OBITUARY NOTICES OF FELLOWS DECEASED.

Carl Friedrich Wilhelm Ludwig was born on the 29th of December, 1816, at Witzenhausen, on the Weser, near Cassel. He graduated at Marburg, in 1839, became prosector under Professor L. Fick, in 1841, and the year after obtained the *venia legendi*, offering for his dissertation a paper on the mechanism of the renal secretion. In 1846 he became Extraordinary Professor at Marburg, and in 1849 he was appointed ordinary Professor of Anatomy and Physiology at Zürich. In 1855 he became Professor of Anatomy and Physiology in the Army Medical School (Josef's-Akademie) at Vienna. In 1865, when the new chair of Physiology was constituted at Leipzig, Ludwig was invited to occupy it, and afterwards assumed the direction of the new Physiological Institute, which had been erected under his own supervision. For a time he took an active part in the reorganisation of the teaching of natural science in the University, but subsequently, and for the remainder of his life, devoted himself almost exclusively to the work of the laboratory.

In 1874 a collection of original memoirs was presented to him, in commemoration of the twenty-fifth anniversary of his professorship, by men holding distinguished positions in science, who had worked with him at Marburg, Zürich, Vienna, or Leipzig. A second *Festgabe* of the same kind was offered to him in 1886, on his seventieth birthday. He also received a similar honour from his university, on the fiftieth anniversary of his graduation in medicine. In 1884, Ludwig visited England, to receive the Copley Medal, and was warmly welcomed by his physiological friends, many of whom had been his pupils. As evidence of the influence which his teaching exercised in this country, it may be mentioned that five of the contributors to the *Festgabe* of 1886 (Brunton, Cash, Gaskell, Schäfer, Wooldridge) were English physiologists.

Among the many lines of investigation of fundamental importance which Ludwig initiated, some of the most remarkable depended on the discovery of new methods. Just as the microscope had opened to the anatomist unexplored fields of research, by bringing him into closer relations with objects which were before beyond his scrutiny, so the recent rapid progress of physics and chemistry had placed more exact modes of observation and of measurement within reach of the physiologist. But the application of these methods was

attended with great difficulty; there were no physiological laboratories, no instruments, no capable mechanicians to whom the physiologist could apply for assistance. Under these conditions, ingenuity and resource were indispensable to success, and in these qualities Ludwig, besides being conversant with the physical science of the time, was pre-eminent. Accordingly, we find that two of the most important of his early investigations were as much due to his ingenuity as an inventor, as to his clear grasp of the physiological questions which his inventions were intended to elucidate. The most interesting of these inventions in its bearing on Ludwig's scientific career was that of the kymograph (or, as he called it, the kymographion); for the knowledge gained by it was of so fundamental a character, that it in great measure took the place of all that was before known as to the relations of pressure to flow in the circulation. This instrument, of which the use is to record the oscillations of a mercurial manometer on a surface moving horizontally at a uniform rate, was first described in a paper published in Müller's 'Archiv,' in 1847,* "On the Influence of the Respiratory Movements on the Blood-stream in the Aortic System." Poiseuille had some ten years before used a similar manometer to measure the lateral pressure exerted against the internal surface of the arterial system by the blood contained in it. This pressure Ludwig desired to register automatically, and by so doing to obtain a permanent record of the effects which are produced on the action of the heart and on the circulation by muscular movements. He subsequently proceeded to examine in the same way those variations in pressure and flow which depend on the incessantly varying functional activity of organs, or on the direct action of the nervous system. The general result of these early researches was to pave the way for those later experimental investigations by which we have learned that the blood-stream is so controlled by the nervous system, that the afflux of blood to each part is at every moment adapted to its requirement. The discovery of the kymograph led to much wider applications of the method of automatic inscription. Ludwig himself applied it, as we have seen, to the recording of the movements of respiration, as well as of the variations in arterial pressure. Subsequently, as elaborated by Chauveau and Marey, it became known as the "graphic method," and it serves not only for the investigation of animal movements of every conceivable kind, but even of the transient and delicate electrical changes which are associated with vital action.

Space does not allow me to do more than mention other contrivances of Ludwig for the study of the circulation, such, for

* The original tracing of the first experiment with the kymograph is in the Physiological Laboratory at Turin, having been given by Ludwig to Professor Mosso, one of his most distinguished pupils.

example, as the stream-gauge, for recording the flow in a blood-vessel, or the methods used by him for maintaining artificial circulation in a separated part or organ, so as to observe its function independent of the organism to which it has belonged; all of which have yielded information of fundamental importance.

Another instrument invented by Ludwig, the value of which to physiology has been nearly as great as that of the kymograph, is the mercurial blood-pump, the purpose of which is to separate from a known quantity of blood derived directly from the circulation, the mixture of gases which it yields to the vacuum. It is sometimes stated that Ludwig derived this instrument from Professor Lothar Meyer, who was, at the time he entered on his investigation of the gases of blood, engaged in researches on the same subject in Bunsen's laboratory, and employed an apparatus for the extraction of the blood gases in which the vacuum was produced by the condensation of steam. To Meyer* belongs the credit of having first succeeded (1857) in extracting the blood gases, and of having made the first reliable analyses of them; but it was Ludwig† who, in 1859, invented the first mercurial pump, and described it in a paper published under the name of his pupil Setschenow. After that time the pump underwent various modifications of form but not of principle, in the progress of the extended series of researches on the gases of the blood which Ludwig commenced at Vienna, and carried out at Leipzig; so that the final form—that employed in the Leipzig laboratory by A. Schmidt in his research on the carbon dioxide of the coloured blood corpuscles and usually described as Ludwig's—although the same in principle, differs considerably in aspect from the original one.

The importance of the discovery of an exact method of investigating the gases of the blood could be best judged of if it were possible to submit to the reader a summary of the researches conducted by means of it during the last thirty-five years. Of the enormous amount of work which has thus been done, Ludwig and his pupils have contributed a very large proportion. Of the earlier experiments carried out in the Vienna laboratory, Ludwig himself published a general account in 1865. After his removal to Leipzig, the subject developed in so many directions, that it is impossible to do more than mention certain series of observations such as those on the gases of lymph, on the gaseous exchange of living muscle, on the presence of oxidisable material in the blood and its significance, &c. These and other researches relating to the physiology of the respiratory exchange, which were conducted under Ludwig's direction,

* Meyer, "Die Gase des Blutes," 'Zeitsch. f. rat. Med.,' N.F., vol. 8, p. 277. 1857.

† Setschenow, "Beiträge zur Pneumatologie des Blutes." 'Wiener Ber.,' vol. 36, p. 304, 1859.

illustrate in the most striking way that tenacity of purpose with which he continued, to the very end of his life, the work he had begun thirty-five years before. Not one of the numerous series of investigations relating either to the internal or pulmonary exchange which were undertaken during that period, failed in adding materially to knowledge; yet at the end of it the work was still going on. Thus in the 'Arbeiten' of the Leipzig laboratory for 1894, we find an important research on the respiratory exchange in the abdominal viscera as estimated by temporarily shunting them out of the circulation, and another on the influence of grape sugar on the gases of the blood—both characteristic examples of his method.*

It will be seen from the preceding paragraph that Ludwig's discoveries relating to circulation and respiration, depended largely on his command of physical methods of research. In his equally extensive and important investigation of the conditions which regulate the functions of secreting glands, we have, along with additional evidence of his unrivalled skill as an experimenter, no less striking proofs that he was also a consummate anatomist. The physiology of secretion occupied his attention throughout his whole life. In 1842 it was the subject of the academical dissertation which he presented to the University of Marburg on entering on his career as a teacher; and in 1889 we find him working with Novi at a branch of the same question that he had discussed in his youth. It is forty-five years since he undertook to investigate the fundamental question whether, when a gland is thrown by the stimulation of its nerves into a state of activity while at the same time its supply of blood is increased, the first effect is dependent on the second. An experiment, remarkable for its extreme simplicity, settled the question. Ludwig showed that in the head of a rabbit just severed from the body, stimulation of the nerves of the salivary glands still determined secretion, notwithstanding that there was, of course, no circulation. Later, he proved, by an experiment on the same gland, that the process of secretion, previously supposed to be one of "vital chemistry," could be so transformed experimentally as to do external mechanical work, and he thus established, between work-producing and secreting structures, a relation which had not before been thought of.

It has been mentioned that the earliest of Ludwig's published researches was on the physiology of the kidney. His prolonged investigation of this subject illustrates, perhaps better than any other, the scientific attitude of his mind. The adaptation of structure to function is displayed in this organ in so striking a way, that from the moment that its minute anatomy was successfully made out by Bowman (1842), the teleological explanation which he gave was at

* These two papers appear under the names of Professor Tangl and Dr. V. Harley respectively.

once accepted as adequate. To Bowman these evidences of design afforded complete satisfaction, but to Ludwig they suggested rather a bundle of unsolved problems than an explanation. To him the first step seemed to be to compare the discharge of the renal secretion from the blood, with the filtration of liquid through a non-living membrane of a similar character, in other words, to investigate the then little-known process of liquid diffusion. It was by applying the results of this preliminary physical inquiry to the kidney that he was led to make the experiments which will ever remain the foundation of knowledge on the subject to which they relate, as proving that the two processes—the physical and the organic—are affected in the same way by difference of pressure between the two sides of the membrane. It was not asserted by him that these were the only conditions, or even the most important. Consequently, when Heidenhain, many years after, proved experimentally that the separation of the organic constituents of the urine from the blood was the work of living cells, actuated rather by chemical than physical conditions, Ludwig was able to regard the evidence not as a contradiction of his own results, but as an addition to them. Having promulgated no theory, but merely summed up the relations which his own experiments had proved to exist, he had nothing to withdraw from, nothing to object to. The strongest evidence of all against what was supposed to be his theory of renal secretion was, indeed, obtained by the aid of an experimental method already referred to, which was *par excellence* his own—that of observing the function of an organ in which the circulation is maintained artificially after it has been separated from the body of the animal to which it belongs. For by the application of this method it was more conclusively shown than by any other, not merely that the secreting cells are the indispensable agents in the separation of the organic constituents of the urine from the blood, but that a sufficient supply of oxygen, as well as the presence of urogenetic substances in the circulating blood, are conditions essential to their activity.

Before I attempt to give a general account of Ludwig's character as a teacher and as a man of science, I must say a word or two about his academical lectures, and about the only book, so far as I know, that he ever published—the famous 'Lehrbuch.' The interest of the lectures—often of a couple of hours' duration—consisted (as I have learned from a distinguished pupil of Ludwig) in the fact that, whatever the subject, they were throughout illustrated by the results of work originated by Ludwig himself, and done, often recently, in the laboratory, so that everyone felt, "this is not hearsay; the evidence is here on the spot." It was this that made them of such value to the mature student.

As regards the 'Lehrbuch,' it is necessary to carry oneself back to

the fifties, in order to appreciate its extraordinary influence, for it was in 1852 that the first volume appeared. To those whose conceptions were derived from the current teaching of that time, it seemed destructive. It made no mention of "vital forces;" it did not even give a definition of life or of organisation, but defined physiology as "the determination of the activities (*Leistungen*) of the living body, with reference to the elementary conditions on which they are dependent;" the writer's aim being to resolve these conditions into their simplest elements, and so bring them within the scope of chemical and physical laws. Accordingly, he began his treatise with the discussion of the elementary endowments of the structure or framework of the living body, and of the processes in which these endowments manifest themselves; seeking to arrive at a clear conception of each vital function by regarding it as a complex of constituent processes severally definable in chemical or physical terms. The book set forth no "biological principles," and therefore had no interest for the general reader. It was for students and for students only, but for them it was a revelation, a forecasting of the physiology of the future for those who were about to make it. The second edition was published in 1862, but had no successor.

It may be noted here that Ludwig, like Darwin, avoided controversy. When, in 1875, certain important investigations which had been conducted in the Leipzig laboratory were made the subject of inconsiderate criticism, he persistently resisted the importunity of friends, who pressed him to reply, saying that he preferred to leave the future to judge between him and his distinguished opponent.

In the preceding pages I have endeavoured to give the reader an idea of the chief directions which Ludwig's activity followed. No one who is acquainted with the modern development of the science of physiology can fail to see that the history of Ludwig's life work was in truth that of the science which he did so much to create; for its development from small beginnings during the second half of the century has coincided in a remarkable way with his career as a discoverer. In certain lines of investigation, as, for instance, those which relate to the localisation of the higher functions of the nervous system, Ludwig took little part, but in every other direction his researches have been of fundamental importance. All the great functions of the organism, whether mechanical or chemical, have been investigated by him collectively and separately—as processes by themselves, in their relation to the whole organism, and as regulated by the nervous system.

That one man, however endowed, should have been able even in so long a period of activity to accomplish so great a work can only be explained by referring to the conditions of the earlier years of his academical life. At Marburg he was engaged in two lines of work

which we are not now apt to associate together. He was a physicist on the one hand, and an anatomist on the other. At the very time that he was engaged on his first great discoveries as to the mechanism of the circulation, it was his official duty, as prosector, to teach medical students in the dissecting room. Being no less a master of the new methods which the invention and introduction of the microscope had rendered available, than he was of descriptive anatomy, he was able to approach his great subject—the animal machine and its working—from the physical and anatomical sides at the same time. That in his lifework the former was the more emphasised, arose from the circumstance that the advance of physical science had rendered so much possible, that before could not be attempted. There were, however, other circumstances which led him in the same direction; such, for example, as his intercourse with Bunsen—until 1851 Professor of Chemistry and Physics at Marburg—a man who could not fail to exercise a powerful influence on such a mind as Ludwig's. To him there is reason to believe that he was indebted for that technical knowledge of gas-analysis which in later years was to bear such magnificent fruit. Still greater importance must, I think, be attached to an incident which occurred in 1847, when, in the course of a visit to Berlin, he made acquaintance for the first time with his three life-friends—Helmholtz, Brücke, and du Bois-Reymond. For it was not Ludwig alone that created modern physiology. It was rather the four friends (in mentioning whom the name of another contemporary—that of Donders—though he was not one of them, must not be forgotten), all of whom, as we know from their writings, were of one mind in determining so far as they were concerned, to throw speculation overboard, and to pursue the methods of the exact sciences in their place. That four such men should be brought together at the very turning point of their several careers, was, indeed, a memorable event in the history of physiology. The time was come for its reunion with the exact sciences, and these were the men who were to effect it. The change would, we may be sure, have eventually been brought about, had these leaders of thought and work not been to the front, but it cannot be doubted that its progress would have been retarded.

The wide range of Ludwig's researches made them more fruitful for the advance of physiological knowledge than those of any of his contemporaries. For although some of the discoveries made, *e.g.*, by Helmholtz* and du Bois-Reymond may have surpassed any of his in brilliancy, neither of these distinguished men could claim to be master of so many subjects in physiology. No one, during so long a period of activity, held in his hands so many threads of investigation without losing hold of any of them or allowing them to interfere with each

* In the comparison here made between Helmholtz and Ludwig, reference is made exclusively to the physiological work done by the former.

other. It was this perfect grasp of the whole subject—this familiarity with the infinite complexities of structure and function in the animal organism, and with all possible methods of exploring them, that made Ludwig *unsurpassable* as a teacher. The motive which attracted men to him was the assurance that they would receive the best guidance in whatever line of physiological inquiry he advised them to pursue, and that they would, if genuine students, be welcome not only to his laboratory, but to his sympathy and friendship. Ludwig in the midst of his students seemed to be happier than anywhere else, for here two joys were experienced together—the joy of satisfying that thirst for discovery which was as insatiable in the last years of his life as it had been in his best days, and that of communicating his own enthusiasm to younger men and engaging their energies in new work.

The Leipzig Institute was an observatory, the work of which was carried out in a way which such a director as Ludwig can alone make successful. For every worker he found a problem, reserving the more intricate ones for those who were possessed of the requisite training and insight, but at the same time finding simpler, but not useless, occupation for those who, with less gifts, had patience and industry. In the publication of the final results of the researches which he initiated, he gave, as is well known, more than full credit to his junior coadjutors; so much so that even the most important papers in the well-known 'Arbeiten' appear under the names of the students who carried out the work under his direction.

In can readily be understood that Ludwig's relation to his young friends, as he called them, was of an exceptional kind. For he was felt to be as superior to other men in respect of his moral nature as he surpassed them in genius. The same sincerity and earnestness which actuated him in his laboratory work were also the motives of his ordinary life. Thus it happened that those who worked with him, however grateful they might be for the advantages they enjoyed and the instruction they received, esteemed it to be an even greater privilege to come into personal relation with one who possessed so noble, so loving, and so lovable a nature.

J. B. S.

CHARLES CARDALE BABINGTON, was born at Ludlow, on the 23rd of November, 1808. His father, who was originally a member of the medical profession, afterwards becoming a clergyman of the Church of England, took considerable interest in botany. Whilst his son was still a schoolboy he retired from work and settled at Bath. The subject of our present memoir entered St. John's College, Cambridge, at the age of eighteen, took his B.A. degree four years later, and his M.A. at the age of twenty-five. At first his inclinations were towards

entomology, but he attended Henslow's lectures on botany, and like so many others, fell under his magnetic influence. He joined the Linnean Society in 1830, and for a short time after the death of the Rev. L. Jenyns, was its oldest Fellow. His first botanical book was the 'Flora Bathoniensis,' published in 1834. He visited Ireland in company with the late Mr. John Ball, in 1835, and gave an account of his tour in the 9th volume of the 'Magazine of Natural History.' His 'Primitiæ Floræ Sarnicæ' was the result of excursions taken during two long vacations, in one of which the Rev. W. W. Newboald was his companion, and was published in 1839. In company with Professor J. H. Balfour, he visited the Outer Hebrides, in 1841, and reported on their scanty vegetation. His work was almost entirely confined to British Botany, but he published in the 18th volume of the 'Transactions of the Linnean Society,' a monograph of the Indian Polygonums, and in the 11th volume of the 'Journal of the Linnean Society,' a paper on the "Flora of Iceland," giving a complete list of the Phanerogamia of the island, which he had visited during the year 1841. The first edition of his *magnum opus*, the 'Manual of British Botany,' appeared in 1843. This work ran through eight editions during his life-time and was for fifty years almost universally used as a hand-book and standard of nomenclature by local botanists who made a study of critical British plants. The special feature of the work was a careful study of the difficult genera by means of the books and fasciculi of dried specimens published by the critical botanists of the neighbouring continental countries. In the early editions he relied mainly upon Koch, Fries, and Reichenbach, and in the later to these were added the writings of Grenier, Godron, Boreau, Jordan, and Lange, and the Exsiccata of Reichenbach, F. Schultz, and Billot. This book brought him into frequent communication with nearly all the active collectors in different parts of Britain, and entailed upon him a mass of correspondence as referee, which occupied a large proportion of his time. The writer of the present notice remembers with feelings of gratitude the kind and patient way in which the Professor helped him in his difficulties when, between forty and fifty years ago he was beginning the study of British botany, and was living in a small country town where there were no herbaria or books of reference. Professor Babington generally spent his long holidays in exploring some rich botanical district at home, such as the Snowdon country, Braemar, and Teesdale, and in this way made acquaintance in a living state with most of the plants with which he had to deal. Amongst the genera and subgenera that he revised may be mentioned Atriplex, Arctium, Fumaria, Batrachium, Cerastium, Dryas, Armeria, Saxifraga, Hieracium, Potamogeton, and especially Rubus. He contributed about 150 papers, mainly on critical British plants, to different periodicals and societies.

In 1846 he published the first edition of his 'Synopsis of British Rubi,' and a much-enlarged second edition in 1869. This was intended to have been illustrated by a series of plates drawn by Mr. J. W. Salter, but the preparation of these was stopped by Salter's death, and they were not published.

In the "Ray Society," which was founded in 1844, as an enlargement of a "Ray Club," which was started in 1836, he took an active interest, serving on its council and helping in the editing of some of its publications, especially the volumes devoted to the memoirs and correspondence of the great naturalist from whom the Society took its name.

He was elected a Fellow of the Royal Society in 1851, the same year in which Huxley, Sir James Paget, and Lord Kelvin were also elected, but he never took any active part in the affairs of the Society.

In 1860 he published his 'Flora of Cambridgeshire,' in which the distribution of the species through the different districts of the county is traced out very carefully, and the changes in the vegetation caused by the drainage of the fens are dwelt on.

On the death of Professor Henslow in 1861, Babington succeeded him as Professor of Botany at Cambridge, and held the Chair up to the time of his death, on the 22nd of July, 1895. His lectures dealt mainly with organography and systematic botany, and were not accompanied by laboratory work. They were discontinued for several years before his death, and as years went on the teaching of botany in the University passed into the hands of the men of a younger generation, with different ideals and different plans of work.

A very full biography, with a portrait, will be found in the September number of the 'Journal of Botany.'

J. G. B.

John Syer Bristowe was born June 19, 1827, at Camberwell, where his father was a general practitioner. He was educated at King's College School, and subsequently entered St. Thomas's Hospital as a medical student. He showed, however, a predilection for art and literature, for at an early age he published a book of poems, and his drawing showed considerable power. He nevertheless worked hard in his destined career, and obtained prizes both in his own school and at the University of London, where he took his M.B. degree in the year 1850, and the M.D. in 1852. He was occupied with various minor positions in the medical school until he was appointed Assistant Physician in 1854, and full Physician in 1860. He became Fellow of the Royal College of Physicians in 1858, and a Fellow of the Royal Society in 1881. The honorary title of LL.D. was also conferred upon him by the University of Edinburgh. He devoted much of the

early part of his medical life to the study of pathology, in which he became most proficient. The greater part of his work was published in the 'Transactions of the Pathological Society.' His communications are enriched by illustrations; the drawing being the work of his own hand, and some of the microscopic plates are exceedingly beautiful. All his communications were of great value, as they always added considerably to the knowledge of the subject then current. Although he made no great original discovery by which his name could be attached to a particular disease, yet he did much more than many that have thus become immortalised by enriching a large number of subjects by his skilful hand and clear understanding.

In 1876 a London publisher, seeing there was much need of a work on medicine, applied to Dr. Bristowe as the man most capable of undertaking a work involving a knowledge of the whole range of subjects required. He fulfilled his task, and produced a large volume on the 'Theory and Practice of Medicine.' This was at once received by the profession as a treatise of the highest excellence, and became a text-book for students. So conscientious was the author that, in order to make the work entirely his own, and as practical as possible, he became for some time a diligent student at Bethlem. He was thus enabled to write his chapter on insanity from personal knowledge, and produced an essay on the subject of the first order. It may be added that Dr. Bristowe was especially inclined towards the study of nervous diseases, and consequently his chapters on these affections are amongst the best in his treatise, and equal to any that can be found in ordinary English works. Every subject which he took up he studied thoroughly, so that had he wished he might have become a specialist in many departments, as for example that of skin diseases, of which he had a remarkable knowledge.

He delivered lectures at the College of Physicians and at various societies, and he did excellent work for the Privy Council Office, Sir John Simon having the highest opinion of his scientific power. We may mention amongst the subjects which he investigated, that of phosphorus poisoning, cattle plague, and the sanitary state of English hospitals; the report on the latter subject, made in conjunction with Mr. Holmes, becoming a standard of reference. He was indeed a great authority on sanitary matters, having been one of the first medical officers of health appointed in London; an office which he retained until his death, forty years afterwards.

Amongst a great number of collateral subjects which interested him was that of speech, and he embodied his views in some admirable papers on the mechanism of articulate speech. In one of these he clearly showed that the Welsh Ll is not to be pronounced by the

combination of any English letters, but is a distinct elementary Welsh sound.

Dr. Bristowe has been President of many societies, and was, at the time of his death, a member of the Vaccination Commission. On leaving St. Thomas's at the expiration of his term of office money was subscribed to institute a prize medal to his memory, and a portrait, painted by his daughter, was hung in the committee room. It may be here stated that Dr. Bristowe left behind him several sons and daughters, all of whom are of exceptional ability. During the Chartist riots he was sworn in as special constable, and he might then have been seen in the night patrolling before his father's house, baton in hand.

Dr. Bristowe may be said to have been not only artistic in his nature but endowed likewise with the true scientific spirit; so that everything he did was sound and accurate, and contained a solid addition to existing knowledge. He not only had a profound acquaintance with disease as presented by the living subject, but an equally profound knowledge of pathology, so that he had no rivals in this subject, nor was there a better clinician. He was of amiable and kindly disposition, and in every respect a good and an honest man; although possessed of superior knowledge, he was too modest to assert himself, still less put forth his opinions as positive or final; he felt many problems in medicine were too difficult for solution, and therefore he did not make the same impression on his students as more confident and dogmatic men often do; but the older and more advanced students thoroughly appreciated his excellence. For the same reason he never had a large private practice, for here positiveness and assertion have considerable weight. It can be well understood that a man of this character could not give credence to the efficacy of the numberless drugs which are every day prescribed, and that he, consequently, was called a sceptic in reference to therapeutic treatment. His scientific mind tended rather to discover the causes of diseases and remove them than administer a doubtful remedy. He sought for nothing but truth, and therefore stated his opinions fearlessly; it happened in consequence that during his lectures at the College of Physicians, he drew from the President remarks which almost amounted to a reproof for the manner in which he spoke of medicines generally. He was much endeared to his friends owing to the honesty, purity, and simplicity of his character. With his artistic nature and intellectual endowments he combined the highest moral qualities. He was thus an ornament to the medical profession, which has gained in strength in every way by his membership.

S. W.

Sir John Tomes was born in 1815, and died at Caterham, where he had for many years resided, on July 29 of this year.

Although his father was a man of high intellectual attainments, and was deeply versed in the metaphysical literature of his day, neither he nor any other member of the family had ever shown any bent in the direction of biological or medical science. Nevertheless, Sir John in his boyhood acquired a good knowledge of field natural history, to which he was partly led by being a most enthusiastic sportsman, a taste which lasted the whole of his life, and he early decided to take up the profession of medicine.

With this in view, he was apprenticed to a hard-riding, hard-drinking country practitioner of the old school, and after acquiring a rough and ready acquaintance with the practice of that period, he came to London and entered at the Middlesex and King's College, in 1836, where he became acquainted with Todd, Bowman, Kiernan, Quekett, Carpenter, Edward Forbes, and with almost all of the band of workers in histology who were then rising into notice.

In a fragmentary diary which he then kept he is found spending his evenings with one or other of these friends, and, whilst living upon scanty means, purchasing a microscope of Powell (afterwards Powell and Lealand).

Owing to the scarcity of competent men, it happened that he spent two years as house surgeon at the Middlesex Hospital, and during this period he fed a nest of young sparrows and a sucking-pig upon madder; the sections of their bones still exist, and are as good as any sections of hard tissues that have been since made with all the improved appliances of modern times.

Whilst house surgeon he had a great many teeth to extract, and finding the instruments then in vogue clumsy and ill-adapted to the purpose, he invented the forceps now in universal use the world over. His marked mechanical ability led the late Sir Thomas Watson and the late Mr. Arnott to advise him to adopt dental surgery as his profession, and in doing so his attention became especially directed to the histology of teeth and bone, with which thenceforward his scientific researches were almost exclusively concerned.

In 1849 he contributed a paper upon the structure of the dental tissues of the Marsupialia, and in the following year one upon the dental tissues of Rodentia, which were published in the 'Phil. Trans.' In 1852 he was elected a Fellow of the Royal Society, and in the same year communicated an important paper, written in conjunction with his intimate friend the late Campbell de Morgan, upon the structure and development of bone. In 1856 he discovered the existence of the soft fibrils in dentine, which have since been known as Tomes' fibrils; this paper also appeared in the 'Phil. Trans.'

Besides these papers, he made numerous communications to the

Microscopical Society, of which he was one of the earliest members, and to other periodicals. His work has stood the test of time, and, like that of his friend Bowman, remains to this day in all essentials unshaken. It is very remarkable that the work of these early investigators, working with instruments that nowadays would be thought very imperfect, and with the methods of histological research all in their infancy, were so accurate and sound as time has proved them to have been.

Although this is not the place in which to speak at any length of his work in the field of dental surgery, yet no notice of his life would be at all complete without some mention of that to which his energies in middle and later life were so largely devoted. Having, by his lectures on dental surgery and physiology, inaugurated the scientific teaching of the subject, he worked on with unfailing tact, singleness of purpose, and pertinacity, to improve the education of, and to get recognition for, dental surgeons.

It was to his efforts, far more than to those of any other person, that the institution of an examination and the conferring of a diploma by the Royal College of Surgeons was due; and this effected, the subsequent passage of a Bill regulating the practice of dental surgery through the House of Commons was his next aim. This, largely owing to Sir John Lubbock's warm espousal of the cause, was successfully done in 1878. His efforts won the fullest recognition from the members of his own profession, with whom his influence was almost unbounded, and in 1883 the Royal College of Surgeons exercised their right of conferring the Honorary Fellowship of the College by electing the late Professor Huxley and Mr. Tomes. In 1886 he was offered the honour of knighthood, which, though caring little for such distinction personally, he deemed it his duty to accept as an honour to his profession.

His methods, whether in scientific or public matters, were never controversial, and he was one of those few people able to fully retain the friendship and liking of those from whom he might have occasion to differ most strongly. Through never having enjoyed robust health, he was little seen in social gatherings; nevertheless, he lived to see the accomplishment of almost all that he had worked for, and passed away in the fulness of years and the enjoyment of all his faculties.

C. S. T.

Henry Austin Bruce, Lord Aberdare, whose death occurred on February 25, 1895, was elected a Fellow of the Royal Society on becoming a Privy Councillor in 1864.

He was born on April 16, 1815; and was called to the Bar in 1837. In 1846 he was appointed Stipendiary Magistrate at Merthyr, which

post he resigned in order to enter Parliament as member for that borough in 1852. He held this seat for sixteen years. At the General Election of 1868 he was defeated; but soon afterwards he was offered a seat in Renfrewshire, which he held for five years.

In 1862 Lord Palmerston selected Mr. Bruce for the post of Under-Secretary of State for the Home Department, and in 1864 he became Vice-President of the Committee of Council of Education.

On the formation of Mr. Gladstone's Government in 1868, Mr. Bruce became Secretary of State for the Home Department, and, in 1873, Lord President of the Council, being at the same time raised to the Peerage.

After the fall of Mr. Gladstone's Government he ceased to take an active part in political life, and devoted himself chiefly to social and educational questions, in which he had always shown a deep interest. In 1875 he presided at the meeting of the Social Science Congress at Brighton, and chose for the subject of his address, "Crime and its Remedies."

In 1859 he became a Fellow of the Geographical Society, and, in 1880, was elected President of that Society, an office which he held—with the exception of one year—till 1887.

Lord Aberdare was President of the University Colleges of Aberystwyth and Cardiff, and took an active part in the foundation of the University of Wales, of which he was nominated the first Chancellor.

The last important work in which he engaged was the Royal Commission on the Aged Poor, of which he was Chairman. On this inquiry, and the conclusions to be drawn from it, he bestowed from first to last the same keen interest and ungrudging labour which he had always given to practical questions affecting the welfare of mankind; but he did not live to sign the Report. The charm of his character and conversation will live long in the memory of those who knew him.

A. V. H.

George Edward Dobson was born September 4, 1848, in county Longford, Ireland, being the eldest son of Mr. Parke Dobson, of Killinagh, county Westmeath. He was educated at the Royal School of Enniskillen and at Trinity College, Dublin, where he graduated B.A. in 1866, M.B. and M.Ch. in 1867, and M.A. in 1875. He was first Senior Moderator and first Gold Medallist in Experimental and Natural Science, Classical Honourman and Stearnes Exhibitioner, and Member of the Senate of Dublin University. He entered the Army Medical Service in 1868, and retired in 1888, with the rank of Surgeon-Major. His "Essay on the Diagnosis and Pathology of the Injuries and Diseases of the Shoulder-joint," was awarded the Gold

Medal of the Dublin Pathological Society, in 1867. He also wrote 'Medical Hints to Travellers,' published by the Royal Geograpical Society, which reached a sixth edition in 1889. But the work by which he will be best remembered in the annals of science, and which formed the main occupation of twenty years of his life (from 1871 to 1891) was his laborious and painstaking investigation into the structure and classification of two groups of mammals, the Chiroptera and the Insectivora, on both of which he became the recognised authority of his time. While stationed in India in his professional capacity, he availed himself of every opportunity to study the bats of that region. His first published paper on them was entitled "On four new species of Malayan Bats from the collection of Dr. Stoliczka," which appeared in the 'Proceedings of the Asiatic Society of Bengal,' for 1871. This was followed by numerous memoirs upon various members of the group in the same journal, and in the 'Proceedings of the Zoological Society,' and the 'Annals and Magazine of Natural History.' In 1876 the trustees of the Indian Museum, brought out his systematic and copiously illustrated work, a 'Monograph of the Asiatic Chiroptera.' This led, on his return to England, to his engagement by the trustees of the British Museum to undertake a still more important work called a 'Catalogue of the Chiroptera in the Collection of the British Museum.' This is in reality a complete monograph of the order as far as the materials then available permitted, and still remains the standard work on the anatomy, classification, and nomenclature of bats, although the 400 species described in it have been considerably added to by subsequent investigators. Dr. Dobson's appointment to the Royal Victoria Hospital, Netley, where he was placed in charge of the museum, gave him further facilitities for pursuing his zoological studies, for which he frequently expressed his obligations to the then Director-General of the Army Medical Department, Sir William Muir. About this time he began to extend his range of observation to other groups of mammals, and took up some interesting investigations into muscular anatomy, which resulted in an important paper "On the Homologies of the Long Flexor Muscles of the Feet of Mammalia, with remarks on the value of their leading modifications in classification," published in the 'Journal of Anatomy and Physiology,' 1883.

Having in the British Museum Catalogue exhausted all the material then available for working at the Chiroptera, he undertook another and allied order, the Insectivora, and was doing excellent service in elucidating the most interesting points in the structure of its members, and unravelling the difficulties of their synomymy. He wrote several valuable articles in the ninth edition of the 'Encyclopædia Britannica,' which were afterwards incorporated in Flower

and Lydekker's 'Introduction to the Study of Mammals (1891), but he mainly concentrated himself upon a work which, if completed, would have ranked as one of the most thorough and exhaustive treatises on any group of mammals in existence. The publication of his 'Monograph of the Insectivora, Systematic and Anatomical,' commenced in 1882, was unhappily interrupted before it was quite finished by a slow, insidious, and ultimately fatal illness, which compelled him not only to give up his position as an army surgeon, but also to abandon all scientific work, and to live for several years in complete retirement, gradually failing in strength until death intervened on November 26, 1895. Thus was lost to science one who, in his too short career, had proved himself an industrious, careful, and conscientious worker, and whose upright and amiable character had won for him the sincere esteem of all who were brought into personal contact with him. He was elected a Fellow of the Royal Society in 1883, and was a corresponding member of the Academy of Natural Sciences of Philadelphia and of the Biological Society of Washington.

W. H. F.

HERMANN VON HELMHOLTZ was born in Potsdam, on August 31, 1821. His father was a Professor of Literature in the Gymnasium of that town. His mother, whose maiden name was Caroline Penne, was of English descent, and was connected with the founder of Pennsylvania. For the first seven years of his life he was delicate, but during these early troubles, and throughout his boyhood, his parents tended him with the most unremitting attention and affection.

In the well known 'Autobiographical Sketch,' contained in an address delivered in 1891, on the occasion of the attainment of his seventieth year, an interesting account of his mental development was given by von Helmholtz himself.

He went to school at the Gymnasium in which his father taught, and, as he himself thought, found unusual difficulty in mastering and remembering "disconnected things," such as "irregular grammatical forms, and peculiar terms of expression." "To learn prose by heart was martyrdom." Whether this defect was or was not as great as von Helmholtz supposed, he appears to have had an unusually good memory for things which he cared about, and to have had the gift of caring chiefly for the best. He could repeat whole "books of the Odyssey, a considerable number of the odes of Horace, and large stores of German poetry."

In the study of science, "the laws of phenomena" afforded the conecting link which his memory needed. He gained his earliest knowledge of geometry by playing with wooden blocks, but the first fragments of physics which he learned in the Gymnasium exerted

upon him an attraction greater than that of pure geometry or algebra. By the time that the lad was of age to go to the University his interest in physics had developed into "an absorbing impulse, amounting even to a passion."

As, however, a livelihood could not be gained from a knowledge of that branch of science, his father advised him to undertake the study of medicine, a sensible proposal, to which the younger man assented without much difficulty. He therefore entered the Friedrich-Wilhelm Institute, a military medical school, in which the poorer students were materially aided while preparing for their future profession. He was thus led to take an interest in physiology, and was fortunate in that he came under the immediate influence of Johannes Müller. Among his fellow students were E. du Bois-Reymond, E. Brücke, C. Ludwig, and Virchow.

His career as a student and teacher, brilliant as it was, may be dismissed in a few words.

In 1848 he was permitted to withdraw from the Army Medical Service, and was appointed Teacher of Anatomy in the Academy of Arts, in Berlin. Only another year elapsed before he received the more important appointment of Professor of Pathology and Physiology in Königsberg. This was followed by the Professorship of Anatomy in Bonn (1855), of Physiology in Heidelberg (1858), and of Physics in Berlin (1871). During the last years of his life he held one of the highest posts to which any scientific man in Germany can aspire. He was Director of the new "Physikalisch-Technische Reichsanstalt" at Berlin.

Amid this unruffled professional success, Helmholtz published a long series of papers and books. It has been said that as each of seven cities contended for Homer, so seven sciences, mathematics, physics, chemistry, physiology, medicine, philosophy, and æsthetics claimed Helmholtz for their own,* and it is interesting to note how early he took the comprehensive view of science which justifies this rhetorical statement.

His first paper (apart from an inaugural dissertation) showed a clear appreciation of the necessity of distinguishing vital from non-vital phenomena. In this memoir, published in 1843, he proved the two negative propositions, that fermentation and decay are not merely chemical processes due to the oxygen of the air, and that they are not propagated by dissolved substances which can diffuse through a porous membrane. He concluded that they resembled vital phenomena "by the similarity of the materials in which they occur, by the mode in which they spread, and by the similarity of conditions which are essential for their preservation or for their destruction." It is evi-

* 'Gedächtnissrede auf Hermann von Helmholtz.' T. W. Engelmann. Leipzig, 1894.

dent that in 1843 the young doctor was on the track which Pasteur has since followed with such marvellous success.

But though the first work of Helmholtz was to prove that the intervention of life was essential to produce certain results which had been wrongly ascribed to direct chemical action, he was soon occupied with a group of converse propositions which show that the action of life itself is subject to the laws of mechanics, physics, and chemistry. The celebrated essay on the "Conservation of Force," or as we should now call it the "conservation of energy," was published in 1847. Helmholtz was led to the discussion of this subject by reflections of the nature of "vital force." He had convinced himself that if it were true that living organisms could restrain or liberate the action of chemical and physical forces, perpetual motion would be realised. This compelled him to ask what relations must exist between the various kinds of natural forces for perpetual motion to be possible, and then to enquire if these relations actually exist.

The argument, though addressed to physicists, was intended for the benefit of physiologists. The first part is devoted to a proof that if all natural forces are "central," *i.e.*, act towards fixed centres, and depend solely on the distances and masses of the mutually reacting bodies, the law of the conservation of energy must hold good. The latter part of the paper is a masterly discussion of the application of the law to the different branches of experimental science. It is unnecessary to trace these applications in detail, or to define precisely with respect to each of them the relative positions of Helmholtz and of his most distinguished contemporaries. The essay is worthy of such minute discussion, but its value is to be judged chiefly by the effect it produced. Though von Helmholtz was not an originator of the doctrine of the conservation of energy; though when his essay was written he knew little of Joule, and nothing of Mayer, he viewed the whole question from a standpoint far in advance of the majority of the scientific men of that day. His paper was refused admission to 'Poggendorff's Annalen,' and he has told us that Karl Jacobi was his only supporter among the older members of the Physical Society of Berlin. The result of a sharp struggle was entirely in favour of the young doctor. William Thomson, in England, and Helmholtz, in Germany, within a few weeks, compelled their respective countrymen to listen to, and before long to accept, the doctrine that energy, like matter, can neither be created nor destroyed, that though protean in form, it is unchangeable in quantity. The last application of this principle was characteristic of the author. The problem of "vital force" led him to the doctrine of the conservation of energy; the last of the questions which he discussed in the light of that doctrine was the conservation of energy in animals and plants

No scientific career, however, can be fruitful which is devoted exclusively to the cultivation of wide generalisations. The success of his essay did not tempt Helmholtz to stray into the easy path of speculation. Profoundly conscious of the importance of general principles, he nevertheless felt that the surest way to gain true knowledge of the relations between mechanics and life was to investigate the mechanism of living bodies in detail. He had already (1845) studied the chemical changes which accompany muscular activity by showing that both the aqueous and alcoholic extracts of the muscles of a frog's leg were different, according as it had, or had not, been stimulated by a long series of electrical discharges. To this was added, in 1847, a memoir on the production of heat during muscular action. It had been held that the increase of temperature in an irritated muscle is due to the more rapid supply of arterial blood. Experiments to test this view had been made on warm-blooded animals, but Helmholtz pointed out that less elevated temperature and persistent irritability were qualities which made the muscles of the frog more fitted to be the subjects of a crucial experiment. He succeeded in proving that the rise in temperature is due to work within the muscle itself, and that the amount of heat thus evolved is far greater than any which may be generated in the nerves through which the stimulation is produced.

From these investigations Helmholtz passed to another difficult problem namely that of the rate of propagation of nervous action. It had been thought that this was practically instantaneous, but in a paper published in 1850 he proved that if two different points of a motor nerve are equally stimulated, the interval of time that elapses before any given stage of the resulting muscular contraction is reached is greater as the point of stimulation is more distant from the muscle. The lag thus detected was very small, being less than two thousandths of a second on the short lengths of nerve employed, but the velocity of propagation, instead of being infinitely great, was found to be less than a tenth of the velocity of sound, the most probable value being 26·4 metre-seconds.

Various problems connected with this research continued to occupy the mind of Helmholtz for some time. In 1850 he discussed the "Methods of measuring very small intervals of time, and their application to physiological purposes." This was followed in 1851 by a memoir on the duration of induced electrical currents, and in 1852 by a further investigation, conducted with improved apparatus, into the velocity of the propagation of nervous action. The result was 27·25 metre-seconds, thus confirming the substantial accuracy of his earlier work.

Helmholtz had thus opened up an entirely new field of research, which alone would have been sufficient to absorb all the energies of a

smaller man. Nevertheless, while these investigations were progressing, he found time to invent the ophthalmoscope (1851), to begin his observations on compound colours (1852) and to write an elaborate essay on the nature of sense impressions (1852).

Thus, in 1852, at thirty-one years of age, Helmholtz had surveyed a large part of the wide field in which he was to achieve so much. His life work lay mapped out before him. He had already shown the extraordinary many-sidedness which was the most remarkable characteristic of his genius, and had entered upon most of the inquiries in the prosecution of which his fame was to be won. If an exception is to be made to this statement, it is that in 1852 Helmholtz had hardly given to the world a proof of the mathematical ability which was afterwards to solve the problem of vortex motion.

It would be impossible in a notice of this kind to enter into any detailed account of his investigations, or to discuss critically the arguments for and against theoretical views which have not in all instances commanded universal assent. The most that can be attempted is to give as briefly as possible a *catalogue raisonné* of his principal achievements.

The measurement of the velocity of propagation of nervous action was probably his most important work in the domain of experimental physiology. He returned to this subject, and about the year 1870 wrote several papers in conjunction with Herr Baxt on questions suggested by it. The original investigation was extended so as to include observations on the living human subject, the measurement of differences in the speed of transmission in different parts of the body, and the effects of temperature. In another memoir published in 1871, Helmholtz discussed experiments, conducted in the laboratory at Heidelberg by Herr Baxt, on the interval of time which elapses between the presentation of a visible object to the eye and the production of the sense-impression to which it gives rise.

In 1864 and 1866 he published two papers on the sound which a muscle emits when contracted. In the first of these he showed that if the muscle be stimulated by intermittent electrical shocks the sound given out corresponds in pitch to the frequency of the stimulations. In the second he determined the correct pitch of the sound given out by muscles, contracting naturally under the influence of volitional impulses, by communicating the vibrations of the muscle to springs or to paper strips. He found that these responded best when their natural period was from 1/18 to 1/20 of a second. This was important as showing that observers who had stated that the natural muscle sound corresponded to from thirty-six to forty vibrations per second had heard the octave of the true fundamental which was itself below the lower limit of audition.

The investigations of Helmholtz on physical and physiological

optics were closely intertwined. In his collected works twenty-four papers are included under these heads, but most of his conclusions are published in his 'Handbuch der Physiologischen Optik.' The first edition of this great work was completed in 1866. Another was passing through the press at the time of the author's death.

Adhering approximately to the chronological order of his inquiries into these subjects, the two first were devoted to the ophthalmoscope and to colour mixtures respectively.

The problem of how to see the interior of the eye did not involve any novel or recondite principles, but was practically extremely difficult. A hint as to the method of solution was obtained from the recorded fact that an observer, wearing spectacles, noticed that a friend's eye appeared to him to glitter when light was reflected into it from his own glasses. The account which Helmholtz gave of the invention of the instrument, to the construction of which this afforded the clue, is a model of the combination of mathematics and experiment to attain a practical result.

A modification of the original ophthalmoscope devised by Th. Ruete, in which the observer looks through a hole in the middle of a mirror, is that now used by oculists. Helmholtz published a paper on this form of the instrument, but though he regarded it as the best for medical purposes, he preferred the earlier type for research.

On the theory of mixed colours and the closely related problem of colour blindness, Helmholtz does not seem to have held with pertinacity the views generally attributed to him, to which, no doubt, he at one time inclined.

In his earliest experiments he combined the spectra produced by two slits inclined in opposite directions at 45° to the vertical. He concluded that at least five colours, viz., red, yellow, green, blue, and violet, were necessary if all the colours of the spectrum were to be matched by compound tints, but that if three only were to be selected the best choice was that of Thomas Young, viz., red, green, and violet. He also explained the reasons why the tints resulting from a mixture of coloured powders are different from those produced by mingling rays of homogeneous light. Later and more refined experiments led him to adopt unreservedly the theory of three fundamental colours.

He rejected Brewster's view that three kinds of light—red, green, and blue, co-exist in each homogeneous ray. The explanation of compound colours as due to the mixture of three fundamental tints could only be understood as applied to subjective phenomena not as describing external physical facts. It is the reduction of colour sensations to a mixture of three fundamental sensations, not the analysis of light waves into three fundamentally different types of vibration.

"In this sense," he said, "Thomas Young solved the problem cor-

rectly, and in fact his assumption gives an extraordinarily simple and clear view and explanation of all the phenomena of the physiological theory of colours" ('Phys. Opt.,' 1867, p. 291). So much was Young in advance of his age that his theory was hardly noticed, "bis ich selbst und Maxwell wieder auf sie aufmerksam machten" ('Phys. Opt.,' 1867, p. 307).

From this standpoint von Helmholtz did not recede; but in his later years he was impressed with the difficulties which attend the explanation of colour blindness on the hypothesis that in such cases one of the three fundamental sensations is weakened or absent.

In the twentieth appendix to the first edition of the 'Physiologische Optik' he proved that he did not even at that date (1867) consider this to be the only possible explanation, but that the phenomena of colour blindness might be due to the fact that the forms of the intensity curves of the three fundamental colours were abnormal, rather than to an incapacity to appreciate one of the fundamental sensations.

It is unnecessary to discuss here the modifications of his earlier views of which there are proofs in the latest edition of the great work on optics. In January, 1893, a very interesting comparison of the opinions expressed in the two editions was contributed to the 'Philosophical Magazine' by Dr. W. Pole, F.R.S., who is himself a type case of, and an authority on, colour blindness. It is therefore sufficient to say that von Helmholtz gave a new determination of the three fundamental colours, which were found to be red, green, and blue, but to be very much more saturated than any colour which occurs in the spectrum. His final word on the subject of colour blindness was that according to his latest "generalisation of the theory of dichromic vision we get rid of the separation of the green-blind and red-blind into two sharply divided classes, a separation which does not appear to have been completely established by observation. It is also shown that the want of correspondence between the absent colour of the dichromic system, and one of the fundamental colours found by us, does not involve any insoluble contradiction."

In 1853 Helmholtz communicated to the Academy of Berlin a preliminary note "On a hitherto unknown alteration in the Human Eye during a change of Accommodation." He was at that time imperfectly acquainted with researches which had recently been made on the same subject by others, and in his full paper, which was not published till 1856, he frankly admitted that he had been anticipated in several points. In finish and delicacy, however, his work far exceeded that of his predecessors, to which, moreover, he made some important additions. He invented the ophthalmometer for measuring the changes in the magnitudes of the images formed by reflec-

tion at the surfaces of the cornea and the crystalline lens. This instrument could not easily be applied to the faint image formed by the front surface of the crystalline lens, and he devised ingenious methods of indirect measurement by comparing the image with that produced at the surface of the cornea by a weaker source of light.

These observations are classic and placed the explanation of the mechanism of accommodation on a firm basis. They were followed by many others described either in memoirs or in the great work on 'Physiologische Optik' which appeared in parts between the years 1856 and 1866. The geometry of the movements of the eye was discussed in 1863. Three papers were devoted to the form of the Horopter, or locus of points the images of which are formed on "corresponding points" of the two retinæ. Helmholtz also devoted much time to the study of after images, to contrast phenomena, to the theory of binocular vision, and to the relations between the convergence of the eyes and the sense of distance.

A masterly memoir, published in 1874, contained by far the most satisfactory explanation of anomalous dispersion that had, up to that time, been given.

Attempts had been made by Sellmeier and Ketteler, to solve the problem on the hypothesis that material particles are set in motion by the ether. No retarding or frictional forces were introduced and thus difficulties arose when the periods of the material and ethereal vibrations were identical. On the other hand, O. E. Meyer had postulated a viscous ether, but had not introduced the material particles. Helmholtz combined the two hypotheses by assuming a frictional resistance to the motions of atomic matter, which was supposed to be mingled with the ether. The agreement of the results with experiment was sufficiently good to prove that whatever changes may be made in our conception of the mechanical relations between the ether and matter, the equations deduced from the assumptions of von Helmholtz are a close approximation to a satisfactory mathematical expression of the phenomena.

The acoustical work of Helmholtz is more accessible to the English reader than his researches on optics, for, whereas the 'Physiologische Optik' has not been translated, the great companion book 'Die Lehre von den Tonempfindungen' was done into English by the late Alexander Ellis, F.R.S., and was enriched by the notes and appendices added by the accomplished translator with the author's consent.

The first part of the 'Tonempfindungen' was chiefly devoted to harmonic upper partials. It was shown that they "are not, as was hitherto thought, isolated phenomena of small importance, but that, with very few exceptions, they determine the qualities of tone of almost all instruments, and are of the greatest importance for those

qualities of tone which are best adapted for musical purposes." In connection with this subject special mathematical and experimental researches were instituted on the various modes of producing musical sounds. The theory of oscillations in open tubes was discussed from the point of view of the velocity potential, the mode of treatment being at that date (1859) of a very advanced type. Another interesting investigation threw a flood of light on the behaviour of violin strings. A bright point on the string was observed through a microscope the object glass of which was attached to the prong of a tuning fork and vibrated parallel to the string. A study of the curves produced by the luminous point led to the discovery that the motion which produces the fundamental vibration consists of a displacement at uniform speed in one direction, and a return movement, at another constant velocity, to the original point of departure.

From problems such as these Helmholtz passed to the study of vowel sounds. In his view the mouth when adjusted to produce a particular vowel resounds to a particular note, and the quality of the sound is determined by the tendency to reinforce that note. He constructed the well-known apparatus for the synthesis of vowel sounds, and arrived at the important generalisation that the quality of a sound depends only on the intensities of the harmonics and not upon their relative phases.

But, as in the case of optics, Helmholtz was not content without following up the external physical phenomena to the point where they affect the nerves. He published several papers on the anatomy of the ear, and developed a theory as to the mechanism by which the appreciation of musical pitch is developed in that organ. In the first edition of his work (published in 1863) it was to the fibres of Corti that he looked for the origin of this sense, but as it was afterwards proved that they are absent in birds and amphibia, Helmholtz concluded that it is probably "the breadth of the *membrana basilaris* of the cochlea which determines the tuning."

The second part of the 'Tonempfindungen' was devoted to the theory of consonance. Helmholtz explained the dissonance of compound tones as caused by beats between the fundamentals or some of the more important partials. In the case of simple tones the necessary means of discrimination was found in beats due to the presence of combination tones. Of these Helmholtz discovered the summation tone, and though holding that under favourable conditions, which depend upon the mode of production of the notes, the combination tones are objective, he developed a theory of their formation in the ear depending on the asymmetry of the vibrations of the drum-skin. Somewhat lengthy controversies have arisen on several of these points into which this is not the place to enter, but

even if some of the views of Helmholtz should hereafter be decisively rejected, his work on the theory of sound will always stand out in the history of the science as the first great attempt to explain the foundations of music by the laws of acoustics and by the construction of the ear. The discussion was extended in the 'Tonempfindungen' from the cause of dissonance to the theory of musical scales; a natural relationship between the tones being sought, as before, in the relationship of their partials.

But though he maintained that these theories explained the physical "reason of the melodic relationship of two tones," the author of the 'Tonempfindungen' was careful to point out that the principles he enunciated had not always determined the structure of the scale and do not determine it everywhere now. The selection of a series of notes, which were afterwards found to obey certain natural laws, was voluntary. The scale itself is not natural in the sense that it is not a necessary consequence of the construction of the ear; on the contrary, it is the product of artistic invention. Thus music is not a mere branch of mechanics, but an art. The architect and the composer alike deal with materials which are subject to mechanical laws, but they are alike free to fashion from them forms determined, not by calculation, but by the sense of beauty.

In the subjects which have been noticed experiment was used by Helmholtz at least as much as mathematics. In his researches on vortex motion and electricity the mathematical powers of his genius were predominant.

The great paper on vortex motion made an extraordinary advance in the theory of the movement of perfect fluids, a subject which had already attracted the keen scrutiny of the greatest mathematicians. It is to Helmholtz that we owe the discovery of the fundamental laws which govern vortex motion, and of those peculiar qualities which have made it play so large a part in theories as to the constitution of matter and of the ether.

The electrical work of Helmholtz began early, and extended over many years. An important paper, published in 1851, on the duration of induced electrical currents, has been already referred to. It was a bye-product of his researches on the propagation of nervous action.

In 1869 he published an account of some experiments on electrical oscillations. A falling pendulum completed two circuits at times separated by a very small though measurable interval. Electrical oscillations were thus induced in a secondary circuit, the terminals of which were connected with the coatings of a Leyden jar. Immediately afterwards the secondary circuit was itself broken, and at the same instant it was connected with a shunt which led through the nerve of a frog's leg. As the time between the completing of the primary and the breaking of the secondary circuit was increased in

successive experiments, alternations in the violence of the twitchings of the muscle were noticed, and in all forty-five maxima were observed. Helmholtz claimed for this method the advantage that the electrical oscillations were established on an unbroken conductor between the coatings of the jar, so that no spark was produced.

Two years later (1871) he returned to the same subject, and determined an inferior limit to the rate of propagation of electromagnetic induction. Blaserna had published some experiments, from which he concluded that in air this velocity was only 550 metre-seconds. Helmholtz then proceeded to investigate the matter for himself, using, with some modifications, the apparatus described above. It is evident that if the time interval between the breaking of the two currents were adjusted so as to give the maximum effect, the same result would only be obtained when the distance between the two circuits was increased, if the time interval were itself augmented by an amount equal to that required for the induction to travel across the additional space. Helmholtz found that the same adjustment was equally good at all distances, and concluded that the velocity of propagation must exceed 314,400 metre-seconds. These experiments acquire an additional interest when we remember that Hertz was a pupil of von Helmholtz, and was thus brought up in a laboratory in which electrical oscillations had been the subject of careful study. The seed sown by the earlier efforts of the master brought forth fruit a hundred-fold.

From 1870 onwards, Helmholtz published an important series of papers on the theory of electro-dynamics. His point of departure was the discussion of the mutual action of two current elements. An expression for the potential of two such elements had been formulated by F. E. Neumann, which differed from those deduced from the theories of W. Weber and Clerk Maxwell respectively. All three gave identical results in the case of closed circuits.

Taking the elder Neumann's formula as the groundwork of his investigations, Helmholtz sought to find the terms which must be added to it, so as to produce the most general expression consistent with the known behaviour of closed circuits. The result was an expression consisting of the sum of two terms, which were multiplied respectively by $1+k$ and $1-k$, where k is an undetermined constant.

The expression is equivalent to that given by Weber when $k = -1$, to that given by F. E. Neumann when $k = 1$, and is in accord with Maxwell's theory when $k = 0$.

It was then proved that if k is negative the equilibrium of electricity at rest must be unstable, so that motion, when once established, would increase of its own accord, and lead to infinite velocities and densities. The assumption was, in fact, a violation of the law of

the conservation of energy in the sense that two electrified particles starting with a finite velocity would within a finite distance acquire infinite speed, and therefore infinite energy.

An account of the controversy which followed on the publication of this view has been given by Maxwell. It is, therefore, sufficient to say that Helmholtz maintained his position against all attacks.

It remained, however, to discuss the case when k was equal to or greater than zero. The most interesting part of this investigation was the application of the generalised formula to the propagation of electrical and magnetic disturbances through bodies capable of electrical or magnetic polarisation. These properties were treated independently. The conclusions arrived at may be summed up as follows.

Both longitudinal and transversal electric disturbances can be propagated in unmagnetisable dielectrics. The velocity of the transversal undulations in air depends on the absolute susceptibility of the medium. If this is very large the velocity is the same as that of light. The velocity of the longitudinal waves is equal to that of the transversal waves multiplied by the factor $1/\sqrt{k}$ and by a constant which depends on the magnetic constitution of the air. In conductors the waves are rapidly damped. If the insulator is magnetisable, the magnetic longitudinal oscillations have an infinite velocity, the transversal magnetic oscillations are perpendicular to the transversal electrical oscillations, and are propagated with the same velocity.

In the particular cases when $k = 0$ the longitudinal waves of electricity have also an infinite velocity, and the theory is then in close accord with that of Maxwell, provided that the absolute specific inductive capacity of the air is great enough to make the common velocity of the electrical and magnetic transversal undulations equal to that of light.

The paper, of which an account has just been given, was entitled "On the Equations of Motion of Electricity in Conductors at Rest," and was described by Maxwell as "very powerful." It was followed four years later (1874) by a memoir, in which the theory was applied to conductors in motion, and by several others of a more or less controversial character.

An interesting result of the attention given by Helmholtz to the theory of electricity was a series of experiments, carried out in his laboratory by Professor Rowland, to determine whether the magnetic effects of an electric current were identical with those produced by the displacement of matter carrying an electrostatic charge. As these experiments were planned by Rowland, it must suffice to state that it was proved that a revolving charged conductor behaves like a ring-shaped electric current.

Helmholtz also developed the theory of electrical double layers at the surfaces which separate two bodies of different potentials, and applied the theory to the explanation of electrical convection and other phenomena.

His views on galvanic polarisation and electrolysis were expounded in the Faraday Lecture, delivered before the Royal Institution in 1881.

In the year 1886 he published a paper "On the Physical Meaning of the Principle of Least Action." This was the first of a series in which he extended the application of that principle to electrodynamics.

His last paper was on this subject. It was communicated to the Academy of Berlin on June 14, 1894, less than three months before his death, and appears in his collected works as an unfinished fragment. It is characteristic of the wide view which he took that this final effort was an attempt to deduce the principles of electrodynamics, "von Maxwell aufgestellten, von H. Hertz ausführlicher formulirten," from a generalised form of the principle of least action.

An account of von Helmholtz would be incomplete without a reference, however brief, to his philosophical views.

He adhered to the opinion that our senses convey to us only symbols of the truth. In an eloquent passage ('Wissenschaftliche Abhandlungen,' ii, p. 608) he declared that the sensations of light and colour have just as much and just as little relation to external facts as the name, or even the handwriting, of a man have to the man himself. The only difference between the symbolism of speech and that of our senses is that the former is more or less arbitrary, while the latter is a universal language, without dialects, which nature prescribes.

But von Helmholtz also held that our most fundamental conceptions are based not upon innate ideas, but upon the use of these inexplicable sensations, conveyed to us by organs which, though they tell us all we need to know, are from the instrument maker's point of view imperfect. But while describing Kant's theory of the *a priori* origin of the geometrical axioms as "eine unerwiesene," "unnöthige," and "gänzlich unbrauchbare Hypothese," he gave greater weight to the principles of mechanics, as enabling us to judge of the relations of real things. As soon as these principles, he said, "are conjoined with the axioms of geometry we obtained a system of propositions which have real import. . . . . If such a system were to be taken as a transcendental form of intuition and thought, a pre-established harmony between form and reality must be assumed."

These and various other points, both scientific and philosophical, were discussed by von Helmholtz, not only in papers addressed to

expert audiences, but in successive series of luminous popular lectures.

Of the literary style in which all this was done, Professor Engelmann must be allowed to speak. "Besseres Deutsch ist nicht geschrieben worden. Helmholtz' Sprache ist von vollendeter edelster Natürlichkeit, von ruhigstem Flusse und gleichmässigem Wohllaut. Er liebt die kurze gerade Redeweise, verschmäht prunkvolle Worte und den häufigen Gebrauch von Bildern und erhebt sich doch, wo es der Gegenstand mit sich bringt, zu poetischer Wärme des Ausdrucks."

Such is a brief record of the more salient results of the work of von Helmholtz. If judged both by their variety and importance, they have, perhaps, never been equalled. They secured for him in his lifetime the admiration and respect of the whole civilized world. Honours were showered upon him. In particular, he was ennobled by the German Emperor, was a Foreign Member of the Royal Society, and in 1873 was awarded the Copley Medal. He attained his seventieth year in 1891, and the occasion was celebrated in Germany almost as a national festival, while outside the limits of the German Empire "learned societies," to quote his own words, "spread over the whole world, from Tomsk to Melbourne," expressed by diplomas and addresses their sense of the importance of his scientific work.

He outlived the celebration by three years only. His death deprived the world of one of the most notable of the leaders of the science of the nineteenth century.

A. W. R.

The death of Sir James Cockle removes from our midst a man eminent as a lawyer and a judge, and no less eminent as a mathematician. Of the work which he did, and the distinctions which he won at the bar and on the bench, something may be said here; though he was known to us chiefly by his writings on subjects far removed from his professional life. He commenced his legal career fifty years ago as a special pleader, and on being called to the bar joined the Midland Circuit, where he gained the good opinion and esteem of all with whom he came in contact. He was especially admired for his justness of thought, clearness of view, refinement of mind, and elevation of character. These characteristics, combined with a sound knowledge of law and unwearied industry, eminently fitted him for the high position he was afterwards called to fill as first Chief Justice of Queensland. This advancement he owed to the influence of Sir William Erle, then Chief Justice of the Court of Common Pleas, who had formed a very favourable opinion of his capacity and character. Erle, on being applied to by the Colonial Office,

"named" Cockle for the post "on account of the estimation and regard in which he was held by the good men on his circuit."

Cockle commenced his judicial labours amid difficulties not of his own creating, for they had arisen before his arrival in the Colony, and were in no way connected with any action of his, but by courtesy, tact, and decision, he speedily overcame them, and his subsequent course was comparatively smooth. For fifteen years he presided over the Supreme Court of Queensland, and throughout the whole period he enjoyed the respect and confidence alike of his colleagues on the Bench, the members of the Bar, and the community in general.

Chief Justice Erle, who watched his Australian career from the beginning to the end with interest and satisfaction, often testified to the excellence of his judicial administration. "With regard to the duties of his office," he wrote to the Duke of Buckingham and Chandos when Cockle had been for some years at work in Queensland, "I am confident that he has done 'what to justice appertains according to law' with zeal and ability, setting a good example of the dignity and motives which become the office. But, besides the work included in his judicial contract, he has been indefatigable as a legislator, systematising the law there, and bringing it up to the best improvements here." In this letter Erle enumerates some thirty statutes consolidated mainly by Sir James Cockle, and points to his endeavours to diffuse the culture which, as a Trinity (Cambridge) man, strong in mathematics, he "imported" with him, and had "imparted in lectures and publications." "He set out in troubled waters—from the clash of legislative and judicial powers—which were soon calmed by his discretion. I have had much knowledge of judicial men, and I am sure the Queen has never had a servant who more thoroughly earned every farthing of the wages he hoped to receive."

Equally emphatic testimony was given by men on the spot who had been long and intimately associated with Cockle in his judicial administration. When he was about to return to England, and before it was known that he intended to resign his official connexion with the Colony, the journalists of Queensland testified in warm terms to the general appreciation of his public services and private worth, and expressed the hope that his absence would be but of brief duration. And although seventeen years have since elapsed, Queensland still remembers with grateful feeling her first Chief Justice. When the news of his death was cabled out to the colony, the daily papers gave immediate expression to the public sense of loss. The occasion served to revive old memories: the Judge, his dignified and courteous bearing, his unwearied labours, the fidelity with which he dispensed justice according to law, his varied services to Queensland, the pro-

fundity of his learning, and his mathematical distinctions—all were passed under review. "No community could desire to build up their series of Chief Justices upon a more upright and steadier foundation-stone than the late Sir James Cockle." ('Brisbane Telegraph,' January 30, 1895.) The following estimates of his character and work as a Judge are from the pens of his successors, both of whom had exceptional opportunities of knowing him in that capacity. Sir Charles Lilley, who was his colleague on the Bench and succeeded him as Chief Justice, writes:—

"Sir James Cockle's services to Queensland as Chief Justice were of a high order. He was an excellent lawyer, and the dignity and urbanity of his judicial presence upheld the tone and character of the Bench and Bar as one of the highest institutions of the country. He felt and manifested the feeling that his decisions should be absolutely just. At times those who observed his anxiety to give exact and righteous judgment, thought it savoured of weakness and hesitancy, but this was an entirely erroneous idea of his character and conduct. It arose from his settled idea that if by any labour of his own he could do right according to law, it was his duty to apply himself with diligence to the necessary task. He was fearless, and eminently successful in his administration of the law, few appeals resulting from his decisions, and only two being successful during his fifteen years' presidency of the Supreme Court of the Colony as Chief Justice. He was courteous to the Bar, loyal and helpful to his colleagues on the Bench, and an example to the world of a righteous judge. He rendered great service to the Colony by a consolidation of large portions of the statute law, especially of the criminal law, the mercantile law, constitutional law, and of the procedure of the courts. He took an interest in the charitable institutions of the country, and for some years was Chairman of the Brisbane Boys and Girls' Grammar Schools. He was President for some time of the Queensland Philosophical Society. Indeed he was one of its founders, and took an active part in its proceedings by contributing some valuable papers, since included in a publication collected from his contributions to several periodicals. He rendered in many ways most important services to Queensland. Not least was the example of his life as a Christian judge and gentleman. My friend was a sincerely convinced Christian. He had satisfied himself of the truth and beauty of Christian Faith and Hope, founding his belief on the proof of the great central miracle of the Resurrection. The news of his death was received with profound regret throughout the Colony, and from the Bench of the Supreme Court his life and services were eloquently eulogised by his successors. I regard my own loss of my old friend as irreparable."

Sir Samuel Griffith, the present Chief Justice, and a former Attorney-General of Queensland, writes:—"The position of Sir James

Cockle as the first Chief Justice of Queensland, was one of some difficulty. He was appointed over the head of Mr. Justice Lutwyche, an able and experienced lawyer, who had previously been the sole Judge of the Court, and who continued to receive for several years a considerably larger salary than the new Chief Justice. No signs of any friction between them, if any existed, were, however, allowed to become manifest to the public. The condition of the Bar, at first extremely small in numbers, and of the colony, which had been constituted at the end of 1859, and was very rapidly increasing in population and wealth, combined to render the office of the President of the Supreme Court one of special importance, it falling to his lot to a great extent to form, by precept and example, what were to be the future traditions of the Court, and to earn for the Bench that respect which, although generally associated by Englishmen with the administration of justice, was in the first instance acquired, and can only be maintained, by the personal qualities of the Judges. In this task Sir James Cockle was eminently successful. He earned and enjoyed the most profound personal respect. Implicit confidence was felt in his intense desire to administer justice with absolute impartiality.

"His courtesy and kindness to the profession, especially to the junior members, who were without the advantage of training in English Courts, were admirable, as was, on the occasion of his death, pointed out by two of them who now occupy seats on the Supreme Court Bench.

"His habit of accurate thinking was impressed upon his judgment, which erred, if at all, in being perhaps too laconic, just sufficient words being used to convey to a reasoning mind the logical conclusion from the premises. When sitting with juries, on the other hand, it was sometimes complained of him that it was impossible even to conjecture, from any indication of the inclination of his own mind, on which side he thought the balance of probability lay, so careful was he to avoid the appearance of partiality.

"Essentially of a shy and retiring disposition, and perhaps diffident of his own ability, he took little part in public affairs, except for a time as Chairman of Trustees of the Brisbane Grammar School. But this very aloofness was, probably, in the special circumstances of the colony an advantage, in that it prevented any imputation, always difficult to avoid in a small community, of undue friendship between judges and suitors. Of his ability as a sound and able lawyer no doubt has ever been felt, and the still more important work already referred to, of giving an initial direction to the administration of justice in Queensland, and establishing a lofty standard of duty in the Courts, could not have been in better hands."

It was, however, as a mathematician, and not as a judge, that Sir

James Cockle was best known to us. He wrote on the Indian Astronomical Literature, on the Indian Cycles and Lunar Calendar, on the date of the Vedas and Jyotish Sastra, and on the Ages of Garga and Parasara. He also published four elaborate memoirs on the Motion of Fluids, and some notes on Light under the Action of Magnetism, &c., but in general he confined himself to problems in pure mathematics. His analytical researches were concerned for the most part with two subjects, viz., Common Algebra and the Theory of Differential Equations. In algebra he worked mainly among the higher equations, and for many years his labours in this department were inspired and directed by the hope of being able to "solve the quintic," or, in other words, to express a root of the general equation of the fifth degree by a finite combination of radicals and rational functions. The problem had long engaged the attention of mathematicians, and was attacked by the most celebrated analysts of the last century with great skill and vigour, but without success. In the early part of the present century, Abel, the young and gifted Norwegian mathematician, attempted to show that a finite algebraic solution of the problem was impossible. Cockle considered the argument with care, and reproduced it as modified by Sir W. R. Hamilton, in the 'Quarterly Journal of Mathematics' (vol. 5). To prove a negative, however, is proverbially difficult, and despite Abel's "demonstration," and the non-success of preceding investigators, Cockle for many years clung to the conviction that what had been done for the lower equations might be done also for the equation of the fifth degree. He laboured long and hard at the problem; and although he failed, as others before him had failed, to effect a general solution, and came finally to the conclusion that such a solution was "absolutely unattainable," yet his labour was not lost. He found not the thing he sought for, but other things which amply repaid the toil of effort, and he opened up new methods of working, and new lines of research which are of acknowledged value in themselves. A result which he obtained in the fifties attracted much attention at the time, on account of its remarkable simplicity. By an indirect but ingenious process he succeeded in determining the explicit form of a certain sextic equation on the solution of which that of the general quintic may be shown to depend. The accuracy of this sextic or "auxiliary" equation (whose coefficients are all monomials save one, which is a binomial) was shortly afterwards confirmed by an independent calculation. The writer of this notice was led to consider the problem in connection with some researches of his own on the finite solution of algebraic equations, in the course of which he calculated Cockle's sextic by a direct process. His researches were published in the 'Memoirs of the Manchester Literary and Philosophical Society,' to which Cockle had contributed his remarkable result, and the subject

was followed up by the same writer in two papers on the "Theory of Quintics," in the 'Quarterly Journal of Mathematics,' and also in an exposition of Cockle's "method of symmetric products" in the 'Phil. Trans.' for 1860. The study of these papers led the late Professor Cayley to investigate the subject, and his results were embodied in a memoir entitled "On a New Auxiliary Equation in the Theory of Equations of the Fifth Order," which appeared in the 'Phil. Trans.' for 1861. Cockle had calculated the auxiliary equation for one of the trinomial forms to which the quintic may be reduced without any loss of generality, hence the simplicity of his result. Cayley, employing an invariantive process, calculated the same equation for the complete quintic, that is, the quintic not deprived of any of its terms, and not modified in any of its coefficients. The result is, of course, less simple than that for the trinomial form, but it has the advantage of being absolutely complete. Thus Cockle's labours on the quintic invested the theory with a new interest, and the methods he devised, and the results he obtained, largely directed the course of subsequent speculation on the subject.

His mode of dealing with the theory of differential equations was equally marked by originality and independence of mind. Not confining himself to the beaten track, he pushed his way into unexplored regions, and succeeded in bringing to light important relations and analogies between algebraic and differential equations. Two examples may be given. He found that from any rational and entire algebraic equation of the degree n, whereof the coefficients are functions of a single parameter, we can derive a linear differential equation of the order $n-1$, which is satisfied by any one of the roots of the algebraic equation. Out of this germ has grown the theory of Differential Resolvents. To Cockle also belongs the honour of being the first to discover and develop the properties of those functions called Criticoids or Differential Invariants, so called because they remain unaltered when the differential equation is transformed by a change of one of the variables, and are therefore analogous in this respect to the critical functions or seminvariants of common algebra. Criticoids seem destined to play an important part in the theory of linear differential equations.

But it would be impossible, within the limits at our disposal, to discuss in detail Cockle's various discoveries in algebra and the calculus. Enough to say here that his work was eminently initiatory. He started theories, but left others to elaborate and perfect them. Of his eighty or ninety papers given to the mathematical world, many are no doubt slight and fragmentary, but there are few, even among the shortest and least complete, which do not contain original and valuable suggestions. He struck out ideas which have taken root in other minds and borne fruit.

The leading events in his life are soon told. He was born on January 14, 1819, being the second son of the late Mr. James Cockle, of Great Oakley, Essex. From 1825 to 1829 he was educated at Stormond House, Kensington; thence he was sent to Charterhouse, where he showed considerable power in making Latin verses. At the end of his second year he was removed and placed under the tuition of the Rev. Christian Lenny, D.D., of St. John's College, Cambridge, who was the first to discover his mathematical talent. In November, 1835, when he had nearly completed his seventeenth year, he went abroad, and was absent from England about twelve months, visiting the West Indies and the United States of America; at Cuba he acquired some knowledge of the Spanish language. Returning home he entered Trinity College, Cambridge, October, 1837, and graduated as thirty-third Wrangler in 1841. His position in the Tripos gave no indication of his future eminence as a mathematician, nor is the circumstance to be wondered at when we consider the character of his preparatory training, and the long break in his studies before he went up to the University. He proceeded to the degree of B.A. in 1842, and of M.A. in 1845.

Mr. Cockle was entered as a student at the Middle Temple in 1838. He practised as a special pleader from 1845 to 1849, was called to the Bar at the Middle Temple in 1846, and joined the Midland Circuit at the Nottingham Spring Assizes in 1848. On August 22, 1855, he married Adelaide Catherine, eldest surviving daughter of the late Mr. Henry Wilkin, formerly of Walton, Suffolk. In 1862 he drafted the "Jurisdiction in Homicides Act" (Imperial); and in the following year (1863) he was appointed by the English Government first Chief Justice of Queensland. Allusions have already been made to the fact that the position at the outset was very trying, but the circumstances deserve more particular notice, as throwing light on the character of the man. Mr. Justice Lutwyche, who during the previous year or two had come into collision on several occasions with the Governor, and also with the Government of the Colony, and whose claims to the supreme place on the bench were in consequence passed over by the home authorities in favour of the English barrister, naturally felt himself aggrieved at the appointment of a younger man who had had no judicial experience. The story, as told in the Brisbane papers on the death of the old Judge in 1880, reflects equal credit on both men: "The late Judge made no secret of his mortification at the appointment of Mr. Cockle. A few years of association, however, entirely obliterated any feelings of hostility to the Chief Justice that this event may have originally engendered, and the two Judges became sincere and attached friends. Sir James always paid a very marked deference to the opinion of his learned brother, and the amiable disposition of the Chief Justice so

wrought upon the sterner nature of his colleague that, when Sir James left for Europe two years ago, the parting was a severe trial to Mr. Lutwyche, who was extremely affected at bidding good-bye to a friend whom he rightly divined he was never to see again."

The Chief Justice was Senior Commissioner for the consolidation (effected in 1867) of the statute law of Queensland. He was knighted by patent in 1869. In 1874 the Legislative Assembly of Queensland showed their appreciation of his services by passing an Act giving him a substantial increase of salary.

Sir James Cockle's professional occupations at this period were numerous and exacting, yet he did not neglect his favourite science. He turned to mathematics as a relaxation, and devoted the intervals of official labour to researches in algebra and differential equations, embodying his results in papers which appeared from time to time in the 'Manchester Memoirs,' the 'Quarterly Journal of Mathematics,' the 'Philosophical Magazine,' and other periodicals in England, and in the 'Proceedings' of the Royal Societies of New South Wales and Victoria, in Australia. He also wrote and published a number of presidential addresses delivered before the Queensland Philosophical Society (now incorporated into the Royal Society of Queensland) in which he dealt with questions in philosophy, logic, and mathematics.

In 1879 he resigned his position as Chief Justice of Queensland, having a few months before returned to England with his wife and children. The remainder of his days was given to mathematical writing, the business of several learned bodies, and the society of his friends; but he was never really strong after his return home, his health suffering perhaps from the change of climate.

He was elected a Fellow of the Royal Astronomical Society in 1854, and served on its Council from 1888 to 1892. He was elected a Fellow of the Royal Society in 1865, a Corresponding Member of the Manchester Literary and Philosophical Society, and a Member of the London Mathematical Society in 1870; he filled the Presidential Chair of the latter Society from 1886 to 1888. He was President of the Queensland Philosophical Society from 1863 to 1879, and was elected an Honorary Member of the Royal Society of New South Wales in 1876. He was a Commissioner for the Queensland Section of the Colonial and Indian Exhibition held in London in 1886; and was nominated to represent the Australian Colonies at the Washington Prime Meridian Conference in 1884, but was unable to accept the position.

Of his personal and social qualities the writer may be permitted to speak from personal knowledge. He looks back with pleasure to an acquaintance begun nearly fifty years ago, which soon ripened into a friendship, never clouded even for a moment by the slightest

misunderstanding; drawn together by similar scientific tastes, the two men had otherwise little in common. In their political and ecclesiastical opinions they differed fundamentally, and had it not been for Cockle's imperturbable temper and graciously tolerant spirit, these differences would often have disturbed their cordial relations. Controversy, however, was distasteful to him, and he avoided the conflict of argument. When it was suggested on one occasion that he should offer himself for a seat in Parliament, he said playfully: "My address to the electors shall run thus,—Gentlemen, I am in favour of making things agreeable all round—all round!" The saying revealed his spirit. He desired to live peaceably with all men, and so far as we know he had not a single enemy. A man of somewhat phlegmatic temperament, he was not easily excited. His features in repose were calm and serious, and to strangers he usually gave the impression of being very reserved; but when conversing with his friends on congenial subjects his countenance would light up with a pleased expression, and his manner become animated, while his fulness of knowledge and ripe and varied experience imparted to his observations an interest of their own. He was extremely cautious in offering a definite opinion on any debateable question, a habit probably due to his legal training. When not engaged in mathematical investigations, his leisure was mostly devoted to problems in metaphysics and theology, studies which had for him a special attractiveness, and in both of which he was deeply versed. His modesty was remarkable; rarely speaking of his own work, he was ever ready to recognise and do full justice to the work of others. There was in him none of the petty jealousies which haunt meaner minds. The writer remembers with gratitude how, when he entered fields which Cockle might be said to have made his own, he was not treated as an intruder or a rival, but welcomed as a friend and fellow-worker, and how he received from his elder an amount of encouragement and help which he can never sufficiently acknowledge.

Cockle was an excellent correspondent, his caligraphy was clear and good, and when writing on congenial themes he would often wax truly eloquent. He had a positive enthusiasm for mathematics, and the discovery of a new theorem or a new method always gave him intense delight. The writer has preserved most of the letters he received from him, and placed them bound in several goodly volumes among the choicest of his literary treasures.

Something of a recluse, Sir James astonished many of his friends, both in England and Australia, by the zest with which, during the last ten or twelve years, he threw himself into the club life of the Metropolis. He became a member of the Garrick, the Savile, and the Savage, and an *habitué* of all three, being particularly attached to the last, of which he was Treasurer from 1884 to 1889. Queens-

landers visiting London could hardly look on gravely when they saw how their old Chief Justice adapted himself to what one of them called his "Bohemian and brilliant environment." But even there he was not unmindful of the claims of science. "In the Savage Club" writes one, "it was a familiar sight to see him quietly working at some algebraical research on the back of an envelope or some odd scrap of paper, though always ready to break off and offer a genial welcome to one of his friends." He was a devoted "brother" of the Masonic Order, and soon after his return home from Australia he became a joining member of the Nine Muses Lodge, in which he rose rapidly through various offices to the highest position, being installed W.M. of that lodge on the 12th February, 1889.

Sir James Cockle was a man of upright character and simple tastes, of amiable disposition and courteous bearing, constant in his friendships and faithful in all the relations of life; being absolutely devoid of ostentation, vanity, or pretence, his whole life was a beautiful illustration of the motto on his crest—*Esse quam videri.* He died at his residence in Bayswater on Sunday, the 27th January, 1895, and was buried at Paddington Cemetery on the following Saturday. He had nine children, of whom eight survive; Lady Cockle also survives him.

R. H.

WILLIAM PENGELLY, who died in 1894 at the ripe age of eighty-two, was one of the last survivors of a scientific type represented by Sedgwick, Lyell, Phillips, Murchison, and the other old heroes who laid the foundation of geological science. He belongs to the heroic age of geology, to that group of men who found British geology almost a *terra incognita*, and left it so completely explored that there is little left for their successors but to correct mistakes and fill in minute details.

Pengelly was born in 1812, at East Looe, in Cornwall, of a Quaker stock, and lived all his life in the west country. Like Professor Dana, he took to the sea, and served before the mast. Having, however, a decided taste for mathematics and geology, he gave up seafaring and settled down as a teacher in Torquay. Here, for some sixty years, he threw himself into the work of higher education, and more especially in the direction of natural science. In 1837, through his energy, the Torquay Mechanics Institute, which had fallen on evil days, was organised and put on a satisfactory working basis. Seven years later he founded the Torquay Natural History Society, and in 1863 he extended the range of his personal influence by establishing the Devonshire Association, which took root and flourished exceedingly, and has been of great service in the west of England. It is impossible to read any one of the many volumes published by the

Association without realising how great has been his influence in bringing natural knowledge within reach of the people. The museum at Torquay is also an enduring monument to his energy, which will continue to teach when his name is forgotten.

Pengelly was, however, beyond all other things, a geologist devoted to the study of Devonshire. The collection of Devonian fossils in the Oxford Museum is spoil of his hammer. He collected also the materials for the "Monograph on the Lignite Formation of Bovey Tracey," a joint publication with Dr. Heer, that has thrown so much light on the Miocene forests which clothed the slopes around the Lake of Bovey. During the second quarter of the present century, the question of the antiquity of man was steadily coming to the front. In 1847 Boucher de Perthes published his discovery of flint implements along with the extinct mammalia in the river gravels of Amiens and Abbeville. Similar discoveries in Kent's Hole by Mr. McEnery, made some time between 1825 and 1839, had been verified by Godwin-Austen in 1840 and the Torquay Natural History Society in 1846. So strong, however, were the prejudices against the antiquity of man that the matter was not thought worthy of further investigation until the year 1858. Then it was determined that a new cave at Brixham, near Torquay, should be explored by a joint committee of the Royal and Geological Societies, consisting, among others, of Lyell, Falconer, Ramsay, Prestwich, Owen, and Godwin-Austen, with Pengelly as the superintendent of the work. The result of the exploration established, beyond all doubt, the existence of the palæolithic man in the Pleistocene age, and caused the whole of the scientific world to awake to the fact of the vast antiquity of the human race. From this time Pengelly's energies were mainly directed towards cave exploration. In 1865 he undertook the superintendence of the exploration of Kent's Hole by a committee of the British Association. Day by day, except when the work was stopped, he visited the cave, and recorded on maps and plans the exact spot where each specimen was found, for no less than sixteen years. The vast collection of palæolithic implements and fossil bones, each of which bears traces of his handiwork, is represented in most of the museums in this country, and the annual reports, listened to with so much pleasure by crowds at the meetings of the British Association, are the most complete that have ever been published. It may be objected that the accumulation of so much evidence of the existence of man in the Pleistocene age in the south of England was unnecessary. It was, however, necessary to sweep away the mass of prejudice, and this could best be done by repeating the evidence. Had this not been done man would not occupy the recognised position which he now holds in the annals of geology. The rest of Pengelly's life was mainly given up to the researches in the other caves in

Devonshire. In estimating his scientific work it must not be forgotten that it was done in addition to the daily task of bread-winning.

There remains one other side of Pengelly's many-sided character which deserves remark. He was a fluent and genial speaker and lecturer. For many years he was a leading figure at the meetings of the British Association, and there are but few large centres where he was not known as a lecturer and not welcomed as a friend. Some of his *jeux d'esprit*, such as, for example, his saying in treating of the thorny question of man's antiquity, "That you may be as naughty as you like," will long be remembered. He died full of years, and with his services honourably recognised by his private friends and by the scientific world.

W. B. D.

By the death of Sir George Buchanan this country has lost one of its most prominent leaders in the branch of preventive medicine and public health. George Buchanan was the son of a medical practitioner in Islington, in which parish he was born in 1831. Very early in life he gave evidences of marked ability, both in classical and mathematical pursuits, and after graduating B.A. at the London University, he entered University College Medical School, where he became a scholar and a medallist, and from which he took his M.B. and M.D. London, with honours and distinction.

His early medical career fitted him eminently for the task of his maturer years. He became Resident Medical Officer, and subsequently Physician, to the London Fever Hospital, where he acquired an intimate acquaintance with the various infectious fevers, and he joined the medical staff of the Hospital for Sick Children, where his clinical knowledge of disease became considerably enlarged. Thus, he was able to bring to bear upon the etiological research, to which he was soon to devote the best years of his life, a thorough practical and clinical knowledge of disease. As far back as 1857, he became Medical Officer of Health to St. Giles. In many parts of London the duties of such a post were limited to the supervision of a few unimportant administrative details, but many of the annual and special reports issued by Dr. Buchanan during his tenure of office in St. Giles's remain to this day examples of the best type of administrative counsel based on knowledge, in a scientific sense, of the various conditions which affect human health and life. Indeed, it was the quality of these reports that led to his employment as a temporary medical inspector under Mr. (now Sir John) Simon, then Medical Officer to the Privy Council, who, in later days, in his work on 'English Sanitary Institutions,' says of Buchanan, that he was "the author of reports which have become classical in sanitary literature,"

that he "had always shown himself of extraordinarily active and discriminating mind, and always intent on that exactitude which is essential to scientific accuracy."

For some seven years Buchanan acted professedly as a temporary and occasional Inspector in the Medical Department of the Privy Council, but his services were such that he soon came to be continuously engaged in the work of the Department, and in 1868 he received permanent appointment. In 1871 he was, with his chief and others, transferred to the newly formed Local Government Board.

During his seven years of temporary duty Buchanan undertook a number of important enquiries. One, which stands out very prominently, was a comprehensive investigation into the effects on health which had resulted from the large works of drainage and water supply carried out in a number of our towns and cities. As this enquiry progressed, Buchanan came to see that the influence which had been exerted by these works was by no means limited to a reduction in the rate of mortality from those diseases which are commonly associated with the pollution of air, soil, and water, and he soon arrived at the conclusion that the drying of the soil brought about by works of deep drainage had been associated with a marked reduction in the mortality from phthisis. This point was worked out in much detail, and Buchanan was able to show that the diminution in the phthisis death-rate bore a distinct relation to the extent to which the level of subsoil water was lowered as the result of works of drainage; that the phthisis death-rate oscillated in one and the same town with variations—of sufficient duration—in the oscillations of the subsoil water; and that where, owing to physical or other circumstances, no lowering of the subsoil water had resulted from works of drainage, there had been no material diminution in the amount of death from phthisis.

This discovery has had an important influence on public health administration, and it has formed the basis of codes of bye-laws having for their object the exclusion of moisture and sub-soil emanations into the interior of dwelling-houses. It also became the more interesting when it was ascertained, after the publication of Buchanan's reports, that Dr. Bowditch, of Massachusetts, had been making enquiries on somewhat the same lines, and had independently arrived at very similar conclusions.

As has already been said, through his studies at the London Fever Hospital, Buchanan came to possess a highly scientific knowledge of the various continued fevers; and he was the writer of the article on typhus fever in Reynolds' 'System of Medicine,' in addition to a number of official reports having to do with the etiology of the specific fevers. Perhaps one of the most important of these was one relating to an outbreak of enteric fever at Caius College, Cambridge, in which

it was clearly proved that the specific poison of that disease could be distributed in water pipes, as the result of an intermitting water-service.

In 1880 Buchanan was appointed to the post of principal medical officer to the Local Government Board, and during the twelve years which followed he made it the aim of his administration that all advice rendered by his department should have a definitely scientific basis. This principle he instilled into all his co-workers, and he was careful to employ the small annual grant made to the Medical Department for auxiliary scientific research to this end. In this way, research of the highest scientific value, as, for example, the investigations of Dr. Klein and Dr. Sidney Martin into the life-history of the specific organisms of disease, was undertaken side by side with that epidemiological field-work into the causes of epidemics, which forms so large a portion of the labours of the medical inspectors attached to the Public Health Department of the State. In this way alone Buchanan rendered services of an eminent kind, not only to the State but to science, and his labours in this respect were deeply appreciated by co-workers in both hemispheres.

In April, 1892, Dr. Buchanan retired from his official post, and on this occasion he received the honour of knighthood. But he had already undertaken to serve as a member of the Royal Commission on Tuberculosis, and to this and other self-imposed duties he now devoted himself. On the death of Lord Basing he became chairman of the reconstituted Royal Commission, and the report which was only issued just before his death is known to have been largely compiled by him.

During his official career Sir George Buchanan was in 1882 admitted F.R.S.; he became President of the Epidemiological Society; he was elected to the Senate of the University of London; and on his retirement he received the honorary LL.D. of Edinburgh, and became a censor of the Royal College of Physicians. His death, which occurred suddenly during convalescence from a surgical operation, took place on the 5th of May, 1895.

R. T. T.

General James T. Walker, C.B., F.R.S., died on the 16th of last February, having reached his seventieth year. The son of a Madras civilian, who was for some time Judge at Cannanore, Walker was born in 1826, and, having passed at Addiscombe, he was appointed to the corps of Bombay Engineers in 1846. He did good service during several of the wars in which our armies have been engaged in India. At the siege of Multan he performed an act of gallantry, which, in later times, would probably have secured for him the Victoria Cross. He served at the battle of Guzerat, and in the sub-

sequent pursuit. He was severely wounded at the siege of Delhi, and was actively engaged in several of the wars with frontier tribes. But his real life-work was connected with the surveys of India, and it is on his services as a scientific geodesist of a very high order that his title to fame must rest. His military reconnaissance and survey of the Trans-Indus Region, during which he mapped upwards of 8,700 square miles of previously unexplored country, almost single handed, between 1849 and 1853, though most important in itself, was mainly a preparation for his work connected with the Great Trigonometrical Survey of India.

Walker entered that department in 1853, and served in it for thirty years. This long time of most arduous service is divided into two distinct periods. For the first eight years he was under Sir Andrew Waugh, while for the remaining twenty-two years he was himself in charge, and latterly he undertook the control not only of the trigonometrical, but of all the other surveys of India. While under Sir Andrew Waugh, from 1853 to 1861, he was engaged on the northern section of the Indus series of triangles, and also on the measurement of the base near Attock, and afterwards on the Jogi Tila meridional series. When Sir Andrew retired, in 1861, Major Walker was marked out as his successor. He had already received high praise for the character of his geodetic work, and his chief had thus addressed him :—" The brilliant success which invariably attends your undertakings is a proof of the high professional qualifications, the foresight, and judgment which you bring to bear on the important geodetical work on which you are engaged."

Walker assumed charge of the survey at a time when much revision had become desirable owing to improvements in the instruments, and also at a time when it was within the range of reasonable hope that the great triangulation might be completed within the period of his own term of office. He first completed the last three meridional series in the north of India, and measured the Vizagapatam base, and then turned his attention to a revision of Colonel Lambton's early triangulations in the Madras Presidency, and to a re-measurement of the Bangalore base. In 1873 he began to devote much attention to the dispersion of unavoidable minute errors in the triangulation, and it is an acknowledged fact that no trigonometrical survey in Europe equals that of India in accuracy. A great work was undertaken under Colonel Walker's auspices, with a view to preserving a complete record of the various operations of the Great Trigonometrical Survey, which is now contained in twenty volumes. The first nine were edited by Walker himself, the first appearing in 1871, and he wrote the introductory history of the early operations of the survey, the accounts of the standards of measure and of the base lines, the descriptions of the methods of procedure and of the

instruments, and a history of the pendulum observations which he inaugurated, two convertible pendulums having been lent to him by the Imperial Academy of Sciences of St. Petersburg.

While Colonel Walker conducted, to the very best advantage, the most intricate and exact measurements, he also took a broad-minded view of the rougher systems of exploration and reconnaissance. Primarily he was a geodesist, but he was also an enthusiastic geographer. Survey parties were, in his time, carefully organised to accompany every military expedition. Native explorers were trained as surveyors, and despatched into unknown parts of Central Asia and Tibet. Their work was reduced, critically examined, and utilised on their return. Numerous valuable general maps were published at the office of the survey at Dehra Dùn, and Walker's map of Turkistan, which went through many editions, was the leading authority for upwards of twenty years. Walker also established friendly relations with the Russian surveying authorities, there was an exchange of publications, India was well supplied with geographical information from St. Petersburg, and there was a cordial feeling of co-operation between the surveying officers of the two countries. During his short periods of leave in England, Colonel Walker did not cease to work zealously for his department. On one of these occasions he fixed the difference of longitude between London and Tehran; on another he investigated the condition of the plates of the Indian Atlas, and wrote an important memorandum on the scale of the atlas and on the projection. From 1878 Colonel Walker undertook the onerous and difficult post of Surveyor-General of India, in addition to his laborious and absorbing duties as Superintendent of the Great Trigonometrical Survey. At last the strain became too great. His health broke down, and although the Viceroy was most anxious that he should remain, it became absolutely necessary for him to leave India. He retired in 1883, and became a General in 1884.

In his retirement, General Walker continued to "live laborious days." He conducted a large correspondence with geographers and explorers, and with officers connected with geodetic surveys in various parts of the world, always being ready to furnish all the information and advice that his great experience could suggest. He became a Fellow of the Royal Society in 1865. He was also a very active member of the Council of the Royal Geographical Society for upwards of ten years, and wrote several valuable papers on subjects relating to the geography of Central Asia and Tibet. He had a happy knack of communicating his knowledge to others unostentatiously, and in the most effectual way. His kindness and patience in this respect were much appreciated by the officers who served under him. While he raised the standard of high excellence of the geodetic work in India,

and of the maps, and maintained it, thus leaving his mark on every branch of the survey, at the same time he is remembered with affectionate regret, and his loss is deeply felt by the officers who had the honour of serving under him, as well as by a large circle of friends and acquaintances.

C. R. M.

THOMAS HENRY HUXLEY was born at about eight o'clock of the morning of the 4th of May, 1825, at Ealing, then a quiet little country village, now a part of the suburbs of London. He was the seventh child of George Huxley, himself a seventh child, and his mother's maiden name was Rachel Withers. In many instances, the striking qualities of a distinguished man have seemed to be derived from the mother, and Huxley has spoken of himself as completely the son of his mother who was "a slender brunette, of emotional and energetic temperament, excellent mental capacity and distinguishing rapidity of thought."

His father, of whom he could find in himself hardly a trace, was second master at the school of Dr. Nicholson, in Ealing, then an establishment of very high repute, at which Cardinal Newman, and other distinguished men found part of their education, and to which the young Thomas Henry was for a while sent; but his stay there was brief. Nor was he sent to any other school; the greater part of his education he got for himself, reading desultorily and fitfully, but closely and to good purpose, whatever books came to his hand.

From an early age he took a great interest in mechanical problems; and had he been able to follow his own wishes would probably have adopted the profession of engineering. But circumstances were against this choice, and the fact of two brothers-in-law being doctors seems to have, in part at least, determined his entering the medical profession. He began his professional studies at an early age, under the care of his brother-in-law, Dr. Salt, at that time practising in London, to whom he became, according to the then practice, "an apprentice."

It was probably in the beginning of this apprenticeship, when he was as yet only 13 or 14 years old, that he attended his first *post mortem* examination; and to this his ardent curiosity led him to pay such prolonged and close attention that he became in some way or other poisoned, falling into so bad a state of health that he had to be sent into the country, into Warwickshire, to recruit. Returning after some months to London, restored in health, he resumed his duties as apprentice to his brother-in-law, taking in so much of the learning of the profession as the opportunities of his situation offered, and at the same time storing and strengthening his mind with varied reading, not only of science but also of literature. Among other things he

acquired during this time a knowledge of French and of German. There is no record of his having received any other than professional tuition, and it must have been chiefly at least by his own efforts that he acquired his large knowledge of other than professional subjects.

In October, 1842, he and his elder brother, James, having obtained free entrance Scholarships, entered Charing Cross Hospital. Here he came under the influence of Thomas Wharton Jones, then the lecturer at the Medical School there on Anatomy and Physiology, whose physiological researches, especially those on the ovum and on blood corpuscles, are of classic value. He threw himself with ardour into anatomical and physiological studies, as is shown by his obtaining in the Honours division of the 1st M.B. examination at the University of London, the second place (with a Medal), in Anatomy and Physiology, the first place (with the exhibition) being taken by W. H. Ransom, now the distinguished physician of Nottingham. It is still more clearly shown by his having contributed, at Wharton Jones' suggestion, to the 'Medical Gazette,' of November 28, 1845, a small paper, in which he demonstrated the existence of a hitherto unrecognized layer in the inner root sheath of hairs, since known as Huxley's layer. And indeed his subsequent career proves that he must have already possessed at the time of his leaving the hospital an exceedingly large and exact knowledge of comparative anatomy, and of the then existing anatomical methods. Nor had he neglected the more distinctly professional studies, though his heart was not in these; for in the winter of 1845–6, having completed his course, he was prepared to offer himself at the examination for the membership of the Royal College of Surgeons, but being as yet under twenty-one years of age could not be admitted as a candidate.

Wondering at this time what he "should do to meet the imperative necessity for earning his own bread," he was urged by a fellow student (the now Sir Joseph Fayrer) to offer himself for the medical service of the Royal Navy. He accordingly wrote directly to Sir William Burnett, then Director-General for the Medical Service of the Navy, was accepted, and on the 13th March, 1846, was entered as assistant surgeon on the books of Nelson's old ship, the "Victory," for duty at Haslar Hospital. At about the same time he passed the examination of the Royal College of Surgeons. At Haslar, where he had for a messmate the late Sir Andrew Clark, he attracted the notice of his chief, Sir John Richardson, the Arctic traveller, who shewed his appreciation of the merits of the young navy surgeon by keeping him at Haslar until an opportunity offered of placing him in some post suitable to his obvious talents. Such a post was that of Assistant-Surgeon to H.M. surveying ship "Rattlesnake," then about to start under the command of Captain Owen Stanley, brother of the late Dean Stanley, and son of the Bishop of Norwich, to survey

Torres Strait, the Louisiade Archipelago, and the S.E. Coasts of New Guinea. Huxley, with joy accepted the offer and sailed from Plymouth, on December 12, 1846. Though he had been chosen by his chief by reason of his scientific promise, his position in the expedition was simply a professional one. The post of Naturalist to the expedition was filled by Mr. J. Macgillivray; Huxley was merely the ship's surgeon.

Calling on her way at Madeira, Rio Janeiro, the Cape, and Mauritius, the "Rattlesnake" reached Sydney on July 15, 1847. Here, during a stay of nearly three months, while preparations were being made, the young naval surgeon, seeking relaxation from his professional work and his studies in the society of Sydney, became a general favourite, and happily for himself met, loved, and gained the love of the lady, then Miss Henrietta A. Heathorn, who was to be afterwards for so many years his devoted helpmate. On October 11 the vessel started on its cruise, but reaching only as far as Port Curtis and Cape Upstart, returned to Sydney in the following March. In April it started on its second cruise, reaching Cape York, landing Kennedy's ill-fated expedition on its way at Rockingham Bay, going on to Port Essington, and returning by the Timor Sea and Indian Ocean to Sydney, which was reached in January of 1849. In the following May the vessel started on its third cruise to explore the Louisiade Archipelago and the S.E. coast of New Guinea, returning to Sydney in the following March, 1850. Here the lamented death of Captain Stanley led to their being ordered home. Leaving Sydney on May 2, returning by the South Pacific and Cape Horn, and calling at the Falkland Islands and the Azores, the "Rattlesnake" reached England and was paid off at Chatham on November 9. A full narrative of the voyage was published by Mr. Macgillivray in 1852, most of the illustrations being reproductions of drawings by Huxley.

The career of many a successful man has shown that obstacles often prove the mother of endeavour, and never was this lesson clearer than in the case of Huxley. Working amid a host of difficulties, in want of room, in want of light, seeking to unravel the intricacies of minute structure with a microscope lashed to secure steadiness, cramped within a tiny cabin, jostled by the tumult of a crowded ship's life, with the scantiest supply of books of reference, with no one at hand of whom he could take counsel on the problems opening up before him, he gathered for himself during these four years a large mass of accurate, important, and in most cases novel observations, and illustrated them with skilful pertinent drawings. Even his intellectual solitude had its good effects; it drove him to ponder over the new facts which came before him, and all his observations were made alive with scientific thought.

From time to time he sent home to the Linnean Society papers embodying the results of his researches. One on Physalia was read in November and December, 1849, and another, on Diphyes was read at intervals from January, 1849, to February, 1850, and of each the briefest abstracts appeared in the 'Proceedings,' the author being erroneously named *William* Huxley. The full papers remained under consideration until September, 1851, when the MSS. of both were, at the author's request, returned to him. With the exception of a small note on the blood of Amphioxus which appeared in the 'British Association Reports' for 1847, and which recorded observations made in the previous autumn while he was still at Haslar, the only papers of his which were published before his return to England in November, 1850, were, in the first place, the memoir "On the Anatomy and Affinities of the Family of the Medusæ," which had been communicated to the Royal Society by the Bishop of Norwich (to whom, at Captain Stanley's suggestion, he had sent the MS.), and published in the 'Philosophical Transactions' for 1849, and, in the second place, shorter communications on Trigonia, on Firola and Atlantis, and on Medusæ, which appeared in the 'Proceedings of the Zoological Society,' the 'Annales des Sciences Naturelles,' and the 'Annals and Magazine of Natural History,' during the year 1850.

The "Rattlesnake" having been paid off, Huxley applied to the Admiralty to be "borne on the books" of H.M.S. "Fisgard" at Woolwich, that is to say, to be appointed Assistant-Surgeon to the ship "for particular service," so that he "should not be obliged to remain on board, but might live in town and work up" the observations made during the voyage "into a well-digested and consistent whole." In a letter to Sir W. Burnett, who remained his staunch friend, he described the investigations which he thus desired to elaborate as being chiefly those on the anatomy of certain Gasteropod and Pteropod Mollusca, of Firola and Atlantis, of Salpa and Pyrosoma, of two new Ascidians, namely, Appendicularia and Doliolum, of Sagitta and certain Annelids, of the auditory and circulatory organs in certain transparent Crustacea, and of the Medusæ and Polypes; of the latter he had carefully examined and figured species of between 40 and 50 genera.

The request was granted. For the next three years Huxley, nominally a navy surgeon, lived in London with his brother, George Huxley, devoting his time to the purpose just mentioned; and during the years 1851–3 he published the results of his labours in numerous papers, the most important of which are the memoirs on Salpa and Pyrosoma, on Appendicularia and Doliolum, and on the morphology of the Cephalous Mollusca, which appeared in the 'Philosophical Transactions' for 1851 and 1853.

One effect of the publication of these researches was to bring their author, almost at a single bound, into the first rank of English anatomists. Within eight months of his return, namely, on June 5, 1851, he was elected a Fellow of the Royal Society, and in the following year he received a Royal Medal of the Society "for his papers on the anatomy and affinities of the Medusæ," the other Royal Medal of the same year being given to Joule, the Rumford Medal to Stokes, and the Copley Medal to Humboldt.

In judging of the value to be allotted to these early papers by Huxley, the condition of comparative anatomy at that time in England must be had in mind. There were several zealous and active systematic zoologists, but in comparative anatomy itself, in the science of animal morphology, the only notable labourers were Richard Owen and John Goodsir. The latter, moreover, was working on so special a line, and in so special a manner, that the former may be said to have been almost standing alone. And his work consisted, on the one hand, of detailed descriptions, possessing the highest merit and greatest value, of the structure of an immense number of animal forms, and, on the other hand, of generalisations and speculations of a metaphysical kind, based largely on the philosophy of Oken, and, as time has since proved, of a fruitless, barren nature. The more sober method of determining the true homologies of animal structures, and the true affinities of animal forms, of which the criteria had been furnished by the labours of von Baer, and which was being fruitfully worked in Germany by Johannes Müller, the method which led the anatomist to face his problems in the same spirit in which the physicist faced his, was almost unknown, or at least unused, in England. It was, it is true, appreciated by Carpenter, but he at that time was much more of an expositor than an investigator. Of the value of this method, to the knowledge of which he must have been led by his solitary readings in his old Charing Cross days, Huxley's early papers came as a startling and convincing proof, and in the words with which the then President of the Royal Society, the Earl of Rosse, accompanied the presentation of the Royal Medal, it is not difficult, reading between the lines, to recognise the appreciation of a new spirit of anatomical inquiry, not wholly free from a timorous apprehension as to its complete validity. "In those papers you have for the first time fully developed their (the Medusæ) structure, and laid the foundation of a rational theory for their classification." "In your second paper, 'On the Anatomy of Salpa and Pyrosoma,' the phenomena, &c., have received the most ingenious and elaborate elucidation, and have given rise to a process of reasoning, the results of which can scarcely yet be anticipated, but must bear in a very important degree upon some of the most abstruse points of what may be called transcendental physiology."

Nor was it only in impersonal academic distinctions that Huxley found his worth recognised. In the scientific world of London he soon formed warm friendships, both among the older and the younger generations. By Edward Forbes, in whose nature there was much that was kin to his own, and with whom he had had some acquaintance before his voyage, he was at once greeted as a comrade, and with Joseph Dalton Hooker, to whom he was drawn at the very first by their common experience as navy surgeons, he began an attachment which, strengthened by like biological aspirations, grew closer as their lives went on. In the first year of his return, in the autumn of 1851, he made the aquaintance of John Tyndall at the meeting of the British Association at Ipswich; and the three, Hooker, Huxley, and Tyndall, finding how much in common were all their scientific views and desires, formed then and there a triple scientific alliance. Nor were older and more influential friends wanting; and these made repeated efforts to induce the Admiralty to at least contribute to the expense of publishing Huxley's researches. But in vain; and after three years the young naval surgeon, whose scientific abilities were thus giving trouble, was ordered to join a ship for active service. This he declined to do, and, though absolutely without private resources, boldly threw himself into a scientific life. For a year or so he appears to have maintained himself by his pen, fighting with it a double fight, labouring on the one hand to make the results of his inquiries known to the scientific world, and struggling on the other to secure his daily bread. A candidature about this time for the Chair of Natural History in the University of Toronto proved unsuccessful, as did also a like candidature for the Chair of Physics, in the same University, by his now close friend John Tyndall. But in 1854 his chance came. Edward Forbes, who held the posts of Palæontologist to the Geological Survey, and Lecturer on General Natural History at the Metropolitan School of Science applied to Mining and the Arts, subsequently called the Royal School of Mines, had just left these to fill the Chair of Natural History at Edinburgh, and Sir H. De la Beche, the then Director-General of the Geological Survey, offered both the posts to Huxley, who in June and July of that year had given lectures at the school in place of Forbes. Of this he has said himself,* "The former post (that of Palæontologist) I refused point blank, and accepted the latter (that of Lecturer) only provisionally, telling Sir Henry that I did not care for fossils, and that I should give up Natural History as soon as I could get a physiological post." As he himself has said, "there was very little of the genuine naturalist in me; I never collected anything, and species work was a burden to me. What I cared for was the architectural and engineering part of the business; the working out the wonderful unity of

* 'Autobiography,' p. 15.

plan in the thousands and thousands of diverse living constructions, and the modifications of similar apparatus to serve diverse ends." And there can be little doubt but that had a suitable opening offered itself, he would have thrown himself into a distinctly physiological career, and the advancement of morphology, due to him, would have been limited to the "Rattlesnake" work. But this was not to be. Though his leanings towards physiology pure and simple broke out at times, as in various lectures and addresses, and in the publication of the little work on "Elementary Physiology," and were shown all his life long in the helping hand and warm sympathy which he always gave to all physiological inquiries, and to all physiological workers, his work in life was destined to be mainly limited to morphology, and conspicuously to the elucidation of the fossils "for which he did not care."

The emoluments of the post of Lecturer on Natural History at the School were but scanty; but they gave Huxley a *pied à terre*, and, moreover, the sagacious De la Beche, foreseeing, it would seem, Huxley's future relations to fossils more clearly than he did himself, since he had refused the post of Palæontologist to the Survey, found for him in April of the following year, 1855, a special place as Naturalist to the Survey, by which a more suitable income was provided for him while an internal arrangement distinguished his duties towards the fossils in the Geological Museum from those of the proper Palæontologist to the Survey, Mr. Salter. Thus, though he had (to the great benefit of palæontology) official relations to the fossils of the Survey, relations which were more precisely defined a little later on—in 1861, by De la Beche's successor, Sir Roderick Murchison—Huxley was never technically Palæontologist to the Survey.

For science and the world at large the important thing is that by the appointment Huxley's career was assured. And the income which by this settled appointment and by other efforts he was able to secure, justified him, he thought, in offering a home to the lady whom he had first met in 1847, whom he had since seen fitfully in the Rattlesnake's visits to Sydney, and whom he had left in that city, in 1850. She at once came to England, and on July 21, 1855, they were married.

For many years afterwards Huxley's life, so far as outward things are concerned, was the ordinary life of a professional man of science in London. He did his duty as Lecturer at the School, and as Naturalist to the Survey, and those who listened to his lectures, and were capable of appreciating them, were witness to the zeal and energy with which he threw himself into the exposition of the new morphology. He as zealously carried out the other scientific duties which came in his way, whether accompanied by emolument or no. Very shortly after his appearance in London, on April 30, 1852, he

gave his first Friday evening lecture at the Royal Institution, on "Animal Individuality." It is encouraging to others to know that the superb lucidity and constrained biting eloquence which later on marked his public speaking were not present from the first. Though he was always helped by his striking skill as a draughtsman, his expositions of his themes seemed at first halting and imperfect. He had, like others, to learn that a public audience do not grasp with avidity a truth presented to them, irrespective of the manner in which it is laid before them. He talked at first "above the heads of his hearers," thinking that they could see things as clearly and as eagerly as he did himself. Indeed there is a tale told that after a lecture in a suburban athenæum, "On the Relation of Animals to Plants," in which he had discussed before his common-place audience some of the most fundamental biological problems, there was a general expression of the desire that the committee would never invite that young man to lecture again.

He held during the years 1856–57–58, the post of Fullerian Professor of Physiology in the Royal Institution, choosing for the title of the first two courses Physiology and Comparative Anatomy, for his face was as yet turned towards physiology. The third course, however, was "On the Principles of Biology."

Then, like most other young professional men of science, he had to eke out his not too ample income by labours undertaken chiefly for their pecuniary reward. He acted as examiner, conducting, for instance, during the years 1856–1863, and again 1865–1870, the examinations in Physiology and Comparative Anatomy at the University of London, making even an examination paper feel the influence of the new spirit in biology; and among his examinees at that time there was at least one who, knowing Huxley's writings, but his writings only, looked forward to the *vivâ voce* test, not as a trial, but as an occasion of delight. And he wrote almost incessantly for all editors who were prepared to give adequate pay to a pen able to deal with scientific themes in a manner at once exact and popular, incisive and correct.

And when he had done all this, to say nothing of the unpaid demands for the administration of science made on him as on all men of science, he had yet to do what was his main work, the prosecution of his inquiries. For the first few years he devoted himself mainly towards completing his "Rattlesnake" work. He followed up the paper on the "Medusæ," mentioned above, by various communications on the same theme, published in various channels, and embodied his results in the monograph "On the Oceanic Hydrozoa," the publication of which, delayed by the hope of obtaining assistance from my Lords of the Admiralty, did not take place until 1858. In 1853 there appeared in the 'Philosophical Transactions' his remarkable

memoir "On the Morphology of the Cephalous Mollusca," in which he investigated the structure of these animals according to the same canons which had guided him in respect to the Medusæ. Other papers, such as those on Doliolum, Sagitta, and Tethyia, were also the outcome of his labours at sea, but part of the programme which he sketched out in his letter to Sir W. Burnett remained for a long time unfinished, and part was never completed at all.

For new lines of inquiry were continually opening up to him. In the first place he was occupying himself with morphological problems presented by invertebrate forms other than those which came before him on the ocean, as shown by his paper on Aphis, published in the 'Linnean Transactions' in 1858, and by others; and with still greater energy did he throw himself into vertebrate morphology, preparing himself for the task by a careful study of vertebrate embryology, a subject then, in spite of the lead given by Allen Thomson, much neglected in England, though most successfully cultivated in Germany by Kölliker and others. One of the fruits of these labours was the Croonian Lecture, "On the Theory of the Vertebrate Skull," delivered November, 1858, in which, following up Rathke, he strove to substitute for the then dominant fantastic doctrines of the homologies of the cranial elements advocated by Owen, sounder views based on embryological evidence. He exposed the futility of attempting to regard the skull as a series of segments, in each of which might be recognised all the several parts of a vertebra, and pointed out the errors of trusting to superficial resemblances of shape and position. He showed, by the history of the development of each, that, though both skull and vertebral column are segmented, the one and the other, after an early stage, are fashioned on lines so different as to exclude all possibility of regarding the detailed features of each as mere modifications of a type repeated along the axis of the body. "The spinal column and the skull start from the same primitive condition, whence they immediately begin to diverge." "It may be true to say that there is a primitive identity of structure between the spinal or vertebral column and the skull; but it is no more true that the adult skull is a modified vertebral column than it would be to affirm that the vertebrate column is a modified skull." This lecture marked an epoch in England in vertebrate morphology, and the views enunciated in it, carried forward, if somewhat modified, as they have been, not only by Huxley's subsequent researches and by those of his disciples, but especially by the splendid work of Gegenbaur, are still, in the main, the views of the anatomists of to-day.

In the second place, led probably by the desire, which only gradually and through lack of fulfilment left him, to become a physiologist rather than a naturalist, he turned to histological themes, as shown

by his papers on the Teeth, the Corpuscula Tactus, and other topics in the 'Quarterly Journal of Microscopical Science' for 1853–54, and more especially by his elaborate paper on the "Tegumentary Organs" in 'Todd's Cyclopædia.' In a striking "Review of the Cell Theory," which appeared in the 'British and Foreign Medical Review' in 1858, a paper which more than one young physiologist at the time read with delight, and which even to-day may be studied with no little profit, he, in this subject as in others, drove the sword of rational inquiry through the heart of conceptions, metaphysical and transcendental, but dominant.

In the third place, the fossils, "for which he did not care," began, owing to his official position, to thrust themselves upon his notice. He found that they, after all, no less than living things, presented morphological problems; indeed, he soon began to see that in many cases they furnished not so much the problem as the key to the problem, sought for in vain among living forms. It was by a sort of irony of fate that the study of the despised fossils, working into the study of recent forms, led him to some of the conclusions by which he has most advanced the natural knowledge of the laws of life.

Nor was it long before the fossils began to exert their power. So early as 1855 he published, in conjunction with Salter, the official Palæontologist to the Survey, a paper "On the Affinities of a Crustacean from the Ludlow Rocks, Himantopterus" (now Slimonia), which was followed up four years afterwards, 1859, by a large memoir, 'On the Anatomy and Affinities of the Genus Pterygotus,' still regarded as the classic work on the subject. In the same year, 1855, he published a paper, "On the Structure of the Shields of Pteraspis," and three years later, in 1858, one on "Cephalaspis," thus clearly establishing for the first time the vertebrate nature of these remains. This was his introduction at once to fossil fishes, to which he was hereafter to pay much attention, and, through palæontology, to geology. He joined the Geological Society in 1856 (having in the same year become a member of the Zoological Society; the Linnean he did not join until 1858), and in 1859 became one of the secretaries. His work on the 'Devonian Fishes' he embodied in a memoir of the Geological Survey, published in 1861, which, though entitled a Preliminary Essay, threw an entirely new light on the affinities of these creatures, and, with the continuation published later, in 1866, still remains a standard work.

The decennium of the fifties may be taken as forming one stage in Huxley's career, for at the end of that period was published that 'Origin of Species' (the paper at the Linnean Society, by Darwin and Wallace, was read on July 1, 1858, and the book appeared on November 24, 1859), which had so great an influence not only on

his own scientific development, but also, and much more so, on his outward scientific activity.

He first met Mr. Darwin soon after his return from Australia, in 1851 or 1852, and the acquaintance rapidly ripened into a close friendship. Next to Lyell and Hooker, it was Huxley on whom Darwin leant for advice and help ; he came to speak of him playfully as " my general agent," and Huxley was one of the few who were privileged to learn Darwin's argument before it was put forth to the world.

From the very first Huxley had felt it as a duty laid upon him to expound by mouth and by pen the teachings of science in general, and of biological science in particular, to that large part of the world which lay outside science, knowing little of it and caring less for it; for there came to him very early the conviction that science was not merely the study of the few, for the sake of the intellectual gratification of the pursuit or the material benefits of the applications, but a thing to be known and, so far as may be, understood of all men as a sure guide for human life. It was this conviction which led him to devote much time and care to more or less popular lectures and addresses, inculcating the broad uses and value of science, such as the the Friday Evening Discourse (at the Royal Institution, which he gave in 1856) on " Natural History as Knowledge, Discipline, and Power." And he at once saw the far-reaching value of the lesson in evolution so forcibly expounded in the 'Origin of Species by Natural Selection.' Beginning with the striking review in the 'Times.' which appeared the day after the publication of the book, continuing with the Friday Evening Discourse, in 1860, on " Species, Races, and their Origin," following up in the same year with lectures to working men at Jermyn Street, on " The Relation of Man to the Lower Animals," and later on with the book on 'Man's Place in Nature,' and by many other utterances, he became known far outside the narrow circle of scientific workers as the powerful champion of what soon came to be called the Darwinian doctrine. One event especially brought his name before the public, and that was the memorable meeting of the British Association at Oxford in June, 1860, at which Samuel Wilberforce, the then Bishop of Oxford, in a discussion on the burning topic, wholly unaware of the new forces which had arisen in biologic science, and thinking to crush the new doctrine with one episcopal blow, was, by his ignorance, delivered into Huxley's hands and smitten by him hip and thigh. Before that the name of Huxley was but little known outside scientific circles and that section of London society which delights to entertain " eminent science ;" after that it became, and rapidly, to be quoted among the people as the name of a leader of men in science.

All this labour of exposition, and the various calls made upon him by his increasing fame as a man of science of unwonted brilliancy and

power, consumed much of his time; but he was now in the prime of his intellectual energy; financial cares, moreover, though still present, were gradually lessening; and during the next decennium he found opportunity to do some of the best of his purely scientific work.

Yet once more the fossils "were upon him," and indeed this decennium of his life was to a considerable extent a geological one. In 1862, at the close of his term of office as Secretary to the Geological Society, he was called upon, in the absence of the President, Mr. Leonard Horner, to deliver the Presidential Address, and in this he shattered an accepted doctrine of the geologists of the day, that a similar succession of organic remains in two distinct regions denoted synchronism of formation in the strata containing them. He introduced the word "homotaxis" to denote what alone was really shown by the facts, namely, that in each region the forms of life had followed each other in the same general order. It is worthy of note that in this same address, though at the time in the very flush of advocacy before the people of the doctrines laid down in the 'Origin of Species' as a reasonable hypothesis of a mode of evolution, he showed his anxiety that his scientific brethren should distinguish between a hypothesis for which there was much to be said, and a clear positive demonstration by the evidence of facts. Knowing his audience, he did not once allude to Darwin; he did not even think it necessary to speak of the imperfection of the geological record; aware that the younger geologists were likely to be carried beyond the evidence by the fascination of the general idea, he used his critical power to show that so far as the then knowledge went there was no case clearly made out by any fossil remains of any one form being progressively modified into others. But his attitude in this was an attitude of judicial caution only. In the address which, as President of the same Society, he had to give in 1868, dwelling on the catastrophic and uniformitarian schools of geological thought, he showed in a most powerful manner how the doctrine of evolution, taking in all that was good of each of the other schools, was destined to be the guide of geologists in the future. Further, in the Presidential Address which again he had to give in 1872, he pointed out how much even ten years had added to palæontological knowledge; so that now at least it might be said, in the case of the higher vertebrates, evolution had been proved as a historical fact. Since that date evidence has rolled in fast, and in his latest utterances, in the year before his death, he was able to point to the plain teaching of palæontological records as affording clear and absolute proof of evolution having taken place, a proof the validity of which could never be shaken by the fate which might in the future await the reasonable hypothesis of Mr. Darwin as to how evolution had taken place.

Though he was at this time prominent among geologists, and they

again and again profited by his advice and advocacy, as when, in 1862, he defended their claims for time against the attack of the physicists, his heart was not with them. Fossils to him were of little value as geological tokens compared with what they were worth as starting points for morphological inquiries. Just as he had used the Devonian fishes as guides for the investigation of piscine morphology, (which led him to the recognition of the peculiar structure of the crossopterygian fin), and, later on, to the distinction of the two types (hyostylic and autostylic) of piscine jaw-suspension, as well as to a comparison of the paired fin of the fish with the pentadactyle limb, so the fossil reptiles of the Elgin sandstones with dinosaurian and labyrinthodontian remains, as well as other amphibian, reptilian, and avian specimens, led him to far-reaching researches into reptilian and avian morphology. Into these researches—for the aspirations towards a physiological career had by force of circumstance by this time quite died away—he threw himself with characteristic ardour. Starting with the first papers on "Plesiosaurus Etheridgii, from Glastonbury," in 1858, on "Stagonolepis Robertsonii, from the Elgin sandstones," and "On some Amphibian and Reptilian Remains, from South Africa and Australia" in 1859, he was occupied during the whole of this decennium and the earlier part of the succeeding one in putting forth numerous papers dealing with the questions arising out of the fossils, which fate, in opposition to his early wishes, had thus thrust upon him. One great consequence of these researches was that science was enriched by a clear demonstration of the many and close affinities between reptiles and birds, so that the two henceforward came to be known under the joint title of Sauropsida, the amphibia being at the same time more distinctly separated from the reptiles, and their relations to fishes more clearly signified by the joint title of Ichthyopsida. At the same time proof was brought forward that the line of descent of the Sauropsida clearly diverged from that of the Mammalia, both starting from some common ancestry. And besides this great generalisation, the importance of which, both from a classificatory and from an evolutional point of view, needs no comment, there came out of the same researches numerous lesser contributions to the advancement of morphological knowledge, including among others an attempt, in many respects successful, at a classification of birds.

In the same decennium Huxley's scientific activity carried him into yet another field of inquiry. One interest of the doctrine of Natural Selection lay in its bearings towards the problem of the relation of Man to the lower animals; it offered a new guide for the study of the Natural History of Man; it awakened a new interest, which Huxley did not neglect to foster, in ethnological and anthropological inquiries. In all his utterances on the general bearings of

Mr. Darwin's work, as, for instance, in 'Man's Place in Nature,' he strove to make it clear that the same measure must be meted to man as to any other living organism; man was to be studied by the same methods as were animals. His published ethnological papers, beginning with a "Letter on the Human Remains found in Shell Mounds," in the 'Ethnol. Soc. Trans.' for 1863, are not numerous, nor can they be taken as a measure of his influence on this branch of study. In many ways he made himself felt, not the least by the severity with which, on the one hand, he repressed the pretensions of shallow persons who, taking advantage of the glamour of the Darwinian doctrine, talked nonsense in the name of anthropological science, and, on the other hand, exposed those who, in the structure of the brain or of other parts, saw an impassable gulf between man and the monkey. The episode of the "hippocampus" stirred for a while not only science but the general public. He used his influence, already year by year growing more and more powerful, to keep the study of the natural history of man within its proper lines, and chiefly with this end in view held the Presidential Chair of the Ethnological Society in 1869–70. It was mainly through his influence that this older Ethnological Society was, a year later, in 1871, amalgamated with a newer rival society, the Anthropological, under the title of "The Anthropological Institute." He had previously, in 1866–67, taken ethnology as the subject of the lectures which during two years he gave, holding for the second time the post of Fullerian Professor at the Royal Institution.

The year 1870 may be taken as marking a turning point in Huxley's career. Up to that time, though having more public demands made upon him than upon most men of science of the same age and standing—though engaged in regular lectures, both at the School of Mines and at the Royal College of Surgeons, at which, in succession to Owen, he was Hunterian Professor from 1863 to 1870, he was able to devote the greater part of his days to scientific inquiry. But about this time the change came. Though after this he did valuable scientific work, his time became more and more taken up by the accomplishment of duties thrust upon or taken up by him, some scientific, others not, and the hours which he could devote to quiet inquiry became fewer and yet more few.

In 1870 he filled the Presidential Chair of the British Association at the meeting at Liverpool, having been President of the Section D (Biology) at Cambridge in 1862, and again at Nottingham in 1866.

In October, 1872, part of, and later on the rest of, the Metropolitan School of Science (which in 1863 had become the Royal School of Mines), hitherto established in Jermyn Street, in conjunction with the Museum of Geology belonging to the Geological Survey, was moved to new buildings at South Kensington. The reorganisation and the

subsequent development of the school at South Kensington made Huxley's hands very full. In particular, he spent much time and energy in so organising a course of lectures which he was called upon to give to selected teachers of the classes examined by the Science and Art Department (he had for many years been examiner in zoology and physiology for the Department) as to make it a model of instruction in the general principles of biology. In that course he selected certain topics relating to the structure and life history of both plants and animals which he judged most fundamental and illustrative. He gave early in the forenoon an expository lecture, illustrated by a few diagrams, but chiefly his own admirable black board sketches; the rest of the day his four demonstrators, W. Thiselton Dyer, Michael Foster, E. Ray Lankester, and William Rutherford, did their best to make each member of the class see for himself or herself, so far as was possible, the actual thing of which the master had spoken. Each lecture, it need hardly be said, was a model of clear, incisive, suggestive exposition; the young demonstrators, who had as yet their spurs to win, did their best to make their work worthy of their chief; and whether they succeeded in teaching much or little, at least learnt much. This course of lectures, subsequently repeated with various modifications, became a pattern for biological instruction in England; it is not too much to say that it brought about a revolution in the teaching of biology in this country.

In 1871 the post of Secretary to the Royal Society became vacant, through the resignation of William Sharpey, and the Fellows learnt with glad surprise that Huxley, whom they looked to rather as a not distant President, was willing to undertake the duties of the office. For the ten years 1871–80 no small portion of his time and thought were (under first his old friend Hooker, and then Spottiswoode, as President) devoted to the interests of the Society; for he had formed a very high ideal of the duties of the Society as the head of science in this country, and was determined that it should not at least fall short through any lack of exertion on his part.

As was said above, one guiding principle in Huxley's life was the deep conviction that science was meant not for men of science alone, but for all the world: and that not in respect to its material benefits only, but also, and even more, for its intellectual good. It was for this reason chiefly that in 1870, on the new educational departure then made, he consented to become a candidate for the London School Board, and, being elected, made himself felt in that assembly as he did in others, bringing to bear on its deliberations all the influence of a man, not only acute and learned, but wise and just.

In spite of the great amount of work which, ever since he landed from the "Rattlesnake," he had daily got through, in the midst of all the distractions of London life, he was never really a robust man;

though he had no serious illness, he from time to time, from his youth upward, suffered from digestive troubles, and again and again abnormal products of the corporeal laboratory, running riot in his blood, gave rise to feelings of gloomy depression and lassitude, unfitting him for intellectual work. All this, however, he could hide from the world; but he could not hide the more serious failure of his health which the intellectual strain of so many duties of such different kinds now brought upon him, and in 1872 he was induced to take a long holiday in a visit to Egypt. He returned much refreshed, and, though his intimate friends confessed to themselves that, in point of bodily vigour, he was not the man he had been, the outside world saw but little evidence of this. In 1872 he had been elected Lord Rector of Aberdeen University, and in February, 1874, he delivered, upon his installation, his inaugural address. In the summers of 1875-76, he delivered at the University of Edinburgh the courses of lectures on Natural History, in the place of Wyville Thomson, who was absent on the "Challenger" expedition. In 1876 he paid a visit to America, delivering an address on "University Education," at Baltimore, on the occasion of the formal opening of the Johns Hopkins University, and giving three lectures on Evolution, at New York. His stay in the States had somewhat of the nature of a royal progress, for whatever city or town he visited, unless he managed to slip in unknown, something in the public way, an address or a reception, was expected of him.

In spite of all the professional and public demands made upon him—and to those already mentioned may be added those of Trustee of the British Museum, to which office he was elected in 1887, and the serving on Royal or other Commissions*—in spite of his now acknowledged fame as one who united profound scientific knowledge with an incisive power of speech sparkling with wit, such as few men of any kind of career possessed, leading his presence to be sought wherever it could be gained, and it was freely given whenever the advance of natural knowledge and the progress of sound thought seemed to him to call for it, he still found some hours left for his anatomical investigations. His most important contributions during

* These were—(1) Royal Commission on the Operation of Acts relating to Trawling for Herrings on the Coast of Scotland, 1862. (2) Royal Commission to enquire into the Sea Fisheries of the United Kingdom, 1864-65. (3) Commission on the Royal College of Science for Ireland, 1866. (4) Commission on Science and Art Instruction in Ireland, 1868. (5) Royal Commission upon the Administration and Operation of the Contagious Diseases Acts, 1870-71. (6) Royal Commission on Scientific Instruction and the Advancement of Science, 1870-75. (7) Royal Commission on the Practice of subjecting Live Animals to Experiments for Scientific Purposes, 1876. (8) Royal Commission to enquire into the Universities of Scotland, 1876-78. (9) Royal Commission on the Medical Acts, 1881-82. (10) Royal Commission on Trawl, Net, and Beam Trawl Fishing, 1884.

this decennium were in part continuations of his former labours, such as the paper and subsequent full memoir on Stagonolepis, which appeared in 1875 and 1877, and papers on the Skull. The facts that he called a communication to the Royal Society, in 1875, on Amphioxus, a preliminary note, and that a paper read to the Zoological Society in 1876, on Ceratodus Forsteri, was marked No. 1 of the series of Contributions to Morphology, showed that he still had before him the prospect of much anatomical work, to be accomplished when opportunity offered; but, alas! the opportunity which came was small, the preliminary note had no full successor, and No. 1 was only followed, and that after an interval of seven years, by a brief No. 2. A paper "On the Characters of the Pelvis," in the 'Proceedings of the Royal Society,' in 1879, is full of suggestive thought, but its concluding passages seem to suggest that others, and not he himself, were to carry out the ideas. Most of the papers of this decennium deal with vertebrate morphology, and are more or less connected with his former researches, but in one respect, at least, he broke quite fresh ground. He had chosen the crayfish as one of the lessons for the class in general biology spoken of above, and was thus drawn into an interesting study of crayfishes, by which he was led to a novel and important analysis of the gill plumes as evidence of affinity and separation. He embodied the main results of his studies in a paper to the Zoological Society, and treated the whole subject in a more popular style in a book on the Crayfish. In a somewhat similar way, having taken the dog as an object lesson in mammalian anatomy for his students, he was led to a closer study of that common animal, resulting in papers on the subject to the Zoological Society in 1880, and in two lectures at the Royal Institution in 1880. He had intended so to develop this study of the dog as to make it tell the tale of mammalian morphology; but this purpose, too, remained unaccomplished.

Richer, perhaps, even than preceding decennia was this one in scientific addresses and general lectures, in which his ripened judgment drew from his immense store of knowledge wise lessons for his younger brethren. Conspicuous among these was one on a theme in which he might feel justifiable pride, the Friday Evening Royal Institution Lecture in 1880, "On the Coming of Age of the Origin of Species."

The decennium of the eighties found him much as the previous decennium had left him, but with official and multifarious duties gathering still thicker round him. In 1881, the Royal School of Mines was incorporated with the newly established Normal School of Science (which later on, in 1891, came to be called the Royal College of Science), and Huxley exchanged the title of Lecturer on General Natural History for that of Professor of Biology. He

was also made Dean of the College, serving actively in that office up to the time of his retirement; indeed, even at the time of his death, he was nominally Dean. In 1881 he accepted, upon pressure, the duties of Inspector of Fisheries, which office he held till 1885, bringing at once exact technical knowledge and acute political sagacity to bear on problems of peculiar difficulty, and, as on other occasions, snatching the fruit of scientific inquiry out of the opportunities of routine business.

In 1880 he had felt it his duty to resign the office of Secretary to the Royal Society, but in 1883, upon the lamented death of William Spottiswoode, he was called upon, by the united voice of the Society, to fill the Presidential Chair. He gladly undertook the duties, for, as has been said, his ideal of the part which the Society ought to play towards that advancement of natural knowledge for which it was founded was of the highest. He looked forward to so using his position as to develop still further the Society's usefulness, and had in his mind plans for changes of a gradual and judicious kind which might safely bring this about. But it was not to be, the ill-health which had seemed to vanish in the seventies came back now with increased force, and in 1885 he felt himself bound to resign the post.

Indeed the ill-health now became grave; the strain caused by the long-continued painful illness of a favourite daughter, ending in her death in 1887, told heavily upon him. Symptoms of cardiac mischief, which had probably been slowly developing for a long time past, now became pressing; and in 1885 he resigned his official duties at South Kensington, retaining the title of Emeritus Professor and, at the solicitation of the authorities, the post, which he had held so long, of Dean of the College. His active connection with the Survey had ceased about 1881, though up to this time his name still appeared as that of naturalist to it.

An attack of pleurisy in 1886, followed by another in 1887, raised grave forebodings among his friends; but judicious care and an innate recuperative power restored him to temporary strength. He found great benefit to his health from occasional visits to Eastbourne, where he afterwards built himself a house, to which he moved in 1890, giving up his London residence.

His experience as Inspector of Fisheries led him to investigate and write, in 1882, an account of the saprophytic diseases of salmon; he also contributed short papers to the Zoological Society, and, in 1887, one to the Geological Society, on *Hyperodapedon Gordoni*, those fossils to which fate had led him against his will occupying his attention almost to the last. But one marked effect of his illness was to produce a condition in which anatomical research became a burden to him. Though he carried about him, as does every man of like calibre and experience, a heavy load of fragments of inquiry

begun but never finished, and as heavy a load of ideas for promising investigations never so much as even touched, though his love of science and belief in its might never wavered, though he never doubted the value of the results which further research would surely bring him, there was something working in him which made his hand, when turned to anatomical science, so heavy that he could not lift it. Not even that which was so strong within him, the duty of fulfilling a promise, could bring him to the work. In his room at South Kensington, where for a quarter of a century he had laboured with such brilliant effect, there lay on his working table for months, indeed for years, partly dissected specimens of the rare and little-studied marine animal, Spirula, of which he had promised to contribute an account to the Reports of the "Challenger" Expedition, and hard by lay the already engraven plates; there was still wanted nothing more than some further investigation and the working out of the results. But it seemed as if some hidden hands were always being stretched out to keep him from the task; and eventually another labourer had to complete it.

Not that the intellectual power was wanting, but that the mind could not work freely in the old fields and on the old lines. A new subject he took up with avidity. Attracted in his walks round Maloja in the Engadine, whither he had been sent for his health, by the various species of gentian, he threw himself with ardour into the study of that genus, and published in the 'Proceedings of the Linnean Society' a memoir dealing with the morphology of the gentian, and proposing a classification based on characters of distinct morphological value. As the work of one who, as he himself has said, had little or nothing of the naturalist in him, and recked little of species, and who, moreover, never had the opportunity of gaining that almost instinctive appreciation of the value of botanical characters which comes to those whose lives are spent among flowers, the memoir is in many respects a remarkable one.

But the new topics in which his mind now moved with the greatest freedom, were those of philosophy and, through philosophy, those of theology. Not that they were really new, for his mind had exercised itself in them from his youth upward, but it was a new thing to him to be able to give his undivided attention to them. And nearly the whole of his time in his retreat at Eastbourne, save that which was given to public demands, such as those of the British Museum, of the University of London, to the senate of which he had for some years belonged, and of the Marine Biological Association, which had been founded chiefly through his powerful influence, and maintained largely through his constant cordial support, was devoted to the study of philosophy and theology, indeed chiefly the latter, the results of his meditations being from time to time laid before the

world in addresses or magazine articles. Philosophy indeed was the occasion of his almost last appearance in public, when, at Oxford in May, 1893, he delivered the second Romanes lecture on "Evolution and Ethics." Not the very last. The last time a large audience gazed upon him was at the meeting of the British Association at Oxford in August, 1894, when, rising to second the vote of thanks to the President, the Marquis of Salisbury, for the Presidential address, he was received with a burst of applause of such a kind as seldom falls to the lot of any man of science or, indeed, to any man at all. A smaller but no less sympathetic and admiring audience greeted him when the Royal Society, at its Anniversary Meeting in November of the same year, bestowed on him the only token of regard left for it to give, but that a most fitting one, the recently established Darwin Medal. Two men, and two men only, had received it before him, Joseph Dalton Hooker and Alfred Russel Wallace; and the Society, in adding the name of Huxley to these, felt glad that it had been put in their power to do honour in their lifetime to the three who, next to Darwin, had had the greatest share in the eventful biological movement of the present age.

At the time he received the Medal his health seemed so good that his friends looked forward to some yet considerable lease of life for him, and, indeed, during the following winter he was cheerfully active in his philosophic and theologic studies, and, besides, continued to exert himself no little in the movement for a Teaching University for London, a matter he had greatly at heart. But, in the early spring, an attack of influenza, from which malady he had on former occasions suffered greatly, prostrated him. His illness was further aggravated by the attempts which he made to complete, in spite of it, a review, of which a part had already appeared, of the Right Hon. A. J. Balfour's work on 'The Foundations of Belief.' And, though he seemed after a while to rally somewhat, disease of the kidney, which, due primarily to his cardiac affection, had probably existed in a more or less latent condition for some time, assumed characters of great severity, pulmonary and pericardial complications followed, and after days of great suffering he expired on June 29, 1895. He was buried in the Marylebone cemetery at Finchley, to the north of London.

Titular honours had no attractions for Huxley, and it is no secret that he at a comparatively early date declined the offer of knighthood. At one time serious efforts were made in the direction of his being created a peer, but financial reasons, if none other, stopped them at the very beginning. Not that he was insensible to the value of a public recognition of his worth, for when, in 1892, Her Majesty was graciously pleased that he should become a member of the Privy Council, he accepted with pleasure so unwonted a signal of the recognition of scientific worth.

The world at large knew Huxley best perhaps in his special character as the fearless and effective exponent of Mr. Darwin's views, and the minds who love to dwell on the "might have been" might linger long questioning how soon those views would have moved the world as they have done had there been no Huxley to expound them. It knew him, too, in his more general character as one who taught to the multitude the power and value of scientific thought in words which, whether spoken or written, while flashing with wit, cut deep with incisive power, reaching ear and eye in the shape of pure manly English, the outcome of a strong, honest, clear-seeing mind; it listened gladly to high themes of science touched by him with an artist's hand; and if, at the close of the century, science stands high in public estimation, that is in no small measure due to Huxley's public utterances, and the close accord which his life showed to his words. But his influence on the world at large formed, as it were, only the diffusion circles of more direct rays of influence concentrated on the narrower field of science and the still narrower field of biology. The foregoing imperfect words have attempted to point out some of the more important steps in the advancement of biologic knowledge which are directly due to him; but these form only a part of what we owe to him. Whatever bit of life he touched in his search, protozoan, polyp, mollusc, crustacean, fish, reptile, beast, and man—and there were few living things he did not touch—he shed light on it, and left his mark. There is not one, or hardly one, of the many things which he has written which may not be read again to-day with pleasure and with profit; and, not once or twice only in such a reading, it will be felt that the progress of science has given to words written long ago, a strength and meaning even greater than that which they seemed to have when first they were read. There is not a biologist of the latter half of this century who has not been helped on his way, directly or indirectly, by some research or by some word of Huxley's. And though those who are coming after can never be fully aware of how great was the personal influence of the man outside his recorded words, the writings which do remain will serve to keep alive the memory of one who, while with his own hand he added many chambers to the growing building of biologic science, did almost as much by a life which taught both his comrades and lookers on the beauty, dignity, and power of natural knowledge.

His record within the annals of the Society is unique. Admitted a Fellow in 1851, he received a Royal Medal in 1852. From 1871 to 1880 he was one of the general secretaries. From 1883 to 1885 he was President. In 1888 the Copley, and in 1894 the Darwin, Medal were given him. The Society could no further go.

M. F.

Henri Ernest Baillon was born at Calais on November 29, 1827, and died suddenly at Paris on July 18, 1895. He was well known personally in botanical circles in this country, having been a frequent working visitor at Kew and the British Museum, to the writer's knowledge, for upwards of thirty-three years; and the extent, scope, and quality of his botanical work have made his name familiar to the botanists of all countries. The following announcement of his death and particulars of his life, most of which have already appeared in 'Nature,' were communicated to me by Mr. A. Franchet the well-known writer on the botany of Eastern Asia:—

"Je vous écris sous une bien pénible impression; M. Baillon est mort hier soir subitement. Dans l'après-midi il était venu au laboratoire selon son habitude. À cinq heures et demie il prit un bain; à six heures son fils rentrant de l'École de Médecine le trouva mort. On croit que le bain, un peu trop chauffé, a déterminé une congestion.

"C'est une grande perte pour nous et pour la botanique. S'il avait des ennemis implacables, il avait aussi des amitiés fidèles. Je ne doute pas que l'avenir ne montre que derrière un esprit, dont les manifestations parfois acerbes visait moins la personnalité que ce qu'il jugeait être l'erreur, se cachait un cœur sensible à l'excès. Il est un bon nombre de ses élèves pauvres qui savent de quelles délicatesses il savait entourer une aumône.

"Quoiqu'il en soit, c'était un grand botaniste; vous le jugez ainsi, n'est-ce pas?

"Ses quatres enfants vont se trouver dans la misère la plus profonde qu'on puisse imaginer. Ce qu'il n'a pas dépensé de sa fortune pour la publication de ses livres a disparu dans la gouffre des dettes de celle qui a porté son nom. Aujourd'hui il ne reste rien."

Having written for further particulars, the following reply was received:—

"Henri Baillon* est né à Calais le 29 novembre, 1827, d'une famille très honorablement connue dans la ville et dans la région. Il fit de très brillantes études au lycée de Versailles et commença ses études médicales à dix-sept ans. En 1854 nous le voyons Interne à l'Hôpital de la Pitié à Paris et ses travaux dans cet fonction, déjà très difficile à obtenir au concours, furent si brillants, qu'il obtint à l'unanimité la médaille d'or de l'Internat, c'est-à-dire la plus haute récompense qui puisse être décernée par la Faculté de Médecine. Son concours pour l'aggrégation fut un triomphe. Admirablement doué pour la parole, il tint véritablement sous le charme son jury tout entier, aussi bien par l'élégance de sa diction que par la pro-

* So far as I know, Baillon never used his second Christian name, or even the initial letter.—W. B. H.

fondeur de ses vues scientifiques. En 1863 il succéda à Moquin-Tandon dans la chaire de botanique de l'École de Médecine, et il a occupé cette chaire jusqu'à sa mort.

"Il ne fût pas membre de l'Académie des Sciences!

"Vous connaissez ses écrits; ils forment à eux seuls une petite bibliothèque. La veille de sa mort il me remettait le fascicule qui complétait le tome XIII de son 'Histoire des Plantes,' et la feuille 51 de son 'Bulletin de la Société Linnéenne.' À cette occasion il me donna la composition du volume XIV de 'l'Histoire des Plantes,' qui devait comprendre les Zingibéracées, les Musacées, et enfin les Orchidées, qui terminaient l'ouvrage au point de vue de l'exposition des familles. Mais ce volume devait aussi fournir l'exposé du plan de 'l'Histoire des Plantes,' de la methode suivie, des caractères sur lesquels il basait sa classification. L'absence d'un document qui eut été si precieux pour l'intelligence complète de celui de ces livres auquel il attachait le plus de l'importance, est une perte irréparable pour la science.

"Certains journaux de basse catégorie ont dit que Baillon s'était suicidé; c'est un mensonge. J'ai passé avec lui toute l'après-midi du mercredi. Il m'a non seulement parlé de ses projets d'avenir pour 'l'Histoire des Plantes,' mais aussi pour Madagascar, et aussi pour ses enfants. Il souhaitait tant vivre pour conduire son fils jusqu'à l'internat des hôpitaux, tout au moins! Et puis en me quittant il m'a récité, ce qu'il faisait souvent et très bien, une ode presque entière d'Horace, et une partie de cette ode Française, "La France a perdu son Orphée," etc. Cette ode il me l'a disait d'une merveilleuse façon au moins cinq à six fois par an.

"Mais je m'arrête, car je pourrais longtemps vous parler ainsi de lui."

I have given the foregoing letters, from one who knew Baillon intimately, in full, because the false rumours alluded to concerning his end were circulated in this country.

The allusion to Baillon's personal character will appeal to the sympathies of those who knew him on this side of the Channel. He was singularly amiable and courteous in his general behaviour;* yet he quarrelled with some of the foremost French botanists of assured position, which led to regrettable and undignified recriminations on his part, and resulted in closing the doors of the Académie des Sciences against him for ever. This embittered his life considerably, and rendered his relations with a section of the botanists of Paris almost unbearable.

In 1875 he was elected a foreign member of the Linnean Society of London, and in 1894 he received the same distinction from the

* From this point I have extracted entirely from my notice in 'Nature.'—W. B. H.

Royal Society. This gave him much pleasure, and consoled him, in some measure, for the implacability of his own countrymen. In 1866 he and a few others founded the Société Linnéenne de Paris. He was elected President, and continued to act as such until his death. For some years the Proceedings of this very small Society were published in Baillon's own periodical, 'Adansonia,' and then a 'Bulletin Mensuel' appeared, and has continued to appear down to the present time, entirely owing to the energy and industry of the President. This organ was not published, but distributed to the leading botanical establishments; hence, there is no record of Baillon's numerous articles therein in the Royal Society's catalogue of scientific papers. Yet, omitting these, the catalogue contains the titles of 230 of his papers, published between 1854 and 1883. But Baillon was a most prolific writer, and covered a considerable range, though systematic botany was his chief study. I need only name his 'Adansonia,' twelve volumes, 1866 to 1879; 'Dictionnaire de Botanique,' four volumes, 1876 to 1892; 'Histoire des Plantes,' 1867–95, and still unfinished. Baillon, too, was the only French botanist who occupied himself on the rich collections of flowering plants in Paris from Madagascar; being the author of the uncompleted 'Histoire des Plantes de Madagascar,' forming a portion of Grandidier's great work on Madagascar.

Baillon was one of the few existing botanists having a good knowledge of the phanerogamic flora of the world. As a writer, however, he was more critical than methodical, and many of his original observations and suggestions have been overlooked by botanists who have subsequently gone over the same ground. This is owing to the fact that the titles of many of his articles do not sufficiently describe their contents. Not infrequently a new genus or a new species is described in the body of a paragraph, and sometimes so informally, that only by careful reading is it possible to arrive at the fact. This often caused the author himself chagrin, especially as he was very sensitive and apt to believe that his work had been purposely ignored. I had almost forgotten to mention that the Euphorbiaceæ were one of his favourite families, and his 'Étude Générale du Groupe des Euphorbiacées" is one of his most finished works. I cannot here enter into a more critical examination of his works, but I may add that the illustrations almost throughout are of a high order of merit.

W. B. H.

William Topley was born at Greenwich, on March 13, 1841. His early education was obtained at local schools; he entered the Royal School of Mines in 1858, and remained a student there till 1861.

His active geological career began in 1862, when he was appointed

an Assistant Geologist on the Government Geological Survey of the United Kingdom. It was a stirring time in the geological world. Hutton had clearly enunciated, and Scrope had emphasized the doctrine that the ground had been carved into its present form by the action of subaerial denudation. But the old notion that the sea had been the main agent in this work still held sway. The question was now again being brought to the front by the vigorous advocacy of Greenwood, and debated by such men as the late Sir A. C. Ramsay, Mr. (now Sir) A. Geikie, and others of less note; but there was to be some fierce fighting before anything like unanimity of opinion could be arrived at. There are very few districts better suited to test the relative value of the two views than the Weald of Kent and Sussex, and it was thither that Mr. Topley was sent to work under the direction of Dr. (now Professor) C. le Neve Foster. In 1865 they furnished one of the most important contributions to the controversy in their paper, "On the Superficial Deposits of the Medway, with Remarks on the Denudation of the Weald." ('Quart. Journ. Geol. Soc.,' xxi, (1865) 443). By this memoir, Mr. Topley, at the very outset of his career, did much towards settling a long-debated point in geological speculation.

In addition to Messrs. Topley and Foster, several members of the staff of the survey took part in the mapping of the Weald; but on the completion of the work all but Topley had resigned, and to him was entrusted the task of writing the memoir descriptive of the area. It gives a masterly account of the great leading features in the structure of the district; liable to modification it has necessarily proved as to details, but it is and will long remain the standard work on the subject.

On the completion of his Wealden work, Mr. Topley was transferred to the Carboniferous district of Northumberland, and in 1880 was recalled to London to superintend the publication of the maps and memoirs of the survey at the office in Jermyn Street. Here and at this work he remained till his death.

But a sketch of Mr. Topley's official career, and a notice of some of his more important work in the domain of pure science gives no idea of the many sided character of his tastes and occupations.

From the first he was strongly attracted towards the practical side of geology. The aid that it can give to the agriculturist occupied a good deal of his attention; and a work on soils, for which he had collected much material, is one of the many projects which his early death prevented him from finishing. He was also an authority on water supply, on which subject his advice was sought by engineers. Among the schemes on which he was consulted, that for bringing water from Mid-Wales to Birmingham, was one of the most important. The writer was here associated with him, and can

bear witness that he was as much at home among the complicated and disturbed rocks of Palæozoic age, as when dealing with he simpler structures of Mesozoic formations.

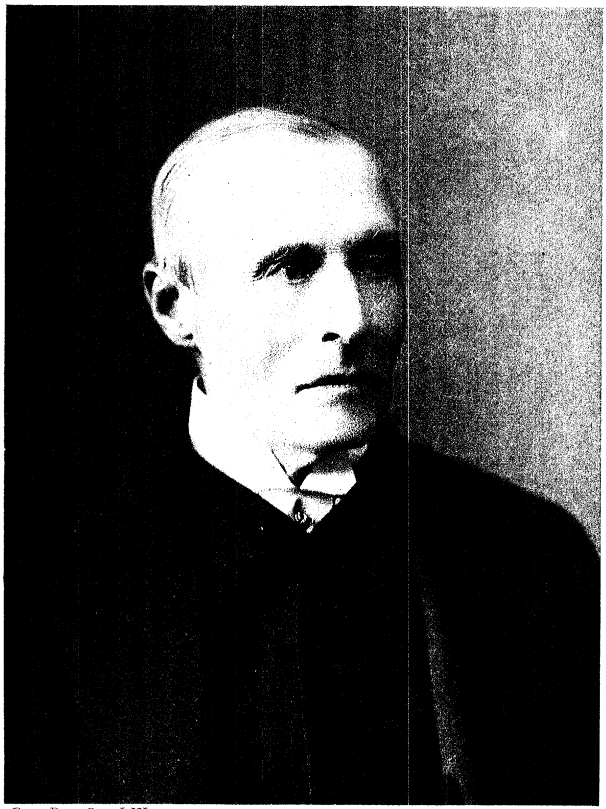
He wrote also on the bearings of Geology on Sanitary Science, on Petroleum, the distribution of Gold and Silver, and the Channel Tunnel. Much of his time was occupied in the share which he took in the work of scientific bodies. For fifteen years he acted as Secretary to Section C of the British Association; and he took an active part in the proceedings of the International Geological Congress, undertaking the laborious duties of Secretary when that body met in London in 1888, and he consented to superintend the British portion of the Geological Map of Europe which is being prepared under its auspices. He also, in conjunction with Mr. Sherborn, acted as editor of the 'Geological Record' during the last two years of its existence; a task as toilsome as it was thankless. He was elected a Fellow of the Geological Society in 1862; an Associate of the Institute of Civil Engineers in 1874; and a Fellow of the Royal Society in 1888.

A goodly record this of work done, or cut short only by the hand of death. Yet it was not to science alone that Mr. Topley's leanings inclined. His appreciation of art in all its branches was a source of pleasure which he delighted to share with those friends whose tastes lay in this direction.

But it was not his indefatigable industry nor his broad culture that endeared him to those who knew him best. In spite of the poignancy of the regret which his unexpected removal must engender, they can look back with pleasure to that sunny geniality, which made him the best of companions; to the unselfishness which led him, no matter at what inconvenience to himself, to give with open hand whatever help he could whenever it was asked for; and to the warmth of heart which was a source of happiness alike to himself and to those who shared his friendship.

The disease which landed him in an early grave appears to have been brought on by drinking contaminated water during a visit to Algiers in 1894. He hurried home more dead than alive, and reached it only to die.

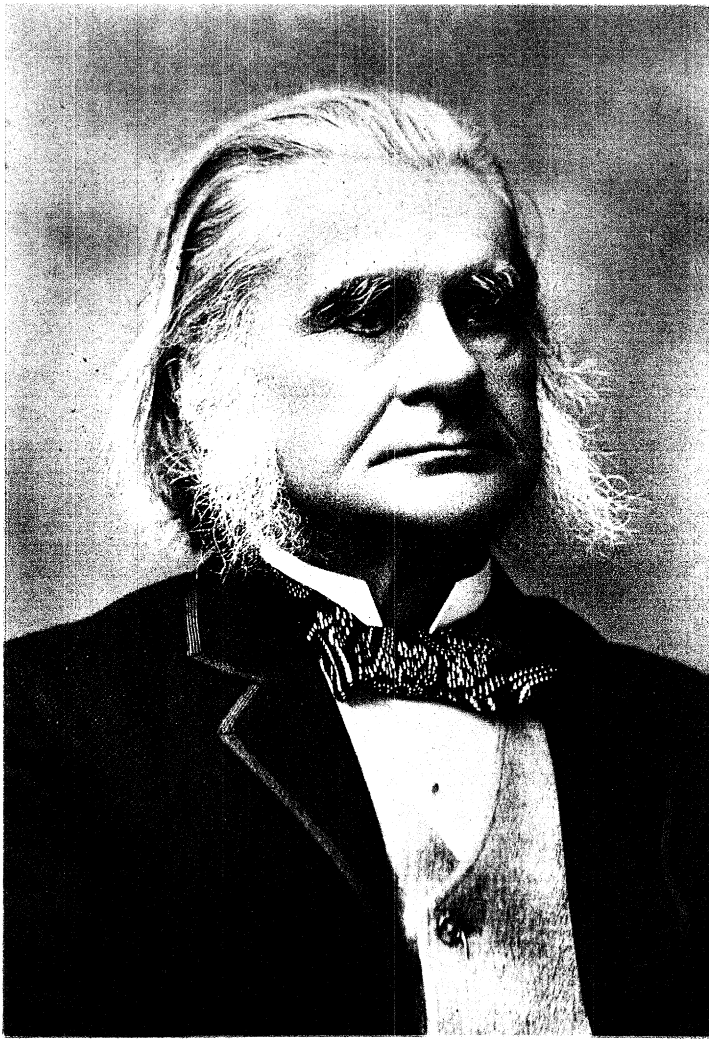
A. H. G.



Proc. Roy. Soc. LIX.

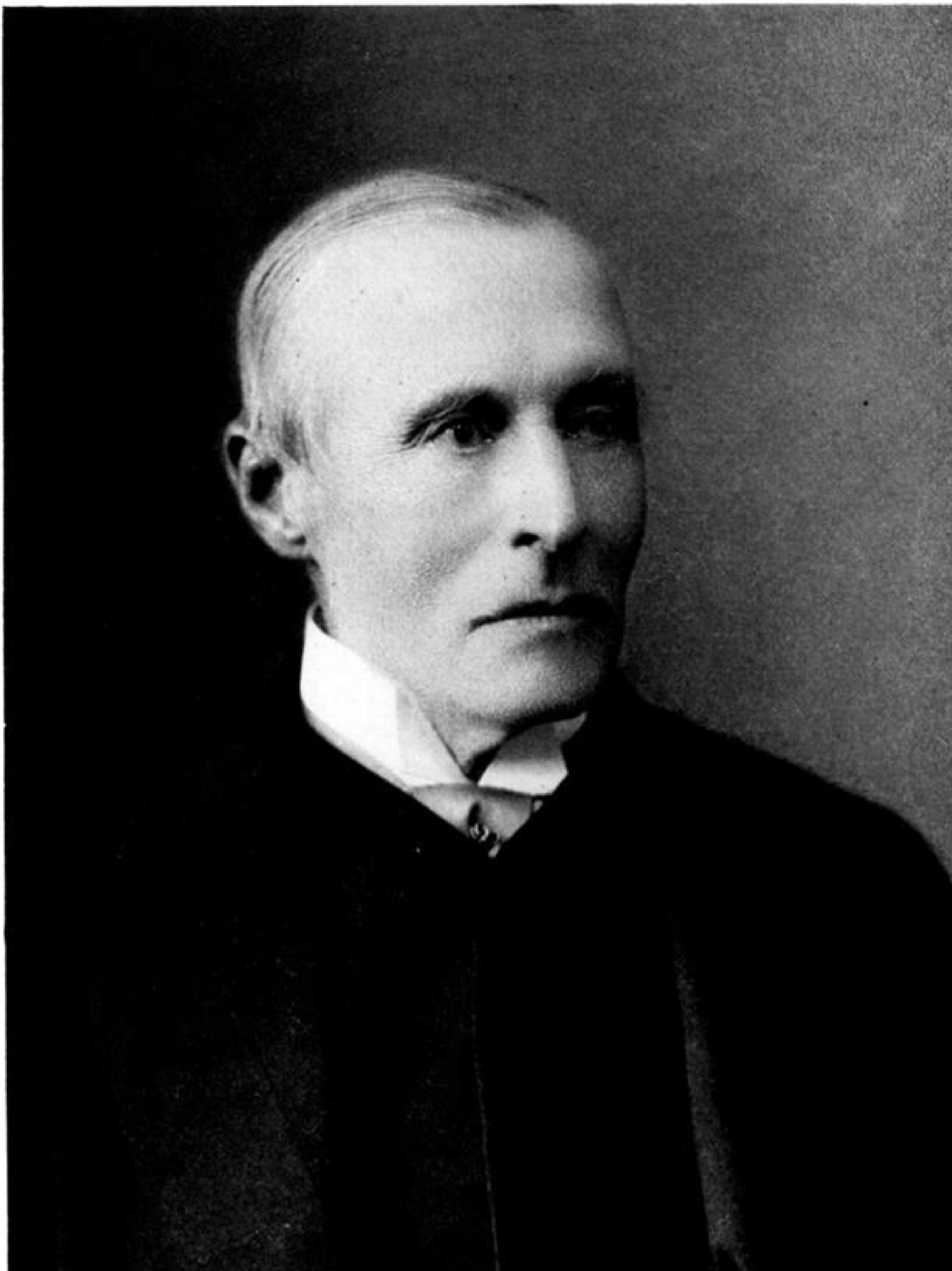
James Cockle.

A.D. 1892. AET. 73.



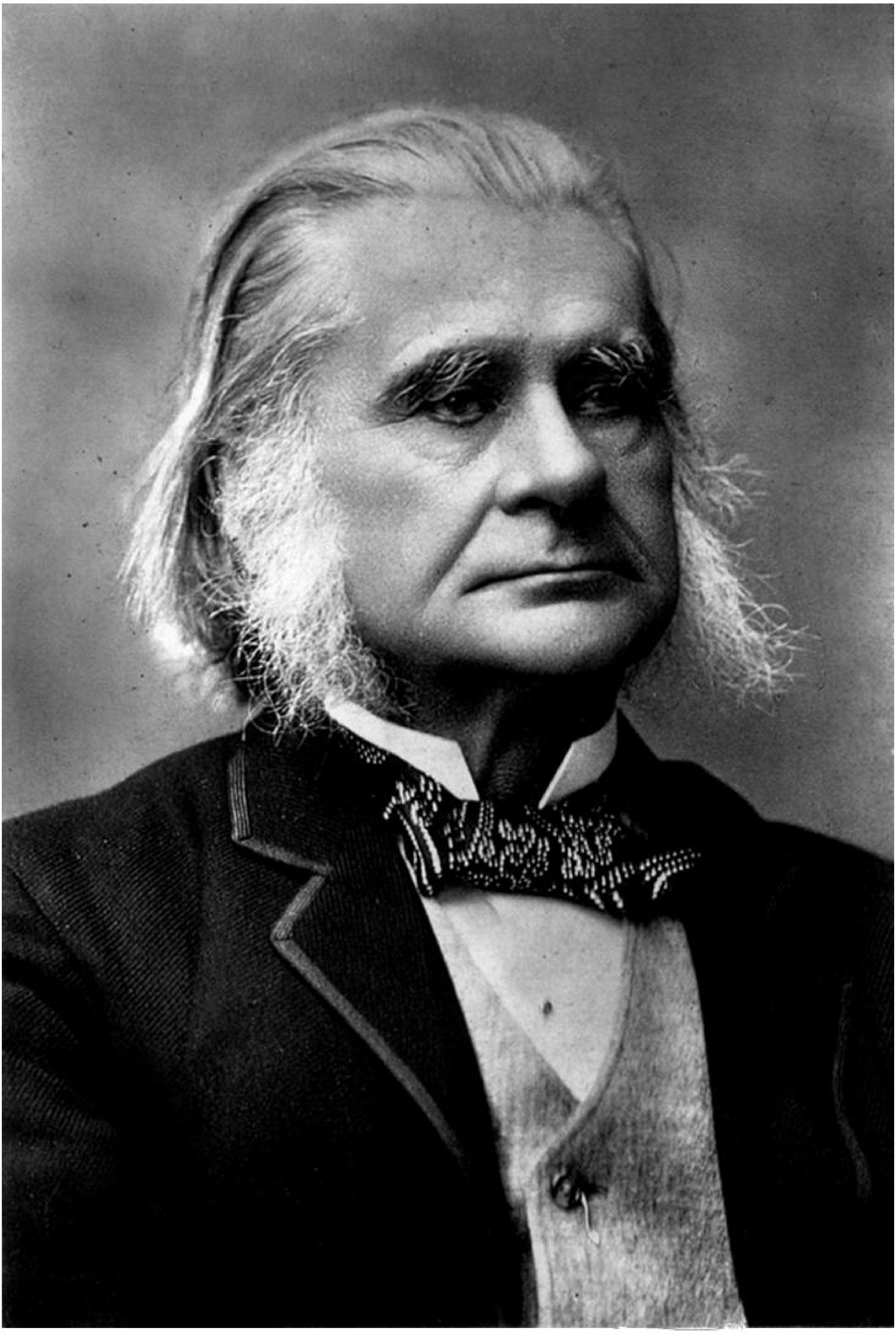
15TH MAY, 1893. ÆTAT. 68.

(From a photograph by Mayall & Co.)



James Cockle.

A.D. 1892. AET. 73.



15TH MAY, 1893. ÆTAT. 68.