OBITUARY NOTICES.

HARFORD MONTGOMERY ATKINSON.

1870—1935.

HARFORD MONTGOMERY ATKINSON was born in Belfast on June 30th, 1870, being the eldest son of Mr. and Mrs. John H. Atkinson of College Gardens in that city. Until his 14th year he was educated privately and afterwards for three or four years at Dunheved College, Launceston, Cornwall, and at Craigmore College, Bristol. From 1891 to 1894 he studied at University College, Aberystwyth, and during this period graduated B.Sc. in the University of London. He then returned to Dunheved College as mathematical master and left there in 1896 to study chemistry in Germany. In 1897 he entered the University of Göttingen and worked under Wallach until 1899 when he took his Ph.D. degree. His inaugural dissertation was entitled " Beiträge zur Kenntniss der Oxaline und über einige Derivate der unsymmetrischen Dipropyl- und Diamyloxaminsäuren."

In 1900 Atkinson returned from Germany and entered the works of the British Aluminium Company, first at the Alumina Works at Larne Harbour, Northern Ireland, and then at the Company's rolling mills at Milton, Staffordshire, where he remained till 1903. In that year he became head of the Chemical Department of the Technical Institute, Limerick, and taught there until 1915 when he came to England to take up munition work. He was first at Queen's Ferry and later for a short time at Waltham Abbey. In June, 1917, with the permission of the Ministry of Munitions he was engaged by the Milton Manufacturing Company and carried out work on the electrolytic production of chlorine. In this capacity he proved a skilful and accurate worker with a wide knowledge of his subject. His managing director, Mr. D. M. Rogers, mentions an accident which occurred to Atkinson about this time when a bottle of ammonia exploded in his face, damaging both eyes. Although suffering acutely, he was very courageous and non-complaining and during his three weeks at Moorfield's Eye Hospital he was a model and cheerful patient.

In March, 1918, a vacancy occurred at the Finsbury Technical College owing to the death of Mr. F. W. Streatfeild and as I had known Atkinson since his Limerick days in 1912 and had fully recognised his sterling qualities as teacher I was very pleased to be able to recommend him for this post, which he held until the College was closed in 1926. As a demonstrator he was highly appreciated by his students, for with his modest self-effacing manner he was a fitting successor to Streatfeild.

He next made a three years' contract (1928—31) with the British Celanese Company and worked for them first at Spondon and then in London, acting chiefly as an abstractor of all chemical literature and publications likely to be of interest in the manufacture of artificial silk. In this advisory capacity his wide knowledge of chemistry and technical literature was of considerable help to his colleagues. A few years ago he declined any further extension of his contract and retired from active chemical work.

In addition to chemistry, Atkinson was greatly interested in literature and music, both English and foreign. He was at one time a keen photographer. Having travelled extensively—walking and cycling—throughout the British Isles and in France, Germany and Switzerland, he had made a special study of scenery in its relation to geological formations. After graduation Atkinson made but little contribution to pure chemistry; his life work lay in the direction of technical chemistry and teaching. The annual inspections (1912—1915) of his department at the Limerick Technical Institute, which I made for the Department of Agriculture and Technical Instruction for Ireland, convinced me that he was a capable teacher with a thorough and comprehensive grasp of his subject. These high qualifications were so far appreciated locally that when he volunteered for war work his post was kept open until 1919.

Atkinson had a charming personality, conscientious, sympathetic and unassuming with a keen sense of humour.

Atkinson's health began to fail in 1934 and after a short illness he died on January 5th of the present year. He was unmarried and is survived by a brother and a sister.

G. T. MORGAN.

WILLIAM BARLOW.

1845—1934.

WILLIAM BARLOW was born in Islington, London, on August 8th, 1845, and inherited from his father, Frederick Barlow, a business dealing with estate and building property; by the exercise of notable acumen in affairs he realised the business and thus found himself early in life possessed of considerable means. Barlow was educated privately; he had a taste for physical science and marked mathematical talent, but cultivated the latter unsystematically and perhaps rather too exclusively.

Barlow thus found himself in his early thirties with an independence, with a genius for handling geometrical problems of a particular kind, and with ample leisure to devote to the study of crystal structure, which had become the subject of his choice. He had not, however, received that rigid disciplinary training through which most students of physics and chemistry acquire a broad sense of contemporary knowledge of the physical universe. In some respects this was a hindrance but in others an advantage; it left a powerful intellect unhampered by authority and led a logical mind to pursue its inquiries into difficult and obscure paths which might intimidate the more conventionally trained. Towards 1888 Barlow came into contact with Prof. H. E. Armstrong, from whom he received much encouragement and help; he also met Mr. (now Sir) H. A. Miers and the writer and acquired from them most of his knowledge of formal crystallography. He afterwards spent some time with his family in Germany and made the acquaintance of Paul Groth, then the leading German crystallographer, occupying himself with the geometry of crystal structure; on returning to England he devoted himself to crystallographic work of a theoretical kind until his death at Stanmore on February 28th, 1934.

In summarising the work of an unconventional genius, it is not easy to proceed chronologically; Barlow did not attack problems in the order which they would naturally take in a text-book or in the present notice. His work on the homogeneous partitioning of space may therefore be first reviewed.

For a century past the view has prevailed that crystal structure consists in the similar repetition throughout space of identical units without regard to their shape or constitution. Continuing earlier work by Bravais and others, Sohncke in 1879 introduced the idea of a regular point-system as one in which the pencils of lines drawn from each point of the system to all the remainder are congruent with each other; the regular point-systems, if classified according to the position and nature of their axes of symmetry (whether screw-axes or axes of rotation), are 65 in number. The 65 Sohncke systems, if built up of mathematical points, do not account for all the types of symmetry represented by the 32 crystal systems; it will be seen at once that the structure of hemimorphous crystals, in which a polar axis is present, cannot be described by a Sohncke system without some further assumption, such as that of polarity of the points or of the component atoms or molecules.

The development of Sohncke's work needed to provide a complete geometrical theory of crystal structure was undertaken independently by Schönflies, Fedorow, and Barlow; all three solved the problem but by different methods, and the line of attack adopted by Barlow may be now briefly indicated. Each Sohncke system is characterised by certain coincidence movements, these being translations and rotations about an axis of symmetry, which leave its appearance unchanged; further, a number of the Sohncke systems are enantiomorphous, that is, not identical with their mirror-images. Barlow duplicated the enantiomorphous Sohncke systems by intercalating the mirror-image in such a way that the coincidence movements of the two component point-systems coincide; he worked out the geometrical methods, three in number, by which this duplication can be effected. The 65 Sohncke systems thus became increased by another 165 to a total of 230; these are known as the 230 space-groups and represent all the types of symmetry possible in crystal structures.

Each of the 32 crystal systems corresponds to one or more of these space-groups. With the proof that the space-groups number 230, the geometrical theory of crystal structure becomes practically complete and the foundation is provided upon which any mechanical or physical theory of crystal structure must be erected. Whilst the methods used by Barlow in carrying out this difficult and laborious piece of work are perhaps less elegant than those of Schönflies, they offer certain advantages by the lucidity with which they reveal the geometrical properties of the space-groups.

Although Barlow published his work on the space-groups in 1894, he had for long been engaged on the second part of the problem of crystal structure, that of the mechanical nature of the structure (*Nature*, 1883, **29**, 183, 205). He assumed that equilibrium requires the atoms composing a crystal structure to be arranged in closest packing and showed that two closest-packed homogeneous assemblages of equal spheres exist; one of these has full cubic symmetry and is known as the face-centred cubic packing, whilst the other has full hexagonal symmetry. It is indicative of the difficulties inherent to problems concerning space partitioning that so acute an observer as the late Lord Kelvin concluded that Barlow's hexagonal closest-packing of equal spheres is not a simple homogeneous assemblage (*Proc. Roy. Soc. Edin.*, 1888—1889, **16**, 712). Modern X-ray analysis has now shown that most of the crystalline metals possess one or other of these closest-packed structures, although some are in the looser body-centred cubic packing. The recognition that equilibrium demands that similar spherical atoms shall arrange themselves in one or other of the two closest-packed assemblages, and that these occur in many of the metals, was the first definite success achieved in associating specific geometrical structures with specific crystalline substances; the importance of this result has been too little appreciated.

In his paper of 1883, Barlow discussed the crystal structure of diatomic compounds, and suggested as one possibility for sodium chloride a body-centred cubic arrangement in which one kind of atom lies at the cube centres and the other at the corners; this structure has now been shown by X-ray analysis to belong to cæsium chloride but not to sodium chloride. It is of interest to recall Sohncke's objection to this structure; he says (*Nature*, 1884, **29**, 383): " Thus eight atoms of Na stand in exactly identical manner around an atom of Cl (and also eight atoms of Cl around an atom of Na). The atom of Cl seems consequently to be in equally close connexion with eight atoms of Na; it has exactly the same relation to those eight atoms. It appears therefore as octavalent, certainly not as univalent; for it would be entirely arbitrary to suppose any two neighbouring atoms of NaCl in an especially close connexion and to take this couple for the chemical molecule of NaCl. By this example we see that from Mr. Barlow's point of view both the notion of chemical valency and of chemical molecule completely lose their present import for the crystallised state." This, which was an objection fifty years ago, is now regarded as one of the merits of the accepted cæsium chloride structure; Barlow's reply to Sohnke (*ibid.*, p. 404) states the modern view.

Barlow expanded his earlier notions on crystal structure in a long paper entitled " A mechanical cause of homogeneity of structure and symmetry " published in the *Proceedings of the Royal Dublin Society* for 1897 under the auspices of Prof. G. F. Fitzgerald; this provides a great deal of information as to possible symmetrical structures. Later, with the present writer, the conception was introduced that the atoms, supposed spherical, occupy volumes in the crystal structure proportional to their valency and, in papers published between 1906 and 1910, a large mass of experimental crystallographic data was reviewed. It was found possible, with the aid of the closest-packing valency volume hypothesis, to correlate many morphotropic relations with chemical constitution and crystal structure.

In 1912, however, the first observations on the diffraction of X-rays by crystalline substances were made and opened the way to direct methods for determining structure; these, brilliantly handled by W. H. and W. L. Bragg and their followers, have furnished precise experimental data as to the arrangement of the atomic centres in a vast variety of solid structures. It is now clear that Barlow's mechanical theory was stated in too simple a form to be applicable to any but the most simple cases; it seems now impossible that crystal structures are, in general, close-packed assemblages of spherical atoms. In this connexion it is significant to note that if the cubic closest-packed assemblage of equal spheres is symmetrically partitioned into tetrahedral groups of four spheres, the centres of these tetrahedral groups form the well-known Bragg structure for diamond; the diamond may thus be pictured as a close-packed assemblage of atoms which have the symmetry elements of the regular tetrahedron. Although X-ray analysis has increased our knowledge of crystal structure in an astounding way and has proved a most powerful tool, it has not led to a mechanical theory of crystal structure; it reveals the atomic arrangement but offers no reason why the component atoms seem to be closely packed in some crystalline structures and only loosely in others. The required mechanical theory of crystal structure may be found in some kind of generalisation of Barlow's conception of equilibrium conditions.

Barlow was elected into the Royal Society in 1908 and was president of the Mineralogical Society from 1915 until 1918. He was a man of simple tastes, very happy in his family life and happy in his friends; he was an expert cabinet-maker and this was helpful in the construction of complex models of crystal structures. It was never easy to follow his train of thought because he invented his own ways for attaining results; thus he rarely used the classical methods of spherical trigonometry in crystallographic calculations but devised special ones of his own for each case which arose. Whilst Barlow's friends will remember his single-mindedness and his kindliness of heart, he will always rank among the master builders of the geometrical theory of crystal structure.

W. J. POPE.

HUBERT VERNON BETTLEY-COOKE.

1885—1934.

HUBERT VERNON BETTLEY-COOKE, who died at Canterbury, Sydney, N.S.W., on October 29th, 1934, at the age of 49 years, was born at "The Hollies," Winsford, Cheshire, England, on April 16th, 1885. He was the eldest son of Mr. Frank Bettley-Cooke, grandson of Surgeon Lieut.-Colonel John Blackburn, M.D., and nephew of Drs. V. K. and E. W. Blackburn of Barnsley, Yorkshire, and was educated at "The Holme," Upper Norwood, London. With a view to qualifying in medicine he continued his studies at the London Hospital, after matriculation, but, being blind in one eye, was reluctantly compelled to desist after three years owing to the excessive strain upon the sound eye. He then served 8 years with Evans, Sons, Lescher and Webb, after which he left for Sydney, Australia, in 1915—where, on arrival, he accepted an appointment as Dyes Chemist to C. H. Slade & Co., Ltd., for 7 years and subsequently entered the service of Bostock's Polishes Ltd. The last 8 years of his life were spent with Abel Lemon & Co. as chemist and technical adviser to this firm's clients.

In addition to his Fellowship of the Chemical Society, London, he was a member of the Royal Society of New South Wales for 15 years, being Hon. Secretary to the Section of Industry for the last 7 years. He was also an Associate of the Australian Chemical Institute for 14 years, a member and Committee man of the Sydney section of the Society of Chemical Industry and for about 17 years a member, councillor and past-President of the Sydney Technical College Chemical Society.

By nature a thoughtful kindly man, of frank and friendly disposition, he was always ready to help others or assist in any movement for the benefit of the chemical profession; although not given to the publication of papers, he contributed liberally in the way of addresses, exhibits and demonstrations to the meetings of the various local Societies.

His leisure time was spent in a private laboratory at his home upon problems connected with his daily avocation and, although weakened physically by a severe illness of comparatively short duration, he endeavoured to complete an investigation a few days before he finally succumbed to heart failure.

He is greatly missed by his many colleagues, but much more so by the devoted wife who survives him.

R. W. CHALLINOR.

JULIUS BEREND COHEN.

1859—1935.

Julius Berend Cohen and his twin brother Adolf were born in Manchester in 1859. Their father was Sigismund Cohen, one of the numerous Germans who migrated to this country to take up the career of merchant shipper. It was his expectation and his desire that his twin sons should enter what had become his prosperous business. After early education in a day school at Manchester, they attended a general matriculation course at Owens College from 1875—76. With a view to their preparation for business, one of the twins, Adolf, spent a year in Lisbon and the other, Julius, a year in Paris. The following year the brothers entered their father's business, but it appealed only to Adolf. Julius had in his school days practised experimental chemistry in school-boy fashion at home and acquired a strong interest in science. After a year of his father's business he decided to prepare for a career in chemical industry and returned to Owens College for two years, 1878—1880, principally to study chemistry.

By happy circumstance, as the future showed, Arthur Smithells * was at this time working in the chemistry department and Cohen and he struck up an acquaintance which rapidly ripened into intimate and life-long friendship. At the end of the two years' course of qualitative and quantitative analysis Cohen reverted to industry, entering the works of the Clayton Aniline Company. This seems to have been one of the few unhappy periods of his life. In an article in the Journal of the Old Owensian Association describing some of his early reminiscences, Cohen said of this experience: "We worked there in a noxious atmosphere of fumes and in indescribable mud and filth (no paved roadways). More unhealthy, dismal and repulsive surroundings it is difficult to conceive." After two years of what must have been in great part mental torture, a fortunate reprieve occurred. In 1882 Smithells had decided to go to Germany for further study. Cohen showed pathetic envy that his friend should be able to do this and when the latter suggested that they should go together the idea was too attractive to be put aside. Cohen's father at first proved reluctant, but Smithells joined in the efforts at persuasion and consent was finally given. A close mutual friend also agreed to make one of the party. This was undoubtedly a turning point in Cohen's career, for in Germany he received that first inspiration which he was in later days so successful in transmitting to his students.

In that day there was a brilliant array of German chemists from which to choose—Kekulé, Hofmann, Fittig, Bunsen, Lothar Meyer, Baeyer, Wislicenus, Emil Fischer—and under Roscoe's advice the trio proceeded to Baeyer's laboratory in Munich, a week being spent in Paris on the way. Arrived in Munich, lodgings were sought out, the addresses put into a hat and lots drawn. Smithells had the pick of the bunch, Frl. Kolb, a woman of splendid character and rich in all domestic virtues, who in her earlier days as an operatic artist had been particularly befriended by Franz Liszt. Cohen and the third member of the party were less fortunate and had soon to change their quarters. A year later when Smithells left Munich to go to Heidelberg, Cohen went to live with Frl. Kolb and was with her for the rest of his stay. From that time on she had a line of students all connected with Cohen and Smithells either by friendship alone or as pupils. Their gratitude for her kindness and their affection for her personally caused them to raise a small pension to assist her in later years when her health failed.

At that time in Munich the chemical department consisted of four large laboratories, two devoted to inorganic and two to organic chemistry. In one of the latter the students were engaged in making a variety of simple organic preparations. H. von Pechmann was in charge and that was the laboratory in which Cohen first worked. Although at Owens College there was a separate Professor of Organic Chemistry—the distinguished Carl Schorlemmer—the course in this subject consisted only of lectures. It was therefore with considerable surprise that Cohen found some seventy or eighty students in the two organic chemical laboratories in Munich devoted to its practical study.

The laboratory hours were from 8 a.m. to noon and 2 p.m. to 6, and there was through-

* The writer is greatly indebted to Professor Smithells for the recollections of student days.

out an atmosphere of steady industry. There is no doubt that in spite of the long hours Cohen and his friends found themselves in a most genial environment. Amongst their fellow students at Munich were W. H. Perkin, Bamberger, Curtius, Duisberg and Friedländer.

On account of its present-day interest one incident of these student days may be mentioned. The Manchester trio came to know in social life of a private dancing society called the " Euterpe Gesellschaft." They decided to join it, but got no further when it was discovered that one of the rules was to the effect that " Juden haben keinen Eintritt." Notwithstanding this, in an article on his student days in Munich which Cohen wrote a short time before his death he spoke feelingly of his happiness there—" . . . in spite of my obviously Jewish name, I received every kindness and consideration from the staff and from my fellow-students and in short from all with whom I came into contact."

Cohen remained for two years in Munich and before returning to England took his Ph.D. degree. The one unhappy event which cast a shadow over his stay there was the death of his twin brother, which caused him to go through the special pangs of bereavement characteristic of such relationship.

On returning to Manchester he was appointed a Demonstrator in Chemistry at Owens College. In the second year laboratory he found that the old system of quantitative analysis of a series of metallic salts was still followed and, being anxious to extend the teaching to practical organic chemistry, he introduced a short course of organic preparations. It was at this time that the idea of writing a student's handbook on the subject was conceived and in 1887 his small " Practical Organic Chemistry " was published by the Macmillan Co.

In 1885 Arthur Smithells had been appointed Professor of Chemistry at the Yorkshire College, Leeds, and being desirous of seeing a real department of organic chemistry established, the cherished hope of the two friends was realised in 1891 when Cohen joined the College as Assistant Lecturer in Organic Chemistry. He was promoted to a Lectureship in the following year. The venture was highly successful. Although the premises found for Cohen were at first only meagre and he had no private room or laboratory, he made the best of them. The organic chemistry department grew so rapidly under his inspiration and guidance that provision for the students had to be made by dividing the large chemical lecture theatre and using the dismantled back section as a temporary home. It was in this laboratory that a great part of Cohen's work was done. Being an improvisation gave it something of the interest of a home-made abode and there was a tendency of the organic chemistry students who worked in it to mark themselves off as a peculiar people or an upper class. An annual " Organic Dinner " was established and the menu on these occasions was embellished with amusing cartoons for which Cohen was responsible. In this laboratory Cohen had his own bench in one corner —he shunned the interruptions of a " private room "—there were research students at other benches and students just beginning their organic preparations or doing organic analysis at others. Over all a bust of Liebig looked down upon the scene. Those who were privileged to work in this laboratory will never forget its good fellowship, contact with research work of various kinds and above all the kindly example of Cohen himself. It was as if Cohen had been able to transfer to Leeds some of the atmosphere that had so stimulated him at Munich where the traditions founded by Liebig still held sway. When in 1904 the Yorkshire College became the University of Leeds, it was possible to give Cohen the position long overdue, that of a full professorship of organic chemistry. This change of status made no difference to his mode of life. His heart was in his laboratory and during normal University hours he was almost always to be found there, either carrying out some chemical manipulation himself or instructing his students. Committee work or meetings which took him away from his laboratory he hated.

From the time of his appointment as Lecturer until he retired from his Chair in 1924, he gave all the lectures in organic chemistry. They consisted of two courses, General and Honours; but in addition he took over a course of lectures to honours students on the history of chemistry and became a serious student of that subject. One of his investigations in this sphere, which was never published except as a pamphlet for private distribution, showed how the work of Mayow on combustion had been exaggerated. When it is remembered that for many years Cohen had no demonstrator to assist in the practical teaching, it can well be realised that as a teacher he carried a considerable burden. He was a clear lecturer, severely practical and direct and devoid of any showmanship. In spite of his manifold routine duties he never allowed his research work to flag, and throughout his whole period as a teacher he was a frequent contributor to the *Journal* of the Chemical Society, in which most of his work was published.

Cohen was elected a Fellow of the Chemical Society on March 5th, 1885. He served as an Ordinary Member of Council from 1908 to 1912. In addition he served on the Council from 1920 to 1922 and from 1925 to 1928, in which periods he was also a Vice-President. In 1911 he was elected a Fellow of the Royal Society. In 1924 when he retired from his Chair at Leeds he was made Emeritus Professor and the University honoured itself in admitting him to the honorary degree of D.Sc. In 1930 he received the honorary degree of LL.D. from the University of Glasgow. During the later years of his tenure of the Chair of Organic Chemistry, Cohen had been working on the antiseptic action of substances allied to acriflavine—an antiseptic used in the Great War. This was supported by the Medical Research Council. When Cohen retired from the Chair he still carried on this investigation on behalf of the Council in a small suite of laboratories in York House, attached to the Leeds Medical School. In 1932 the Medical Research Council decided to discontinue this line of work. Cohen now gave up active laboratory work and he and his wife went to live at their country cottage beside Coniston Lake. After only three years of retirement in these beautiful surroundings, a retirement which was so perfect in its provision of activities in which Cohen delighted—writing, walking, gardening, painting, music, doing odd jobs in his workshop—that he described it as " blissful," he died on June 14th, 1935, after a few weeks' illness. He was then 76 years of age.

In 1892 Cohen married Hilda, daughter of William and Mary Hughes of Manchester, and they had two sons and two daughters. The elder son Adolf died on active service in France during the War, but the other children survive him. Cohen was a great believer in the new Universities, and partly for that reason, but also because of his loyalty to it, all his children were students at Leeds.

Cohen's first published papers appeared in the *Berichte* in 1884. They were in collaboration with von Pechmann and were concerned with compounds of phenols with ethyl acetoacetate. No further publication occurred for three years and it seems probable that on his return to Manchester from Munich he was engaged in the preparation of his book on practical organic chemistry. This was first issued in 1887. From this time until his retirement from active work in 1932 Cohen contributed a long series of papers to the *Transactions* of the Chemical Society and, in his later years, to the *Proceedings* of the Royal Society. His early work dealt with isolated problems that were unconnected with each other and some of these undoubtedly arose in working out methods for his " Practical Organic Chemistry." During part of this time, he was engaged in teaching inorganic as well as organic chemistry and several of his papers dealt with analytical methods. After 1890 practically all his work was in the organic field. A variety of miscellaneous problems excited his interest and these form the subject of about a third of his published papers. The papers in this category show his wide interests, but none of them led to a more sustained investigation in the fields in which they lay. There were, however, certain subjects in which he showed a continuous interest. One of these was the aluminium–mercury couple and its uses in organic chemistry. Although first used simply as a reducing agent, it was soon discovered that the couple could replace aluminium chloride in the Friedel and Crafts reaction and was advantageous in several directions. It was next employed as a carrier in the bromination of benzene, toluene and paraffin hydrocarbons. Finally Cohen and Dakin between 1900 and 1904 carried out a systematic and detailed investigation of the products obtained by the chlorination of toluene in presence of the aluminium–mercury couple. As a result of these studies they were able to establish the sequence of events as toluene and its chloro-derivatives were successively chlorinated, and the probable factors which determined the positions taken up by the chlorine atoms entering the nucleus. The subject had been in a state of considerable confusion before this work was undertaken and it was very much clarified by the results obtained. These studies in chlorination, in which the influence of halogen atoms in the benzene nucleus in determining the results of further substitution was the main theme, led to others of a similar kind. They may be grouped in general as (*a*) further studies in chlorination and bromination, and (*b*) the influence of various substituents in the nucleus on the orientation of halogens or nitro-groups subsequently introduced. The idea underlying them was to discover the factors and if possible establish the laws governing substitution in benzene derivatives. During this period a few papers were also published on the effect of nuclear substitution on oxidation in the side chain. In these, interesting data concerning steric hindrance were obtained.

Cohen was intensely interested in the problems of optical activity and a large proportion of his published papers deal with certain aspects of this subject. Almost the last paper he wrote was on "Asymmetry and Life." The first one in this field, published with C. E. Whiteley in 1900, dealt with attempts to produce optically active compounds from inactive substances. Although in the main the methods used were unsuccessful, they showed ingenuity and imagination of a pioneering kind. This piece of work is regarded by some as amongst the best that Cohen carried out. It was followed by a long series of papers on the effect of position isomerism on the optical activity of menthyl esters of substituted benzoic acids and later of phthalic acid. With the assistance of several of his pupils a considerable number of isomeric series were prepared and investigated, and although no numerical rule was established which would account for the observed optical activity it was shown that certain qualitative regularities occurred with sufficient frequency to make them almost predictable.

In 1921 a paper describing a new method for the resolution of asymmetric substances was published by Shimomura and Cohen. It described a method which was successful in producing optically active derivatives from racemic compounds by combining the latter with some optically active substance and then carrying out a replacement reaction, *e.g.*, active phenylchloroacetic acid was prepared from inactive mandelic acid in this way. This was the last experimental work done by Cohen in this particular field.

During the War the organic chemistry laboratory at Leeds as in many other universities was called upon to prepare substances necessary for medical treatment that had previously been obtained from abroad. This was carried out with great success and it may be mentioned in particular that large quantities of novocaine were made on an almost commercial scale. In 1915 H. D. Dakin, who was working at Compiègne with Alexis Carrel on the antiseptic treatment of war wounds, asked for a large series of substances to be made for trial. With the assistance of the Medical Research Council upwards of 100 were prepared by Cohen and his assistants, and sent out. Amongst these were various chloroamines, some of which were found to be very efficacious. This was the beginning of Cohen's interest in antiseptics and he continued to work on them until 1932. The introduction in the War of acriflavine as a surgical antiseptic led him to begin a systematic examination of a long series of acridine and phenazine derivatives, together with pyridine and quinoline compounds which might be expected to exhibit antiseptic activity. C. H. Browning acted as his chief collaborator on the bacteriological side and was responsible for the testing of the antiseptic potency of the various substances which Cohen and his research assistants made. Although a number of very active products were synthesised, no marked advance on acriflavine itself was realised. The publication by Sir W. J. Pope in 1921 of his work on photosensitising dyes derived from quinoline led to the observation by Cohen that the substituent groups which led to extension of photosensitising action were identical with those which tended to an increase in antiseptic power in the acridine group. He therefore commenced the synthesis and examination of a long series of dyes known to have sensitising or desensitising action, a task which occupied him and his collaborators until his retirement in 1932. In addition to dyes of the cyanine group a large series of styryl- and anil-quinoline derivatives were made and tested. Whilst the anils when suitably substituted were very powerful antiseptics, one of them, quinanil, having been of considerable service clinically, the styryl compounds were found to possess marked trypanocidal activity. This led to the further study of the effect of chemical constitution on trypanocidal action and several compounds were discovered in which the ratio of the tolerated to the curative dose was high. A branch of this line of work which was followed in Cohen's later years was the examination of organic bismuth compounds for spirochaeticidal activity. The carrying out of this work on antiseptic and trypanocidal action, in which several hundred compounds were made, illustrates well Cohen's phenomenal patience and industry. The work of Ehrlich and others has shown that in the present state of knowledge regarding chemical constitution and biological activity there is practically no other road to pursue than that travelled by Cohen. Many might have the vision to plan such a task, but he is amongst the few who have had the capacity for patient industry which enabled it to be carried out in spite of the disappointments entailed inevitably in work of this kind.

It is well known that a number of Cohen's pupils on his advice took up the subject of biochemistry, or physiological chemistry, as it used to be called. The first of these was H. D. Dakin and it came about in a somewhat curious way. Cohen's interest in optical activity and especially in the biological resolution of inactive substances had made him a great student and admirer of the work of Pasteur. He had also had the curiosity to make cultures on gelatine plates of air-borne organisms in connection with his work on atmospheric pollution. H. D. Dakin, who was a research student at the time, was naturally affected by these phenomena. When it became possible for him to gain further research experience abroad, he suggested that he might take up physiological chemistry. Cohen met him more than half way and suggested a term with Duclaux at the Pasteur Institute. Duclaux agreed, but later ill health forced him to decline and he suggested that Dakin might work with Bertrand. At that time Bertrand was not very well known in this country and Cohen therefore recommended Dakin to go to his old teacher, von Pechmann. The tragic death of von Pechmann prevented this and finally at the suggestion of Sir Henry Roscoe, who was a governor of the Jenner (now the Lister) Institute, Dakin went there. He worked at first with S. G. Hedin on the phenomena of enzyme action but especially on the use of enzymes for the resolution of inactive substances. This work, as may well be imagined, so impressed and delighted Cohen that for many years his research students who were able to travel elsewhere for post-graduate work were advised to enter the field of biochemistry.

Whilst still on the staff at Owens College Cohen became interested in the smoke problem and this interest was maintained actively throughout his life. He realised early the need for data concerning the composition of soot, the quantity emitted from domestic and factory chimneys, its journeys in the air and its effect on health, vegetation and buildings. The facts required could only be ascertained by experiments and he set about doing them. In the course of these experiments observations were made at ten different stations in the Leeds district, varying from industrial to suburban. Besides the collection of rain and atmospheric deposits and their analysis, a variety of plants were grown in the different centres, the deposit from leaves was examined, and the intensity of the light was measured. These facts are only mentioned to give an idea of the seriousness with which the problems were tackled. It would have been unlike Cohen to do otherwise. He had several collaborators in this quest, the chief of them being A. G. Ruston of the Department of Agriculture at Leeds University. The results, together with many cognate ones obtained by others, were collected and published in book form under the title "Smoke, a study of town air." This ran through two editions, the first being published in 1896 and the second in 1925. It gives as complete an account as one could desire to have of the nature of soot and its effects, as well as those of the gaseous impurities which are emitted from chimneys along with it. In addition, the dispersal of soot, the production of fog, and the effect of pollution of the air on vegetation are dealt with. It is a *vade mecum* of experimentally ascertained facts about smoke and its results which must have been of inestimable value to those interested in the suppression of this blight of the industrial age. It is typical of Cohen with his æsthetic temperament, combined with an essentially practical outlook, that he should spend so much time in laborious experiments to try and convince others of the ravages wrought by smoke, quite apart from its offensiveness to eye and nose. In addition he frequently gave lectures to societies and conferences on smoke abatement and from 1915 served as a member of the Local Government Board Departmental Committee on the pollution of the air by smoke and other noxious vapours.

Cohen's reputation as a teacher was not gained solely through the school of organic chemistry at Leeds. It came to him also and perhaps in greater part through his books. His first endeavour in this sphere was the little "Practical Organic Chemistry," written after his return from Munich and first published by Macmillan in 1887. After being reprinted twice, it gave place to a similar but more advanced book which was first published in 1901 and went through three editions, the last being in 1924. This and the earlier book have guided innumerable students in their first ventures in practical organic chemistry and they must have made Cohen's name familiar the world over. Another of his books which is less known but found a useful place in schools was entitled "Class Book of Organic Chemistry." It included an elementary outline of the subject with practical exercises. There were two volumes, the first published in 1917 and the second in 1920. The latter was intended for medical students and contained matters with a direct bearing on medical subjects. His two theoretical books "Theoretical Organic Chemistry" and "Advanced Organic Chemistry" (3 volumes) have been extremely popular, as can be estimated by the numerous editions and reprintings through which they have gone. The former was intended for students reading organic chemistry for the Ordinary Degree and the latter, known popularly as the "big Cohen," for Honours and post-graduate students. Historically, probably the most important of these books was the small "Practical Organic Chemistry." It was the first of its kind in this country and it undoubtedly gave an impetus to the introduction of the practical teaching of organic chemistry in the chemistry schools in the eighteen-nineties. The two theoretical books which followed it also played a great part in the development of the teaching of the subject, for they appeared when there were few others in the English language to choose from. Their popularity, which still persists, is a sufficient indication that they met the need for which they were designed.

No account of Cohen's influence and work would be complete without a reference to the Working Men's Institute in the York Road district of Leeds which owes its existence to him and his wife. A few years after their marriage, desiring no doubt to bring some brightness into the drab lives of some of the children who lived in the overcrowded areas of Leeds, they established what was at first called a "Lads' Club." It was started by borrowing two rooms from All Saints Church, York Road, and in these weekly meetings were held. The rough "tykes" who attended were entertained principally by games and music. The two rooms soon became overcrowded and a back-to-back house in All Saints Street was taken. This in turn gave place later to a loft over a stable, which was used as a general club room, but Mrs. Cohen also established a Mothers' Meeting there. In 1911 a new building was erected, the gift of Professor W. Stroud, and was presented to the University in order to provide for the club in perpetuity. It was extended later by the addition of an extra storey. As the original members of the club grew to manhood, their attachment to it and to its founders was so great that they would not leave it, hence the eventual change of name to "Working Men's Institute." The present caretaker, now a grandfather, was, as a boy, one of the original members. The Club possesses its own athletic ground and is now a self-governing institution. Throughout a great part of its period of existence Professor and Mrs. Cohen were greatly helped in their work for the Club by Mr. Joseph Wood. The success in achieving the aims for which it was founded and the efforts made to guide its destiny during the 37 years in which he worked in it brought great satisfaction to Cohen, and of his social activities in Leeds it was probably the one that gave him most pleasure. When he retired the members of the Club as a sign of their esteem and affection presented him with a bird bath for his garden at Coniston Lake.

What has been written above indicates the many-sidedness of Cohen's activities. That he was able to accomplish so much was due in large part to his ability to concentrate entirely and exclusively on the thing he had in hand. If he had a motto it must have been "Do it now." These activities would be sufficient in themselves to fill the life of most men, but not so with him. He had gifts which took him into other spheres. Music was a great delight to him and he was an excellent performer on the violin. He was particularly fond of chamber music, and at his home friends both from the University and from outside were often entertained by him and his wife at musical evenings. Painting in water colour was one of his hobbies, and he was very skilful in this medium in catching the spirit of the Lake District around his country cottage at Coniston. He was an active member of the Leeds Fine Art Society, a small club which met periodically for sketching out of doors or held exhibitions in the home of one or other of the group for criticism and discussion. He was an active gardener and found especial pleasure in this pursuit after his retirement. On a country walk he was a delightful companion. The varied phenomena of the countryside had a great attraction for him and he was able to infect others with it too. Tennis was one of his ways of getting exercise and he played a good average game.

The administrative and routine side of University life offered no temptations to him. Indeed he was often critical of those who saw the need for devoting time which had to be spared from laboratory hours to this kind of work. He was also a severe critic of his chemical brethren and above all of those who he thought were out for public glory or private gain. He was impatient with those who were nominally but imperfectly equipped with science and particularly also with the medical profession. The foreign students of the University were always welcome at his home and unconsciously in this way he was able to give them an impression of English life which otherwise they would have missed. Many of them must be grateful for the friendship thus shown to them.

It is undoubted that he played a great part in the Yorkshire College and the Leeds University which succeeded it, by developing as fine a tradition of service both to the institution itself and to the community in which it lay as one could hope to find. It was impossible to be associated with Cohen in any enterprise without being affected by his austerity, his kindness of heart and his high-mindedness. He was never a preacher and what his pupils learnt from him was through example rather than precept. Such charm of character allied to modesty and single-minded devotion to duty is but rarely seen.

H. S. RAPER.

CHARLES FREDERICK CROSS.

1855—1935.

CHARLES FREDERICK CROSS, whose name will always be associated with cellulose, was born on December 11th, 1855, and died on April 15th, 1935. His father, Charles J. Cross, J.P., of Brentford, a director of the firm of T. B. Rowe, the well-known soapmakers of that town, no doubt directed his footsteps in the direction of science and its industrial applications. He was educated at King's College, London, taking prizes in chemistry, geology, and German, and in 1873, at the age of 18, embarked on a chemical career, becoming chemist to the Phosphate Sewage Company, of Barking. An early assertion of independence is found in a letter of this period, protesting against certain of his results being published until they had been more definitely established.

Following further experience on sewage farms in the Midlands, he spent the year 1876 in Zurich, studying organic chemistry at the University and the Polytechnikum, improving his knowledge of German and making many friends. Thus prepared, he entered Owens College, Manchester, where he joined a group of active workers under Roscoe and Schorlemmer, whose brilliance had made Owens College a recognised centre of chemical science at that time. Cross, energetic, lively, and musical, took an active part in the life of the college, speaking at the Union Society and the College Chemical Society, the records of which, for example, on March 1st, 1878, show him contributing a paper on the life and work of Liebig, with Dr. Roscoe in the chair. He graduated B.Sc. London in 1878, and spent a post-graduate year upon various investigations, one of which, a research "On the Hygroscopic Properties of Bodies," was published in 1879. He was now looking for an opportunity to specialise, and the opportunity soon came. Dr. Roscoe had been asked by the Barrow Flax and Jute Company of Barrow-in-Furness to send them a young chemist likely to solve difficulties connected with the bleaching of fibres. He told Cross it was just the task for him, and from 1879 to 1881 Cross worked with eagerness and success, partly at Barrow and partly at Owens, on the problems which he found to hand. Always very quick and imaginative, he realised at once that a vast field must exist for the application of science to the study of vegetable structures, and he resolved to make the field his own. How rapidly he did so is shown by publications on the Chemistry of Bast Fibres (Owens College, 1880), on Cellulose and Coal (with E. J. Bevan; *Chem. News*, 1881), the Chemical Technology of Jute (*J. Soc. Chem. Ind.*, 1882), and by his correspondence with Vines of Oxford, Mills of Anderson's College, Glasgow, and others, on the chloro-compounds of bast fibres, the nature of dyewoods and logwood, and the constitution of lignin. The reactivity of jute, ever after his favourite lignocellulose, to chlorine led to the discovery of the well-known chlorination process for the isolation and estimation of cellulose—a process which after innumerable critical investigations still remains a standard.

At Owens a friendship had grown up between Cross and Edward J. Bevan, a student of the same age and the same keen outlook upon research. Bevan had gone to the Musselburgh Mill (Cowan and Co.), where he was afforded every facility for acquiring a knowledge of paper manufacture. The two young men kept closely in touch, and, fired by the thought of the innumerable problems awaiting solution in the cellulose field, determined jointly on a great adventure. In 1883 they gave up their posts in order to spend a period on pure research, and through the influence of Thistleton Dyer, Hugo Müller, and H. E. Armstrong, they secured places in the Jodrell Laboratory, Kew (1883). Their investigations here dealing with lignification and the oxidation and hydration of cellulose were published in a series of memoirs. The result of this close association was the formation in 1885 of the consulting practice of Cross and Bevan, with laboratories at New Court, Lincoln's Inn, still the home of the firm.

The partnership was dissolved only by the death of Bevan in 1920, and the loyalty of the two men to each other was such that Cross might easily have written of Bevan in the words used by Liebig in speaking of Wöhler: "I had the great good fortune to gain a friend of similar tastes and similar aims. Without envy and without jealousy, hand in hand we pursued our way; when one needed help the other was ready. . . . Many of the investigations which bear our joint names were done by one alone. They were charming little gifts which each presented to the other" (Liebig, Autobiographical Sketch).

They early specialised on paper, taking an active part in the investigations needed for the development of the Ekman bisulphite pulping process. Their "Text Book of Papermaking," still a standard work, was published in 1887. Bevan gradually concentrated, however, on the analytical side, becoming public analyst for the County of Middlesex. Cross, devoted to research, spent as much time as possible at the bench. He carried out important work on nitrocellulose and the sulphuric acid esters of cellulose and the influence of mixed esters formed during nitration on the stability of the products. Impressed with the importance of obtaining cellulose in a soluble form, he prepared other esters and the discovery of cellulose acetate (1889) and benzoate (1890) was the prelude to the great achievement of his career, the discovery of cellulose sodium xanthate (viscose), the preparation of which was patented in the joint names of Cross, Bevan, and Beadle in 1892.

Cross at once realised the importance of this product, soluble in dilute alkaline solutions and capable of simple reversion to a dispersed form of cellulose, and his correspondence reveals the tremendous energy with which he set to work. The autumn of 1892 was consequently a period of intense activity. Clayton Beadle was making "viscose," as Cross began to call it, at St. Mary Cray, and was testing it for paper coating and sizing. The variation of viscosity with the age of the viscose solution was being investigated. Arrangements were made with Messrs. Griffin and Little to handle the material in the U.S.A., and Cross made enquiries for a machine suitable for producing viscose films. But the earlier applications of viscose were in the direction of a "filler" for textiles and paper to provide waterproof and other finishes, and to prepare strong boards for bookbinding and similar purposes.

A Viscose Syndicate was formed under the chairmanship of A. W. Pears and later of

Cross, and a product, Viscoid, suitable for the manufacture of such articles as umbrella handles, was developed and licences granted for manufacture at home and abroad.

A. D. Little had written to Cross in August, 1893, asking whether he had considered spinning viscose in place of nitrocellulose, but owing to more pressing claims no experimental work was done in this direction till 1896, when the Viscose Syndicate was consulted by C. H. Stearn, of the Zurich Incandescence Lamp Co., with works near Kew Gardens, as to the possibility of producing from viscose a thread suitable for electric lamp filaments. Under the guidance of Cross and Bevan, Stearn, with the skilled co-operation of his assistant Topham, soon produced a suitable monofil, and this naturally suggested the manufacture of a multiple thread which could be used for textile purposes. Although they had little, if any, knowledge of spinning machinery, Stearn and Topham rapidly overcame the difficulties involved, and early in 1898 they succeeded in spinning a multiple twisted thread from viscose at a reasonable speed. The invention of the centrifugal spinning box by Topham in 1900, and the acid bath of Stearn and Woodley (E.P. 2529, 1902), made the production of artificial silk a commercial reality. As Cross himself says in the Mather Lecture to the Textile Institute (1921), this development of viscose spinning by Stearn and Topham is "one of the most extraordinary cases in technical history." The original Topham box reposed for years in a dusty corner of Cross's laboratory, but it now happily takes its place as a piece of technical history in the Science Museum, S. Kensington.

The Viscose Spinning Syndicate arranged for the production of viscose by financial groups in America, Germany, and France, whilst Messrs. Courtaulds Ltd., with whom Cross remained closely associated, took over the production in this country in 1903. He was also greatly interested in the development work of the Société Française de la Viscose, spending his summer holiday in Dieppe so as to be near their works at Arques la Bataille, and during the initial period of the Société Russe de la Viscose he visited them in Moscow and St. Petersburg.

Although viscose and viscose rayon remained a dominant interest, Cross during the period from 1912 to his retirement about six years ago took an active part in almost every phase of cellulose development; new processes for the manufacture and standardisation of wood pulps, in particular, and the utilisation of the waste materials of the bisulphite pulp process, occupying his attention. By writing, lecturing, and the steady output of original research also, he did everything possible to encourage the study of cellulose and its problems.

The Cross and Bevan "Text Book of Papermaking" (1887) has already been mentioned. The classical "Cellulose, an Outline of the Chemistry of the Structural Elements of Plants," was published in 1895. It was written by Cross during a three weeks' holiday, and to those who knew him it is Cross himself thinking, speaking, full of imagination; his ideas pouring out, often haphazard, but everywhere giving inspiration for future work. He agreed with the present writer that the book should stand as a phase of cellulose history and should not be brought up to date, but schemes for the writing jointly of another book on modern lines never materialised. He wrote rapidly in a characteristic style, and his handwriting, which, as a young man, was like copybook, later became a spidery gothic, easily read only by the experienced.

During the fifty years 1879—1929 over a hundred memoirs appeared under Cross's name. They include contributions to the constitution of lignin and the lignocelluloses, flax, esparto, kapok, and the cereal straws; raffia and the cutocelluloses; the nitric, formic, and other esters of cellulose, and his theory of the ketonic constitution of cellulose. All are characterised by rapid clean-cut experimental method.

His pioneer work and his persistent and successful efforts were duly recognised. He was elected a Fellow of the Royal Society in 1917, and was awarded the Research Medal of the Society of Chemical Industry "for conspicuous services to chemical industry" (1916), and the Medal of the Society of Dyers and Colourists (1918). He served as President of the latter Society 1917—18, and was awarded the Perkin Medal (1923). He was also the first honorary fellow of the Textile Institute and the Technical Section of the Paper Makers' Association. Always interested in the work of the Royal Society of Arts, he gave the Cantor Lectures in 1920 on "Recent Research in the Cellulose Industry," and served on their technical committee dealing with the durability of paper.

Cross was of a kindly dry humour and always intensely alive with a gentle charm of manner and a brightness of outlook which appealed to everyone. As a young man he was very fond of rowing and fishing, and the love of the river and of the open country always remained. He was a keen observer of nature and delighted in flowers and the garden. Love of music too was a dominant factor in his life. He played excellently on the organ and the piano and was a natural musician in the sense that he turned to music as a form of expression. He extemporised admirably, the influence of Bach being always observable, and would generally relax after a long day, in this manner. He also had the gift of entering into the spirit of life abroad and this, coupled with the ready way in which he spoke and wrote foreign languages, had much to do with the success of his work in other countries and the close contact he preserved with them.

He married in 1890 a daughter of General C. R. Stainforth of the Madras Cavalry, who with their two sons and daughter survives him. During the last few years he lived in retirement at Hove and, being prevented by an arthritic affection from walking much, he owed everything to his wife's devoted care.

To have given the world a new structural material—viscose cellulose—was a great achievement. That it came as early as it did was due to the combination of the two opposites which dwelt in Cross—the visionary and the practical scientist.

CHARLES DORÉE.

THOMAS CUTHBERT DAY.

1853—1935.

THOMAS CUTHBERT DAY was born at Burton-on-Trent, where his father, the Rev. Cuthbert Day, was headmaster of the Grammar School. In his early days he was engaged in the brewing industry and came to Edinburgh about 45 years ago as head maltster to a firm of brewers. Later, in company with the late Bailie John Hislop of Leith, he founded the firm of Hislop and Day, the well-known photo-process engravers. In this work his knowledge of chemistry was most valuable. Colour reproduction was then in its infancy and he was successful in developing this work on thoroughly scientific lines. Many of the plates in the *Proceedings* and *Transactions* of the Royal Society of Edinburgh are the work of his firm.

Day had a wide general interest in science, but it was geology which attracted him. He specialised on the geology of the volcanic formations along the coast of the Firth of Forth and recorded many hitherto undiscovered volcanic vents. It was on the chemical aspects of geology that he did his most important work, and his chemical analyses of igneous rocks, chiefly rocks from Fife and the Lothians, form a contribution to petrographical knowledge of outstanding merit and importance. He was a popular lecturer on geological subjects and wrote an interesting account of the Arthur's Seat volcano as a guide to beginners.

He was of a quiet yet genial disposition and his death removes a well-known figure from the scientific societies of Edinburgh.

Day was elected a Fellow of the Society in 1881 and died in Edinburgh on June 14th, 1935, in his 83rd year.

A. LAUDER.

PETER FENTON.

1875—1935.

PETER FENTON, a pharmacist of exceptional ability, died at Coatbridge on June 14th, 1935, aged 60. He was elected a Fellow of the Chemical Society in December, 1919. After a brilliant studentship he was appointed to a lectureship in botany and materia medica at the Glasgow School of Pharmacy. He was an enthusiastic and inspiring teacher. Thereafter he engaged in the successful practice of pharmacy, latterly in Kirkcaldy, ultimately returning in 1932 to his native town, Coatbridge, where his scientific bent found expression in his devoting himself exclusively to optical and refraction work. Possessing all the optical qualifications available in this country, Fenton was one of the founders of the Glasgow Refraction Hospital and his continued keen and practical interest in that beneficent institution was rewarded by his appointment, shortly before his decease, as chief of staff. He had a talent for botanical research, rather than chemical, and read many papers to naturalist societies in the county of Fife. An accomplished and cultured man of genial personality and lovable disposition, Fenton was honoured and greatly esteemed. Sincere sympathy is felt for his widow and family in their sudden and sorrowful tribulation and severance.

W. MAIR.

HAROLD FOLLOWS.

1862—1935.

HAROLD FOLLOWS, second son of Alderman John Follows (for many years one of Stafford's best-known public men), was born in Stafford on December 10th, 1862, and died at his residence in Victoria Park, Manchester, on May 8th, 1935. He was educated at Stafford Grammar School, distinguishing himself particularly in classics and chemistry, and continued his studies in chemistry and physics at the evening classes held at Owens College in Greek Street, off Downing Street, in 1879—1880, 1880—1881, and in 1881—1882 in Rosamond Street East under Professor Henry Roscoe. In 1878 Follows was appointed to his first position as junior chemist to H. D. Pochin & Sons, Ltd., Chemical Manufacturers, Salford, and remained on the technical staff of this Company until 1889, when he was appointed salesman and representative of Hardman & Holden, Ltd., Tar and Ammonia Distillers and Chemical Manufacturers, Miles Platting, Manchester. He held this position until his retirement in January of this year. Follows was an extremely likeable man, full of energy and determination to serve his company loyally and faithfully. His ready wit and his ability to talk learnedly on scientific matters made him a very successful representative. He could speak fluent French and German and had a useful business knowledge of other languages. His death is much regretted by all who had intimate contact with him. He was elected a Freeman of the Borough of Stafford on August 25th, 1884, and a Fellow of the Chemical Society on May 7th, 1885. Follows carried out water analysis for the Stafford Corporation during the period 1887—1888 when they contemplated a new water scheme. He leaves a widow and many friends to mourn his loss.

A. F. CAMPBELL.

VICTOR JOHN HARDING.

1885—1934.

BIOLOGICAL chemistry suffered a grievous loss by the death on July 3rd, 1934, of V. J. Harding, D.Sc., Professor of Pathological Chemistry in the University of Toronto. He had a first attack of coronary thrombosis in September, 1930, a second in June, 1932, and a third in October, 1933. In May, 1934, he developed bronchitis and pleurisy and died of heart failure during this illness. Following each of the former periods of ill-health, he engaged with his customary zeal and ardour in his research activities and even directed them from his sick bed. His wonted cheerfulness and enthusiasm were at these and all times an inspiration to those about him, and even those who knew him best could only admire and wonder at his fortitude and courage under circumstances trying in the extreme both to the body and mind.

Victor John Harding was born on October 23rd, 1885, at Fishpool, near Bury, Lancashire, and was the son of Frederick James and Sarah Halstead Harding. From Bury Grammar School he proceeded in 1903 to the University of Manchester and graduated with first-class honours in chemistry in 1906, being awarded a graduate research scholarship and the Le Blanc Medal. He was a research pupil in the laboratory of Professor W. H. Perkin from 1906 to 1910. In his final post-graduate year at Manchester he served as research assistant to Dr. C. Weizmann, with whom, as also with W. H. Perkin, he published several papers concerned with organic syntheses. Among the more important of these was his share with Perkin in the synthesis of 1-methyl*cyclo*hexylidene-4-acetic acid (J., 1908, **93**, 1943). He graduated M.Sc. in 1907 and D.Sc. in 1912.

Harding was appointed Demonstrator in Chemistry at McGill University, Montreal, in 1910. In his first year on the staff he taught general chemistry under the direction of Professor J. Wallace Walker, and in the following year he was persuaded by Professor R. F. Ruttan to accept appointment as Lecturer in Biological Chemistry. He was unfamiliar with this branch of chemistry, although it had always had attractions for him. To extend his knowledge in this field he spent his vacations during one or two summers in England and engaged in biological research both at St. Mary's Hospital, London, and at the Lister Institute. One outcome of this work was a paper on the action of enzymes on hexose phosphates (*Proc. Roy. Soc.*, 1912, *B*, **85**, 418). A series of memoirs followed on the ninhydrin reaction, published in the *Transactions of the Royal Society* and in the *Journal of Biological Chemistry*, 1915—1917. His service in developing a colorimetric method of estimating α-amino-acids is well known. He applied it with success in researches on protein hydrolysis and found it to be comparable in accuracy with the microgasometric method of Van Slyke. The possibility of error in his method led him to study the specificity of the reaction and the conditions under which it could be utilised in the presence of amines and ammonium salts. As applied to amino-acids, the reaction was traced to the formation of the corresponding glyoxal and ammonia, which then combined with triketohydrindene hydrate.

His services at McGill University were recognised by his appointment as Assistant Professor in 1913, and as Associate Professor of Biological and Physiological Chemistry in 1917. He maintained his close association with Professor R. F. Ruttan, engaging actively and with marked success in the teaching of medical students in the Department of Chemistry. His courses included physiological chemistry with special reference to problems of nutrition and metabolism, and the properties of colloids. From among his students he selected those who had worked most creditably and induced them to attend an advanced seminar course in which he discussed the more recent advances in his subject. Endeavouring to foster the research spirit, he described the new contributions of individual investigators in the field of pathological chemistry and while giving credit to each author he never attempted to be dogmatic or critical. His methods were intended to arouse latent interest in the application of chemical methods to pathology and with Ruttan he succeeded along these lines in instituting one of the first practical courses of biochemistry given in Canada.

From 1916 until the end of the War his researches were interfered with owing to his spending vacations in war service at the munitions factory of Messrs. Curtis and Harvey, Dragon, Quebec, but during University terms he contrived, in close association with clinical workers in the hospitals, to continue his investigations. With J. W. Duncan in 1918 he published a report on the effect of high carbohydrate feeding on the nausea and vomiting of pregnancy (*Can. Med. Assoc. J.*, 1918, **8**, 1057). This was his first research in pathological chemistry and it was followed immediately by a paper with C. A. Fort on the amino-acids of the human placenta which showed the combined protein fraction to have a high arginine content (*J. Biol. Chem.*, 1918, **35**, 29). This subject was continued in several publications in association with his pupil, Dr. Elrid G. Young, now of Dalhousie University, to whom the present writer is indebted for much of the information in this memoir. The ready digestibility of this protein fraction was demonstrated both *in vitro*, and *in vivo* in the dog. They studied also the influence of this protein on the purine metabolism of the growing dog. Alongside this problem Harding and Young investigated the influence of high and low protein diets on creatine–creatinine output in the dog (*J. Biol. Chem.*, 1918, **36**, 575; 1919, **40**, 227).

In 1920 Harding was elected to the Chair of Pathological Chemistry in the University of Toronto. Here, as at McGill, working in close co-operation with the Department of Obstetrics and Gynæcology, he engaged in a long and noteworthy series of investigations which will always remain an outstanding contribution to obstetrical knowledge, particularly on the toxæmias of pregnancy. His early papers on the nausea of pregnancy introduced and established the now wide-spread use of glucose therapy in *Hyperemesis Gravidarum* and his studies in dehydration covered a field which had hitherto been unexplored by other investigators. The theory of carbohydrate deficiencies stressed the essential common metabolic basis of all degrees of the nausea and vomiting of pregnancy. The result of Harding's teaching was the application of new methods not only to *Hyperemesis* but also to the milder cases (*Lancet*, 1921, **2**, 327; 1922, **2**, 649). These papers were followed by studies in ketogenesis in pregnancy and it was when the accumulated evidence showed that the carbohydrate deficiency hypothesis could not be established as the primary ætiological factor in the disease that Harding directed his efforts to the elucidation of the dehydration factor. Subsequently he turned to the question of the later toxæmias and made important contributions, including " The effects of hypertonic saline on the toxæmias of pregnancy " (*Brit. Med. J.*, Oct., 1930) and " Researches in the toxæmias of later pregnancy " (*Amer. J. Obstetrics and Gynæcology*, 1932, **24**, 820). He was invited to read the latter paper at the meeting of the New York Obstetrical Society in 1931, an honour which was appreciated by his colleagues as a marked recognition of his valuable work. The present-day treatises on obstetrics bear witness to his influence on modern treatment in this field.

Problems of carbohydrate metabolism claimed his interest in subsequent years and, finding that the existing methods for the identification and estimation of sugars in body fluids were inadequate for his purpose, characteristically he devised other valuable analytical procedures. His study on the metabolism of galactose with G. A. Grant (*J. Biol. Chem.*, 1933, **99**, 629) belongs to this period, as also his work on the benign *glycosurias*. Among his latest interests was an endeavour to ascertain the nature of the reducing sugars in normal urine, but the completion of this investigation was interrupted by his untimely death. In preparing himself for this problem he devised a scheme for using micro-organisms in quantitative and differential sugar analysis and these experimental methods will be found of permanent service and value (*Biochem. J.*, 1933, **27**, 1082). His last publication was on carbohydrate material in fasting urine (*Can. Chem. & Met.*, 1934, **18**, 105). In all, Harding published some 70 papers in the scientific journals, most of them in collaboration with his own pupils.

Harding married, at Montreal in 1914, Mary Marshall, daughter of Captain Archibald Browning Smith, of Seaforth, Liverpool, whose helpfulness and grace were always a comfort and inspiration to him. She and a son and a daughter survive him. He was elected, in 1922, a Fellow of the Royal Society of Canada, and he was a Fellow of the Chemical Society and an Associate Fellow of the Academy of Medicine of Toronto as well as an Honorary Member of the American Urological Association. Other learned societies of which he was a member were the Biochemical Society, the American Society of Biological Chemists, and the Canadian Medical Association. He was one of the pioneers in the creation of a biochemical section of the Canadian Chemical Association and was the first president of the Toronto Biochemical Society. Harding was a man of reserve and modesty, which he combined with great strength of purpose and sincerity. A humour, always kindly and gentle, pervaded his judgment of men and things. Perhaps this was best seen in his frequent quotation from the creations of W. S. Gilbert's genius. He had a disarming candour which was always effective. His great earnestness of character gave him a wide and humane sympathy. He brought to the hospital ward an overwhelming desire to allay suffering. His co-operation with clinical practitioners to seek a remedy for human ills gave him an abiding satisfaction.

The University of Toronto has expressed gratitude for his great services as a teacher and has put on record in a memorial from the Faculty of Medicine its high appreciation of him as a man and as an investigator of signal merit and achievement. The concluding paragraph of this memorial may here be quoted. " The memory of a gentleman who quietly but persistently pursued and finished a considerable work, who spread an influence among his students and associates of kindly earnestness, and who lived in harmony with his own high ideals, will remain and merge with that vast body of tradition which gives a University a soul." Many in this country will mourn the untimely loss of one whose elevation of spirit, and love of things beautiful and seemly, possessed a very special flavour for all who enjoyed his friendship.

W. N. HAWORTH.

FREDERICK BEAUMONT HIRST.

1875—1935.

FREDERICK B. HIRST was born in Dewsbury in 1875, and was educated at Batley Grammar School. On leaving school he was apprenticed to a local chemist, Alderman Smith-Ward. He qualified as a chemist and druggist and later as a pharmaceutical chemist. One of his early appointments was on the staff as demonstrator at the old Turner's College of Pharmacy, Manchester, where he gained experience of great value in after-life.

After holding many posts of a responsible nature, Hirst became associated with the co-operative movement in 1908 and was appointed chemist to the Batley Co-operative Society. This position he held until 1918; he then moved to York to fill a similar position with the York Co-operative Society. In 1921 he again transferred his activities to Bolton—still in the co-operative movement—where he had the responsibility of opening the department. This post he held until shortly before his death on May 18th, 1935.

During the War he was actively associated with the Volunteer Defence movement, holding the rank of Captain.

A man of numerous interests, he will probably be known to most as a result of his lecturing engagements. From early days this had been his favourite occupation outside business, and of late he had fulfilled numerous engagements in the north of England, speaking on dietary matters, a subject upon which he was well informed. Some two years ago he was the subject of much publicity in the national newspapers as a result of an association he had made with the world's loneliest island—Tristan da Cunha. The inhabitants of Tristan knew Hirst as their "fairy godfather" on account of his having taken every opportunity for a long time of sending them medical necessities, etc., and corresponding with them.

He was married in 1900, and has left a widow and a son and a daughter. The interment took place at Dewsbury Cemetery on Tuesday, May 21st.

Hirst was elected a Fellow of the Chemical Society on June 21st, 1923.

C. SALT.

CARL LANGER.

1859—1935.

CARL LANGER, who died on March 3rd, 1935, on the "Homeric" while returning from a cruise to the West Indies, was the son of a manufacturer and was born on December 1st, 1859, at Sonnenthal near Brüsau in Moravia, a province of Austria. Shortly afterwards his parents moved to Budapest and he became a Hungarian subject. On leaving school he went in 1877 to study chemistry under Victor Meyer at the Polytechnikum at Zurich; two of his contemporaries there were Treadwell and Karl Markel. In 1880 he received the diploma of Technical Chemist and in the following year was appointed Meyer's assistant. In 1882 he obtained his Ph.D., his thesis being entitled "Ueber Gesetzmässigkeiten bei der Substitution aromatischer Amine." He was also engaged with Meyer on research on the dissociation of gases at high temperatures. On his professor's recommendation he was, in 1883, offered a post with the Badische Anilin und Soda Fabrik at Ludwigshafen; while there he discovered an orange aniline dye, but his name is not mentioned in the patent. In 1885 Ludwig Mond, requiring a research chemist, at Markel's suggestion, approached Langer; Mond eventually obtained permission from the B.A.S.F. for Langer to terminate his agreement and he settled in England, working at Mond's private laboratory at Avenue Road, St. John's Wood, London. He was engaged on two main problems: (1) the generation of cheap electric current and (2) the recovery of the chlorine which was at that time wasted in the ammonia-soda process. It is well known how research on the latter problem, followed by further work on a method for eliminating carbon monoxide from hydrogenous gases which were wanted for use in Mond and Langer's gas battery, resulted in the discovery of nickel carbonyl. The carbonyl patent was taken out in 1890 and from then on Langer worked continuously, perfecting the process.

Arrangements were made with Henry Wiggin & Co., nickel refiners of Birmingham (now a subsidiary of the Mond Nickel Co.), for the erection by Dr. Ludwig Mond at their red lead and tin oxide works at Smethwick of an experimental plant, where Langer worked out the details of the process. In 1898 he went to Canada with Dr. B. Mohr and negotiated for Mond the purchase of the Victoria and Garson Mines in the Sudbury area.

The erection of a refinery at Clydach near Swansea was decided upon in 1899 and was commenced in September of that year. The layout and most of the plant were designed by Langer and remain as a monument to his genius as a chemical engineer. On the formation of the Mond Nickel Co., Ltd., in 1901 he was appointed Managing Director and as such supervised and directed the refinery operations of the company until his retirement.

Langer was a man of distinguished bearing, strong character and of very firm opinion. He was much respected and liked by his staff and workpeople, and was keenly interested in all matters affecting the welfare of them and their families. In this connexion he was responsible for the Mond Nickel Co.'s housing scheme at Clydach. One of his main hobbies was photography, in which he was an expert and often made his own cameras; he was very clever in working in wood and metal and spent a large portion of his leisure hours in his workshop at home.

He lived close to the works at Ynyspenllwch, Clydach, but in 1916 moved to Bucklebury Place near Reading, as he had to leave the district by his doctor's orders, following a serious operation which he underwent in Swansea in the previous year; he, however, continued to visit the works frequently. In 1928 he retired and went to Switzerland, where he built a house on the shores of Lake Zug. In 1887 Langer married Miss Marie Dukat of Agram, Croatia, by whom as well as by two sons and one daughter he is survived. He was elected a Fellow of the Chemical Society in 1885.

C. M. W. GRIEB.

JOHN WALTER LEATHER.

1860—1935.

THE life of the late J. W. Leather will be deservedly kept in memory as that of one of those who have been largely responsible, during the last forty years, for the development of scientific work in relation to Indian agriculture, as well as to tropical agriculture generally.

Leather was born in 1860, at Rainhill in Lancashire. After leaving school, he entered his father's chemical factory in St. Helens and there served an apprenticeship to chemical work. He was sent, in 1883, to study chemistry under Kekulé at Bonn, where he stayed for three years. Leaving the University in 1886 with the degree of Ph.D., he accepted the appointment of senior assistant to Dr. J. A. Voelcker, the consulting chemist to the Royal Agricultural Society of England, and held this post for six years. During this time he developed very high technical skill in chemical work in connexion with agricultural problems, a skill which he retained throughout his career. He originated several new methods, and at least one of these—a process for the detection of castor seed in feeding stuffs—is universally used to the present day. In 1891 he became professor of chemistry at the Harris Institute, Preston, but the call to his real life work, in India, came before he had settled down there.

In 1892, as a result of the recommendations of Dr. Voelcker in his report on the improvement of Indian agriculture, the Secretary of State for India decided to appoint a chemist and an assistant chemist to the Revenue and Agricultural Department of the Government of India, and Leather was selected for the former appointment. It was in this position of agricultural chemist to the Government of India, and in that of Imperial agricultural chemist which followed it in 1906, that Leather did what may be regarded as his life work. There he remained with few intervals until, in 1916, he retired and settled down in Malvern.

Leather's appointment in India led to his having to range the length and breadth of the country, studying and advising upon the various problems which were placed before him by the authorities of the several provinces. He was, in fact, the only chemist attached to the agricultural departments in India. His activity was very great, but it was an almost impossible position. His publications during this period (most of which appeared in the *Agricultural Ledger*, then edited by Dr. G. Watt) were varied and numerous. They include the first general account of Indian soils, the first series of analyses of Indian manures, studies of alkali and salt lands, studies of sugar-cane, including the composition of the Indian varieties, and a multitude of other questions. On the whole, the conditions under which Leather worked at this time did not permit him to push any of his many inquiries to a final issue in the improvement of methods or the better utilisation of Indian resources. A summary of his work during this stage of his Indian career is contained in his final report on the first five years of the work of the Agricultural Chemist to the Government of India, issued in 1897.

The more congenial part of Leather's Indian work came in 1904, when the Imperial Research Institute at Pusa was founded, and he settled down as head of the chemical department of that Institute, as Imperial Agricultural Chemist. As a result of his activities there, we have a series of publications, most of them in the Memoirs of the Department of Agriculture in India. These deal with such subjects as the water requirements of crops in India, the composition of Indian rain and dew, soil temperatures in India, the problems of drainage and the loss of water from the soil in the tropics, and the interchange of calcium carbonate and carbon dioxide in soil under tropical conditions. It cannot be said that the work he did was of a spectacular character, but he gave us a valuable collection of data that did not exist before, and which nobody else has gathered together. With its limits, his results are always reliable, and for several generations many workers will bless the name of Leather for the careful observation which can form a sound basis for real agricultural advances to be initiated by others.

So far as his work generally was concerned, Leather was essentially a laboratory worker. Of his industry there was no doubt, and he was highly revered by the various assistants who worked under him during the long period of his activities in India. He had a great capacity for friendship, and there are many who look back to their association with him as a time when they were initiated into accurate laboratory investigation and into that close study of a limited objective which was a special characteristic of his work.

H. H. MANN.

SIDNEY MORGAN.

1878—1934.

THE writer's acquaintance with Sidney Morgan extends back to October, 1905, when he entered the Royal College of Science as a student in training. He followed what was then the usual three years' course, taking chemistry, physics and mathematics in the first session, mechanics and geology in the second, and finally the third year chemistry course. His student career was highly creditable and in July, 1908, he was awarded the A.R.C.Sc. in Chemistry. On leaving college, Morgan obtained a teaching post at the Jewish Free School, Bell Lane, Middlesex Street, and held it until 1910, when an opportunity occurred of recommending him for the post of a scientific officer to the Rubber Growers' Association. He seized with eagerness this chance of securing appointment in an important branch of chemical technology and his subsequent career thoroughly justified this selection. On proceeding to the Federated Malay States he became Resident Assistant Chemist to the Malaya Research Fund of the Rubber Growers' Association. In this capacity it was his function to place the technique of the preparation of plantation rubber in Malaya on a scientific basis. The practical importance of this pioneering work was highly appreciated by the Rubber Growers' Association and the first edition of Morgan's treatise on "The Preparation of Plantation Rubber" was published by this Association in October, 1913.

In the same year Morgan was appointed Senior Scientific Officer in Malaya and from that time until the date of his resignation in 1920 he continued to make important scientific contributions to problems of rubber preparation. Many of these are described in the editions of his book which appeared in 1922 and 1927. These two editions were written in collaboration with Dr. H. P. Stevens. The treatise is an indispensable work of reference to all interested in the technology of rubber.

In 1910 Morgan joined the firm of Macfadyen Wilde & Co. as a visiting agent and at the time of his death he was a director of several plantation rubber companies.

Sidney Morgan had an essentially realistic mind and was more attracted to the practical aspects of rubber problems than to the more fundamental questions of rubber chemistry and physics. Although somewhat reserved in discussions of such matters, he was personally keen and enthusiastic about his technological problems. He served the Rubber Growers' Association faithfully and played a considerable part in establishing its prestige. For this Association he compiled a digest of patents dealing with rubber and latex as applied to road construction. This summary was published by the Association in 1933. During 1934 he visited the East again to inspect the rubber estates in which he was interested and he died on Christmas eve of that year.

Sidney Morgan was discriminating in the choice of his friends, but those who knew him intimately bear tribute to his genial personality and kindness of heart.

In October, 1917, he married Nell, second daughter of the late Mr. and Mrs. A. F. G. McQuade of Sydney, New South Wales, who survives him.

G. T. MORGAN.

SIR JAMES WALKER.

Born April 6th, 1863; Died May 6th, 1935.

CHEMISTRY in general, and physical chemistry in particular, suffered a great loss in the death of Emeritus-Professor Sir James Walker, President of the Chemical Society in the years 1921—1923. Walker had retired from his occupancy of the Chair of Chemistry at the University of Edinburgh in 1928, but relinquished none of his interests in the science on retiral. Not only did he continue to function actively on many committees—such as the Advisory Council for Scientific and Industrial Research, the University Grants Committee, and the Carnegie Trust—but he maintained for several years an active connection with his old department, visiting it almost daily, participating in a most stimulating way in its various research activities, and lightening the administrative load of his successor by wise and kindly advice whenever solicited.

His many friends hoped that this Indian summer of a busy life-time would prove of long duration, but it was not to be. As his body weakened, his visits grew regretfully rarer, but his keenness of intellect and cheerfulness of outlook persisted practically to the very end. An instance may be given in relation to a meeting held by the Chemical Society of the University of Edinburgh only two weeks before his death. This gathering was originally planned as a diamond jubilee celebration, the official records of the society going back only to 1874, but just before the function was held it was discovered that the society actually existed in 1785, when Joseph Black was professor of chemistry, and was thus in fact celebrating its sesqui-centenary as the oldest chemical society in the world. Sir James, although too ill to attend, delighted the society with the following message: "I send my congratulations, and I feel sure that although the origin was 'Black,' the members of the Edinburgh University Chemical Society will make the future brilliant."

As the protagonist of the classical school of physical chemistry in Great Britain during the last forty years, it fell to Walker's lot to prepare the obituary notices for the Chemical Society of several of his distinguished continental colleagues, and to deliver the Memorial Lectures for van 't Hoff and Arrhenius, with both of whom he had been on terms of intimate friendship since the start of his scientific career. It is characteristic of Walker's innate thoughtfulness and generosity that, bearing in mind the difficulties he had encountered in establishing many of the personal details contained in these notices, he prepared after his retiral a collection of " Autobiographical Notes " expressly in order to lighten the burden of his own future historian. To those who knew Walker, these notes will reproduce his spirit far more vividly than any words of another could possibly succeed in doing. To those who did not know him, they will provide an insight into the human side of his character which could be obtained by no other means. They are therefore reproduced *verbatim* below.

* * * * * * * * *

My people are all Angus people. My grandfather, James Walker, came from Brechin to Dundee to start business as a master flax-dresser. My father, James Walker (b. 26 May, 1842; d. 6 April, 1904), followed in the same line, but as hand-dressing was gradually superseded by machine-dressing, he became ultimately a flax merchant. At the age of twenty he married Susan Hutchison Cairns (b. 1 March, 1838; d. 25 April, 1901), daughter of Arthur Cairns, Dundee. The only child of this marriage, I was born in Dundee on 6 April, 1863. I first went to the school attached to St. Peter's church, but as I was thought to be somewhat delicate in health at the time I was on medical advice withdrawn, and did not attend school again until I entered the High School of Dundee at the age of ten. In those days there was no school curriculum and each department had its independent head. In selecting classes the main object which the boys, if not their parents, had in mind was to secure if possible two consecutive play-hours. By resolutely avoiding classics I was in some years fortunate enough to attain the desired end. In retrospect the teaching seems to me to have been very good, English, French and Science standing out specially in my memory. A few years before I left school a science department was instituted under Frank W. Young, a young man full of enthusiasm and an inspiring teacher who instilled into many of us boys an enduring love for science.

At the age of sixteen I had passed the St. Andrews University local examinations, but I had no desire to continue formal study at a university, and chose rather to enter a business office. I was apprenticed to David Low, a flax and jute spinner, and during three years of my apprenticeship gained some knowledge of business men and business methods. The time spent in acquiring this experience I have never regretted. I found the office work by no means heavy and had plenty of time for discursive study as well as amusement. Mr. Young in addition to his school duties conducted evening science classes in the Y.M.C.A. building of the town, and there with a number of my old schoolfellows I attended lecture and laboratory courses in continuation of what we had done under him at school. My serious interest in science was increasing during this time, and at the end of my apprenticeship when I was offered the choice of going to Russia to gain acquaintance with the flax trade there, or of entering a university, I chose after considerable hesitation the latter course, the balance being turned by a chance meeting with a former school friend, somewhat senior to myself, who described in glowing terms the delights and advantages of student life in Edinburgh. A by-product of my connection with the flax trade, which afterwards proved of use to me in chemistry, was that in view of my possible move to the Baltic Provinces I had acquired a slight reading knowledge of Russian. This enabled me later to read Mendeleieff's treatise on chemistry when no translation into Western languages existed, and also to earn an honest penny as abstractor of Russian papers for the Journal of the Chemical Society.

An obstacle to my entering the University was my previous neglect of classics, for at that time Latin was a compulsory subject of the University entrance examination, and I had had no Latin at school. However, two months' intensive study was sufficient to see me through, and I matriculated at the University of Edinburgh in October, 1882.

There was no Faculty of Science in the University, although degrees in Science were given. The classes which I took out were Mathematics (Chrystal) and Natural Philosophy (Tait) in the Faculty of Arts, and Chemistry (Crum Brown), Botany (Dickson) and Natural History (Cossar Ewart) in the Faculty of Medicine. The last two were for me quite subsidiary subjects and I paid little attention to them. In the others I liked the work and took a respectable place in the classes. Chrystal's incisive style and Tait's clear exposition I greatly enjoyed, but my leaning was towards Chemistry. The elementary class of Chemistry was then attended almost entirely by medical students who had in general no interest in the science and made life a burden to the Professor. The first year's laboratory

Sir James Walker.

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instruction was a rigid mechanical system of test-tubing, of no practical or educational value, and much below the level of the instruction I had had years before under Mr. Young. Entering the advanced class was like entering a new world. Crum Brown's lectures on organic chemistry were of enthralling interest. He selected a few topics, and discussing them in detail really gave one an idea of what scientific method meant. The laboratory up to the degree stage was entirely devoted to qualitative analysis, but it was qualitative analysis properly taught under the direction of John Gibson, a pupil of Bunsen, who rigorously adhered to the exacting principles of the master. One was forced to understand what one was doing, and the occasional introduction of the rarer elements made separations by systematic routine tables impracticable. As it happened, my attendance in the laboratory was at a period of transition when conditions were far from normal. In the summer of 1884 I worked in the advanced laboratory, and should have attended it in the following winter also. The Chemistry Department, however, was in process of transference from the Old College to the University New Buildings (the Medical Buildings in Teviot Place), which were nearing completion and were partly occupied. At the beginning of the academic year 1884—1885 the former chemistry department in the Old College had been wholly given over to Zoology, and while the lecture-rooms and the elementary practical class-room had been for some time ready in the new buildings, the advanced laboratory was still in the hands of the workmen, with the result that during the winter session 1884—1885 no laboratory instruction was given at all. I was anxious to take the final B.Sc. Examination in April, so to meet my case Crum Brown arranged in the early spring that I should have, in a small room which had just been finished, an intensive course of analytical chemistry from John Waddell, the junior laboratory assistant. It was an excellent plan; I duly passed the examinations in Chemistry and Physics, and graduated as B.Sc. in April, 1885.

As it was now definitely my intention to seek an academic career in chemistry, I continued to work in the laboratory, mostly at quantitative analysis. Amongst fellow-workers were Orme Masson, Hugh Robert Mill, P. C. Ray, and Alexander Smith. With Smith I formed an early and lasting friendship.

In those days the degree of Doctor of Science could be obtained one year after graduation as Bachelor. When I asked to be allowed to begin a research with the object of presenting a thesis for the degree, I was told (rightly enough, no doubt) that I was not yet ripe for that. But I had neither time nor money to spare, and thought that if I were not permitted to do such work in Edinburgh I might manage to get it done in Dundee, where the University College had been founded in 1882. I went to interview Thomas Carnelley, the professor of chemistry there, and told him my difficulties and plans. Carnelley had built an admirable laboratory, and was eager to receive research workers. Accordingly, he accepted me and I spent the University vacations in his department, working early and late, and was able to send in a thesis to the University of Edinburgh, entitled "The Dehydration of Metallic Hydroxides by Heat," which was accepted. I was then allowed to proceed to examination, and graduated as D.Sc. in April, 1886, along with John Waddell and Hugh Robert Mill. The following summer and winter I spent as demonstrator in the Edinburgh laboratory. Amongst the students were John Shields, who afterwards collaborated with Ramsay in the well-known work on surface tension, and Hugh Marshall, the discoverer of the persulphates, and later my successor in the Chair of Chemistry at Dundee.

In those days it was practically imperative on anyone seeking a career in academic chemistry to spend some time in a German University, and the question arose as to which University I should choose. My own inclinations were towards physical chemistry. In 1887 I had read Lothar Meyer's "Theoretische Chemie," van 't Hoff's "Études de dynamique chimique," and the first volume of Ostwald's "Lehrbuch der allgemeinen Chemie." I was greatly attracted by Ostwald's book, and should have liked to work under his guidance, but Baltic friends advised me against going to Riga, where Ostwald then was, and I decided in the end to follow Alexander Smith to Baeyer's laboratory in Munich. As I had done little practical organic chemistry in Edinburgh—my first and almost only organic preparation was phenylhydrazine at a scholarship examination—I resolved to spend the summer session of 1887 in Carnelley's laboratory, where I received from him a very thorough training in combustion analysis. On arriving in Munich I soon performed the necessary organic preparations and combustions, and was placed as a research student under Ludwig Claisen, who set me to work on the condensation of benzaldehyde and ethyl acetate under the influence of sodium ethoxide. Baeyer I scarcely ever saw; as far as I remember, he spoke a few words to me only once.

Meanwhile, I learnt from the *Zeitschrift für physikalische Chemie*, the first volume of which was then appearing, that Ostwald had been appointed as professor of physical chemistry at Leipzig in October, 1887. Although I liked working with Claisen, the prospect of studying physical chemistry under Ostwald had superior attractions, so at the end of the winter session I left Munich for Leipzig.

Ostwald received me in a very friendly manner, and set me a theme which I was able to finish in the summer session. The old laboratory in Brüderstrasse which Ostwald occupied afforded ample room for the small number of research workers who had then found their way to him. We were of very varied nationality with seldom more than one example of each nation. As far as my recollection goes, there was only one genuine German research student in the physico-chemical laboratory. I worked in the same room as Wilhelm Meyerhoffer, who later became van 't Hoff's chief collaborator. We were greatly stirred by a visit which Arrhenius paid to Ostwald that summer, and were overjoyed to make his acquaintance. Although few worked in Ostwald's physico-chemical laboratory, his lectures on physical chemistry were well attended by students who came from the main institute, which was under the direction of Wislicenus. Ostwald's chief assistant was Ernst Beckmann, who was, however, in a practically separate section of the department, being concerned with the teaching of students of pharmacy, nominally under Ostwald's charge. Walther Nernst was appointed assistant in the physico-chemical laboratory, but little call was made on his time for teaching. When I returned to Leipzig in 1888 after the summer vacation, the number of workers in physical chemistry had considerably increased. Arrhenius came as an independent researcher, and relieved Nernst when the latter had to take an enforced holiday on account of an illness. He took a keen interest in the work on the affinity constants of bases on which I was engaged, and used some of my results in his fundamental paper on salt-hydrolysis.

I graduated as Ph.D. in July, 1889, and left Leipzig to take up the post of research assistant to Crum Brown, a post which I held for three years. I was not on the University staff, but was permitted to deliver courses of lectures to advanced students on various branches of physical chemistry. My work with Crum Brown was interesting, if arduous. He was brimful of ideas and my chief difficulty was to get one tried out before the next came along. He had lost touch with laboratory work and had little appreciation of practical difficulties. One day he mentioned that he had once electrolysed sodium ethyl tartrate in the hope of preparing, by an electro-synthesis analogous to that of Kolbe, the ethyl ester of a tetrahydroxyadipic acid, but had not succeeded in obtaining anything. This seemed to me an admirable notion, and I thought it would be worth while to apply it to a simpler case. As it happened, Crum Brown succumbed to the influenza epidemic then raging and by the time he returned to duty I was able to show him a quantity of diethyl succinate which I had prepared by the electrolysis of potassium ethyl malonate. This opened the way to an extensive research on the electro-synthesis of dibasic acids, and on this I was engaged during the rest of my stay with Crum Brown. He was very generous in allotting me time for my own work, so that I was able also to continue research on the physico-chemical lines of Leipzig.

In this period falls the inauguration of the Alembic Club. The assistants in Chemistry used to lunch together in a dark and dismal chamber of execrable proportions imposed by the handsome external architecture of the building. They were John Gibson, Leonard Dobbin, Hugh Marshall, Alexander Smith, and myself. We constituted ourselves into a club and arranged meetings outside working hours to discuss chemical problems of interest. Later we undertook the publication of fundamental papers of historical interest bearing the title of "Alembic Club Reprints." Leonard Dobbin, our secretary, played the chief part in this venture, which met with a gratifying success.

As Ostwald's first English pupil I was of some assistance in introducing the modern physico-chemical theories into this country. I had translated into English Ostwald's "Grundriss der allgemeinen Chemie," and helped in the defence of the new theories against the violent opposition which they at first encountered. The meeting of the British Association at Leeds in 1890 was distinguished by a historic discussion of these theories in which van 't Hoff and Ostwald took part. Arrhenius was unable to be present, but he entrusted me with the reading of a paper, which was well received. This meeting was for me personally of decisive importance, for there I made the acquaintance of William Ramsay, who was the solitary well-known chemist in this country to accept with heartiness the theories of osmotic pressure and electrolytic dissociation. Ostwald introduced me to him, pronouncing my name in the German fashion, and we conversed for a long time in German before Ramsay discovered he was speaking to a fellow Scot. I promptly made up my mind to work in Ramsay's laboratory as soon as an opportunity offered.

At the time of the meeting of the British Association which was held at Edinburgh in 1892 and was attended by Ramsay, Arrhenius, and Ostwald, the research on electrosynthesis with Crum Brown was practically at an end, and I now resolved to carry out my plan of working with Ramsay. Crum Brown offered me a post on the University teaching staff, but I felt that the time had come for me to make a move, and so in the autumn I entered Ramsay's laboratory in University College, London, as a research worker in the hope of sooner or later obtaining a teaching post there. This hope was after a year fulfilled, and in 1893 I became Ramsay's second assistant. My teaching duties were heavy, and I had practically to give up research work in the Laboratory. Norman Collie was my senior on the staff, and amongst the students were Morris W. Travers and E. C. C. Baly. Annie Purcell Sedgwick, who afterwards became my wife, was a research student under Collie.

In 1894 the Chair of Chemistry in University College, Dundee, fell vacant through the resignation of Percy Frankland, and I had the good fortune to be selected to fill this post in my native town. The first few years of my tenure were made memorable by an academic dispute of an unusual nature. The College had been "affiliated and made to form part of" the University of St. Andrews. On my appointment I became, therefore, a member of the Senatus of the University, and took part in University business. The validity of the legal steps which had been taken to effect the union was, however, contested, and it was decided by the House of Lords that the union was invalid. The professors of the College then ceased to be members of the University, and their classes no longer by right qualified for St. Andrews degrees. Other measures were now taken to carry out the union in a more binding fashion, and after a protracted contest, the House of Lords ultimately decided that the affiliation in its new form was valid, and once more we were admitted to the Senatus of the University, and our position as University teachers regularised. This occurred in the year 1897, and on 1 September of the same year I was married. My son, Frederick Walker,* was born on 18 August, 1898.

During the fourteen years of my occupation of the Dundee chair, the Chemistry department grew, though somewhat slowly. I was happy in having excellent assistants (Fred. J. Hambly, J. R. Appleyard, and their successors John Lumsden and J. K. Wood) who made the teaching work of the Chair light, and collaborated with me in research. Of the kindness and helpfulness of my chemical colleague in St. Andrews, Thomas Purdie, I have the warmest and most grateful memory. He was indeed one of the salt of the earth.

Amongst those who studied with me were John Johnston, who became head of the chemistry department of Yale University, S. A. Kay and A. C. Cumming, a research student from Melbourne, both of whom were afterwards associated with me in Edinburgh. The laboratory planned by Carnelley was not completed in his time. Frankland added a portion, and I the remainder. The experience of laboratory construction and equipment I gained stood me in good stead later.

In 1908 Crum Brown resigned and I was appointed to succeed him in the Chemistry Chair at Edinburgh. By this time the science laboratories of the Edinburgh department had become, notwithstanding recent extensions, hopelessly inadequate for their purpose.

* Now Lecturer in Geology and Geography in the University of St. Andrews.—J. K.

It was difficult to secure sufficient accommodation for ordinary advanced students, and such research students as there were had to be squeezed into any odd corner.

I at once began to agitate for a new department, but had to be content for some years with a little additional space at High School Yards (another University site), at first in a wooden shed, and later in the basement of an addition to the Physics Laboratory. This splitting of the department was very inconvenient but had to be endured, as any further extension of the main laboratories was impossible.

On assuming office in Edinburgh I found that two practically identical elementary courses of lectures were given, one to a large number of male students, mostly medical, and the other to a small number of women students, who were not at that time permitted to attend any medical class. I was empowered to rearrange matters by splitting the elementary class not according to sex, but according to faculty. A purely medical lecture course was established, the teaching being adapted to the requirements of the students in their medical curriculum. Science students attended the other course, which was of a more general character, and this course the medical women also attended until, nearly a decade later, they were allowed to mix with the medical men.

In 1913 the University Court allocated a site at High School Yards for a new chemistry department, plans for which were nearing completion when the Great War broke out. Beyond a sudden depletion of the classes due to students volunteering for service, the war did not at first make any great change in the department, but as time went on and the prolonged nature of the struggle was realised the male students practically disappeared except in the medical class, which owing to the demand for doctors in the army remained almost normal.

In 1915 there was a sudden outcry for high explosives, the supply of which had been found very defective. Trinitrotoluene (T.N.T.) in particular was demanded in large quantities, and it occurred to A. C. Cumming, now a member of the Edinburgh Chemistry staff, that it might be possible to utilise a local manufacture of sulphuric acid in producing this explosive. At that time very little oleum (fuming sulphuric acid) was made in this country, and it was imperative that the nitration of toluene should be carried out not by means of oleum but by means of ordinary concentrated acid which was available in much larger amounts, and could be recovered from spent acid. Published literature showed that the use of oleum could be avoided and experiments were started to evolve a suitable two-stage process for the production of the explosive on a large scale. Negotiations were entered into with the Munitions branch of the War Office, and in order to effect a contract with the Government a private company was formed, the Lothian Chemical Company Limited, of which the members were A. C. Cumming, J. W. Romanes and myself. Romanes, an Edinburgh graduate, had had considerable industrial experience, and was at the time engaged in the laboratory on technical research work. A process was worked out and plant installed in a disused chemical factory, which was staffed, as far as management and chemists were concerned, entirely by teachers, graduates or students of the University. From the beginning the working of the factory was successful, and we had the distinction of being the only firm to complete its original contract for T.N.T. within the specified time. Other contracts followed, but as the factory was in a populous district of the city, the liability to explosion led the Ministry of Munitions to ask us to act as managers in the construction and running of a Government factory in the outskirts. We selected as site the yards of disused quarries at Craigleith, and the works erected there were known as H.M. Factory, Craigleith. They remained active till the end of the War.

The period of demobilisation and the years succeeding it brought grave problems to the University. There was the certainty of a huge influx of ex-service men as students into the already insufficient laboratories. In 1912 I had urged the foundation of a new chemistry Chair devoted to the requirements of students of medicine, and it was now seen that the establishment of this professorship would at least ease the burden of teaching in the crowded years to come. The University Court therefore founded the Chair of Chemistry in Relation to Medicine, and George Barger became its first incumbent in the summer of 1919. The other problem was the immediate provision of laboratories. It was seen that the scheme of 1913 was unsatisfactory, as the site would not give the necessary accommodation. After a careful inspection of all the ground available in the neighbourhood of the University, it was evident that no suitable site existed there for the purpose. I suggested that the most convenient location of new buildings would be near the then terminus of the tram-line running south past the Old College. A piece of land adequate to the needs of the chemistry department could be obtained there, but it was felt that a larger scheme providing for other scientific laboratories should be envisaged, and ultimately the adjacent King's Buildings site of 112 acres, situated on West Mains Road, was purchased. A beginning was made to build the chemistry department in November, 1919. The laboratories were constructed and occupied in three successive sections to meet the needs of the large classes as they passed from year to year. The foundation stone of the building was laid by the King in July, 1920, and the completed building was opened by the Prince of Wales in December, 1924. One of the main features in the new laboratories was the provision made for research workers. Hitherto it had been impossible to encourage students to remain on after graduation, but now that space was available, and now that the research degree of Ph.D. had been instituted, an increasingly large number of post-graduate workers made use of the laboratories.

After twenty years' service in the University I resigned the Chair in September, 1928, and was succeeded by James Kendall, whom I had found in Edinburgh as a second year student when I took over the Chair from Crum Brown.

* * * * * * * * *

It remains to discuss briefly the scientific work of Sir James Walker. Although his pre-eminence in physical chemistry overshadowed his activities in other fields, Walker was also a skilled organic chemist, and his success in attacking purely technical problems was exceptional. As an example of his investigations in organic chemistry, his remarkable work on the electrosynthesis of dibasic organic acids (*Trans. Roy. Soc. Edin.*, 1891, **36**, **211**; 1893, **37**, 361) may be cited. Regarding his prowess on the industrial side, it will suffice to record the statement of the Department of Explosives Supply that the nitrogen economy figures for H.M. Factory, Craigleith, during the months of September and October, 1918, constituted a record for the country. Nevertheless, to quote from a lecture which Sir James was invited to deliver before the Chemical Society in 1919, describing the operation of this factory (J., 1920, **117**, 382), " it is certain that if the production had continued, an immediate further saving of at least 2 per cent. would have been effected, the alteration in the plant necessary to secure this being practically ready at the date of the armistice."

In physical chemistry itself, Walker's contributions are too numerous for detailed mention. Fortunately, it is altogether unnecessary to describe his research work here at any length, since he seemed to possess an uncanny facility for selecting simple problems of fundamental importance, with the result that his main investigations—for instance, on ionisation constants, on hydrolysis, on amphoteric electrolytes, and on many other aspects of the van 't Hoff–Arrhenius theory of solutions—have long been incorporated in the standard text-books and are familiar even to beginners. Walker's own " Introduction to Physical Chemistry " first appeared in 1899 and has since passed through ten editions. Its most significant feature is its " readability " and it has probably assisted more students towards an easy, yet sound, appreciation of the subject than any other single volume. The writer of this article has even seen it in use in a small town in the interior of Finland as a text-book for a class in English !

Simplicity was, indeed, the key-note of all Walker's activities; in research, in teaching, and in administration he always took the direct route. Another striking point in his character was his tenacity; no problem, once taken up, was dropped until every single detail connected therewith had been made clear. In his last years he was still unofficially supervising work at Edinburgh on Kolbe's electrosynthesis and the transformation of cyanates to ureas—topics which he had first attacked forty years ago. A third trait was his adaptability. In an age of increasing specialisation, he retained to the end an unusually wide range of interests, and kept himself up-to-date in a great many diverse fields. Remembering the bitter controversies in which he participated, as a young man, while the revolutionary ideas regarding the nature of solutions were being forced upon his reluctant seniors, he was always particularly open-minded with respect to the work of the new generation of physical chemists which has recently effected a second revolution in this same field. As he remarked quite complacently, " if they can see further than Arrhenius, it is, after all, only because they are standing on Arrhenius's shoulders."

Walker's abilities and services to chemistry were recognised by many honours. The Royal Society of Edinburgh awarded him the Makdougall-Brisbane Medal at the beginning of his career in 1895, and the Gunning-Victoria Jubilee Prize at its close in 1933. He was elected to the Royal Society of London in 1900, and gained its Davy Medal in 1926 " for his work on the theory of ionisation." His presidency of the Chemical Society in 1921—1923 was distinguished by two notable addresses, the first on " The Rôle of the Physicist in the Development of Chemical Theory " (J., 1922, **121**, 735), the second on " Symbols and Formulæ " (J., 1923, **123**, 939). He was also president of the Chemistry Section of the British Association in 1911, vice-president of the Royal Society in 1927—1928, vice-president of the Royal Society of Edinburgh in 1916—1919, and vice-president of the Society of Chemical Industry in 1925—1928. He was an LL.D. of the Universities of St. Andrews and Edinburgh, and in 1921 he received a knighthood.

Few men, however, had less desire for honours than Walker. He carried his learning with singular modesty, and worked unselfishly always for his department and for his profession. His scientific progeny, including not only those who have studied directly under him but those who have been inspired by his writings, are legion. Wherever physical chemistry is mentioned among English-speaking chemists, the first name that springs to mind is the name of Sir James Walker.

JAMES KENDALL.

JAMES ALFRED WILKINSON.

1873—1934.

IN Prof. J. A. Wilkinson, who died in Johannesburg on October 1st, 1934, after a very brief illness, South Africa loses a man of many parts who exercised an important rôle in the development of science and education in the country of his adoption.

He was born at Shawforth, near Manchester, on June 10th, 1873, and was educated at Manchester Grammar School and at Caius College, Cambridge. Not only did he win many scholastic and academic distinctions, but he was athletic champion of his school for three years in succession. Going to South Africa in 1902 as Science Lecturer to the Transvaal Education Department, he became Professor of Chemistry at the newly constituted Transvaal Technical Institute in Johannesburg in 1904 and held that rank in the various institutions that evolved from that body until it attained the status of the University of the Witwatersrand, latterly adding to his duties that of the Chair of Chemical Engineering. He retired from the two Chairs on reaching the age limit in 1934. He married Lilian Elizabeth Hudson in 1912 and had one son, who is, at present, an undergraduate in the Faculty of Medicine at the University of the Witwatersrand.

As was inevitable where a centre of learning had to be built up from practically nothing, Wilkinson was unable to engage in original work, but he contributed a number of papers on general scientific and educational subjects to various South African Journals. He laboured greatly, however, in the cause of higher education in Johannesburg and the strong position of the University, in general, and of its chemical department, in particular, owes much to his untiring efforts and great organising ability. His lectures were always interesting, as he took great pains in their preparation and in the devising of experiments, but his special pride was his school of Chemical Engineering, which he built up alone and in the face of some opposition and which has produced men who have been conspicuously successful in after-life.

Wilkinson had a good deal of the pugnacity and doggedness of the north-countryman, but he was always on the side of the angels and fought many a battle for what he thought were the bests interests of the University and its students. He was greatly admired by all those who worked under him and was unstinting in his efforts to help his students while at the University and in after-life; the last letter the writer received from him after his retirement and shortly before his death was a very detailed one on behalf of one of his old students who was coming to England. He also gave of his time to the athletic activities of the students and was President of the University Athletic Club.

In spite of his manifold activities at the University, Wilkinson played an important part on the larger stage. He took a great interest in Red Cross work and was first Chairman of the Executive Committee of the South African Red Cross Society. He was also a member of the Committee of the Johannesburg Public Library, of the Commission for the Preservation of National and Historic Monuments in South Africa, and of the Council of St. Mary's Cathedral. He was a past-President of the Chemical, Metallurgical and Mining Society, the South African Chemical Institute, and of the South African Association for the Advancement of Science, and as Chairman of the Executive Committee of the last body in 1929 he did much to make a success of the visit of the British Association for the Advancement of Science to South Africa in that year.

O. L. Brady.

JAKOB MEISENHEIMER.

1876—1934.

Jakob Meisenheimer, Professor of Chemistry in the University of Tübingen, who died on Dec. 2nd, 1934, in his 59th year, was born at Griesheim a.M. on June 14th, 1876. He came of peasant stock. His father, like most of his forefathers, was a farmer, leading a simple life on the land. Yet his was a remarkable family, for both he and his elder brother Johannes (who held the Chair of Zoology at Leipzig) attained scientific distinction and were called to University Professorships at comparatively early ages.

Meisenheimer's school years were spent at a classical gymnasium, but from his boyhood he had shown a strong inclination towards natural science. He studied chemistry at the University of Heidelberg from 1895 to 1896 and then went to Munich. At Munich he found himself in a most inspiring environment. He worked in Baeyer's laboratory and carried out his course of research for the doctor's degree under Thiele, of whom he always spoke with great admiration. Willstätter was working in the same room, and here began his friendship with Wieland, a friendship which lasted throughout life and found beautiful expression in a lecture which Wieland gave in his memory in the *Horsaal* of the Chemical Institute at Tübingen.

Meisenheimer passed his doctor's examination at Munich in October, 1898, *summa cum laude*, and he used to relate with some pride that Baeyer examined him for nine minutes only instead of the hour prescribed. It was at Munich also that he found that love of the mountains that he never lost. Every year, except the War years and those immediately following, when travel was difficult, he spent several weeks in his beloved Tyrol. He was a skilful climber and made many ascents, always without a guide, accompanied only by a friend or, after his marriage, by his wife.

After taking his doctor's degree he remained in Munich as assistant to Thiele until 1904; he then went to Berlin to take up the post of lecturer at the *Landwirtschaftliche Hochschule* under Buchner. When Buchner left in 1909 to become Professor of Chemistry at Breslau, Meisenheimer was appointed to succeed him as Professor of Chemistry and Director of the Institute. While *Assistent* at this Institute he met his future wife, Fräulein Elmire Thiel, daughter of Ministerial-Direktor Dr. Hugo Thiel of the Prussian Ministry of Agriculture. She had begun a *Doktor Arbeit* under his direction, but left it uncompleted on account of her marriage, which took place in July, 1909. With two sons and two daughters she survives him.

At the outbreak of war in 1914 he took the field with the *Garde-Reserve-Korps* and served in Belgium, East Prussia, Poland and France, finally as Captain and Commandant of a Pioneer Battalion. He received the iron cross, second and first class.

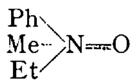
In the last year of the War—in April, 1918—he was appointed to the Professorship of Chemistry at Greifswald, and in October, 1922, he received the call to succeed Wilhelm Wislicenus as Director of the Chemical Institute of the University of Tübingen.

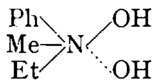
Here in this ancient university town on the Neckar—that still retains so much of its mediaeval charm—he lived in the official house of the Director adjoining the Laboratory in the Wilhelmstrasse and very near to the old Chemical Institute where his predecessors in his office, Lothar Meyer and von Pechmann, had lived and worked.

Meisenheimer had an intense love for his work and strictest honesty in it. His work was characterised by scientific thoroughness.

He was very reserved, but his kindliness, his unaffected manner, and his quiet humour made his personality one of no little attractiveness. He had no great worldly ambition; the position he filled was such as he would have desired, and his life was a happy one. Even in the last two years when he came under the shadow of a mortal disease he had his happy hours and he never lost his sense of humour. He worked almost till the end.

His scientific investigations extended over a wide variety of subjects, but he will be remembered chiefly for his outstanding contributions to the stereochemistry of nitrogen. The earliest of these was his demonstration of the molecular dissymmetry of suitably substituted amine oxides (*Ber.*, 1908, **41**, 3966). On general principles the presence of four different radicals in a non-planar compound should suffice to produce molecular dissymmetry. In the ammonium salts, however, the experimental evidence (due largely to H. O. Jones and his co-workers) indicated that all five of the radicals attached to the nitrogen must be different for the salt to be resolvable into antimeric forms; if two were alike, the salt was non-resolvable. Meisenheimer now showed in a paper entitled " A new kind of asymmetry associated with the nitrogen atom " that methylethylaniline oxide (or its hydrate) could be resolved into optical antipodes through its bromocamphorsulphonate.





He regarded this as proving that a compound of quinquevalent nitrogen could in fact exist in antimeric forms although two of the valencies were attached to like radicals. In the compounds examined by Jones the two like radicals were linked to the nitrogen by valencies of the same kind (co-valencies), whereas in the amine-oxides (written with a double bond between the oxygen and the nitrogen where a " co-ordinate " or " semipolar " link is now believed to exist), and in their hydrates, they were attached by valencies of different kinds (one electro- and one co-valency). Meisenheimer regarded this discovery as giving strong support to Werner's new theory of ammonium salts and indicating the probability of a tetrahedral configuration of the ammonium ion. He was thus led to adopt what is now the general view of the structure and configuration of these substances. With his pupils he subsequently resolved many other compounds of this type, and one of these, methylethylallylamine oxide (*Annalen*, 1922, **428**, 252), is of special interest as the only purely aliphatic ammonium compound which has as yet been resolved into optical antipodes.

He also gave an ingenious experimental proof that the fifth valency in quinquevalent nitrogen compounds was chemically different from the other four (*Annalen*, 1913, **397**, 273). Trimethylamine oxide gave a methiodide which with sodium hydroxide generated the base (I). On the other hand, the hydriodide of this amine oxide gave with sodium

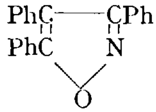
(I.) $[Me_3N{\cdot}OMe]OH$ $[Me_3N{\cdot}OH]OMe$ (II.)

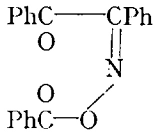
methoxide the isomeric methoxide (II). The isomerides were distinguished by their behaviour on heating; (I) gave formaldehyde, trimethylamine, and water, (II) gave methyl alcohol and trimethylamine oxide.

Investigation of the corresponding phosphorus compounds, the phosphine oxides, led also to interesting results. In contrast to the ammonium salts, phosphonium salts of the type $[PR_1R_2R_3R_4]X$, R_{1-4} being hydrocarbon radicals, have resisted all attempts at resolution, but Meisenheimer found that the salts of the phosphine oxides $[PR_1R_2R_3(OH)]X$ could be resolved without any special difficulty (*Ber.*, 1911, **44**, 356; *Annalen*, 1926, **449**, 213) and these are still the only optically active compounds known which owe their dissymmetry to phosphorus.

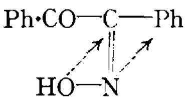
His most valuable contribution to stereochemistry was probably his proof of the correct spatial interpretation of the Beckmann transformation of oximes. The observation that in this intramolecular change stereoisomeric oximes give structurally isomeric amides was made by Hantzsch in 1891, but the assumption on which he based the method almost exclusively used for fixing the configuration of the ketoximes—that in this transformation the hydroxyl of the oximino-group changes places with the radical on the *same* side—went without experimental control for nearly 30 years, although its entirely arbitrary character was well recognised (*e.g.*, Werner, " Lehrbuch der Stereochemie," Jena, 1904, 277; Bucherer, " Lehrbuch der Farbenchemie," Leipzig, 1914, 202).

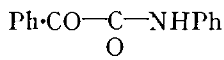
Meisenheimer put this assumption to experimental test and found that it was erroneous: his work thus led to the reversal of the whole system of oxime configurations built up on the results of the Beckmann change. The proof depended on a highly ingenious method which he devised for determining the configuration of the benzil monoximes, based on the only reliable chemical means for fixing the configuration of geometrical isomerides—that of relating one of the isomerides to a cyclic compound. His plan consisted in the oxidative fission of the ethylenic linking in triphenyl*iso*oxazole (III) by formation of an ozonide and its decomposition by water. This should yield the benzoyl derivative of a benzil monoxime which would necessarily have the *syn*-configuration (IV). The fission was found to proceed

(III.) 

 (IV.)

smoothly at room temperature and the product proved to be the benzoate of the β-monoxime. The β-monoxime is therefore the *syn*-isomeride. Hantzsch, however, had assigned the *anti*-configuration to it, since its Beckmann transformation yielded benzoylformanilide. The production of benzoylformanilide from the *syn*-isomeride proves that that in this case the Beckmann change involves the *trans*-, not the *cis*-interchange of the radicals. Meisenheimer pointed out that if a *trans*-interchange is taken to represent the

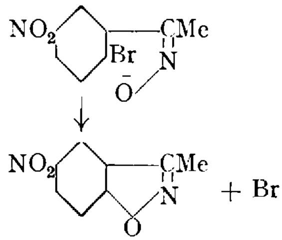


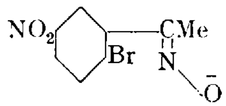


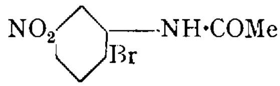
normal course of the transformation, the chemistry of the oximes becomes in many ways more reasonable and intelligible than if the configurations based on the older view are assumed.

He made it exceeding probable that the *trans*-interchange was indeed the normal course of the transformation by showing that in two other cases also—each of a very different type from the foregoing—it likewise proceeded in the *trans*-manner.

The former of these cases was that of the isomeric oximes of 2-bromo-5-nitroacetophenone, and the method employed was analogous to that used by Brady and Bishop to determine the configuration of an aldoxime. Meisenheimer showed (*Annalen*, 1926, **446**,

(V.)  (VII.)

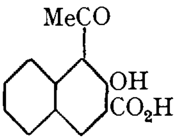
 (VI.)

 (VIII.)

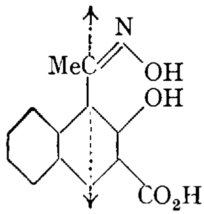
207) that in alkaline solution, in which the oximes exist as anions (V) and (VI), one form undergoes ring-closure with elimination of a bromine ion and formation of a benz*iso*oxazole (VII) very much more readily than the other. This reactive form must necessarily have

the *syn*-(aryl)-configuration (V). The other, more stable isomeride, which must accordingly be the *anti*-(aryl)-form (VI), was found to pass by the Beckmann change into the substituted acetanilide (VIII). The Beckmann change therefore here also takes the *trans*-course.

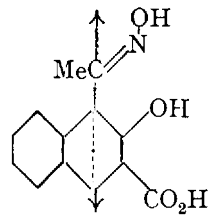
The third case was one of special interest by reason both of the ingenuity of the design and of the experimental skill required for its execution. He prepared the substituted naphthyl methyl ketone (IX) and showed that it gave rise to two isomeric oximes (X and XI). He then determined their configurations in a very ingenious way. He had been



(IX.)



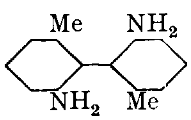
(X.)



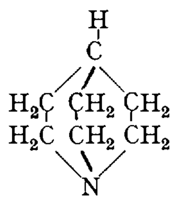
(XI.)

much interested in the optical isomerism discovered by Kenner in the diphenyl series and had independently formed the view that it was due to a purely mechanical steric hindrance of intramolecular rotation. He had moreover provided important experimental evidence in favour of this view by resolving the compound (XII) (*Ber.*, 1927, **60**, 1425), in which the substituent groups are of such a nature that they scarcely offer the possibility of any other kind of interaction. On the basis of this view he predicted that the oxime (X) would have restricted rotation about the axis indicated and should be resolvable. The isomeride (XI), having of course free rotation, should be non-resolvable. He showed that one of the oximes could in fact be resolved, its optical activity disappearing, as it should do, somewhat rapidly, while the other was non-resolvable. The configurations of the two oximes were thus determined, and here also the Beckmann transformation was found in each case to take the *trans*-course.

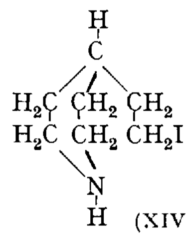
He devoted a very great deal of attention to the stereochemistry of compounds of the amine type. He had long been convinced of the pyramidal arrangement of the nitrogen valencies in substances of this class and he provided indirect experimental evidence of it by his synthesis of quinuclidine (XIII), the tricyclic compound from which one half of the molecule of the cinchona alkaloids is derived (*Annalen*, 1920, **420**, 190). He found that it was formed with such readiness from the piperidine derivative (XIV) and showed such



(XII.)



(XIII.)



(XIV.)

extraordinary stability and resistance to chemical attack as to indicate strongly that it must have a strainless molecule: and if its molecule is strainless, then the normal arrangement of the nitrogen valencies must be pyramidal. He did a great deal of work in endeavouring to resolve different types of unsymmetrically substituted amines and in disproving various supposed cases of isomerism attributed to non-planar 3-co-ordinate nitrogen. A paper which he read at the meeting of the British Association at York in 1932 dealt with the former topic.

Among the variety of other subjects which he investigated was alcoholic fermentation. He had worked on this with Buchner and never lost his interest in it, returning to it several times in later years. The subject in which he was chiefly absorbed towards the end of his life was one on which he had been engaged while still a young *Assistent* at Munich—the problem of substitution and addition. He had hoped with the aid of the newer physico-chemical methods to probe more deeply into the nature of these fundamental processes, but death overtook him before he could bring his investigations to a completion.

W. H. MILLS.