OBITUARY NOTICES

OF

FELLOWS DECEASED.

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PAUL EHRLICH, 1854—1915.

It is fitting that some account should be given, in the publications of this Society, of a Fellow so eminent in Science and of an influence so great as Paul Ehrlich. We shall first give the chief facts of his life and afterwards consider the nature and significance of his work.

Ehrlich was born in 1854 at Strehlen, a small town in Silesia. He was of Jewish extraction, like so many others who have risen to fame, and was a cousin on his mother's side of Carl Weigert, with whom he enjoyed an intimate and valuable friendship, lasting till Weigert's death. Ehrlich's early education was received in his native place, and afterwards in the Gymnasium of Breslau. At Breslau also he attended the University for a semester and then went to Strasburg, where he took up the study of medicine. Amongst his teachers there were Waldeyer, the anatomist, whose attention he attracted by his application of aniline dyes to the staining of tissues, and Biermer, the distinguished physician. After completing his curriculum, he worked for a year in the Pathological Institute, under the stimulating direction of Cohnheim and Heidenhain, and in association with Weigert, Salomonsen, and Welsh. There is little evidence, however, that he was much swayed by the influence of any one man; certainly his field of work was chosen and laid out by himself.

In 1878 he went to Berlin, to take up his duties as chief assistant in Frerich's clinique. There he worked for several years and published his papers on hæmatology (which were the first to establish his reputation) and on *intra-vitam* staining, etc. The growing science of bacteriology naturally presented many problems which appealed to his bent of mind; but when engaged with these, he found, in 1888, that he had contracted tubercle and he had to give up work for a time. Fortunately, a satisfactory cure was effected and he was enabled to resume his researches, which were now to be occupied for some years with questions of toxic action, immunity, etc.

Returning to Berlin in 1890, he worked in a laboratory of his own for a time, but afterwards obtained a post in the newly established Institut für Infectionskrankheiten. In 1896, on the establishment of the new Serum Institute, he was appointed Director on the recommendation of Althoff, the Prussian State Minister, who early recognised his genius. When the Institut für Experimentelle Therapie was established at Frankfurt, in 1899, he was transferred to its directorship. His earlier work had been done almost single-handed, but we now see him surrounded by a large body of able workers, and equipped for his requirements in a manner which at that time had hardly been equalled. He continued his researches on immunity and instituted on a large scale experimental investigations on cancer. The latter were not of his own choosing, but were taken up at the request of the Government. He was, however, fortunate in being able to carry on contemporaneously that

remarkable series of researches in chemo-therapy which culminated in his discovery of salvarsan in 1910. He died suddenly in August, 1915, in the full height of his mental activity and vigour.

Ehrlich's earlier work, belonging as it did to no distinct branch of science, did not for a time receive the attention it merited, but the importance of his researches on immunity could not be ignored, though his views involved him in much controversy, sometimes not devoid of bitterness. It was, however, his great therapeutic discovery that set him on a pinnacle in his own country, and from this time he was acclaimed as one of the greatest. The recognition of his work in this and other countries was a great gratification to him. He was Croonian Lecturer in 1900, he gave the Harben Lectures in 1907, and he delivered a remarkable address on chemo-therapy at the International Medical Congress in London in 1913, meeting with an ever-growing welcome and appreciation. In 1908 he was awarded, jointly with Metchnikoff, the Nobel prize, and in connection with this he delivered an address on "The Partial Functions of the Cell," which contains the latest development of his views on the receptor-apparatus of cells.

Although Ehrlich thus worked in and enriched various fields of science, it is not difficult to trace a unifying principle throughout his investigations. One subject led to another; his work as a whole is an evolution. This leading principle is the affinity of the constituent molecules of living matter for various chemical substances brought into relationship with them. To treat the substance of cells according to the principles of the organic chemistry of the day did not lack in boldness, and if we waive the question as to the correctness of theory, we must admit that the results were striking. This principle in Ehrlich's work is seen first when he was a student of medicine, in connection with a paper on lead poisoning by Heubel, who claimed that the organs in which the lead accumulated had also after death the property of fixing the metal. Ehrlich recognised the difficulty of this problem of fixation and distribution by quantitative methods, and conceived the idea of using a coloured substance for the study of the problem, introducing fuchsin for the purpose with success. The period of his work at Berlin was largely occupied by this question of selective affinity, and though it supplied methods of great value to the histologist, one must recognise that a dye was employed simply as a suitable means in the study of the larger question. His duties in connection with clinical medicine, though not in themselves attractive to him, supplied problems of interest and also material for research, and in this way many of his investigations at the time came to be in connection with diseases of the blood.

By his method of "Farbenanalyse" he was for the first time able to recognise and differentiate classes of cells. He discovered the specific granulations of the leucocytes, classified these cells, and especially insisted on the fundamental difference between the lymphoid and myeloid tissues. It is still a matter of dispute in what sense this difference is to be accepted, but there is no doubt as to the fundamental importance of Ehrlich's observations both

regarding the sources of leucocytes and their behaviour in reactive processes. Further, he published numerous papers on the anæmias, leucæmias, etc., and his observations still constitute the foundation of hæmatology. We owe to him also many important methods, *e.g.*, those for demonstrating glycogen in cells, for estimating the reaction of the blood, the diazo reaction, etc. And we owe to him also the demonstration of the acid-fast character of the tubercle bacillus and the method of demonstrating it, which is still in general use.

We have already indicated that Ehrlich's researches on stains had as their object not so much the obtaining of new histological methods, though these were supplied, as the throwing of light on the larger question of combining affinity, and it was accordingly natural that he should extend his principles to the living organism. The outcome of research in this domain was twofold, viz., the discovery of the *intra-vitam* method of staining, and his work on the oxygen requirements of the organism. By the injection of methylene blue into the circulation, or by placing a small living organism in the dye, he was able to demonstrate the processes of certain nerve-cells down to their finest endings. It is unnecessary to refer to later developments of this method, or to emphasise the importance which it has had in biological study.

His monograph, 'Das Sauerstoffbedürfnis des Organismus,' published in 1885, contains an account of experiments on the relative affinity of the tissues for oxygen. In these he employed two dyes, viz., alizarin blue and indophenol, both of which are reducible to leuco-compounds, the latter being the more readily reduced. On introducing one of these dyes, in a colloidal state, into the circulation of an animal, and killing the animal some time afterwards, he found that some organs were coloured blue, whilst others had reduced the dye and contained the leuco-product. Further, some organs which ordinarily did not reduce the dye did so when a state of asphyxia was established. By these methods he was able to deduce the relative reducing powers of different tissues for oxygen, and he explained the results on the supposition that there exist side-chains in the cell protoplasm, whose function is the fixation of oxygen for cellular needs, and that the affinity of these varies in different organs. In this work the germ of his side-chain theory is found.

The next period of Ehrlich's work was occupied chiefly by the study of the action of toxin and antitoxin. In 1891 he published papers on ricin and abrin, in which he showed that antitoxins to these vegetable toxalbumens could be produced by feeding certain animals with sublethal doses. Accordingly, antitoxin production was not peculiar to the case of bacterial toxins, nor was it essential that the poison should be introduced parenterally. He showed also that immunity to these poisons could be transmitted from the mother to the offspring, and that this was due to the direct passage of antitoxin from the blood of the former, chiefly through the milk; in other words, the immunity is of the passive order. From a study of toxins and their action, Ehrlich formed the view that a toxin has essentially a dual constitution, and that there are two essential factors in its action. It

possesses an atom group, by which it is linked to the side-chains of the cell protoplasm, and a toxic group which produces the characteristic lesion or symptoms; and, by an extensive series of researches, he studied the changes occurring in a toxin in the process of deterioration. He early formed the view, and demonstrated by test-tube experiments, that antitoxin combines directly with and neutralises toxin—not by destroying its toxicity, but by satisfying its combining affinity, so that it no longer unites with the cell protoplasm; and it may be noted that the dissociation of toxin from antitoxin has since that time been demonstrated. He further supposed that toxin molecules resembled in constitution, or masqueraded as, foodstuffs, and were fixed to the cell protoplasm by its side-chains. Owing to their being lost for the purposes of the cell, they are cast off, still in combination with the toxin; the side-chains are reproduced, and, when the process is continued, they are reproduced in excess, and set free in the blood. These free side-chains or receptors are then in a position to act as antitoxin, combining with any toxin present, and preventing its union with the cells. Such, in brief, is Ehrlich's side-chain theory as evolved in the case of antitoxins; it was afterwards elaborated to explain more complicated anti-substances, agglutinins, lysins, etc.

We have seen how, in his earlier researches, Ehrlich demonstrated the selective affinity which definite chemical compounds have for the constituents of cells in the living or dead condition, whilst in antitoxic action there is an affinity which has within certain limits a specific character. This specific character he supposed to rest on the complicated structure or configuration of living matter, and the essential point in his side-chain theory is that anti-substances pre-exist in the cells, and become free as the result of the stimulus to over-production. The number of anti-substances is apparently without limit, and it may be objected that their pre-existence is inconceivable; but, in view of the established facts, the same criticism applies to any theory that can be put forward. The determination of the value of Ehrlich's side-chain theory will, however, be attained by future experimental work, not by discussion. The natural sequence to the work on antitoxin action was an extension of the enquiry to other anti-substances more complicated in their structure and mode of action, and the outcome at a later date was a long series of researches dealing especially with the lysins. These are of an intricate nature, but are essentially dominated by his previous views, and are characterised by imagination and ingenuity of plan.

Apart, however, from Ehrlich's theoretical contributions to this biological problem, his work on antitoxin led to practical results of the highest value. One of the chief features of his genius was the combination of imaginative outlook with the power of detailed working out; rarely has this combination been so remarkable as in his case. Standardisation of antitoxin was essential to successful therapeutics, and Ehrlich, in his capacity as Director of the Serum Institute, attacked the problem with his usual thoroughness.

He investigated in great detail the phenomenon of antitoxin production in the body, the nature of its neutralising action, and, what was a problem of great complexity, the changes which occur spontaneously in toxins. The last, in fact, were a bar to standardisation of toxin for practical purposes, but Ehrlich solved the problem by introducing a standard antitoxin, which, by means devised by him, could be kept practically unchanged. His method of standardisation has remained the one in general use down to the present day, and by means of it the method of antitoxin treatment has been stabilised and the value of the results enhanced.

Ehrlich's official work at Frankfurt, from 1901 onwards, included investigations into the nature of tumour growth, and these were carried out on a very extensive scale. Though no therapeutic results were reached, there followed many important additions to knowledge on the biological side, with regard to the conditions of growth and virulence of malignant tumours, the production of immunity, modifications of structure in relation to virulence, etc.; in fact, for a time, the work from Ehrlich's laboratory may be said to have largely dominated this field of research. The principles of immunity to bacterial disease were naturally brought to bear on the question, and methods were devised by which an analogous active immunity could be produced against a tumour otherwise invasive—the tumour, needless to say, being derived from another animal. The natural immunity possessed by an animal against a tumour from a different species is in a different category; it is due, not to destructive powers on the part of the animal, but to failure of the cancer cell to draw nourishment from the fluids of the host. The special feature of the cancer cell, according to Ehrlich, is an excessive avidity for nourishment, yet the cell fails to grow in any but the fluids of the species of animal from which it has come. Here again, just as in the case of anti-substances to proteins introduced parenterally, species-differences in molecular structure are brought out in a striking way.

To the failure of growth of the cancer cells in the conditions last mentioned, Ehrlich applied the term *athrepsy*, and described it as due to a want of correspondence between the cell receptors and the available food molecules. He analysed this athrepic immunity, and found that the same principles held in various bacterial and protozoal infections, as well as in chemotherapy, there being in all of them examples of the want of fixation as a preliminary to the necessary action.

It will be gathered from what has been said above, that Ehrlich drew a close parallel between the taking up of food molecules by a cell and the fixation to the cell of certain substances which act as poisons—both depend upon the presence of suitable side-chains or receptors in the cell protoplasm. Accordingly, the failure of a poison to act as such is often due to non-combination with the cell.

The application by Ehrlich of the principles just explained led to remarkable results. Using, in the first instance, trypanosome infections as the test, he found that a large number of substances had a marked

parasiticidal action, one of the most striking effects being found in the case of trypan red, a single dose of which might cure a mouse otherwise fatally infected with the trypanosome of *mal de caderas.* In certain cases, after treatment with a drug, a relapse occurred, and he found that the parasites which had survived its action were no longer susceptible; in other words, they had acquired drug-fastness. That this had a chemical basis was shown by the fact that the fastness usually applied to substances of the same chemical group, but not to those of other groups, one of which included the arsenic compounds.

Amongst these last, atoxyl, which had been introduced as a trypanocidal substance by Breinl and Thomas, came to be studied, and what proved to be an important discovery was made, when Ehrlich, along with Bertheim, showed that this substance had not the constitution of an acetanilide, as was supposed, but was para-amino-phenylarsenic acid, a substance from which a great number of derivatives with different actions could be formed. For example, by introducing an acetic acid group, a substance, arsacetin, was obtained, which had a much greater effect on trypanosome infections, but was much less toxic to the tissues than atoxyl. Ehrlich thus came to formulate the view that the different atom groups, existing as side-chains in such substances, functioned in different ways; some were concerned in fixing the substance to the parasite or body-cell—were parasitotropic or organotropic—whilst others produced the toxic effect; but in every case combination or fixation was essential.

The problem thus came to be how to vary the structure of the chemical compound so as to produce maximum affinity for the parasite and minimum affinity for the cells of the body, along with the necessary toxic action on the former. Ehrlich further showed that the pentavalent arsenic, in which form it exists in the atoxyl series, has little direct parasiticidal effect until it is reduced to the trivalent form. He thus directed his attention to compounds containing trivalent arsenic, and found that their toxic action on the tissues was reduced when two molecules were combined by means of the arsenic group.

After the preparation and testing of a large number of substances, as indicated by the numbers, the goal of a satisfactory spirochæticidal substance was reached in dioxydiamidoarsenobenzol (No. 592), the hydrochloride of which is salvarsan (No. 606). In this substance the hydroxyl, aided by the amido groups, bring about the maximum affinity for the spirochætes, while, of course, the arsenic group leads to the parisiticidal effect. Although Ehrlich did not directly concern himself with practical medicine, no one appreciated more fully than he the precautions necessary for therapeutic success, and the importance of detail as regards administration. Was the drug really without harmful action on the tissues? Might not a drug-fast strain of spirochætes be developed? These and many other questions had to be answered, and a vast amount of experimental work was entailed. Ehrlich gave salvarsan to the medical word in 1910, and experience of the drug during the years since then has pronounced as to its value.

In a notice such as this it is impossible to do justice to many of Ehrlich's investigations;* his extraordinary industry is indicated by the several hundred papers which he published. We have selected only the main subjects, and have endeavoured to show their dependence on a common principle. Of his originality, of the extent and quality of his work, and of the practical results obtained, there can be, we think, only one opinion: they are all of the first order. From the outset he marked off a field of work for himself, and this was not confined to any one science, but encroached on the domain of several. A worker in biology, he called to his aid the services of chemistry, and his knowledge of both departments was immense. To what extent his application of purely chemical conceptions to certain vital processes was justified is a question which can be answered only in the future, but this does not affect the value of the actual attainments by his methods. Originality and boldness of conception are apparent in his earliest researches, and, as we have already indicated, his whole life's work is an evolution from these. His outlook was never utilitarian: "Science for its own sake" might have been his motto, and the practical fruits fell off incidentally, as it were. With remarkable imaginative power there was combined in equal degree the faculty of intensive work, and each problem was worked out by him down to minute details. The researches, guided by his master mind, which led up to the discovery of salvarsan, stand in a sense by themselves in the history of medical science. He saw that scientific investigations in certain departments must nowadays be "on a Dreadnought scale," as he himself put it in one of his characteristic phrases, and fortunately the great requirements of his later work were satisfied and success was attained. And apart from Ehrlich's actual discoveries, it must be recognised that at the beginning of the century there was no more potent and far-reaching influence than his in the domain of medical science.

R. M.

* A full account and analysis of his work are given in the volume "Paul Ehrlich," a Festschrift published on the occasion of his 60th birthday in 1914.

S. SCHWENDENER, 1829–1919.

SIMON SCHWENDENER, who was elected a Foreign Member of the Royal Society in 1913, was a native of Switzerland. He was born on February 10, 1829, at Buchs, in the Canton of St. Gallen. His father was a farmer, but the son showed a preference for the pursuit of knowledge rather than for the practice of agriculture; consequently, on the completion of his school-education, he became, not a farmer, but a teacher in the elementary school of his native town. A bequest from his grandfather made it possible for him to enter upon a University career. With this end in view, he went to Geneva, where he studied Botany under Alphonse de Candolle. Unfortunately, his means were insufficient to enable him to complete his University course, and he was compelled to have recourse to school-teaching for a time. In 1856 he removed to Zurich, to resume his botanical studies under Oswald Heer, and on August 8 he took his degree with a phænological thesis, begun at Geneva, 'Ueber die periodischen Erscheinungen der Natur, insbesondere der Pflanzenwelt.'

Shortly before this Naegeli had come to Zurich, and, under his guidance, Schwendener began to study the microscopical anatomy of plants. So well did teacher and pupil agree that, when in 1857 Naegeli was called to the Chair of Botany in Munich, he took Schwendener with him as his assistant. After ten years with Naegeli at Munich, Schwendener was nominated Professor of Botany at Basle. Ten years later, in 1877, he moved on to Tübingen, where he succeeded Hofmeister; and in 1879, on the death of Alexander Braun, he became Professor of Botany at Berlin, where he remained for the rest of his long life. He died on May 27, 1919. He was never married.

The consideration of Schwendener's work may well begin with the important book 'Das Mikroskop,' in which he collaborated with Naegeli, published 1865–7 (2nd ed., 1877), a book which exercised considerable influence upon botanical thought. Naegeli wrote the part relating to the structure and morphology of plants, whilst Schwendener was responsible for the part dealing with the mechanism and the optical theory of the microscope. His remarkably efficient treatment of the subject revealed the natural bent of his mind towards mathematics, which was so marked that it is somewhat a matter of surprise that he did not take up physical science as his special study, rather than botany. At this time he was also engaged upon definitely botanical work, the only work of the kind that he produced, which made him famous. He had undertaken, no doubt at Naegeli's suggestion, an investigation into the structure of Lichens, the results of which, with the title "Untersuchungen ueber den Flechtenthallus," appeared in Naegeli's 'Beiträge zur wissenschaftlichen Botanik,' 1860–3–8. In the course of his work, he devoted special attention to the

cells containing chlorophyll, which form an essential constituent of the thallus, the "gonidia," as they had been termed. It had long been known that the gonidia closely resemble certain free-living organisms which had been described as Algæ. Inasmuch as the gonidia were assumed to be developed from the colourless filaments of which the thallus mainly consists, the view was held that many of the simpler Algæ were in reality nothing but the gonidia of Lichens, which had become free and had continued so to live. With regard to the rest of the thallus, the similarity between the colourless filamentous tissue and the mycelium of Fungi had been recognised, as well as that between the spore-bearing fructifications of the Lichens and those of the Ascomycetous Fungi. But no definite idea as to the nature of the Lichen-thallus had been reached. Then it began to be realised that there might be another, inverse, interpretation of the nature of the gonidia which might lead to some satisfactory conclusion. The position was well stated by De Bary in the following passage taken from his 'Morphologie der Pilzen, Flechten, etc.,' 1866. Speaking of certain Lichens, he says: "The Lichens in question are either the fully developed fructifying states of plants, the incompletely developed forms of which have hitherto been regarded as belonging to the algal groups Nostocaceæ and Chroococcaceæ—or these Nostocaceæ and Chroococcaceæ are typical Algæ, which acquire the form of Lichens, because they are invaded by certain parasitic Ascomycetous Fungi, the filamentous mycelium of which penetrates into the developing thallus, and often becomes attached to the coloured cells." In the last of his papers published (1868) in Naegeli's 'Beiträge,' Schwendener, as the result of his observations on the gonidia, expressed himself strongly in favour of the latter of the above alternatives, and promised a further statement on the subject as soon as it became possible for him to resume his work, which had been interrupted by his removal to Basle. This promise he redeemed by the publication, in 1869, of his celebrated pamphlet "Die Algentypen der Flechtengonidien," in which he adduced convincing evidence that the gonidia do not originate in the thallus as developments of the filamentous tissue; but, on the contrary, are Algæ which have become imprisoned in, or invaded by, the mycelium of a Fungus, forming the thallus, in which, as it grows, the algal cells multiply by division. This led to the remarkable inference that a Lichen is a composite, not a simple, organism, consisting of Alga and Fungus living together in a relation which, on the whole, is one of mutual advantage; an altogether new biological conception, subsequently designated "symbiosis" by De Bary. This conclusion aroused the most lively opposition from the professed lichenologists. For years the famous controversy raged, conducted with much acuteness and no little acrimony, nor has it even now completely died out. It may be added that such new facts as have since been discovered, notably, the observations of Stahl (1877), have contributed to strengthen the position of the Schwendenerian theory, and that it has long been almost universally accepted.

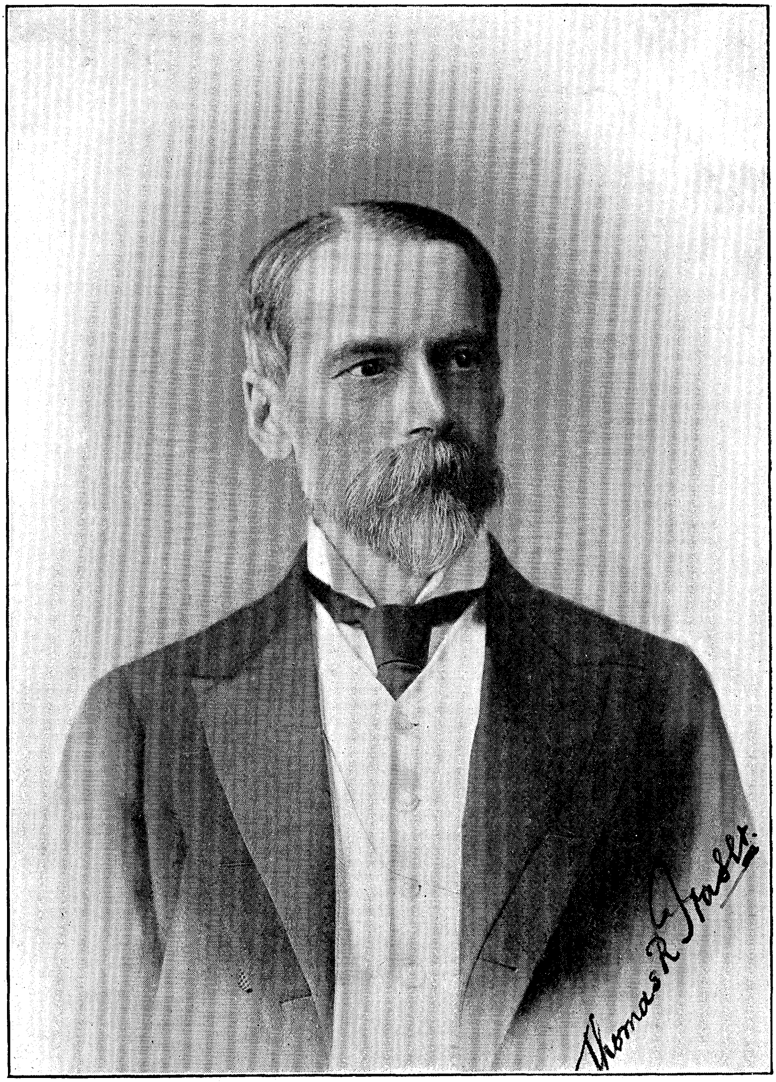
Schwendener took no part in the controversy, nor did he publish anything more on Lichens. Whilst still at Basle, he turned his attention to a line of research more in accordance with the natural bent of his genius, the study of the mechanics of the structures of plants. The first fruit of his labours in this direction was his 'Das mechanische Prinzip im anatomischen Bau der Monokotylen' (1874), in which he demonstrated that the anatomy of these plants, and more especially the distribution in them of the supporting tissue (stereom), is in accordance with the recognised principles of constructive engineering. This line of research he pursued to the end of his career, applying mechanical principles to the elucidation of various structures and physiological processes. Before he left Basle in 1877, he published a considerable work on phyllotaxis, considered from this point of view, 'Die mechanische Theorie der Blattstellungen.' When at Berlin, he wrote a number of papers on such subjects as the twining of plants, the ascent of sap, the mechanism of the stomata and of the pulvini of leaves. Nearly all of these appeared in the 'Monatsberichte' of the Prussian Academy; they were republished in two volumes in 1898.

Not only was he active in research, but he was also successful as a teacher. He inspired a number of his students to prosecute research along the lines that he had laid down. He inaugurated, in fact, that study of structure in relation to function, which has since been so brilliantly developed, in particular by Prof. G. Haberlandt, once his pupil, now his successor, at Berlin.

Schwendener's name will always hold a distinguished place in botanical history, as that of the discoverer of the true nature of Lichens, and of the founder of the study of the physiological anatomy of plants.

The information concerning Prof. Schwendener given in this notice was kindly supplied by Prof. Dr. Hans Schinz, Director of the Botanic Garden, Zurich.

S. H. V.


Thomas R Fraser

SIR THOMAS RICHARD FRASER, 1841–1919.

SIR THOMAS R. FRASER, M.D., F.R.S., and F.R.S.E., died at his residence in Edinburgh on January 4, 1920, in his eightieth year. Only sixteen months previously he had relinquished his Professorship of Materia Medica, in Edinburgh University, which he had held since 1877.

He was born in 1841, in India, a country to which he was destined to return, many years later, as member and President of a Plague Commission. His early education was obtained at private schools and was succeeded by his entrance as a medical student at the University of Edinburgh. As a student in that university he had come under the influence of Sir Lyon Playfair, Professor Hughes Bennet, Sir James Y. Simpson, and Sir Robert Christison, who, together with other members of the medical faculty, formed at that time a galaxy of talent probably never excelled in the history of any British medical school. It may well have been owing to such inspiring influences that many who had the advantage of studying in that era were destined to make their names worthy of a place amongst those of their renowned teachers. The most eminent toxicologist of his time, Sir Robert Christison, impressed his students by his exceptional power of observation and deduction coupled with the resources of the widest knowledge of the action of drugs, whether as poisons or as therapeutic agents; but he lived and taught before the analytical methods of pharmacology had been satisfactorily established. Thus, whilst his description of his own symptoms, subjective and objective, occasioned by a toxic drug of unascertained action (the Calabar ordeal bean) is extraordinarily exact and instructive, it was not to be at his hands that the elucidation of these effects was to be accomplished, but by the subsequent work of Fraser, who subjected the poison which had occasioned them to exhaustive pharmacological research.

This experience of Christison's, which but for his presence of mind, might have ended in disaster, occurred in 1855, and seven years later Fraser offered for graduation a thesis entitled "The Action and Uses of the Calabar Bean." He had still to devote much thought and experimentation to the subject, but on this, his first comprehensive investigation, his title to be ranked among the pioneers of pharmacology may justly be founded. Christison was not slow in recognising the early promise thus displayed by his former pupil, who became his Assistant in the University Department of Materia Medica, in the year subsequent to the appearance of the thesis. During his tenure of this position Dr. Fraser made further advances in investigating the action of drugs, especially of Calabar bean. Working largely with the extract containing the alkaloid, physostigmine (often referred to also as eserine), he recognised the altered condition of vision it occasioned, the failure of motor-cord conduction, as well as of cardiac and respiratory activity, and he indicated its application to ophthalmic practice, to the treatment of toxic

spasm, and as possessing in certain intestinal conditions the value of a cathartic.

From this research there was a further development. The symptoms produced by Calabar bean and the cause of their occurrence suggested the probability that atropine, which antagonises the action of physostigmine on the eye, would also show itself antidotal with regard to the more serious effects which the former occasions. This anticipation proved fully justified by the results, and much light is shed by the long series of experiments on the relative antidotal efficiency of atropine when administered before (prophylactically), simultaneously with, or subsequently to the toxic Calabar bean extract. This work, published just fifty years ago, is a model of its kind, of care in design, and precision in accomplishment. It was also in the late "sixties" that Prof. Crum Brown and Dr. Fraser began collaboration in an experimental enquiry into the connection between chemical constitution and physiological action. Selecting certain alkaloids possessing well-defined toxic actions, amongst which spasm is conspicuous, they introduced an ethylic or methylic group into the molecule of these by substitution for hydrogen. On submitting such salts to pharmacological tests, the observers established the fact that whilst the toxicity of these was enhanced as contrasted with that of the alkaloids individually from which they were derived, the condition of spasm was no longer present, but in place a paralytic state attributable to a peripheral action on motor nerves. Confirmation with amplification of these observations were subsequently obtained in the case of atropine and coniine, of which the dimethylic iodide was found to be inferior in toxicity to the monomethylic.

The appointment of Dr. Fraser, in 1870, to an assistant physiciancy in the Royal Infirmary of Edinburgh was the beginning of a prolonged connection with that institution, as (exclusive of three years, during which he was acting as Medical Officer of Health for Mid-Cheshire) his services, first as assistant and subsequently as physician and one of the professors of clinical medicine, extended over a period of forty years.

When, in 1877, he was appointed to the Chair of Materia Medica, in succession to Sir Robert Christison, Dr. Fraser was in his thirty-seventh year. The post, with its high traditions, offered great opportunities to the enthusiastic investigator and teacher; he had, it is true, lectured "extra-murally" on his chosen subject, but he had now to meet a much larger class, to which he owed the duty of making the wide and exacting group of subjects taught from the Chair of Materia Medica* not merely intelligible but attractive. Prof. Fraser was not long in establishing his reputation as a successful and inspiring lecturer; clear, deliberate, and incisive in style, methodical and thorough in handling fact or theory, he infused vitality into what the medical student is too apt to regard as the dry bones of pharmacognosy by his descriptions of the action upon tissues and organisms of which

* The professor was responsible not only for the teaching of pharmacognosy, but also of pharmacology, therapeutics, and pharmacy.

drugs are capable, and of the therapeutic employments which are thus indicated. His lectures, no less than his devotion to research, were calculated beyond merely instructing to produce a stimulating and enduring impression upon students in the third year of their curriculum. Many of them came into contact with him again at a later period of study in the clinical wards and lecture theatre of the Infirmary. Here Prof. Fraser was in a sphere entirely congenial to him; his minute attention to detail, thoroughness of method, and wide experience rendering him a diagnostician of high order, whilst his intimate acquaintance with the potentiality of therapeutic agencies gave peculiar point to the line of treatment which he advocated.

Pharmacological research, to which he was devoted, occupied a large share of Prof. Fraser's time, and brief reference may be made here to some of the directions in which it was prosecuted. An investigation into the action of *Strophanthus hispidus* (Kombé) is closely associated with his name. The toxic properties of the plant had been known to travellers, who had ascertained that certain South African tribes used arrows, both in warfare and in the chase, which had been anointed with an extract derived from Kombé. Some of these poisoned arrow-heads, having been sent home, came into the hands of Sharpey about 1862, who recognised the extract upon the points as having the effect of a cardiac poison, whilst three years later Hilton Fagge, with Stephenson, expressed a confirmatory opinion, grouping the new poison with digitalis and squill. The further investigation which was to result in the addition of a new and valuable therapeutic agent was taken up at this point by Dr. Fraser, who, in 1870, published certain results which he had obtained with the poison from the arrow-heads; whilst, many years later (in 1885), at a meeting of the British Medical Association, held at Cardiff, he indicated the directions in which the activities of the drug might be used with advantages therapeutically.

In two extensive papers which appeared in the 'Transactions of the Royal Society of Edinburgh' in 1890 and 1892 respectively, he gives an exhaustive account of strophanthus as used as an arrow poison, discussing also its botanical features, especially the morphology of the seeds—as the source of the extract, and indicating the properties of the active glucosideal constituent, strophanthin, which he had separated from them. Whilst recognising the similarity of action which strophanthus and digitalis produce upon the heart, he emphasised the relatively feeble effect of the former upon the blood-vessels, those of the frog contracting not at all to extract solutions of 1:20,000, and but slightly to 1:1000. He also drew attention to the anæsthetic action of the glucoside on the conjunctival surface.

Many years later, another Strophanthus (*S. sarmentosus*) was investigated by him, together with Alister Mackenzie, the results published in 1911 establishing the parallel activity possessed by it with that of the *S. hispidus*.

Poisoned arrows in general had very naturally become objects of interest to him, and many specimens from widely separated sources found their way to the Edinburgh laboratories, and there were subjected to examination.

Amongst these were the weapons of the Abors and Mishmis of N.E. India, upon the points of which the toxic element was discovered to be an aconitine (probably pseudaconitine from *A. ferox*), but Sir Thomas found that the latter tribe also employed a poison which, when injected, produced hæmorrhagic conditions similar to those originated by snake venom. This poison he identified as croton oil.

He bestowed much time upon various researches into the action of snake venom, the antidotal treatment of conditions due to its absorption, and the endeavour to establish immunity by treatment anterior to venom inoculation. A series of papers embodying his results and conclusions was published in the years 1895–1897. Over a period often extending to many weeks, gradually increased doses of the venoms of certain snakes were administered, until a proportion had been arrived at which would infallibly have proved fatal to an untreated animal.

Such preparations were made by feeding,* as well as by hypodermic injection. Prof. Fraser satisfied himself that an animal thus prepared not only acquired protection itself against the action of venom subsequently administered by injection in doses largely exceeding the calculated lethal, but further, that the serum (whether liquid or desiccated) of such an animal had the property of immunising other animals against venom action. By varying the time-relationship of administration of the antivenin and of the potent venom, he sought to ascertain the limit within which the former might prove effective as a prophylactic or as a curative agent. That the bile of poisonous snakes (African and Indian cobra, puff-adder), as well as that of the non-poisonous, and of all mammals, to which the test was applied, is capable of destroying the activity of venom when a requisite portion of the former is brought into contact with the latter *in vitro*, was demonstrated by Prof. Fraser in an elaborate series of observations. Cholesterin appears to be the inactivating constituent. He entertained some hope that a practical antidotal method of treatment might find its basis on this fact, although estimating that a relatively enormous dose of bile ($\times$ 1600–2000) might be required when inoculation of venom had already taken place.

The venom of several poisonous snakes was subjected to examination more recently in the Edinburgh laboratories. That of the common Krait (*Pongarus cœruleus*), investigated by Sir T. Fraser, with the collaboration of Major Elliot and Dr. Sillar, was found to be inferior in toxicity to cobra venom. Together with Dr. Gunn, the venom of the S. African Colubrine, *Sepedon hœmachates*, and also that of the Viperine, *Echis carinatus*, were examined by him, and the important conclusion was deduced from the results obtained, that the venoms of Colubrine and Viperine snakes, far

* Calmette is at issue with these conclusions in so far as the development of immunity is concerned, he (Calmette) holding that a venom-fed animal develops no antivenin—though he admits the possibility of some degree of protection being conferred upon certain very young animals by this method.

from being identical, are widely different in their actions, the hæmolytic effect predominating in the latter, whilst a paralysant action on both central nervous system and motor nerve endings at the periphery is the dominating effect of the former.

In consequence of a severe outbreak of plague in 1898, a Commission was appointed by the Governor-General, with the approval of H.M. Secretary of State for India, which was charged with conducting an enquiry into the origin of the disease, the manner of its communication, and the effects of certain sera as means of prevention or cure. As such an enquiry, from its nature, demanded the presence of the Commissioners in India, Prof. Fraser, who had been nominated for service as their President, asked and was granted leave of absence from his University duties.

Having arrived in Bombay, the Commission at once began its enquiries, examining numerous witnesses, and making searching personal observations in many of the plague-stricken areas. Much important information was elicited, which finds incorporation in the voluminous report issued in 1901. Therein also the Commissioners make suggestions as to the proceedings most likely to prove effective for preventing or circumscribing plague outbreaks in the future. These suggestions are not adopted in all instances with entire unanimity, and consequently there are presented certain minority recommendations, amongst which is an important expression of opinion by the President.

In addition to his service upon this Commission, Prof. Fraser was called upon at various times to occupy other important positions. In 1881 he acted as President of the Section of Materia Medica and Pharmacology at the International Medical Congress, which was then held in London. He filled the Presidential Chair of the College of Physicians of Edinburgh, and he was appointed by the Admiralty a member of a Committee charged with an enquiry into an outbreak of scurvy, which occurred in Sir G. Nares' Arctic Expedition. He also acted as Consulting Medical Officer to the Prison Commissioners for Scotland. He was appointed Honorary Physician to H.M. the King in Scotland in succession to Sir Wm. Gairdner. The honour of Knighthood was conferred upon him in 1902.

Apart from his professional work, Sir Thomas Fraser served his University in various capacities. His precision in method, administrative ability, and close acquaintance with the requirements of medical education, peculiarly fitted him for the discharge of the duties of Dean of the Medical Faculty, and this onerous post was held by him for twenty years. He was actively concerned in the preparation of memoranda relating to the Scottish Universities' Bill, and in facilitating the work of the Commission of 1889, which held its sittings in Edinburgh. Sir Thomas was elected by his University in 1905 as its representative to the General Medical Council, and acted in that capacity for the ten years ensuing; apart from his contributions to the main business of the Council, he rendered very special and important assistance as a member of the Pharmacopœial Committee of that

body, which was then engaged in the preparation of the current issue of the 'British Pharmacopœia,' which appeared in 1914.

Wide recognition of the services which Sir Thomas Fraser had rendered to medical science and education is evidenced by the many distinctions which were bestowed upon him by learned bodies both at home and abroad.* He was created an honorary M.D. of Dublin and a B.Sc. of Cambridge. In 1877 he was elected a Fellow of the Royal Society and also of the Royal Society of Edinburgh. He was laureated by the Institute of France, by the Turin Academy of Medicine, and by the College of Physicians of Philadelphia.

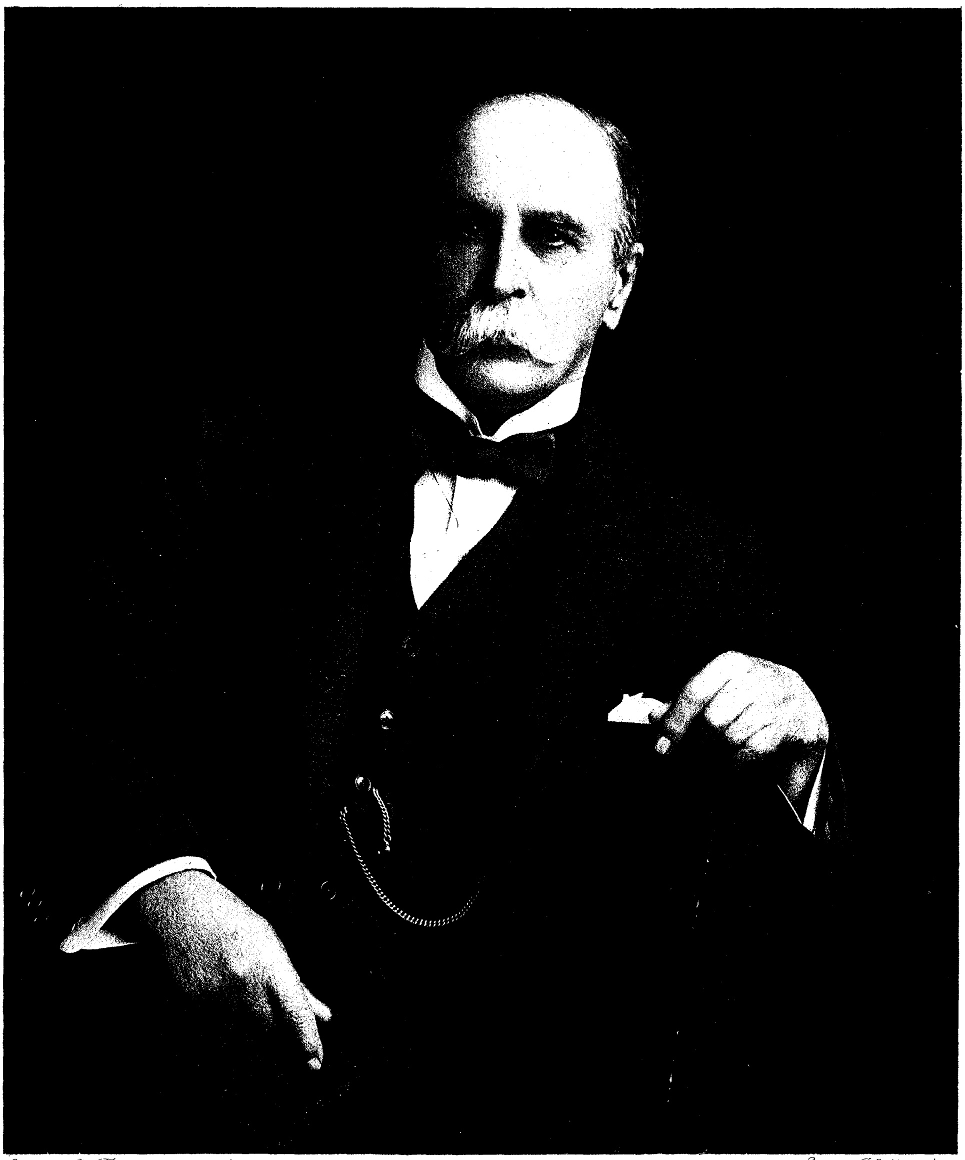
In 1918, at the time of his retirement from the Chair of Materia Medica, which he had occupied for forty years, Sir Thomas Fraser received a double mark of appreciation; from his University, the honorary degree of LL.D. (a similar recognition having already been conferred upon him by Aberdeen and Glasgow), whilst, by many friends and former pupils of his own, he was presented with the replica of a portrait of himself. This portrait will perpetuate for those who knew him the harmonious sensitive features, the keen eye, and the dignified self-possession of posture with which so many generations of Edinburgh students have been familiar.

In his earlier years, if not actually robust physically, he was as alert and energetic in occupying the scanty opportunities which presented themselves for relaxation as in fulfilling the duties of the day. He had many resources for leisure. He was devoted to nature, so that gardening, which enabled him to study the growth and colour development of plants, had a strong attraction for him; hill climbing, not merely as congenial exercise, but for the pleasure of the distant view from the summit, led him at one time or other to make the ascent of all the highest mountains in Britain. Shooting, but more especially trout fishing, in which he was an adept, were sports which he followed with enthusiasm. He recognised the need of outdoor exercise for others, who, like himself, were spending the bulk of their time in class-rooms, laboratories, and infirmary wards, by encouraging rowing amongst the undergraduates, of whose boat club he was for many years the captain.

The sense of pleasure and refreshment, which in more advanced life Sir Thomas experienced when a vacation permitted him to visit his Highland residence, "Druimbeg," in Argyleshire, can be readily appreciated. There, freed for a time from the close succession of professional duties, living amidst beautiful surroundings and in the companionship of his wife and family, he could supervise his garden—the many medicinal plants in which were of special interest to him, and enjoy the congenial opportunities for recreation which were readily available.

But for many years before the end of his active life his health had not been satisfactory. He suffered from a bronchial condition, which was sometimes incapacitating, and must often have rendered the discharge of

* The substance is given here of a paragraph contained in an excellent appreciation of Sir T. Fraser which appeared in the 'Scotsman' newspaper on the day following his decease.



Elliott & Fry, photographers — *Emery Walker ph. sc.*

Wm Osler

duties which involved speaking to large audiences trying to him. When he had reached the age of seventy he had a further misfortune, in experiencing a fracture of the femur, and, though partial use of the injured limb was eventually recovered, his movements remained restricted, and were often attended by discomfort. In spite of these limitations, Sir Thomas never lost his courageous spirit, which supported him in carrying on the performance of his duties and attaining the object he had in view, although in this attempt he might have failed had it not been for the constant and devoted care which Lady Fraser exercised on his behalf. His interest in research work never forsook him, for even after his retirement, and within not many months of his decease, he was planning fresh investigations, of which he was not to see the accomplishment. As his mind remained active and vigorous to the end of life, so in his appearance there was little indication of the advanced age to which he had attained, for his hair was still moderately dark and abundant, whilst his features retained not only the alertness, but much of the symmetry of earlier years.

J. T. C.

SIR WILLIAM OSLER, Bart., 1849–1919.

It is no easy task to write an obituary notice of Sir William Osler which shall be in any degree adequate. No one who did not know him could give a just appreciation of the man, and those who did know him retain so dominant a memory of his vivid personality and charm that they are apt to do less than justice to his achievement, and to those strenuous years of scientific work which earned for him the opportunity of manifesting the gifts which were so pre-eminently his.

The story of Osler's life falls naturally into three main periods. The first thirty-five years were spent in Canada, where he was born and educated, and, after his graduation followed by two years' work in Europe, were devoted to steady work in the study and teaching of physiology and morbid anatomy and the pursuit of clinical medicine. Then followed twenty years in the United States. This also was a period of research and observation, during which he organised the medical teaching of an important new university, and trained a group of able younger men, who were destined to carry on his work and to hand down its traditions.

The last fifteen years, spent in Oxford, constituted a period of fruition, of the cultivation of many interests, and of widespread influence upon the advancement of medicine and of medical education. Success seemed to come

to him without his seeking it, and his was the rare distinction of having occupied professorial chairs in four universities, in three English-speaking countries, each one of which he was invited to fill.

His father, the Rev. Featherstone Osler, a clergyman of the Church of England, emigrated, with his wife, in the year 1837, from Cornwall to Canada, to take up missionary work in that country, and settled in the province of Ontario. There, in 1849, at Bond Head, the subject of this memoir was born, the eighth of a family of nine children, several members of which have attained to positions of distinction. His father, who died in 1895, and his mother, who reached the patriarchal age of a hundred years, lived to see and to rejoice in the successes of their sons. When the boy was nine years old his parents removed to the more settled district of Dundas Valley, where was a good grammar school. At his second school, at Weston, near Toronto, Osler came under the influence of one of those who helped to shape his career, the Rev. W. A. Johnson, the head master, a man keenly interested in natural science and who possessed a microscope. He it was who first awakened in his responsive pupil a zeal for microscopic work and those scientific interests which played so large a part in shaping his later life.

Probably each one of us cherishes the memory of such teachers who influenced us strongly, and although these guiding personalities of our early days tend to assume heroic proportions with the lapse of years, the parts which they play in our lives admit of no question. Osler was fond of recalling three such men, of whom Johnson was one, and to that trio he dedicated his text-book of medicine.

Influenced by his upbringing, Osler contemplated following in his father's footsteps and taking orders. With that intention he entered at the Trinity College, Toronto, where he came under the second of the three teachers, James Bovell, Professor of the Institutes of Medicine, a physician who himself afterwards took orders. Having relinquished the idea of entering the Church, Osler took up the study of medicine, his true vocation, and, after two years spent at the University of Toronto, migrated to McGill University, Montreal, where he completed his medical course, and graduated in 1872.

At McGill he was a pupil of Dr. Palmer Howard, the Professor of Medicine, the third of his outstanding teachers, who probably influenced him most of all.

After taking his degree he proceeded to Europe, and worked in London, at University College, in Berlin and Vienna, under a number of distinguished men, including Jenner, Wilson Fox, Ringer, Virchow, and Nothnagel. The experience thus gained in British and Continental methods of medical education and research stood him in good stead when he was called upon to organize the medical clinic at the Johns Hopkins University.

In 1874 Osler returned to Montreal, to take up the Professorship of the Institutes of Medicine at McGill, at the early age of twenty-five.

The ten years during which he held that Chair were years of active progress of the school, and in that progress he had no small part.

The subjects of his professorial lectures were physiology and pathology, and he also accepted the Chair of Helminthology at the Veterinary College, but ere long he was given charge of a small-pox ward, and in due course was elected a physician to the Montreal General Hospital. Thus he acquired opportunities of clinical teaching, of which he took full advantage, and, ten years after his return to Canada, had gained such a reputation as a physician and teacher of clinical medicine that he was invited to accept the Chair of Medicine in the University of Pennsylvania.

It is evident, from the testimony of his colleagues and pupils, that the influence and stimulating power, which were so characteristic of Osler throughout his life, were fully in evidence during the Montreal period. The ties there knit were never loosened; it was to his *alma mater*, McGill, that he bequeathed his valuable library, and he desired that his ashes should rest within her walls, amidst the books which he loved so well.

His sojourn in Philadelphia lasted only five years. They were years of strenuous work and growing fame as a clinical worker and teacher who always maintained the tradition that to be a good physician a man must needs be a well-trained pathologist. Of Osler, in Philadelphia, Dr. Howard A. Kelly writes:—"Fresh invigorating currents of life and new activities in our stereotyped medical teachings began to manifest themselves, and every sturdy expectant youngster, in short order, lined himself up as a satellite of the new star. Osler breezes were felt everywhere in the old conservative medical centre, and yet it was not without some difficulties that he securely established himself."

In 1889 he accepted the call to take up the posts of Professor of Medicine in the newly established Johns Hopkins University in Baltimore and of Physician-in-Chief to the Johns Hopkins Hospital. The new University had acquired an ideal President in Dr. Daniel C. Gilman, and a group of most able professors. Amongst those who formed the nucleus of a medical faculty were Drs. Welsh, Newell Martin, and Ira Remsen. The other clinical chairs were filled by the appointment of Drs. Halstead and Kelly. Prof. Welsh in the Chair of Pathology was a tower of strength, and the new Professor of Medicine was most fortunate in his colleagues.

At Johns Hopkins Osler found his great opportunity. He could construct a medical clinic *de novo*, unhampered by traditions or vested interests. It was for him to determine the staff required, to select its members, and to shape the methods of teaching and research; and under his guidance there emerged the earliest organised medical unit in any Anglo-Saxon country.

In its construction he adhered firmly to the best and essential feature of the English schools, the contact of the student with the patients, in the wards and out-patient departments, throughout his curriculum, and the teaching of medicine at the bed-side. At the same time he embodied what is best in the German system, the intimate association of laboratories and wards, under one directing head, and the enlistment of a group of highly trained assistants working under the guidance of the director. The advancement of the science

and art of medicine was one of the chief objects aimed at, and even the clinical clerks were taken into partnership for this work, were encouraged to collate cases, to look up references and historical points, and were trained in exposition.

It was only gradually that the structure grew. First the clinical and pathological scheme was constructed, whilst as yet there were no students to profit by it. Then followed a period of post-graduate teaching only, and when, in the end, the first undergraduate students reached the wards, they found awaiting them a clinic fully organised and equipped.

From the great school so built up many others have since been copied or developed, and it is not too much to say that from Johns Hopkins has emanated an influence which has revolutionised the whole system of medical education in America; nor has its influence failed to spread to our own country. Many of Osler's assistants and pupils occupy leading posts in other universities, and the profession pays ungrudging tribute to the work which he accomplished for medical teaching and research. Even if the work which Osler did at Johns Hopkins stood alone to his credit, it would ensure for him an honoured place in the annals of medicine.

Nor were his activities limited to the university, the whole of the medical life of Baltimore and Maryland felt his influence. He infused new life into societies and libraries, and took a leading part in the work of combating tuberculosis in the State, and in the promotion of sanitation. Throughout the length and breadth of the United States his influence was felt, working always for co-ordination of the medical profession and the brotherhood of its members. It was for him a time of literary activity also; and in 1891 his text-book of medicine was published. In England his work and eminence were fully recognised. He had been elected to the Fellowship of the Royal College of Physicians in 1884, whilst still in Montreal, and delivered the Gulstonian Lectures in the following year. In 1898 he was admitted to the Fellowship of the Royal Society.

In 1892 he married Grace Revere Gross, daughter of John Revere of Boston.

The work at Johns Hopkins, coupled with the claims of a large consulting practice which knew no limits of mileage, was very exacting, and after other invitations had been declined, at various times, he accepted, in 1904 the office of Regius Professor of Medicine in the University of Oxford, on the resignation of Sir John Burdon Sanderson.

In Oxford, whither he came in 1905, Osler entered upon a third period of activity, as the occupant of a Chair dating from the reign of King Henry VIII and with very different functions from those of his previous Chairs; a position of high prestige, which had been enhanced by the tenures of Acland and Sanderson, with great opportunities of influence but little clinical teaching. The duties of the Regius Professor are varied, and others are added to them. Osler soon came to his own and held an unrivalled position in Oxford. He took an active part in the administrative work of the University and its

medical school, of the Bodleian Library as a Curator, and of the Clarendon Press as a delegate. Coming fresh from a land of intensive progress he was able to suggest new methods and improvements. At the Radcliffe Infirmary he had opportunities of clinical teaching, kept in touch with medicine, and took an active part in promoting the development of the Pathological Department of the hospital. King James I attached to the Regius Chair the Mastership of the ancient fifteenth century almshouse at Ewelme, and Osler took an intense interest in its beautiful buildings, and its precious muniments, of which he secured the proper care.

Osler rejoiced in the atmosphere of an ancient seat of learning, with its long traditions of men and movements. Before his arrival, he had been elected to a studentship at Christ Church, the college of two of his literary heroes, Burton, author of the 'Anatomy of Melancholy,' and John Locke. One could not help feeling that, whereas his mind was in the forefront of all progress, and his energies were devoted to the interests of the future, he felt equally at home in the past, and found a congenial setting in the Middle Ages.

There were few of those amongst whom he moved who did not come under the spell of his friendship. Recognised as a great physician, a man of science, an eminent teacher of medicine and student of its history, a learned lover of books for their contents rather than their outward form, and a power in educational matters, he easily held his own in any gathering of learned men, and his advice and opinion were valued highly. No greater tribute could have been paid to his versatility than his election, in the last year of his life, to the Presidency of the Classical Association.

He had accumulated a great library of books bearing upon medicine from its earliest days, which contained many rare works, and almost every known edition of his life-long favourite, the 'Religio Medici' of Sir Thomas Browne. He had made considerable progress in its classification and indexing on a novel and original plan, and few things gave him greater pleasure than to show and describe his treasures to an appreciative listener.

A facile orator, Osler could make an apt and interesting speech on any occasion. He could not be tedious, and his speeches, as also his writings, were studded with phrases which fixed themselves in the memory of his hearers, and were permeated by an impish humour which was essentially his own.

Sometimes, indeed, his lighter sayings were misunderstood, and a statement, or rather over-statement, thrown off with a smile in an after-dinner speech, was, on occasion, made the subject of a grave discussion in the daily press.

His house in Oxford was a centre of wide hospitality, wherein he and Lady Osler extended a cordial welcome to their many friends. It was a place of pilgrimage to Americans and Canadians who came to England, and its doors were ever open to the Transatlantic Rhodes scholars.

Nor were his activities limited to Oxford. He was often in London, at the Royal College of Physicians, the medical societies and schools, and at the many important committees of which he was made a member. He took an

active part in the foundation of the Historical Section of the Royal Society of Medicine, and of the Fellowship of Medicine, started during the war to facilitate post-graduate work for officers and colleagues from overseas. He presided over the Medical Section of the International Medical Congress at its meeting in London in 1913, and it was obvious that members from all lands, who attended the Congress, gathered round him not merely as appreciative colleagues but as personal friends. Soon after he came to Oxford, Osler suggested the formation of the Association of Physicians of Great Britain and Ireland, a body of clinical teachers which has done much for the advancement of scientific medicine, and in bringing together the staffs of the various medical schools of these kingdoms. He also was one of the founders, and until his death the senior editor, of the 'Quarterly Journal of Medicine.'

Throughout his career Osler made many contributions to medical literature. His earliest published writings were the outcome of his microscopic work, and dealt with Canadian Diatomaceæ and the platelets of the blood, which he was one of the first to describe. The first to observe them was Max Schultze in 1865. Osler showed, in a paper published in the 'Proceedings of the Royal Society,' in 1874, that the clusters which Schultze had described were formed by the aggregation of separate particles which circulated as such in the blood. Some ten years later, Bizzozero gave them the name of platelets, and indicated the part which they play in thrombus formation.

In 1877 Osler described a verminous bronchitis occurring in dogs, and the causative organism which is sometimes spoken of as *Filaria Osleri.* He fell into the error, however, of classifying the organism as a strongylus, whereas it is a filaria.

Many of his papers are records of individual cases; the earlier ones are mainly pathological, but, after a time, clinical papers are included, and, in the end, predominate. They reflect the subjects which were occupying his attention from time to time, such as the prodromal rashes of small-pox, miners' lung, the changes in the blood in disease, and the forms of splenic enlargement. Not a few papers treat of arterial diseases, of aneurysm, and malignant endocarditis, which last he chose as the subject of his Gulstonian Lectures, delivered at the Royal College of Physicians in 1885.

The clinical papers cover a large part of the field of medicine. Tuberculosis, syphilis, chorea, the cerebral palsies of children, congenital heart disease, pulmonary fibrosis, congenital malformations, ochronosis, gastric and duodenal ulcers, typhoid fever and its complications, malaria and cerebro-spinal fever are among the subjects of which they treat. Some subjects recur at intervals, such as the visceral lesions of the erythema group. Vacquez first described a case of polycythæmia rubra, but it was Osler who recognised it as a definite clinical entity, and it is often connected with his name. An hereditary malady, characterised by multiple telangiectases associated with hæmorrhages, may rightly be styled Osler's disease. The knowledge embodied in these papers, which cover so wide a range, formed an admirable ground-

work for a great text-book, and his Text Book of Medicine was Osler's *magnum opus.* In it he made a judicious use of statistics collected from hospital cases; it gains much from the inclusion of tables and charts, and he made full use of the researches of others. The clear and individual style of the book makes it very helpful to students and practitioners alike, and it has long held the foremost place among such works, both in this country and in America. One great feature is the due weight given to morbid anatomy. The book has passed through many editions, and has been translated into French, German, and Chinese. The ninth edition, upon which he was engaged, has appeared since Osler's death, under the editorship of Dr. T. MacCrea, with whom he had previously edited a System of Medicine.

Valuable as Osler's researches were they cannot be described as epoch-making. What was epoch-making was the work which he instigated and inspired; the stimulating influence which he exercised upon his assistants who worked under his guidance, upon the hundreds of students whom he taught, and upon the thousands who have read his text-book and other writings. He had the gift of being genuinely interested in the work of all his pupils and friends, and spared no pains to help even the least of these in what he was trying to do. From wherever Osler might be there issued telegrams, letters and post-cards, conveying here a word of sympathy or congratulation, there a pat on the back to the writer of a paper which pleased him, or a note on something which he thought would be helpful to a fellow worker.

His scientific writings by no means exhausted his literary activities. Essays and addresses of compelling interest and admirable in literary style, such as were collected into the volumes with the titles of "Aequanimitas" and "An Alabama Student," lay sermons, such as "A Way of Life" delivered to the students of Yale, and introductions to reprints of old books, appeal to a wider audience, and give us a revelation of his thoughts and ideals.

Many honours came to him. Many universities conferred upon him their honorary degrees, and the Academie de Médecine of Paris elected him as a Foreign Associate. He was made a Baronet in 1911, at the time of the King's Coronation. One tribute which gave him special pleasure was the presentation, on the eve of his seventieth birthday, of two volumes of essays written in his honour by pupils and friends upon both sides of the Atlantic. The presentation was made, in London, by his brother Regius Professor of Cambridge, Sir Clifford Allbutt, in a speech worthy of the occasion, which elicited from Osler an equally felicitous reply, spoken with deep emotion and lacking none of his old charm of diction.

The last years of Sir William Osler's life were spent under the shadow of the great war. He felt the incubus deeply, but spared no effort to help. Long railway journeys, under conditions trying to a man of his age, attendance on many committees, inspections of many hospitals, and especially those conducted under American and Canadian auspices, laid a heavy burden upon him. In 1917 he and Lady Osler suffered the loss of their son, their only

child, who fell in Flanders when serving with the artillery. Yet deeply bruised as he was, he carried on bravely, as ready as ever to help others and working as hard as ever. His own loss served only to increase his affectionate sympathy for his friends in like case.

Early in October, 1919, he fell ill, after a long, cold motor drive from the north, where he was held up by the railway strike, and to the illness so acquired he succumbed some three months later, on December 29th, 1919, leaving a gap which cannot be filled, and the memory of a mind and character hardly to be matched in their compelling influence, versatility and charm.

A. E. G.

JOHN GILBERT BAKER, 1834–1920.

JOHN GILBERT BAKER was born at Guisborough in Cleveland, Yorkshire, on January 13, 1834. In August of that year his parents, John Baker and his wife, Mary Gilbert, removed to Thirsk, where young Baker spent his early boyhood. In 1843 he was sent to the Friends' School at Ackworth, where he evinced those interests that were to dominate his life by commencing to make a collection of local native plants. In 1846, at the age of twelve, Baker was transferred to the Friends' School at Bootham, York, which had already acquired a considerable reputation for its encouragement of natural study, a vigorous school natural history society having been in existence there since 1836. When in his fourteenth year, Baker was awarded for his collection of botanical specimens the annual prize at the exhibition of "out of school" work for 1847, and was appointed curator of the school herbarium.

Later in 1847, Baker left school in order to assist his father, and for the next eighteen years was engaged in business at Thirsk. But this occupation did not abate the predilection for natural studies developed at Ackworth and disciplined at Bootham. He collected critically, and we find him, at fifteen, contributing to the 'Phytologist,' in 1849, a new record of a rare *Carex* from Snailesworth Dale. By the time he was twenty, his knowledge of the plants of his county enabled him to collaborate with J. Nowell, who dealt with the mosses, in issuing a supplement to the 'Flora of Yorkshire,' published by H. Baines in 1840. This local supplement of 1854 was followed in 1855 by a thoughtful discussion of the relationship of the flowering plants and ferns of Great Britain to their surroundings, and by 1859 his reputation as an authority on British plants was so fully established that he then became the curator and secretary of the still active Botanical Exchange Club. Among those by whom the extent of Baker's knowledge was already fully appreciated was Daniel Oliver, a distinguished young Northumbrian botanist, four years Baker's senior, who, in 1858, had been invited by Sir William Hooker to

accept the post of librarian at the Royal Gardens, Kew. The ability with which, from 1859 to 1865, Baker distributed the collections of the Exchange Club and drew up its reports enhanced the respect with which he was already regarded, and his reputation as a philosophical natural historian was placed on a permanent basis by the publication, in 1863, of a scholarly series of studies of the botany, geology, climate, and physical geography of North Yorkshire. The work which his connection with the Exchange Club entailed led Baker now to turn his attention to taxonomic problems. He published in the 'Naturalist' for 1864 a review of the British roses, which attracted immediate attention on the Continent as well as in this country. In 1865 he contributed to the 'Journal of Botany' a monograph of the British mints.

In May, 1864, Baker met with a misfortune which was to determine his future career. His work on North Yorkshire, though published in London, had been printed at Thirsk, where the bulk of the stock was stored in his business premises. A disastrous fire destroyed this stock and at the same time consumed his private herbarium and his botanical library. The members of the Exchange Club and other friends at once proceeded spontaneously to replace his lost library and did all that they could to make good the valuable botanical collection. They could not restore the stock of copies of the volume on North Yorkshire. But practical sympathy induced some at least of his friends to suggest to Baker that his misfortune would be a gain to the cause of natural knowledge were he to take the opportunity it offered him of abandoning business pursuits and of devoting himself exclusively to those scientific studies he was so well qualified to prosecute.

There is no reason to doubt that this wise suggestion attracted Baker, though when it was made its realisation must have appeared somewhat hopeless. Baker had given "hostages to fortune." In August, 1860, he had married Hannah Unthank, and their first-born child, Edmund, born on February 9, 1864, was an infant when the fire took place. A chapter of accidents, however, enabled the suggestion to be adopted.

In 1862 the widow of the accomplished W. Borrer had presented to Kew the whole of her husband's fine herbarium. The incorporation in the general collection of Borrer's vascular cryptogams was a matter of urgency in connection with work on which the Director of Kew was personally engaged. The excellence of Baker's treatment of the roses had arrested Sir William Hooker's attention. It was known, too, that in connection with his Exchange Club work Baker was then making a special study of ferns. He was accordingly invited to assist in the arrangement of the Borrer material preparatory to the laying-in of the sheets. In August, 1865, the veteran Sir William Hooker died in his eighty-first year. Among the many tasks undertaken by that eminent botanist, one of the most arduous had been the preparation of his 'Species Filicum,' the five volumes of which had occupied much of his attention between 1846 and 1864. On the completion of this great work, its author, unwearied by the weight of fourscore years, set himself the task of preparing a 'Synopsis Filicum,' and it was in connection with this new

undertaking that the help of Baker in dealing with Borrer's specimens had been sought. Sir William's son and successor, Dr. (afterwards Sir Joseph) Hooker found on the Director's desk the preface to the projected 'Synopsis,' much of its matter in manuscript, and proof copies of the three opening sheets. It was clearly desirable that a work so important should be completed. The multifarious duties and undertakings of the new Director equally clearly precluded him from attempting the task. His experience of Baker's work led the younger Hooker to decide that Baker was singularly qualified to accomplish the work. The task, however, could only be carried out at Kew. This involved the creation of a post in the establishment which Baker could fill. Fortunately, it was possible to arrange that this be done. In 1861, Oliver, the librarian at Kew, had been permitted to supplement the exiguous stipend which public opinion then regarded as an adequate remuneration for scientific service, by accepting the chair of botany at University College, and in 1864, when A. Black, the able keeper of the herbarium at Kew, was compelled, owing to the state of his health, to resign that appointment, Sir William Hooker, with a view to further improvement in Oliver's position, succeeded in obtaining the approval of Government for a proposal to amalgamate the keepership of the herbarium with the librarian's post. This reduction in the strength of the establishment at Kew in 1864 was followed by a further reduction in 1865, owing to the decision that the post of Assistant Director, formerly held by Dr. Hooker, must lapse with his appointment to the directorship. Under the circumstances, however, assent was given to the proposal of the new Director for the creation of a new post, that of first assistant in the herbarium, as from April 1, 1866, and permission was accorded him to engage temporary assistance at once. On receipt of this authority Hooker invited Baker to join the Kew staff. With this invitation Baker complied, taking up his duties as a temporary officer in January, 1866, and being permanently confirmed in the post of first assistant in the herbarium on April 1 of that year.

Soon after his appointment at Kew, Baker was permitted to follow the example of his colleague in the herbarium, in supplementing his income by undertaking teaching work. In 1869 he was appointed lecturer on botany at the London Hospital Medical School, and held this post until 1881. In 1874 he was appointed one of the lecturers to the young gardeners employed at Kew, when the courses of instruction, voluntarily initiated by Oliver in 1859, at last received the recognition of Government. In 1882 he was appointed by the Society of Apothecaries to their lectureship on botany at the Chelsea Physic Garden.

Baker occupied the position of first assistant in the Kew herbarium until, on Oliver's retirement at the age of sixty, he was promoted on June 1, 1890, to the keepership of the library and the herbarium. As keeper he served until his own retirement, at the age of sixty-five, in 1899. The Chelsea lectureship he retained, after his promotion at Kew, until 1896. The Kew lectureship he consented to retain, greatly to the advantage of his pupils, for

five years after his retirement from the keepership, and his demission of this work at the age of seventy in 1904 was a source of unmixed regret. But he still continued his private studies in the herbarium, and, although towards the close of his life his physical strength gradually declined, there was no impairment of his intellectual vigour. He died at Kew, in his eighty-seventh year, on August 16, 1920.

Baker's first task on reaching Kew was the completion of the 'Synopsis Filicum.' This he prosecuted with such industry that the work was published in 1868, and with such ability that he became immediately a leading authority on vascular cryptogams, and was at once invited to prepare the volume in the great 'Flora Brasiliensis,' edited by Von Martius, which deals with the ferns of Brazil. This volume, which appeared in 1870, was but one of many further contributions on the same subject. A new edition of the 'Synopsis' was soon called for, and the work was carefully revised by Baker before its re-issue in 1874. In 1875 he presented to the Royal Irish Academy an account of the ferns of the Seychelles. Sir William Hooker, in 1854, had supplemented the 'Species Filicum' by devoting the tenth volume of the 'Icones Plantarum' to the illustration of a century of new, rare, and imperfectly known ferns. Following this example, Baker supplemented the 'Synopsis Filicum' by completing in 1887 a second century of new and rare ferns, illustrated in the seventeenth volume of the 'Icones.' In 1887, also, he provided, as a companion to the 'Synopsis,' a welcome handbook of the fern-allies, and in 1892 he contributed to the 'Annals of Botany' a summary of the ferns discovered or described since 1874.

Baker's interest in roses was equally sustained. The review of the British species, published in 1864, was followed by a monograph of British roses, published by the Linnean Society in 1869, and by a revised classification of the genus, contributed to the same society in 1902. Baker also wrote the botanical descriptions of the species figured by A. Parsons in the fine monograph of the genus *Rosa*, published by Miss E. A. Willmott between 1910 and 1914.

These, however, were not the only monographic interests that Baker displayed. He became the leading authority of his day on a number of monocotyledonous natural families of plants. In dealing with this important subject, he adopted simultaneously two radically distinct methods, both of which he employed with equal success. Between 1869 and 1899 he contributed to the 'Gardeners' Chronicle' numerous accounts of monocotyledonous genera, designed especially for the benefit of those engaged in cultivation. To the 'Journal of the Royal Horticultural Society' he supplied other contributions of the same kind. As mindful of the needs of scientific workers as he was of practical requirements, he contributed numerous similar papers to the 'Journal of Botany' during the same period. Between 1870 and 1880 he communicated to the Linnean Society instalments of a monograph of the *Liliaceæ*. In 1878 the same Society published his monograph of the *Hypoxidaceæ*, and in 1887 its Journal included his 'Systema

Iridacearum.' In 1893 he contributed to the 'Annals of Botany' a synopsis of the *Museæ.* So highly were these various contributions valued, that Baker was begged to collate and systematise many of his articles in the 'Gardeners' Chronicle' and the 'Journal of Botany,' and re-issue the information in a series of valuable handbooks. One of these, dealing with the *Amaryllideæ* as a whole, appeared in 1885; another, dealing with the *Bromeliaceæ,* was published in 1889; a third, dealing with the *Irideæ,* was issued in 1892.

But Baker did not confine his attention exclusively to monocotyledons and vascular cryptogams. He was the author of numerous similar papers dealing with dicotyledonous plants. One of the most important of these, owing to its bearing on economic problems, was a monograph of the tuber-bearing species of *Solanum,* published by the Linnean Society in 1884.

Before Baker undertook to describe the ferns of Brazil, he had already displayed his aptitude for floristic, as contrasted with monographic study, in the 'New Flora of Northumberland and Durham,' prepared in collaboration with G. R. Tate, and published in 1866. The appearance in 1870 of the volume relating to ferns immediately led to an invitation to elaborate the Brazilian *Compositæ.* The results occupy two volumes, issued by instalments between 1873 and 1884, in the great work of Von Martius. But Baker had already elaborated one natural family for the 'Flora of Tropical Africa' in the first volume, issued in 1868, and another in the second volume, which appeared in 1871. He described the species of twenty-one additional families for the parts of this work published between 1898 and 1906. He also elaborated for the 'Flora of British India' an account of one natural family, published in 1876 and 1878, and of a second family, published in 1890 and 1892. In 1877 appeared his 'Flora of Mauritius and the Seychelles.' In 1885 he published his well-known 'Flora of the English Lake District.' Between 1877 and 1895 he contributed, partly to the 'Journal of Botany,' partly to the Linnean Society, a series of important floristic studies connected with the vegetation of Madagascar, which embody descriptions of over a thousand species previously unknown. He also described the species of five natural families, included in the sixth volume of the 'Flora Capensis,' published during 1896 and 1897.

Notwithstanding the extent and the excellence of Baker's systematic contributions, his interest was not wholly confined to classification. His earliest note, published in 1849, is concerned with the environment rather than with the characters of the plant to which it refers. In the striking paper published by him in 1855, when he had barely attained his majority, Baker attempts to classify British plants "according to their geognostic relations." His work on North Yorkshire, which was published in 1863, is a natural history of the area discussed, of such value that the Yorkshire Naturalists' Union during the period 1888–1906 accorded it the unusual honour of republication in their Transactions in a revised form. Baker took the opportunity afforded him by the preparation of the 'Synopsis Filicum'

to draw up a complementary review of the geographical distribution of ferns which was published by the Linnean Society in 1867. Numerous other papers dealing with questions of distribution were published by him in later years and at the close of his first course of lectures at Kew in 1874 he was invited to contribute to the 'Gardeners' Chronicle' a series of "Elementary Lessons in Botanical Geography" in order "that gardeners and other learners in biology should be encouraged as much as possible to acquire comprehensive and correct ideas of the laws and leading facts of plant distribution." These articles were so greatly appreciated that he was persuaded to permit their publication in 1875 in book form. In 1888 he was jointly responsible with W. W. Newboult for a revised edition of the 'Topographical Botany' by H. C. Watson, to whom the original edition of his own 'North Yorkshire' was dedicated. This natural history interest and instinct, manifest even in those writings which duty compelled him to cast in a taxonomic mould, remained powerful to the end, for the last of his published papers, which appeared in 1917, deals with the botany and the physical geography of Palestine.

Baker's work as a naturalist and systematist received general recognition. In September, 1864, as soon as his review of the British roses appeared, Baker was elected an Associate Member of the Société Royale de Botanique at Brussels. In April, 1866, he was elected a Fellow of the Linnean Society in whose welfare he took a keen interest. He served as a member of Council during 1876–78, 1889–91 and 1893–96, acting as a Vice-president during 1889–91 and again in 1893–94. His work for the 'Flora Brasiliensis' led to his election to the Leopoldinisch-Carolinische Akademie der Naturforscher of Halle. From 1868 to 1886 he served on the scientific committee of the Royal Horticultural Society and in the last mentioned year was appointed a Vice-chairman of the Narcissus Committee, although not then a Fellow of the Society. In June, 1878, he was elected a Fellow of the Royal Society and served on the Council in 1883–84. He became a member of the Yorkshire Naturalists' Union in 1883 and was elected President for 1884–85. In February, 1886, he was elected an Honorary Member of the Literary and Philosophical Society of Manchester. After having been deprived of his help for two years, owing to modification of their bye-laws, the Council of the Royal Horticultural Society in December, 1888, did him the signal honour of appointing him an Honorary Life-fellow, thus enabling them to appoint him to preside over the botanical section of the Rose Conference of 1889 and to benefit again from that year onwards by his services as a member of the scientific committee. In 1890 the Natural History Society of Dumfries and that of Northumberland and Durham made him one of their Honorary Members. In November, 1896, he was elected a British Honorary Fellow of the Botanical Society of Edinburgh. He became a Corresponding Member of the Massachusetts Horticultural Society in July, 1898, and of the New York Academy of Sciences in February, 1899. In 1902 he was elected an Honorary Member of the Royal Irish Academy.

In 1897 the Royal Horticultural Society bestowed on Baker the Victoria Medal of Honour in Horticulture, and in 1899 he was the recipient of the Linnean Medal, the highest honour it is in the power of the Linnean Society to offer. A portrait of Baker, by J. W. Forster, was exhibited at the Royal Academy in 1893.

The service to the State which Baker was able to render as a teacher was marked. The opportunity of imparting useful botanical knowledge to so many men destined to apply it in outlying parts of the Empire has been given to few; no teacher ever took fuller advantage of his opportunity. Recognition of this aspect of his labours came late; it was in 1919 that the University of Leeds conferred on Baker the Honorary Degree of D.Sc.

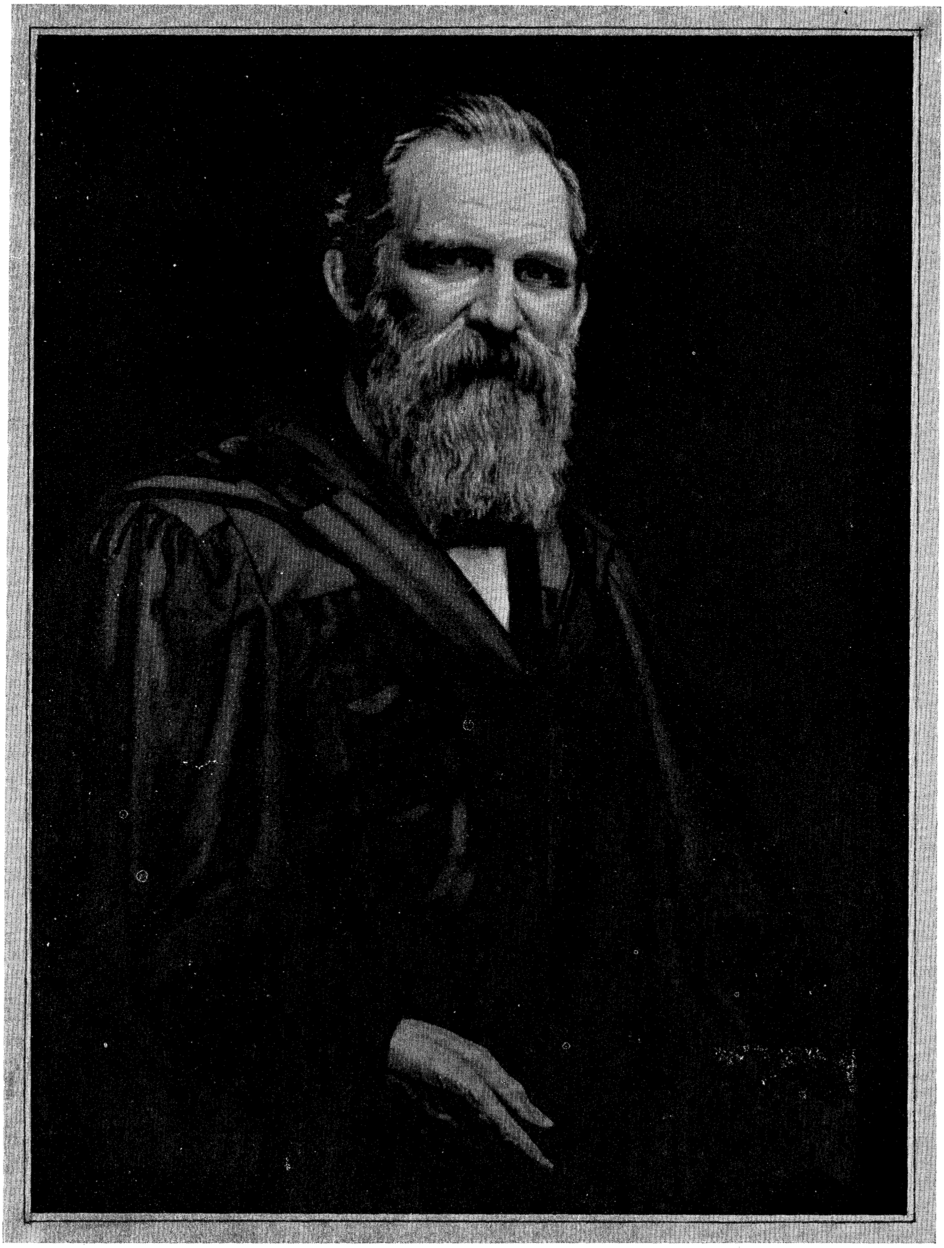
Baker owed his eminence as a systematist largely to the circumstance that his floristic and monographic studies alike are imbued with the spirit of the philosophical natural historian impelled by a sense of duty to attack taxonomic problems. The tasks he undertook clearly gave him the utmost pleasure; the spirit in which they were approached saved him from the error of regarding classification as an end. The identity of the subject of study having been established, his further interest lay in its relationship to its surroundings. He had fully apprehended the effects of environment before Œcology became a special study.

He appreciated as clearly the distinction between floristic and monographic study. He realised that the object of floristic work is to facilitate the identification of the plants that characterise a given geographical area, whereas the purpose of monographic work is to determine the affinities of the plants that constitute a particular natural group. His floristic diagnoses, so drafted as to be employed by the uninitiated in identifying with some certainty the plants in which business may cause them to take a practical interest, are divested of those details that are called for only in a monograph.

The sense of proportion which rendered Baker so distinguished as a systematic writer made him equally effective as a teacher. He used the natural history of plants as a means of educating those on whom he inculcated the importance of botanical knowledge in everyday affairs. His style was lucid and concise, while he possessed the happy gift of ability to emphasise the salient features of his subject without neglecting its details.

Baker's published works ensure the perpetuation of his memory as the last of a singularly gifted circle of systematic botanists. While any of them survive, those who worked with or were taught by Baker will cherish the recollection of one of the kindest and best of men.

D. P.



Chas. Lapworth

CHARLES LAPWORTH, 1842–1920.

In the death of Charles Lapworth in his seventy-eighth year, the science of Geology mourns one who, in the amount and quality of research performed, by the current of fresh ideas with which he was inspired, and in the new direction imparted to the work of his pupils and contemporaries, stands out as one of the leading geologists of his time, worthy to rank with the foremost of the pioneers. When he began his work the glamour of the first fine flush of geological discovery had paled, and it seemed as though the boundaries of the science had been reached, the leading facts disclosed, the main principles laid down. Before his death, the science had been born again, and new discoveries showed that we were but on the threshold of a great development. The effect of his work was most marked in illuminating the structure of our own country and particularly the older part of it, but the work was so thoroughly done, and the principles involved so soundly established, that its reactions were felt in Scandinavia and Bohemia, in America and Australia, in the Dolomites and the Festoon Islands, while it even touched the ocean depths and the Antarctic continent.

His first series of researches brought about the realisation of his forecast that the history told by the Older Palæozoic rocks could be broken up into delicate time divisions comparable with those which heretofore had only proved possible of establishment in the Secondary and Tertiary rocks. This task was effected in the Southern Uplands of Scotland, where he happened to be residing. The rocks in this district are so highly disturbed and convoluted that they had defied the efforts of the best stratigraphers of the time to unravel them. Lapworth was at his best when grappling with a really difficult problem, and, as he saw the necessity for it, he sedulously cultivated those arts of his profession which he realised would alone enable him to solve it.

Large scale maps were essential—he would survey the ground and produce them; lithological variations were minute and obscure—he cultivated his eye and mind to detect and remember them; the geological mapping of these variations must be painstaking and thorough—even though such detail had never yet been attempted in a complicated district he would carry it to completion; distances were long and arduous—no exertion should be too great or hardship too severe; work on sections and maps must be delicate and minute—he would train his hand and eye as an artist; graptolites were the only fossils—he would learn all that was known about them and find out for himself what else there was to be learnt; search for fossils must be thorough and exhaustive, collection laborious and exact—he knew that this was vital and did not shrink from carrying it out.

So he worked for fourteen years; first at Galashiels, then in Roxburgh and Selkirk, next at Moffat, which he realised was the key area, and finally he put

his deductions to the test in the baffling ground of Girvan and Ballantrae. He succeeded in proving that the Moffat Shales—a black, fine-grained, deep-water, deposit, some 300 feet thick—represented in full the thousands of feet of ashes, grits, greywackes, and slates of Upper Llandeilo, Bala, and Llandovery ages in Wales and England: That, in their thin beds, foot by foot and sometimes inch by inch, distinct graptolitic faunas were embedded, succeeding one another in an unvarying order, comparable in part at least, to their succession in much thicker masses elsewhere: That the shale bands themselves were the highest part of a series of rocks underlying the Silurian greywackes of the Uplands, brought to the surface in narrow anticlines, which were generally inverted and always complicated by faulting: That this region, instead of possessing a simple ascending sequence, as had been supposed, was really one of intense plication and convolution, so much disturbed that anticlines had been mistaken for synclines, to the effect that the apparent succession was upside down: And that there existed here mountain structure of the same type and order as was being worked out in the Alps.

In the Girvan region he found confirmation for all his conclusions, using the graptolites that he discovered here, too, as his clue, and mapping the difficult country as faithfully as before; and thus he set in order a group of rocks very much thicker than at Moffat, and containing faunas of trilobites and shells intercalated among his graptolite zones.

By this time he had worked out the life sequence of about half of the Lower Palæozoic rocks, dividing their history into a dated zonal calendar so soundly constructed that events all over the world have been found to fit into it. The same results could have been worked out years before, in half-a-dozen easier regions, where the sequence was laid out in simple order, had it but occurred to any observer that such minute work was either necessary or desirable, and had any geologist tested organism after organism until he hit upon those which best served his purpose. Lapworth effected it in a region where there was not a single straightforward section, among traps and pitfalls innumerable, and where the rocks were as often on their heads as on their heels. Well justified was the note of triumph in his words: "Zonal work is probably destined to effect in the history of geological research a revolution as great and an advance as rapid as those brought about by the use of the microscope in the history of biology."

He recognised the debt he owed to the graptolites for his success, and he was not the man to leave a debt unpaid. He made a careful study of all that was known, or to be known by his own researches, of their biological characters and geological distribution. He arranged his holidays so that he could collect them all over the country, examined those sent him from other areas, whether in Britain or Overseas, classified them afresh and described a host of new forms, and finally established some twenty zones in his own district and outside, which he found were not only of world-wide extent, but so exact that in several instances he was able to correct errors into which local observers in Wales and America had been led by delusive

stratigraphy. His magnificent paper on the 'Distribution of the Rhabdophora' came like a revelation to his co-workers, so many unexpected results did it disclose, and so thoroughly had Lapworth realised his dream of zoning Lower Palæozoic rocks, not merely as satisfactorily as had been done for the Neozoic rocks, but, in truth, far more so. For these zones are more constant over wide areas than are those in newer rocks, the reason, worked out by Lapworth, being that the graptolites were pseudo-planktonic in habit; attaching themselves to seaweeds, they were drifted right over the seas, and became embedded in the fine sediments deposited in the ocean depths.

The influence of this research upon contemporary work is shown in the 'Monograph of British Graptolites' by Miss Elles and Miss Wood, written for the Palæontographical Society under Lapworth's editorship, in which the Lower Palæozoic zones are shown to have increased from 20 to 36, and the number of species dealt with from 284 to 372. The graptolites were also made to yield their contribution to the study of evolution; the minute variations exhibited by the species in ascending sequences, which in the first instance were observed for their geological bearing, being evidently directed towards improved conditions of life and promoting the survival of the fit.

Lapworth's third piece of work relates to the ancient rocks of the Midlands, beginning in 1881, the date of his appointment as Professor at Birmingham. The Ordovician rocks of Shropshire were studied in detail, and paralleled with those of Scotland and elsewhere. The Upper and Middle Cambrian rocks were brought into order, and to some extent zoned. Two distinct types of Lower Cambrian rocks were individualised in Shropshire and Warwickshire, fossiliferous rocks of this age being thus demonstrated to exist in England, and their chief fossil being reconstructed and described. The Welsh Lower and Middle Cambrian rocks were mapped at Harlech and compared with those of the Midlands, just as these in their turn were found comparable with those of the Highlands of Scotland. Deeper still, the rocks of the Longmynd, the oldest massive sediments yet known in the country, were for the first time mapped in detail, proved to be earlier in date than the Cambrian, and compared as to their higher members with the Torridon Sandstone of Scotland, and, as to their lower members, with the problematical rocks of Charnwood Forest in Leicestershire. The underlying rock of the Uriconian group was also mapped in Shropshire, and the occurrence of related rocks proved under the Cambrian of Worcestershire and Warwickshire.

It was only fitting that one so steeped in the facts should lay down the law as to the nomenclature of the Older Palæozoic rocks, and particularly that Middle Group, of which, in Scotland, England, Wales, and Ireland, he had become *facile princeps.* The work of the "old masters" had left this middle group in dire confusion. Lapworth cut the knot, and, impressed by the existence of three great and sub-equal faunas in the Older Palæozoic rocks, he erected his "Ordovician System" to rank with the Cambrian

below and the Silurian above. Thus he earned the grateful thanks of geologists of his own and later times, for a work which could only have been done by one of his great knowledge, and which would never have been accepted but for the respect in which its author was held, and the knowledge, skill, and tact with which he stated his case.

The fourth piece of work, which will always be associated with Lapworth's name, was to overcome the great stratigraphical difficulties which had led to a mis-reading of the complicated sections of Lower Palæozoic and Eozoic rocks in the North-west Highlands of Scotland. Sir Jethro Teall, who (one of very few) had the advantage of seeing Lapworth's methods of work in this region, and who afterwards studied the petrology of the Highland rocks and took part in the publication of the 'Geological Survey Memoir,' has been so good as to furnish the following account of the Highland work:—

> I have been asked to say something about Lapworth's work in the north-west of Scotland, and I do so with great pleasure as it was my good fortune to spend a day or two with him in Eriboll in 1883. But before describing what was to me a memorable experience, it will be necessary to give a brief account of the situation as it stood when he first visited the district in 1882, and of his work in that year.
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> Having established the principles by which the complicated stratigraphy of the Southern Uplands could be interpreted, and applied those principles with brilliant success to the Moffat and Girvan areas, he felt free to turn his attention to the Durness-Eriboll region, where Lower Palæozoic rocks were also known to occur. In the fifties and sixties of last century, a keen controversy had arisen between Sir Roderic Murchison and Prof. Nicol of Aberdeen, concerning the relation of these fossiliferous rocks to the "Eastern Schists" of Sutherland and Ross. According to Murchison there was a gradual upward passage from fossiliferous strata to the crystalline schists, whereas Nicol maintained that the superposition had been brought about by faulting and that the highly metamorphosed "Eastern Schists" were older than the comparatively unaltered sediments—not younger as Murchison supposed.
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> This controversy had recently been revived by Hicks, Callaway, Bonney, and others, and although much new light had been thrown on the subject, the problem had not been definitely solved. The fact that previous observers had arrived at diverse conclusions, for even the opponents of the Murchisonian view were not agreed on all points, convinced Lapworth that some secret lay hidden in the district; and so, equipped with the six-inch Ordnance maps, with a knowledge of mountain structure derived partly from his own work in the Southern Uplands and partly from the work of Rogers in the Alleghanies, Heim in the Alps, and others, but above all with his own genius, skill, and enthusiasm, he set to work on his self-imposed task.

He began in 1882 at Durness where the evidence of the superposition of the Eastern Schists on the fossiliferous rocks is conclusive; but not finding, in that region, a clue to the solution of the difficulty he soon moved on to Eriboll, taking up his quarters on the east side of the loch at a shepherd's house, near Heilem Ferry. Here he found what he wanted; a succession of well stratified rocks divisible into zones suitable for mapping purposes and a district in which the rock-exposures were sufficiently numerous to enable the distribution of the different zones to be recorded on his maps. It must be remembered that the fundamental difference between the work of Lapworth and that of all previous observers is, that he attacked the problem in the only way that it could be satisfactorily dealt with; namely, by geologically surveying the district on maps sufficiently large in scale to bring out the extremely complicated structure. His main work was done on the six-inch maps, but in some areas this scale proved inadequate.

At the end of the short summer holiday he had surveyed a considerable area, extending from Whiten Head to Eriboll House, in sufficient detail to bring out the main structural features and to disprove the theory of the upward succession from the unmetamorphosed fossiliferous rocks to the crystalline schists.

On returning home he began a series of articles in the 'Geological Magazine' (March, 1883), entitled "The Secret of the Highlands," stating his general conclusions in the following words (p. 121):—

"I believe that we have in the so-called metamorphic Silurian region of the Highlands of Scotland a portion of an old mountain system, formed of a complex of rock formations of very different geological ages. These have been crushed and crumpled together by excessive lateral pressure, locally inverted, profoundly dislocated, and partially metamorphosed. This mountain range, or plexus of ranges, which must have been originally of the general type of those of the Alps or Alleghanies, is of such vast geological antiquity that all its superior portions have long since been removed by denudation, so that, as a general rule, only its interior and most complicated portions are preserved to us. In the area partly worked out by myself, the stratigraphical phenomena are identical in character with those developed by Rogers, Suess, Heim, and Brögger in extra-British mountain regions." This series of articles was never completed for reasons that will appear later.

In the following year (1883) he returned to his task, and my acquaintance with him arose in this way. In the early summer of that year the late Prof. J. F. Blake and I met Lapworth at Rhiconich, in Sutherland, by accident, and went on with him to Durness, where we found Peach and Horne, who were then beginning the official geological survey of the district. Lapworth very kindly invited Blake and myself to accompany him to Eriboll, an invitation which we gladly accepted. On the way he called our attention to the unconformable

junction of the quartzite with the Lewisian gneiss, and to the lithological zones in the quartzite. This was necessary because only the upper part of the quartzite—the "pipe-rock" of Nicol—is exposed in Lapworth's type section at An-t-Sron, on the other side of the loch. Our first work in Eriboll was to examine this section. There he demonstrated the normal succession from the pipe-rock of the quartzite, through the "fucoid" beds and the salterella grit, to the limestone, directing our attention to the various zones—seven in all—which he found useful for mapping purposes, and had already described in his paper on 'The Secret of the Highlands.'

During the remainder of the time we were with him, about two days, he took us rapidly over the ground that he had surveyed in the previous year between Eriboll House and Whiten Head. First he proved to us that the zonal succession which he had established at An-t-Sron was repeated on the opposite or eastern side of the limestone basin, with steep dips and other complications that it would have been very difficult to unravel without a knowledge of the true sequence. At Arnaboll hill he showed us the Lewisian gneiss, resting on quartzite, called our attention to the abnormal aspect of both rocks, especially near the junction, and gave us his interpretation of the facts. The gneiss, he said, had been forced westward over the fossiliferous sediments on an overthrust-fault of low hade, and the abnormal aspect of the rocks seen here, and at many other places which we subsequently visited, was due to the fact that they had been crushed and in some cases rolled out by what he called the earth-mill, so as to be almost unrecognisable. He invited me to take specimens and examine them under the microscope, as he had done. Those specimens are now some of my most cherished possessions, for they introduced me to a new petrological world. He suggested that the term "mylonite" would be appropriate for rocks that represented the extreme phase of mechanical metamorphism, and this term, now well established, he finally adopted.

Leaving Arnaboll hill we worked northwards towards Whiten Head, and, to remove any doubt that might linger in our minds as to the identity of the "Upper Quartzite" of Murchison with the "Lower Quartzite," he placed me on the latter with instructions to walk along it, making sure that I never left it. He and Blake took up their positions on the "Upper Quartzite," and moved in a direction roughly parallel to me with the same care. Progress was slow, for in such a disturbed area it was necessary to examine every inch of the ground. Finally we met on quartzite, shook hands, and declared that beyond all shadow of doubt the "Upper Quartzite" was merely the "Lower Quartzite" brought up again by the disturbances of which we had already seen such striking evidence. As a further illustration of these disturbances he took me, after Blake had left us, to the shore north of Heilem, and showed me that the salterella grit had there been

repeated many times on itself by clean-cut faults, a striking illustration of "*schuppen*" or imbricate structure, common in the Alps and elsewhere, but new to Britain.

The relation of the Eastern or "Moine Schists" to the complex of mechanically metamorphosed rocks which lie below them was then engaging much of his attention. He showed us that both groups had been, so to speak, alive with movement, and speculated as to the possibility of the latter representing a phase through which the former had passed, but his views on this great question were not at that time fully developed. The position he finally reached was summarised by him as follows in a communication read at the Geologists' Association on July 4, 1884, and subsequently published.* "I believe at present that the great area of metamorphic schists of Sutherland and the Central Highlands is, as a whole, neither Archæan nor Ordovician. The Sutherland Gneiss—Arnaboll—is Archæan. The Sutherland Schist has been manufactured since Silurian times. For all I know, there may be large areas (in the Central Highlands, etc.), composed wholly of Archæan (Laurentian) rocks, or of Cambrian or pre-Cambrian rocks. When the metamorphism of the Highland area began I think that it is impossible to say, and may be always impossible. One thing seems pretty clear to me—the so-called oldest beds of the Highland succession of the Schistose Series of the N.W. Highlands are the newest in point of time. The zone of intermixture and metamorphism (in Sutherland) travelled to west from east, and the last beds (schists) manufactured are those now in contact with the Assynt Series in Durness, Eriboll, and Assynt" (p. 441).

The subsequent work of the Geological Survey has not confirmed these views, except so far as the occurrence of Archæan (Laurentian) rocks among the Eastern Schists is concerned; but, as stated in the "Memoir on the Geological Structure of the North-west Highlands of Scotland" (p. 600): "The age and origin of the rocks that have been mapped as Moine schists is a complicated problem which has not been finally and definitely solved, but abundant evidence has been accumulated to show that under the influence of the post-Cambrian movements rocks of diverse age and origin have acquired a common type of structure, and that true crystalline schists have been simulated if not actually produced."

The short time we were with Lapworth supplied us with a continual succession of surprises. We did not know which to wonder at most, the remarkable stratigraphy of a type hitherto unknown to Britain, or the skill with which Lapworth was unravelling it. He had been full of energy and enthusiasm, relieved by occasional flashes of humour, during the whole of the time we were with him; but it was obvious that the strain, both mental and physical, was very great and before the end of

* 'Proc. Geol. Assoc.,' vol. 8, pp. 438–442 (1885).

the season he broke down under it. In his feverish dreams, as he lay ill in the shepherd's house below the Arnaboll ridge, he used to fancy that the gneiss was still moving westward on the overthrust fault which he had discovered and mapped, and that both he and the house would be crushed by the advancing mass. So ended Lapworth's work in the north-west of Scotland.

In 1883 the Geological Survey commenced their operations in the Durness–Eriboll region and on November 13, 1884, there appeared in 'Nature' a preliminary account of their work. It consisted of an Introduction by Dr. now Sir Archibald Geikie, in which he frankly abandoned the Murchisonian view, and a Report by Messrs. Peach and Horne which proved that the Surveyors had arrived at practically the same conclusions as Lapworth, both as regards the stratigraphy and the metamorphism. The following extract from a letter to me dated November 14, will show how he received the news that he had been forestalled as regards publication:—

"The matter has ended beautifully . . . I am too late with the publication of my results, but that is a matter of no consequence as the facts are out distinctly and clearly."

Referring to the communication to the Geologists' Association in his final paper "On the Close of the Highland Controversy,"* he wrote, on p. 98:—"It is not referred to in this place as establishing any selfish claim to priority, for the officers of the Survey reached their conclusions in complete ignorance of my results and from a totally different direction, while they have gone far beyond me in working out the details," and on p. 102, "The old subject of dispute has utterly disappeared and there is no longer any reasonable excuse for dissension. We have all been partly right and partly wrong. It is time for a hearty laugh all round, a time to shake hands and be friends."

It was Lapworth's intention to return to the district after the complete results of the work of the Geological Survey had been published, but the opportunity never occurred. This is to be regretted for there are certain statements in his communication to the Geologists' Association which require further elucidation. For example, he states that "in some cases the original dividing plane (either bedding plane or fault plane) of two successive rock-sheets has been twisted into the form of spirals, cornucopias, etc." It must be remembered that he possessed to a remarkable degree, the faculty of thinking in three dimensions. A few facts observed on the surface suggested to him a hypothesis as to the kind of deformation to which a rock-sheet had been subjected, or, in other words, as to its course underground and where it should reappear. The hypothesis was then tested by further observation and at once discarded if its predictions were not verified. He was constantly forming such hypotheses and discarding or modifying them until he

* 'Geol. Mag.,' December 3, vol. 11, p. 98 (1885).

found one that fitted all the facts obtainable. If he had carried out his intention of returning to Eriboll, the somewhat puzzling statement quoted above would doubtless have been expanded and illustrated.

In comparing the published work of Lapworth with that of the Geological Survey, one is struck by the fact that Lapworth approaches the subject from what may be called the "fold" point of view, the Survey from the "fault" point of view. Lapworth did not attach much importance to this difference, for in a letter to me dated September 21, 1885, he wrote:—"The Survey men have gone beyond me in boldly grasping the idea that the same result can be arrived at quite apart from following the theoretical stages downwards inch by inch and simply asserting that under pressure the rock snaps—like a sheet of ice—flakes or shears in parallel slates which slide over each other. In that I agree, though I reach it from an opposite direction. I hold that over-folds, sigmaclines, overfaults, thrustplanes, are *homologous* and pass insensibly one into the other; exactly as the American geologists believe that monoclines and ordinary faults are *homologous* and pass insensibly one into the other. As under certain conditions (excessive tension and torsion) no monoclines are formed but simply *faults*; so under certain opposite conditions (excessive pressure and torsion) no overfolds are formed but simply thrustplanes (my overfaults)."

J. J. H. T.

As the distribution of the humble graptolites—"outils qu'il a forgés lui-même et que d'autres eussent dédaignés"—had established world-wide time horizons, and had unravelled directly the mountain structures of the Uplands—and indirectly those of the Highlands—so were these last in their turn to be applied to the tectonics of the broader features of the earth-crust.

In this, as in his other work, while possessing deep and sympathetic knowledge of the researches of such geologists as Suess, Heim, Bertrand and Brögger, he held steadily to the views of the mechanics of the earth's crust to which his independent thought had led him. In his view the structure underlying rock complication was the "fold," Hogarth's "line of beauty and grace," sometimes tearing into faults or breaking down into cleavage, and of all dimensions from microscopic to mountainous. Lapworth, in his epoch-making address to Section C of the British Association at Edinburgh, and in later addresses, showed that the continents were but the crests and the oceans but the troughs of great earth-waves, with the septum between the two gentle and inactive, or else abrupt, advancing, and alive.

The greater continental crests are generally sagged downwards, and the oceanic troughs buckled upwards, at their centres. The "land hemisphere" of the world, with its central sag, the Atlantic, has its counterpart in the hemispheric Pacific depression, the one divided from the other by the greatest septal line of the globe, the "Pacific girdle of fire," "ablaze with volcanoes and creeping with earthquakes."

The "Challenger" had revealed for the first time the true or planetary contour of the earth crust, discovering a new world larger than that fraction of the globe hitherto known, the land part of it. It had depicted the plateaux on which the visible continents stand, separated by steep (septal) slopes from the deep ocean floors. Lapworth showed that the calculations based on these discoveries were consistent with and were explainable on his "fold theory." The area of ocean bed below the mean line of the septal slope is equal to the area of the earth-crust above it, while the bulk of ocean water below that level is equal to the crust material projecting above it.

Stretching from pole to pole, he states, we have three great crests, America, Eur-Africa, and Asia-Australia, with their three troughs the Pacific, Atlantic, and Indian Oceans. At right angles to them are the Arctic Ocean, the Mediterraneans, and the Southern Ocean, with their continental crests in the latitudes of North America, South America, and what, if the theory is sound, must be an Antarctic Continent. The interference of these two sets of master-folds accounts for the position of the great land-masses, their oblique coasts and their triangular terminations, and for the ocean deeps. Analogies to these may be detected in the moon and planets, and in the sun, while, in theory, they may possibly be found passing outwards and upwards, in the spirals of the nebulæ and in "that most glorious septum of all the visible creation the radiant ring of the Milky Way."

There has been no space to dwell on other sides of Lapworth's character or activities. His gift for teaching in the class-room and the field; his inspiring influence on his co-workers; his services to the State in relation to Geological Surveys and Coal Supplies; his bid for the freedom of research from the shackles of convention and authority; his genius for the grouping of facts, and the scientific use of his vivid imagination as a tool in his own research and a generous stimulant to investigation and discovery in others; his elaborate care that the results of clear thinking should be as clearly and logically expressed in his own writings; and his profound belief in the importance in education and in life of science generally, and his own science in particular, "not only the interpreter of Nature, but also the servant of Humanity."

Though his friends well know that his work, founded on the truest devotion to scientific principles and a passionate love of truth, can never die, they must henceforth miss the kindly and genial presence, the rich stores of many-sided wisdom and experience, the boundless energy and enthusiasm, the flashes of genius and inspiration, the transparently beautiful character of him who is no more.

> "His life was gentle; and the elements
> So mix'd in him, that Nature might stand up,
> And say to all the world, 'This was a man!'".

The portrait is from a painting by Mr. Bernard Munns and is reproduced with the kind permission of the Council of the University of Birmingham.

W. W. W.

Photo: Elliott & Fry.

Leonard Doncaster

LEONARD DONCASTER, 1877—1920.

THE death of Leonard Doncaster at the age of 42 has stopped a career of exceptional promise. He was a natural investigator, driven to research by the impulse of scientific curiosity, and his work will have a permanent place in the history of genetics. Born at Sheffield, December 31, 1877, the son of Samuel Doncaster, manufacturer, and his wife Emma Gertrude, whose maiden name was Barber, both members of the Society of Friends, he was one of the many naturalists produced by that body. Throughout his life the Quaker principles governed his development and bearing towards the world, an influence which naturally became marked during the period of the war. He was sent to the Friends' Public School, Leighton Park, Reading (1890—5), going thence to King's College, Cambridge (1896), where he took an open scholarship. Before coming up he spent six months at Heidelberg, and thus had the great advantage—to a biologist—of some knowledge of German from the start. In the Nat. Sci. Tripos, Part II, 1910, he took a First, with a mark of distinction in Zoology, rarely given.

Though a zoologist by formal choice, he might equally have been a botanist. From childhood there was never any doubt as to the leading purpose of Doncaster's life. The problems of biology were always in his thoughts, and the form in which they presented themselves was to him indifferent. As a young student he was already a competent field botanist and entomologist, with some knowledge also of the domesticated animals and plants, much of which he had acquired in his father's well-known and beautiful garden. But, though his range was wide, anything like vagueness or superficiality was quite alien to his composition. He liked knowledge hard and clear, and his weaknesses were not those of the omniscient or the expansive. Circumstances led him into academic zoology, but he never lost touch with these varied interests. Biology was probably to him, as to so many modern naturalists, rather a challenge than a source of contemplative enjoyment. A hint, which could be used in the attack, might come from anywhere.

After taking his degree he spent some time at the Naples Station (1901—2). He had there two objects in view, the first being to make a fresh investigation of the structure and development of *Sagitta*. In this animal there is an ovary and a testis on each side of the body; and since the two organs, male and female, of each side arise directly by division and differentiation of a single common mother-cell, there was a chance of seeing something exceptionally interesting in the cytological phenomena by which this process is accomplished. A useful paper on *Sagitta* was the outcome of this work, but cytologically the material proved intractable. His second purpose at Naples was to find out why such various and discordant results had been encountered by previous experimenters on the

hybridization of Echinids—a problem which had naturally acquired fresh interest from the recent discovery of the Mendelian system of analysis. Up till that time the observation that a cross between two species did not give a uniform result was accepted without demur, but now such an occurrence was anomalous, and called for special consideration. A great deal still remains to be done in this vexed field. Doncaster, however, was successful in contributing one new fact, namely, that the seasonal changes observed by H. M. Vernon were in the main directly dependent on temperature. His experiments, which were very laborious, also demonstrated a curious series of individual differences in the degree of dominance—a phenomenon which should be examined further.

On his return to England he attached himself definitely to the group of studies now called genetics, and began a number of investigations, most of which involved experimental breeding. At various times he bred rats, cats, pigeons, besides gall-flies, moths, and other invertebrates. The early struggles of Mendelism to obtain a hearing were then in an acute stage, and though strongly inclined to caution, he knew enough of the general course of variation and heredity to be in no doubt as to the essential truth of the new doctrines. His adhesion certainly helped greatly in spreading confidence among his contemporaries. His first paper on a definitely Mendelian problem was a note on the Tortoise-shell Cat, in which he showed that the well-known rarity of tortoise-shell tom-cats is an expression of the fact that whereas the cross between orange and black produces female tortoise-shell, the corresponding male form is orange. About this time he began an investigation into the life-history of the saw-fly, *Nematus ribesii*, and of the gall-fly, *Neuroterus lenticularis.* The latter subject he pursued with intervals for more than ten years. The purpose was to discover the mechanism by which sex was determined in these species. The fertilized eggs of *Neuroterus* give rise to females only, as do those of *Phylloxera* and some other Hemiptera. These females, unfertilised, produce other females of which (again without fertilisation) some produce males, and others females, not a mixture—a condition now known to be parallelled by various insects. The cytological distinction, if there is one, between these two types of females, is still undiscovered. Doncaster's work on these and similar types, and especially the clear exposition which he gave of their intricate polymorphism, contributed much towards the comprehensive codification by which a mass of apparently contradictory records as to the parthenogenetic or agamic and sexual forms of Hymenoptera and Hemiptera has been reduced to order.

He will be, however, best remembered for his experiments on the inheritance of sex in *Abraxas grossulariata*, the Currant Moth. From the Rev. G. H. Raynor, who had long been a fancier of this species, Doncaster learnt that the variety *lacticolor*, distinguished from the common form by a great reduction in the amount of black, was known only in the female. At that time no example of what is now called "sex-linked" inheritance

amenable to experiment had been studied. He at once saw the extraordinary importance of the subject, and, as the result of correspondence with Mr. Raynor, matings were arranged and a critical investigation of the case was begun. It was soon found that by mating *lacticolor* females with F_1 males (from *lacticolor* female × *grossulariata* male), the missing *lacticolor* male could be produced, and the various possibilities tested. The facts proved that the system of inheritance is exactly the converse of that previously known to be followed by colour-blindness and certain other conditions in man. Whereas the linkage of colour-blindness is with maleness, that of *lacticolor* is with femaleness. The first clear proof, carried out by the method of experimental breeding, that sex is determined in the gametes, was then made. Incidentally, another very curious corollary followed, namely, that ordinary wild *grossulariata* females are actually heterozygous for *lacticolor*, though that variety is seen very rarely in nature. But the cytological proof that in certain Hemiptera sex is determined by the gametes of the male had recently been obtained by E. B. Wilson, and this great discovery had naturally impressed Doncaster. Since the spermatozoa of these Hemiptera were visibly dimorphic in respect of sex, and the eggs of *Abraxas* were no less demonstrably proved by breeding methods to be dimorphic, he doubtless felt that this dimorphism must be a condition generally obtaining among the germ-cells of *both* sexes, and he therefore devised a scheme of sex-determination (also proposed independently by Wilson) representing both possibilities. This compromise involved the conception that both sexes should be heterozygous in sex and the supposition that dominance attached to the gamete received from the female. Subsequently, he accepted the emendation by which the female only of *Abraxas* is regarded as heterozygous, as the male is in the Hemiptera and Diptera, the other sex in each case being taken simply to be homozygous, paradoxical though that conclusion is.

The next step was to look for a cytological proof of the dimorphism of the eggs, but this was not to be had. The diploid number was large, 56, and was only established with difficulty. But in the course of this further work a remarkable new phenomenon was encountered—that a certain strain of *A. grossulariata* had two kinds of females, of which one produced females almost exclusively, the other giving the usual mixture of sexes. Entomologists have met with great departures from the normal sex-ratios in Lepidoptera, but none had ever been investigated systematically.

Doncaster was still engaged on this inquiry when his final illness supervened. He found evidence of cytological distinctions between the two kinds of females, the female-producers having 55 chromosomes instead of the normal 56. He attempted an interpretation on the lines followed by Bridges in his paper on "non-disjunction" in *Drosophila*, but, as Doncaster pointed out, the suggestion was not consistently applicable, and much remained to be done.

In these latter years some further progress was made with the old

problem of the tortoise-shell cat, which he had continuously kept in mind since the early days of Mendelism. As mentioned above, he had shown that, as a rule, the heterozygous combination of orange with black is tortoise in the female but orange in the male, a distinction which has not yet been factorially represented. But very rarely a male is produced having the characteristic tortoise-shell distribution of colour. From extensive inquiries among breeders, and from some direct observations of his own, Doncaster came on the interesting and suggestive fact that these rare tortoise-shell tom-cats are almost if not quite always *sterile.*

In his last paper of all, he put forward the original but by no means extravagant notion that perhaps the tortoise-shell tom is a free-martin, owing its peculiarity to intra-uterine influence of other female embryos. This conjecture was made in consequence of Lillie's surprising discovery as to the nature of the bovine free-martin. Most of these subjects were discussed in his useful text-book, 'The Determination of Sex,' 1914, but of course in the six years that he lived after that publication much progress was made. In regard to the chromosome hypothesis his views were at that time in a transitional stage. It may be noticed, for instance, that in discussing the descent of colour-blindness he does not develop the cytological argument. Normal colour-vision is represented as depending on the presence in the male of a single factor N, the loss of which produces in him colour-blindness. The normal female is homozygous in N, the transmitting female, whose sight is normal, being Nn, like the normal man. This was the notation which he had proposed in 1911, and it has been adopted as an improvement on all previous suggestions. But in man the normal distribution of the sex-factor must be the same as that of N. The inference that the two factors, the one for colour-vision, the other for sex, are transmitted in collocation beomes inevitable; though till man is proved to have a distinct sex-chromosome, the nature of the collocation might be left to the imagination. Throughout the book also he never loses sight of the somewhat ill-defined though unquestionable evidence as to the possible modification of sex-ratios by influences of some different order, a circumstance which has hitherto not been reconciled with cytological appearances.

But of the various modes of attack on genetical problems his mind turned perhaps more naturally to cytology than to any other. Laboratory methods were congenial to him. He came to regard the empirical results of experimental breeding more and more as a stimulus to microscopical search for some visible basis of difference to which genetical diversities could be referred. The element of apparent fundamentality which he found in cytology very strongly appealed to Doncaster's analytical mind, and he was therefore from the first greatly attracted by the theory of linkage propounded by Morgan. In the clear and excellent 'Cytology,' which he published shortly before his death, he declares himself an adherent, a judgment which, from a student so slow to form decisions, has special value. At a moment when the claims of cytology are acquiring such prominence,

his loss will be severely felt, for we have in this country no one who combines, as he did, personal experience both in all the branches of genetics and in cytological technique.

He was an exceptionally clear-headed thinker and speaker, full of enthusiasm and faith in the value of his work, and therefore an admirable teacher. From 1906–10 he held a zoological post at Birmingham University, being for the latter part of his tenure of that appointment Lecturer in Heredity and Variation. In 1908 he married Dora, daughter of Walter Priestman, of Birmingham. Returning to Cambridge, he served the University in various capacities, especially as Superintendent of the Museum of Zoology (1910–14), and Lecturer in Zoology (1911–17). When, in 1919, Professor Herdman resigned the Derby Professorship of Zoology in Liverpool University, Doncaster was appointed his successor. To geneticists this appointment was a source of great satisfaction. It seemed that a fresh centre for the development of these interests was assured. He began the work of his new Chair with all his zeal and devotion. But within a year he was struck down with malignant disease, and died May 28, 1920.

In a notice of his "fine young colleague" which appeared in the 'Liverpool Daily Post' of May 29, Professor Herdman wrote:—"Doncaster was a splendid lecturer, and an investigator of the first rank. But what struck one most, beyond these high qualities, was his absolute right-mindedness, his serious conscientiousness, his evident determination to do what he felt to be right under all circumstances. We have all alike been impressed by the care and trouble that he took, by his sound judgment, and the weight of his considered opinion."

Personally, Doncaster was slight in build, and in temperament intellectual, highly strung and somewhat anxious—a combination not rare among the advancers of knowledge. His mind was always working, and he felt and thought of everything with concentration and intensity. The years of the war were, I believe, to him a period more horrible than to most thoughtful men. He held strongly the Friends' attitude of the unlawfulness of war, but feeling that alternative service was a duty, he gave up his researches and qualified as a bacteriologist, working first in one of the Cambridge military hospitals, and afterwards in the Friends' Ambulance Unit at Dunkirk.

Holding these reservations from the common ways of men, he never joined quite easily in ordinary society. That religion was a prominent element in his nature was well known to his scientific friends, but it made no obvious difference in his demeanour towards us. This pre-occupation, latterly, came nearer to the surface, and in Cambridge he occasionally delivered religious addresses, it is reported, with distinction. At Dunkirk also he took part in the Sunday services of the Friends.

In several ways his work received outward recognition. He was Mackinnon Research Student of the Royal Society (1904–5). He was

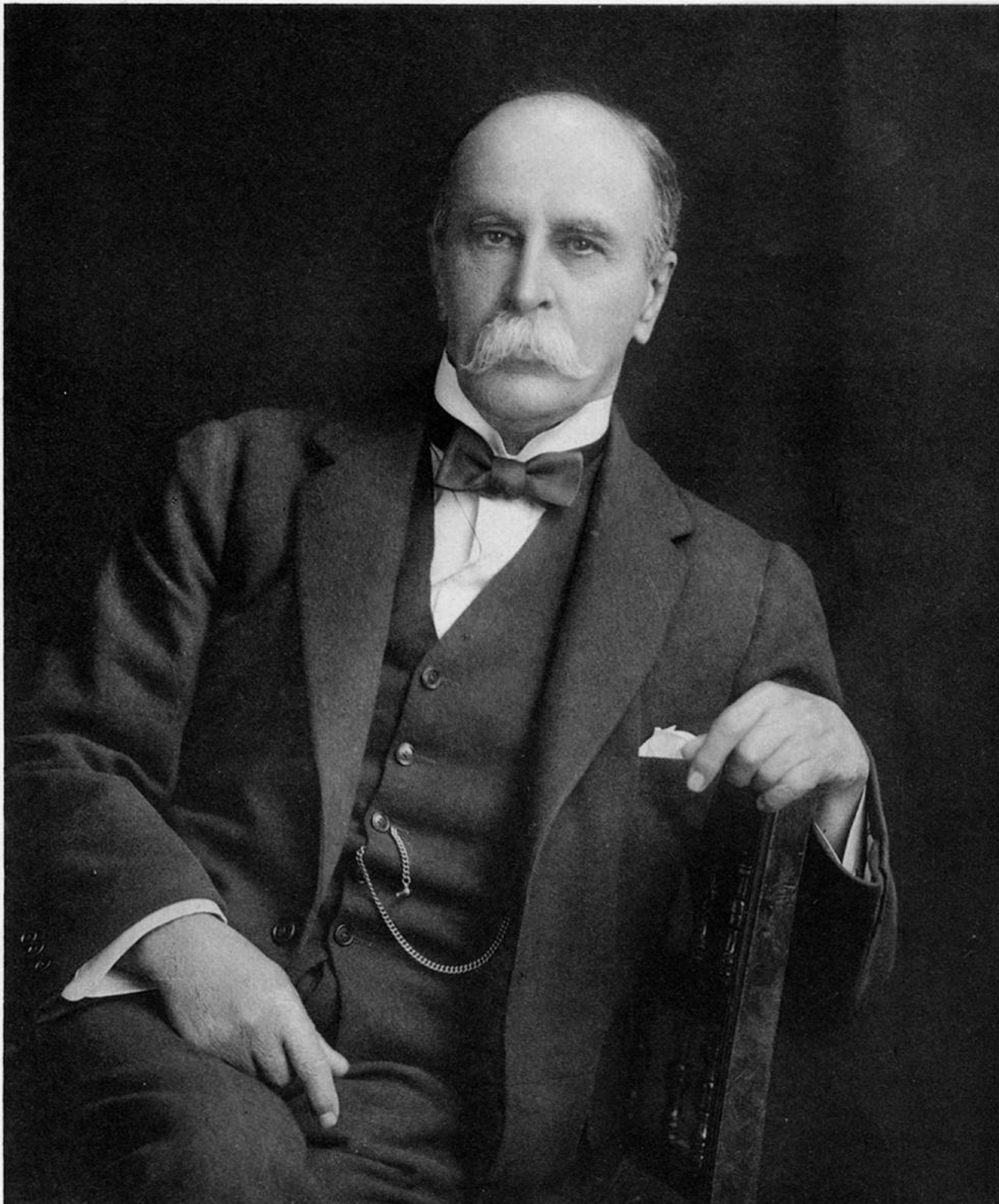
awarded the Walsingham Medal in Cambridge, and the Trail Medal of the Linnean Society.

In 1910 he was elected to a Fellowship at King's College, Cambridge, and in 1915 he became a Fellow of the Royal Society.

He leaves one son and two daughters.

W. B.


Thomas R Fraser.



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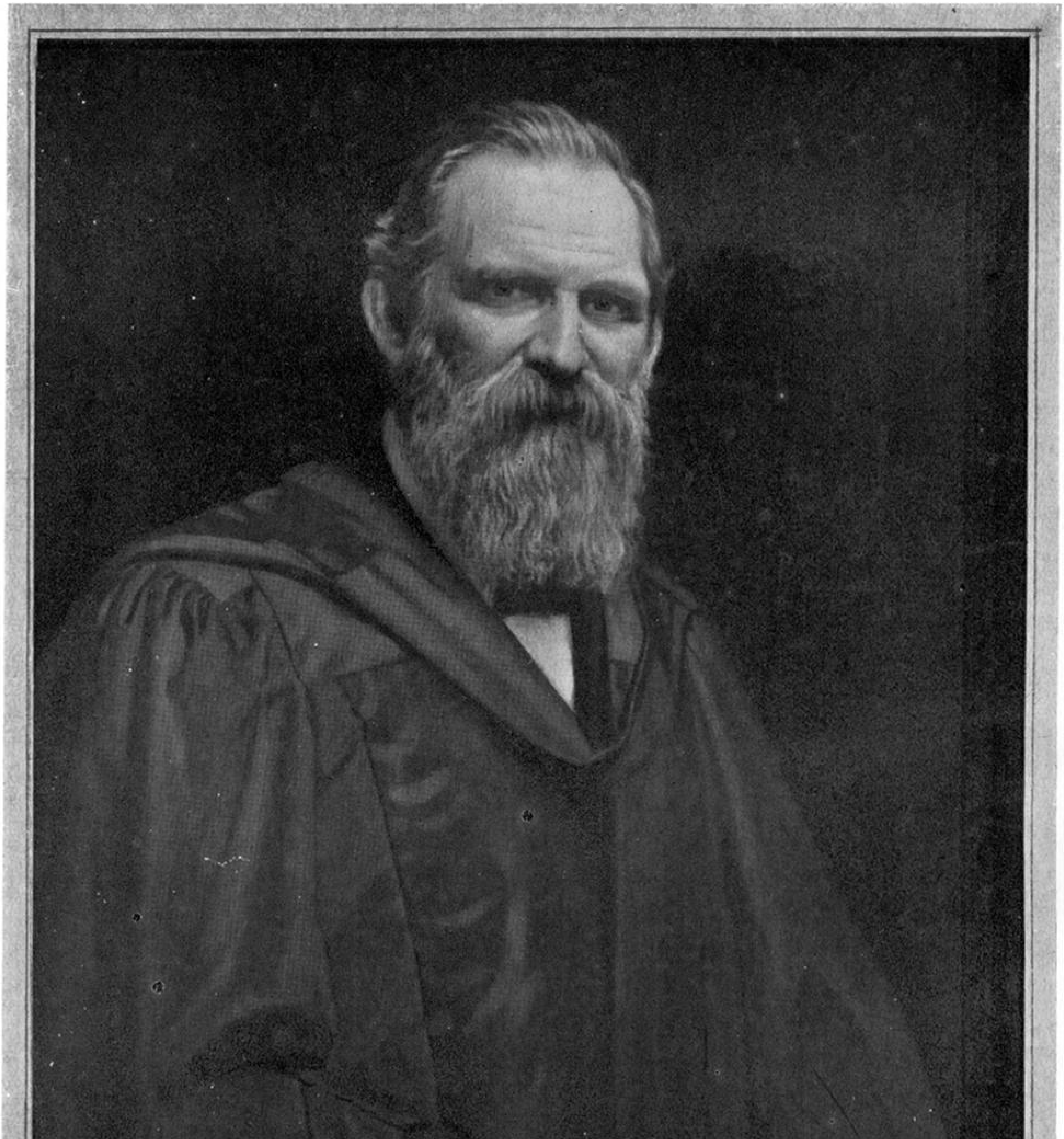

Chas. Lapworth

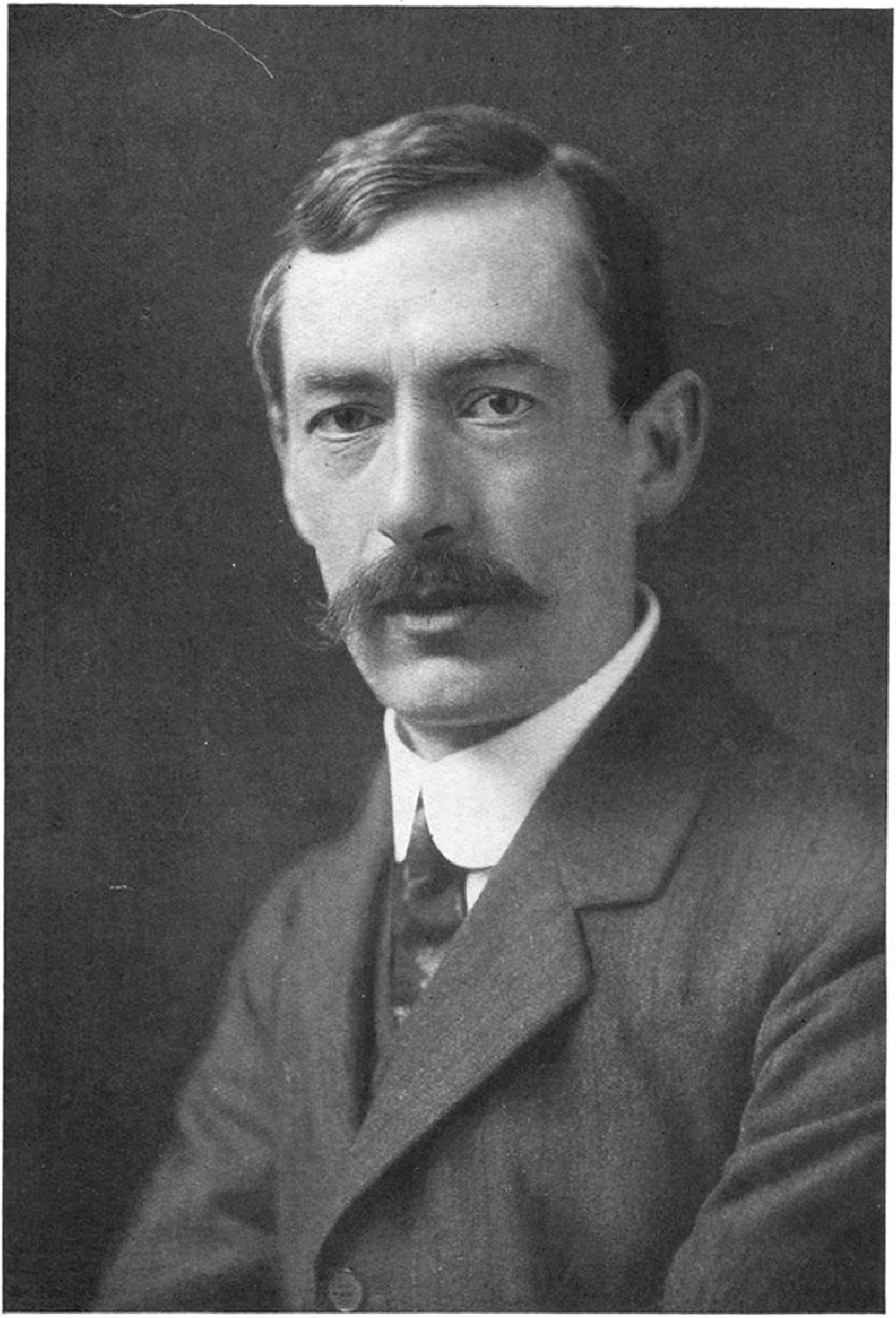

Photo: Elliott & Fry.

Leonard Doncaster