Company. At that time laboratory research was much hampered in England by the difficulty of getting any special instrument made for any particular research. In Germany the instrument makers were willing to make special instruments for which there was not likely to be any further sale, but in England it was very much more difficult. The large instrument makers looked upon it as a special favour to undertake such an order, and often the research was delayed because of the length of time required in making some necessary piece of apparatus. Dew Smith saw that here he could greatly help Foster and physiology, and so he started in Cambridge a workshop expressly to turn out anything required in scientific laboratories as quickly as possible, to which was afterwards added a drawing and lithographic establishment. This workshop, the foundation of which was indirectly due to Foster, has been a great boon to science, especially in its later development as the Cambridge Scientific Instrument Company.

Foster held very strongly the opinion that science was cosmopolitan, that all workers in physiology should be banded together in one brotherhood—the brotherhood of science; the practical carrying out of this belief manifested itself in two ways, both of which have been very fruitful and of great benefit to English physiologists. In the first place, when he started the 'Journal of Physiology,' he determined to obtain American as well as English co-operation, and on the title-page of the first number appear the names of Bowditch, Martin, and Wood, as well as of Gamgee, Rutherford, and Sanderson.

In the second place, when the International Medical Congress met in London in 1881 he and Kronecker together drew up a scheme for a separate International Congress of Physiologists to meet every three years, and a Committee was formed. The first Congress was held at Basel in 1889 and they have been held with great success every three years since that date. Their success is largely attributable to Foster's powerful influence, always exercised and repeatedly emphasised to make the proceedings demonstrational, to the exclusion of papers and to make the social proceedings as cheap and informal as possible.

In 1872 he was elected a fellow and in 1881 he succeeded Huxley as one of the secretaries of the Royal Society, a post which he held until he resigned it in 1903. During these twenty-two years Foster's influence in the world of science was very great indeed; he became personally acquainted with the leading men of science of every department, and with his broad-minded scientific spirit he set himself to further and aid scientific progress in every direction; thus apart from purely biological subjects he took an active part in the establishment of the National Physical Laboratory, in the rearrangement of the Meteorological Office, and in starting the International Congress of Geodesy. In 1897 he was president of the physiological section of the British Association at Toronto and in 1899 he was President of the Association at the Dover meeting.

Perhaps his most important work as Secretary was the establishing of close, confidential, and frequent relations between the Royal Society and Government Departments. Foster believed this to be to the advantage both of the country and of the Society; many of the Society disagreed with him strongly on this point; whether right or wrong in his judgment he carried that policy through until the Society had become expert adviser to a number of Government departments as a routine thing. These departments placed great confidence in Foster and in numberless cases he stood with them for the Royal Society itself. The way in which the Government departments went at once in medical matters to the Royal Society for advice and assistance, as if it were the fountain of medical knowledge as well as scientific, simply represented the great reliance placed on Foster.

All who knew him, especially those who worked with him at the Royal Society during these twenty-two years, felt how intensely Foster cared for and devoted himself to the welfare of the Society, inspiring all of those with whom he worked with a conception of the Society as a living active factor in the life of the nation, as well as the leading scientific club.

He was a member of the Committee appointed by the Colonial Office to advise as to the best means of preventing malaria and other tropical diseases, and his services were recognised on his death by an official letter from the Colonial Office to Lady Foster.

He was appointed by the Government to serve on various Royal Commissions: that on vaccination in 1889, on the disposal of sewage in 1898, and on tuberculosis in 1901, of which latter he was Chairman. For these services and in recognition of his services to the cause of science he was created in 1899 Knight Commander of the Order of the Bath.

Foster, who was a great reader of scientific papers in all languages, always impressed upon himself and others the maxim that the next best thing to knowing a thing is to know where to find it; and he felt strongly that unless steps were taken soon it would become very rapidly more and more impossible to know where to find it. He had already been one of the most active promoters of the Royal Society Catalogue of Scientific Papers, and he threw himself heart and soul into the greater scheme of an International Catalogue of Scientific Papers. He was the soul of the whole thing: he got the money, he raised the enthusiasm, and by the Royal Society starting it and insisting that English is the proper language for it because bound to be the most nearly universal, a stride was made in the right direction and work done that would be more difficult and costly to do later than then.

The undertaking is not yet certain of success but undoubtedly it is one of the utmost importance to the scientific men of all countries, and its very magnitude bears testimony to the boldness and persuasiveness of its author.

Nor were his energies expended only in enterprises directly connected with science. He never forgot his close connection with University College and the University of London, and he was an active member of the Statutory Commission by which the University was reorganised, and teaching functions added to the examining powers which it previously exercised.

Foster's actual additions to our knowledge by way of research are small and not of great importance. He was a discoverer of men rather than of facts, of biologists rather than of facts and theories in biology. He was an impulsive man, he very rapidly decided and acted, especially quickly did he come to a conclusion about a man's character and ability. The judgment of a man once thus rapidly formed he never seemed to relinquish. His judgment in the great majority of cases was curiously correct; in some cases it was wrong and in these it always seemed as though Foster thought that the man himself had changed not that his first impression was incorrect.

With respect to his scientific work, his paper "on the Action of the Constant Current upon the Heart of the Snail," in conjunction with Dew Smith, is a model of the way in which a research should be thought out and a scientific paper written. He it was who was one of the first to study embryology in this country, and he used to tell how he believed he was the first man to cut up an embryo chick, and mount every section in the right order in series throughout. Huxley, he said, was very pleased, and had thought such a feat hardly possible.

It was a memorable day in the history of biology when Foster, talking in the little room of the philosophical library about his future career with Balfour, who wanted to devote himself to science, but was uncertain what line of research to follow, took up an egg, cracked it, showed him the embryo inside, and said "What do you think of working at that?"

Foster's influence extended far beyond the limits of his own country. His 'Text-book of Physiology,' published in 1877, made him known throughout the world. In the excellency of its literary style, and the suggestiveness of its criticism of the unsettled problems of physiology, it was far superior to any other text-book, in recognition of which it was translated into Italian, German, and Russian. In America it was *the* text-book. In 1876, through Foster's influence, H. N. Martin became Professor of Physiology at Johns Hopkins University, Baltimore; he took over with him his enthusiasm for Foster and for his methods of teaching, and, with the aid of the text-book, which appeared in the following year, he revolutionised physiological teaching in the United States. To this day Martin is looked upon in the States as the father of the present-day method of teaching.

Apart from his text-book, Foster was known and loved throughout the physiological world. When he was elected perpetual Honorary President of the International Congress of Physiologists in 1901, there was a great prolonged outburst of applause, that seemed as though it would never stop. At the Congress of 1904, at Brussels, when he was not well enough to be present, he sent a telegram: "Though absent, I am with you." Immediately the International Committee interrupted the business going on in order that the President might read the telegram. There was genuine distress at his ill-health among the people present

This appreciation of Foster throughout the world was manifested by the various distinctions bestowed upon him. He received honorary degrees from the Universities of Dublin, Glasgow, Montreal, Oxford, and St. Andrews, and was appointed honorary or corresponding member of a large number of learned societies both at home and abroad. In 1900 he was urged by many of his friends to stand for the representation of London University in Parliament, and finally, after a good deal of hesitation, accepted and was elected. He had always belonged to the Liberal party; he belonged to a family of strong nonconformist views, but could not side with Gladstone on his views about Home Rule. He was therefore nominally a supporter of the Government, and spoke and voted on that side. His speeches, mainly on scientific matters, were always received with great attention, and his influence in the House was considerable. When, however, the Education Bill of the Government was brought forward, he found himself so strongly opposed to their views on the religious question that he felt he could no longer sit on that side of the House, especially as, in addition, he had no sympathy with any suggestion of Tariff Reform. Instead of resigning, he crossed the floor of the House, urging that he was elected as an Independent Member to represent the University of London, that he was known by all to be a Liberal in politics, not a Conservative, and had been sent to Parliament to supply a crying want—the representation of science.

His opponents made the most of his non-resignation, with the result that in 1906, when he was again a candidate, he was defeated, though by the narrow majority of twenty-four votes.

In many ways his entrance into Parliament was unfortunate. The impossibility of attending to his Parliamentary duties without neglecting his professional ones, or *vice versâ*, combined with the fact that he represented London, not Cambridge University, brought about his resignation of his professorship, in 1903.

In the same year he resigned the Secretaryship of the Royal Society, so that in 1906, when he was not re-elected to represent London University, he found himself stranded. Still, however, he was by no means devoid of occupation, for the Commission on Tuberculosis, of which he was Chairman, and the Sewage Commission, provided work and interest.

It is a matter of great regret that Cambridge University possesses no Professorial Pension Fund. If ever there was a man in the University whose services should have been recognised by a retiring pension, Foster was the man.

His great delight outside his scientific work was horticulture, and it was ever a source of wonder to his many friends that he was so successful in making rare plants grow on a bare chalk hill-side. His special fancy for a long time was the growing of irises, of which he had a wonderful collection ; he was especially successful with the Onocyclus group, of which he manufactured many new hybrids. His patience was remarkable, in many cases the seeds did not germinate till the fifth year, and in some not till the eighth year or even later from the time of planting. In later years he grew especially, in addition to irises, Eremurus of different kinds with great success.

His knowledge of horticulture and his delight in plants was recognised in the botanical world by his appointment as Chairman of the Departmental Committee, to report on the botanical collections at Kew, and at the British Museum. He wrote many articles, especially in connection with irises, for the horticultural journals.

One of the new studies which he was particularly keen to introduce into Cambridge was the scientific study of agriculture. It is mainly due to his energetic advocacy that Cambridge to-day possesses a flourishing agricultural school, and it was a great source of delight to him when the University determined to follow his lead and provide for the teaching of agriculture on a scientific and practical basis.

Zoology shared with botany his active interest; it was due largely to his co-operation with Ray Lankester, and his active support, that the Marine Biological Research Laboratory at Plymouth came into existence; an instrument of research which, with other kindred establishments, has proved of the utmost value to the progress of zoology.

His power of grasping and presenting to others a mass of complicated detail was remarkable. As senior Secretary he had for many years to bring the business to be dealt with before the Council of the Royal Society, and through his skilful presentations of the facts, the Meetings ended with but few arrears. He held his own views tenaciously, but accepted an adverse vote with good humour. He was keenly alive to the desirability of keeping the Council in close touch with the Society, and it was largely owing to him that the existing custom of the presentation of an annual report by the Council to the Fellows was introduced.

Foster was a delightful companion, full of humour, with an irresistible bubbling over kind of laugh, and a twinkle in his eye which was most infectious. He was excellent as an after-dinner speaker, and was usually expected to speak; frequently, some unexpected humorous but never ill-natured allusion to some distinguished person at the dinner, would cause roars of laughter; almost as frequently the laugh would be directed against himself.

Up to the very last he was at work, up to the last he retained his gift of humorous speaking, and on the very day on which he died (January 28, 1907), he had made an excellent speech in London at the Meeting of the British Science Guild.

This last public appearance was a fitting end to a great career. There have been many greater scientific men than Foster. It is hardly too much to say that no man ever devoted himself more whole-heartedly to science, and that if science can be served by strengthening the influence and promoting the spread of the scientific spirit, few have done it better service.

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W. H. G.

CHARLES STEWART, 1840—1907.

Charles Stewart was born at Plymouth in 1840 and died in London on September 27, 1907. After obtaining the qualification of M.R.C.S. in 1862, he returned to Plymouth, where his father and grandfather had been medical practitioners, and for a short time followed the same profession. In 1866, however, he left Plymouth for London, and from that time devoted himself entirely to Zoological and Physiological teaching and investigation. For nearly twenty years he spent his chief energies in lecturing and teaching at St. Thomas' Hospital and at Bedford College, besides, we believe, doing much incidental lecturing in the provinces. At St. Thomas' he held successively the lectureship in Comparative Anatomy, to which he was appointed in 1871, and a joint lectureship in Physiology (with Dr. John Harley) which he obtained ten years later. The necessity of earning an income set him upon this path, which was, however, pursued with skill and success.

There is no doubt that Prof. Stewart was one of the best lecturers in London. This opinion was indeed fully recognised by his nomination to deliver many Friday evening lectures at the Royal Institution and his appointment to the Fullerian Professorship of Physiology in that Institution from 1894 to 1897. Prof. Stewart's abilities as a lecturer were not exhibited in the way of oratorical display and the use of gesture. His merit was a smooth and even flow of language, simple in character and at times not merely unconventional but even familiar. Combined with this was an obvious interest in, and mastery of, the subject with which he was dealing, which carried conviction and aroused sympathy. He possessed also in a high degree that faculty so necessary to a lecturer upon biological subjects, the power to illustrate his statements upon the blackboard. The present writer heard him lecture upon fishes in the Zoological Society's House at Hanover Square on an occasion of afternoon lectures inaugurated by the Society, but since discontinued.

Discarding altogether the use of lantern or wall diagrams, Prof. Stewart depicted with rapidity and accuracy upon the blackboard coloured representations of certain tropical fishes in which even the iridescent hues of those creatures were skilfully suggested with simply a few coloured chalks.

Prof. Stewart's life work was, however, accomplished during his tenure of the office of Curator of the Museum of the Royal College of Surgeons. Though mainly devoted to teaching before his selection to the headship of that incomparable Museum, he had held the minor office of Curator of the

Museum at St. Thomas' Hospital. The performance of the duties of that post indicated a talent for museum work hardly inferior to his powers as lecturer and teacher and one which continually improved by use during his long occupancy of the Curatorship of the College of Surgeons Museum.

The duties, however, of that important office were, at the time of Prof. Stewart's appointment, as they had been in times past, not merely those of a museum superintendent. The Conservator of the Museum discharged also the duties of Hunterian Professor and was expected to deliver courses of lectures. Until the last few years of his tenure of the Conservatorship, Stewart gave annually a course of six or nine lectures upon various topics, though connected, as was natural, with the extensive collections under his control. The first series, delivered in 1885,* were upon the "Structure and Life History of Hydrozoa"; subsequently he lectured upon "Auditory Organs" (in 1886 and 1887), on "Phosphorescent Organs" (in 1890), on "Alternation of Generations" (in 1899), on the "Integumental System" (in 1889, and again in 1896), and on a variety of other subjects, the lectures generally, or at any rate frequently, laying special stress upon recent acquisitions to the Museum.

The wide outlook upon biology implied by these lectures and by Stewart's full appreciation of John Hunter's idea of a museum is perhaps responsible for the comparatively small amount of detailed zoological discovery published by him. Endeavouring to give the most liberal interpretation to the idea of "Physiological Series," Prof. Stewart devoted himself to illustrating copiously from the vegetable as well as from the animal kingdom the facts upon which anatomical and physiological generalisations were based. This left but little time for the writing of memoirs upon new anatomical facts; we find, therefore, that but little share was taken by Stewart during these years in the affairs of learned societies, whether as a contributor or as an office holder. For four years, however (1890—1894), he held the important position of President of the Linnean Society, but his contributions to the publications of that Society had been made in earlier years.

The zoological work accomplished by Stewart, though not large in amount, and chiefly published during the "'seventies," contained some important new matter. His main claim to distinction as a zoologist rests upon his work upon the Echinodermata. He discovered in the genus *Cidaris* certain organs, subsequently called after him "Stewart's organs," believed to be of a respiratory nature, but whose functions are not yet certain. Other papers upon the Echinodermata are: "On the Spicules of the regular Echinoidea," 'Trans. Linn. Soc.,' 1866; "On the minute Structure of certain Hard Parts in the Genus *Cidaris*," 'Quart. Journ. Micr. Sci.,' 1871; "On some Structural Feature of *Parasalenia*, etc.," 'Micr. Journ.,' 1880, and one or two others. His most recent memoir published in the 'Journal of the Linnæan Society'

* The writer is indebted to Mr. R. H. Burne for this and other information.

for 1907 is upon "The Membranous Labyrinth of certain Sharks," and Prof. Stewart also contributed to the 'Quarterly Journal of Microscopical Science' some notes upon the teeth of *Ornithorhynchus* and to the Zoological Society some details in the anatomy of the poisonous lizard *Heloderma*, especially relating to the glands which secrete the poison. In addition to these papers upon Echinodermata and vertebrated animals, Prof. Stewart published a few memoirs upon Corals and other Invertebrates. In all he wrote about twenty papers.

In reviewing Stewart's contributions to scientific literature, the "Catalogues" of the Royal College of Surgeons are by no means to be omitted. These recent Catalogues differ in their form and in many other particulars from the older Catalogues published during the tenure of the Conservatorship of the Hunterian Museum by Sir Richard Owen. The volumes are of smaller size, are illustrated by illustrations in the text (with an occasional Plate), and are most of them at any rate almost of the nature of treatises on the subjects with which they deal.

Stewart was not merely the editor of the Catalogue, in itself a large piece of scientific work, but wrote a great part of it, especially of the first volume.

Although hardly able himself to devote much time to the preparation of scientific memoirs, Stewart was exceedingly generous to others and anxious to assist them with the abundant material at his command. Of this the present writer can speak from his own experience. Stewart's enthusiasm for zoological anatomy was, in fact, perfectly disinterested. A total absence of even the capacity for sordid scheming, and a disposition which was, as a contemporary justly remarked of him, "unassuming to a fault," rendered Charles Stewart an ideal official and endeared him to those who were privileged with his friendship.

F. E. B.