OBITUARY NOTICES.

ROBERT HENRY ADERS PLIMMER.

1877—1955.

Robert Henry Aders Plimmer, who died on June 18th at the age of 78, was the eldest son of Alfred Aders of Manchester. He was educated at Dulwich College and University College, London, graduating B.Sc. in 1899; he then went to Geneva where, under the direction of Professor Carl Graebe he worked on a problem connected with the constitution of euxanthic acid, the yellow substance obtained from the urine of cows fed on mango-leaves and better known as piuri or Indian yellow; to this extent the research had a remote connection with the subject later to become known as biochemistry—a fortuitous example of coming events' casting their shadows before. Actually Aders, as he was then known, had received much encouragement from his stepfather H. G. Plimmer, F.R.S., the Professor of Bacteriology at St. Mary's Hospital, to devote his chemical knowledge and training to physiology and pathology. With this idea in mind he went to Berlin University, where he became a student under Professor Emil Fischer who set him to investigate the products of hydrolysis of gelatine by the, at that time, new technique of separating amino-acids by the fractional distillation of their ethyl esters, under reduced pressure, a method which in the light of modern methods of paper chromatography is only of historic interest. The results of this research were published in the *Annalen* under the joint names of Fischer, Levene, and Aders; it was not until a little time after this that Plimmer adopted his stepfather's name. The experience gained under Fischer was no doubt the inspiration for writing the two monographs on the analysis of proteins some time later. On his return to London, Plimmer was awarded the D.Sc. of London University for a thesis on proteins. His first appointment was to a Grocers' Company research studentship at the Lister Institute; this he held for two years before joining the staff of the Physiology Department of University College under Professors Starling and Bayliss. Starting as an assistant in 1904 he was made Assistant Professor of Physiological Chemistry in 1907 and Reader from 1912 until 1919. During the first World War he was working at the R.A. Medical College on the energy value of foods, which involved a large amount of accurate analytical work the results of which were published by H.M. Stationery Office in 1921. This must in some ways have been a labour after his own heart since all his life he manifested a strong liking for the analytical side of chemistry, his earlier independently published papers being concerned with the quantitative determination of cyanide in presence of chloride and on the phosphate content of phytin. In 1919 Plimmer was appointed head of the Rowett Research Institute, Aberdeen, which had been founded by an old schoolfellow of his whom he got to know at Dulwich College. Here again he had an opportunity for indulging in his liking for analytical work, in assaying the biological value and vitamin content of food stuffs; his interest in vitamins had previously developed while at University College and resulted in the joint publication with his wife, a student of his, of a small book entitled " Food, Health, and Vitamins " which ran into 9 editions. Another most successful publication was the well-known " Organic and Biochemistry " which became the almost universally accepted textbook for 2nd M.B. students and others. From 1922 until 1943 Plimmer held the Professorship of Chemistry at St. Thomas's Hospital Medical School where he was a most successful teacher of inorganic, organic, and biochemistry as well as toxicology. During this time he continued his researches on protein analysis, begun at Aberdeen; on his retirement in 1944 he was made Emeritus Professor. In the same year he was invited to join the Staff of the Postgraduate Medical School of London where he undertook the supervision of the analyses of pathological material. As far back as 1908 Plimmer was appointed co-editor with Professor Sir Frederick Gowland Hopkins, F.R.S., of the well-known " Monographs in Biochemistry." In January, 1911, he and J. A. Gardner called a meeting of biochemists to form a Biochemical Club which later became the Biochemical Society; some years later he wrote the history of the Society for the period of 1911—1949 which was published as a special number of the *Biochemical Journal*; his connection with the foundation of the Society and subsequent development of Biochemistry in this country forms perhaps his best memorial. Plimmer had very strong views on most subjects, a fact which, combined with a somewhat brusque manner, was not conducive to an easy approach and tended somewhat to overshadow his fundamentally generous nature and may have stood in his way of attaining some of the higher scientific honours which were his due.

P. Haas.

WALTER WILLIAM REED.

1885—1955.

MR. W. W. REED was born in Essex and educated at Brentwood Grammar School, passing from there to the University College of North Wales, Bangor. He was awarded a First Class Honours Degree in Chemistry in 1906 and an M.Sc. in 1908. In the same year he took the examination of the Associateship of the Institute of Chemistry, as it was then called, specialising in Organic Chemistry. Three years later he converted this Associateship by examination into the Fellowship of the Institute. On leaving Bangor Mr. Reed joined the staff of the Chemistry Department of the Technical College in Huddersfield where he remained until returning to his native East Anglia to become a Lecturer in Chemistry at the Technical College, Norwich. He stayed at Norwich for the rest of his teaching life, until 1950, being Head of the Science Department and Vice-Principal of the College at the time of his retirement.

Outside the Norwich City College and Art School, Mr. Reed devoted his time to church work and the Church Lads' Brigade. For fourteen years he was People's Warden for the Church of St. Michael at Plea, and since 1912 he had been closely associated with the Church Lads' Brigade.

R. JONES.

SIGMUND OTTO ROSENHEIM.

1871—1955.

OTTO ROSENHEIM's death on May 7th at the age of 84 removes one of the Senior Fellows of the Chemical Society, elected to the Society on May 7th, 1896, and a notable biochemist of uncommon versatility. Rosenheim was born at Würzburg in Germany on November 29th, 1871. In July, 1910, he married Mary Christine Tebb, a Bathurst Student of Girton College and daughter of William Tebb of Rede Hall, Burstow.

Rosenheim took his Ph.D. degree at Würzburg, working under Tafel in Emil Fischer's laboratory. He spent part of the course in Bonn and returned to Würzburg to complete his degree. The subject of his thesis was the oxidation of 6-hydroxyquinoline, and his examiner was Hantzsch who had just succeeded Fischer. After some months of military service he went to Geneva to work with Graebe and there he met Liebermann, Pictet, and Kehrmann. The outlook for one of his persuasion in Germany was not rosy, so Rosenheim decided to make his domicile in England. He therefore wrote to W. H. Perkin at Manchester and Perkin facilitated his entry for research in chemistry at Manchester University in 1895.

In 1896 Rosenheim joined Philip Schidrowitz in practice as Analytical and Consulting Chemists at a laboratory in Chancery Lane, London. Dr. Schidrowitz writes of Rosenheim at that time as a modest and pleasant young man, especially interested in work of a scientific character and in particular in biological chemistry. During their association Rosenheim and Schidrowitz published original observations on a variety of subjects including the optical activity of gallotannic acid, compounds of piperidine with phenols, Fehling's solution, and analyses of modern " dry " champagne.

In 1901 Rosenheim was appointed Research Student of Pharmacological Chemistry in F. W. Tunnicliffe's Department of Materia Medica and Pharmacology at King's College in the Strand. In doing this he was welding an association which had already begun during his practice as consulting chemist. The products of their renewed association were publications on the volumetric estimation of uric acid, the use of piperidine as a uric acid solvent, the influence of borates and of formaldehyde on the metabolism of children, the influence of selenium on certain tests for arsenic, and selenium compounds as factors in a beer-poisoning epidemic. It was at this point that Rosenheim noticed the decomposition of selenium and tellurium compounds by moulds, observations which have been much amplified in recent times by the work of Challenger.

In the autumn of 1904 Rosenheim was appointed Lecturer in Chemical Physiology in Halliburton's Department of Physiology at King's College. Here he collaborated with F. S. Locke in a classical investigation on the effect of certain sugars and of calcium and potassium ions in the perfusion fluid on an isolated mammalian heart. Then followed an investigation into reliable tests for the detection of choline in body fluids. He used the polarising microscope for differentiating choline platinichloride from inorganic platinichlorides and this instrument was never out of reach throughout his career of research. There then followed a prolonged investigation into brain chemistry. In **1906** he evolved a practical method for preparing cholesterol from brain by mixing minced brain with plaster of paris and sand and extracting the cholesterol at room temperature with acetone; under these conditions the phosphatides were insoluble. In the following year in collaboration with Mary Christine Tebb who was gaining research experience in Halliburton's department, they showed that Liebreich's protagon, a crystalline material obtained by extracting brain with certain solvents, was a mixture, a view diametrically opposed to that of Liebreich, Gamgee, Blankenhorn, and Cramer, but in conformity with Thudichum's classical work on the chemistry of the brain. Rosenheim and Miss Tebb resolved protagon by gentle extraction with suitable solvents into two crystalline substances, phrenosin and kerasin, free from phosphorus, and a residue of sphingomyelin containing phosphorus. The separation of phrenosin and kerasin in a state of complete purity is difficult but Rosenheim's introduction of the selenite plate test in conjunction with the polarising microscope greatly facilitated it. A number of communications on the galactosides of the brain culminated in **1916** in a paper in which constitutions for these two substances were advanced very similar to those accepted today.

In the first decade of this century the subject of pressor bases was much to the fore. Dixon and Tayler believed that the placenta had a normal secretion of a pressor substance, but Rosenheim showed clearly in **1909** that if there was no putrefaction the pressor effect was absent. Incidentally, tyramine was identified by Rosenheim as a product of putrefaction.

In **1915** the title of Reader in Biochemistry was conferred on Rosenheim but in **1920** he resigned his Readership to devote all his time to research.

After a few years of quasi-retirement, Rosenheim and his wife came, in the autumn of **1923**, to the Medical Research Council's laboratories at Hampstead with the problem of spermine. They had already worked out a method for preparing the often elusive spermine phosphate crystals first observed in **1678** by Leeuwenhoek, and in collaboration with Dudley and later with Starling they isolated spermine in quantity from pancreas, discovered a new and related base spermidine, determined their constitutions, and finally synthesised both bases.

Rosenheim was deeply interested in vitamin chemistry. In **1908** and **1910** he had worked on rice and the beri-beri problem with Kajima, but in later years the fat-soluble vitamins riveted his attention. With Drummond in **1920** he discussed the possible relation between the lipochrome pigments and the fat-soluble accessory food factors and in **1925** they showed that the well-known colour reaction of cod liver oil with sulphuric acid could be replaced by one using arsenic trichloride. The blue colour produced had a greater permanence, and its intensity was approximately proportional to the content of vitamin A. This in itself was a notable advance; later, results of industrial importance arose from the observation by Rosenheim and Webster, based on colorimetric and feeding tests, that the amounts of vitamin A in liver fats of other species such as salmon and halibut were often **100** times as much as in cod-liver oil.

Rosenheim then turned his attention to vitamin D. With Drummond and Coward he had in **1925** found that the precursor of the antirachitic substance found in foodstuffs on irradiation with ultraviolet light was contained in the sterol fraction. Webster had worked on this problem and had devised a reliable technique for testing antirachitic substances on rats. Rosenheim and Webster joined forces and soon found that dihydrocholesterol could not be activated and that, if cholesterol was irradiated and unchanged cholesterol removed by digitonin, the residue yielded an intensely active antirachitic preparation. On testing many other sterols they found that ergosterol and its acetate were almost the sole substances which could be activated and that ergosterol was in fact provitamin A. The way was thus clear for work on a larger scale, undertaken by Bourdillon, Callow, Webster, and their collaborators, which resulted in **1931** in the isolation and characterisation of vitamin D for which Dale proposed the now accepted name of calciferol.

The constitution of the sterols and bile acids then assumed an added importance. Chemists were familiar with the structures adduced for cholesterol and the bile acids by Windaus and Wieland and were tacitly critical of the blocked quaternary carbon atom in these structures. When the paper by Diels and Gädke appeared in **1927** on the production of chrysene from cholesterol by hot palladised charcoal I drew Rosenheim's attention to it with the comment that I was sure it would revolutionize the structure of cholesterol. To our surprise months passed by without Diels's observation being taken up as of constitutional importance and Rosenheim attributed this to the reverence held for the German masters, Windaus and Wieland, in Germany. In the meantime ergosterol chemistry was being intensively studied by Windaus and Heilbron, and the numerous ready movements of double bonds in this substance convinced Rosenheim and myself that the blocked quaternary carbon atom formula would have to go. Our opinion received further support from the physical side, for early in 1932 Bernal found, on *X*-ray examination of various sterol crystals, that the dimensions of the cells required a long molecule and were difficult to reconcile with the accepted formulæ. Rosenheim pondered deeply over Bernal's findings and kept harping on them to me. When Bernal's second Note appeared, reaffirming his earlier view, Rosenheim anxiously sought my opinion and on the spur of the moment I said to him, " Why don't you take ring II in the Windaus–Wieland structures and put it on the other side of ring I so as to produce a potential chrysene structure ? " Rosenheim took the problem home and next morning came to me and said excitedly " It fits, it fits." My comment was that any new structure was bound to fit some of the facts of degradation; when however we found that Windaus's degradation of ring I led on our formulation to a five-membered ring ketone and not to the four-membered ring which puzzled Windaus we felt we were on the right lines. After further study we were fortunate in arriving at the complete structure of cholesterol and the ring system of the bile acids now accepted universally.

The new formulae effected a revolution in the chemistry of the sterols, the many bile acids, the heart poisons, the sex hormones, and the constituents of the adrenal glands, and for 23 years have formed the basis of countless original communications.

Rosenheim had for many years been puzzled by the nature of Lifschütz's resinous " oxycholesterol " obtained by debrominating cholesterol dibromide with sodium acetate. The blue colour it gave with sterol reagents fascinated Rosenheim. It so happened that from 1933 to 1943 Rosenheim and his collaborators Callow, Starling, Stiller, and Petrow studied the action of selenium dioxide on sterols and problems connected with the known diols and triols derived from cholesterol. Rosenheim and Starling by the action of selenium dioxide on cholesterol esters obtained a *trans*-cholest-5-ene-3 : 4-diol which gave a blue colour with sterol reagents and was very sparingly soluble in organic solvents. The latter property enabled the authors to show that this *trans*-diol was a constituent of Lifschütz's " oxycholesterol."

Rosenheim was equally at home in problems of nutrition and in metabolic studies. With Webster in 1943 and later he made many highly intriguing observations on the metabolism of cholesterol and allied substances in rats.

In the laboratory Rosenheim was an invaluable colleague. He had a phenomenal memory and was a mine of accurate information. He was philosophic, contemplative, and placid; he was sagacious, cautious, and wise, and his opinion was frequently sought by his many colleagues in the Institute and willingly given. He was a born investigator, he was meticulous and methodical, and he had an unusually pronounced flair for colour reactions. For many years he was a much valued member of the Accessory Food Factors Committee of the Medical Research Council. He was a Fellow of the Linnean Society, and his chief hobbies were rock-gardening and photography. He was elected into Fellowship of the Royal Society in 1927.

HAROLD KING.

GWYN WILLIAMS.

1904—1955.

GWYN WILLIAMS, born on 27th November, 1904, at Willesden Green, was the only child of the late W. J. Williams, J.P., of Cae Ffynnon, Llangystennin, Llandudno Junction, North Wales. His father, after a period of work with the Civil Service, was for many years Secretary of Kodak Ltd.

Williams was educated at University College School and at the University College of North Wales, Bangor, where he graduated with first-class honours in 1924. He remained as a post-graduate student with a maintenance grant from the Department of Scientific and Industrial Research, working under Professor K. J. P. Orton, F.R.S., and Dr. F. G. Soper. In 1927, after taking his Ph.D., he went to Cambridge where he held a Strathcona research studentship at St. John's College for two years and for a further two years a Fellowship of the University of Wales. He graduated Ph.D (Cantab.) in 1931, and for the next five years he worked in the Laboratory of Colloid Science in Cambridge. During 1936 and 1937, and again for a short period in 1939, he was a guest research worker in the research laboratory of the Eastman Kodak Co., Rochester, New York.

It was not until the Autumn of 1939, at the age of 34, that Williams took up academic teaching as assistant lecturer at King's College, London. The college was evacuated to Bristol from 1939 to 1943 during which time he succeeded Dr. E. W. McClelland as lecturer in organic chemistry and was awarded the D.Sc. degree of the University of Wales.

In 1946 he was appointed to the Chair of Chemistry at Royal Holloway College, University of London, which fell vacant on the retirement of Professor T. S. Moore. The department flourished under his guidance and became an active centre of research. At first Williams carried a heavy load of teaching in a department that was understaffed and inadequately housed; he resisted the temptation to have temporary additional buildings erected and by 1951 a substantial permanent new building was completed which sufficed for all the needs of the department, the staff of which had been increased.

From the time of his first research under Orton, Williams studied the kinetics and mechanism of chemical reactions. He began with Orton's chloramine problem. The *N*-chlorination and *C*-chlorination of acetanilides were shown to be simultaneous side-reactions, and the work was extended to other substituted acetanilides and benzylacetamides and also to anilides in which the acyl group was widely varied. The results were interpreted in terms of the opposed polar effects of substituents although in some instances they were complex. At Bangor he also determined the basic strength of some chloro- and nitro-anilines by the partition method.

At Cambridge he investigated the reactions of bromine with ethylene and vinyl bromide which were known to take place only on polar surfaces. Both reactions had negative temperature coefficients and complicated kinetics, pointing to a process involving a complex formed on the wall of the reaction vessel. At a Faraday Society discussion he contributed a valuable review of the mechanism and kinetics of additions to olefins.

During this period Williams also made, with Dr. A. S. C. Lawrence, a careful kinetic study of the thermal decomposition of benzylidene azine and ω-azotoluene, the reactions being homogeneous, with activation energies of 53 and 35 kcal. mole^{-1}, showing the difference in ease of rupture of the C=N and C–N bonds.

In the Kodak research laboratories, Williams began an investigation of the polymerisation of styrene in carbon tetrachloride under the catalytic influence of pure stannic chloride, which was continued in England and published in a series of papers the last of which appeared in 1952.

The polymer formed was shown to have a molecular weight of 1200—3000; increased initial concentration of styrene raised both the rate and the degree of polymerisation. The presence of hydrogen chloride (whether added or arising from the presence of water) caused a complete arrest of this process, and induced a new reaction yielding 1-phenylethyl chloride and short-chain polymers of multiplicity 2—5 the structures of which were demonstrated.

The activation energy of the main polymerisation was about 3 kcal. mole^{-1}, and the kinetic results indicated a chain reaction starting from an activated trimer. The short-chain reaction on the other hand had an activation energy of 9·4 kcal. mole^{-1}; its mechanism was also formulated by Williams.

.During the war years at Bristol several members of the King's College staff undertook for for the Ministry of Supply, at Professor W. E. Garner's suggestion, a laboratory study of the nitration of dinitrotoluene designed to throw light on the process of manufacture of trinitrotoluene. Rates of nitration were found to be proportional to the molar excess of sulphuric acid over water present. Williams took a prominent part in this work and it was largely owing to his enthusiasm that it was extended to a more academic study of the kinetics of nitration of dinitrotoluene in homogeneous sulphuric acid solution. Accurate measurements were made at temperatures from 60° to 120° and in acids from 87% sulphuric acid to strong oleum. The effects of addition of nitrous acid and of bisulphates were examined. With varying concentrations of sulphuric acid the rate of nitration had a maximum value at 92% sulphuric acid. This maximum has, however, no significance for the two-phase manufacturing process where the proportions of reagents are very different.

About 1941—1943 the nature of the nitrating agent was also considered. Solutions of nitric acid in oleum or sulphuric acid were electrolysed and movement of the nitric acid away from the anode was demonstrated. As a result of these and other experiments and of a critical re-examination of a wide range of chemical and physical data a convincing case was made out for the view that the active nitrating agent was the nitronium ion formed by the reaction :

$$HNO_3 + 2H_2SO_4 \longrightarrow NO_2^+ + H_3O^+ + 2HSO_4^-$$

Although an ion NO_2^+ had been suggested earlier by Euler and by Walden a detailed demonstration based on a large body of evidence had not hitherto been provided. The same view was arrived at independently by Westheimer and Kharasch in the U.S.A. who showed that the dependence of rate of nitration on sulphuric acid concentration was closely parallel to the behaviour of trinitrotriphenylmethanol as an indicator in the same acid, ionizing thus :

$$Ar_3C{\cdot}OH + 2H_2SO_4 \longrightarrow Ar_3C^+ + H_3O^+ + 2HSO_4^-.$$

During the same period Ingold and Hughes also came to a similar conclusion as a result of studies of nitration in such organic solvents as nitromethane and acetic acid.

The kinetic results on the nitration of dinitrotoluene had been interpreted on the hypothesis that proton uptake by the medium was a rate-determining factor in the process. This, however, was disproved in 1949 when Melander showed that such nitrations were insensitive to the substitution of tritium for protium at the seat of the attack on the nucleus. As these experiments were made under different conditions Williams himself tested the point by velocity measurements with pentadeuteronitrobenzene and verified that its rate of nitration was the same as that of nitrobenzene within the limits of experimental error.

In order to avoid the complication of the presence of nitrous acid formed by a simultaneous oxidation in the nitration of dinitrotoluene Williams extended the work, and measurements were made on the nitration of nitrobenzene, *p*-chloronitrobenzene, and of trimethyl-*p*-tolylammonium and trimethylphenylammonium salts in which oxidation did not interfere. Details of the first two of these have not yet been published.

The maximum rate of nitration of dinitrotoluene at about 90% sulphuric acid had received a plausible explanation by the hypothesis that proton uptake was rate determining, but this had been shown to be incorrect. An alternative possibility had been considered at an early stage namely that nitration in the most concentrated acids might be retarded as a result of increased salt formation between the substance nitrated and the sulphuric acid of the medium. But, as Williams showed, the rates of nitration of nitrobenzene, *p*-chloronitrobenzene, and the trimethylphenylammonium ion were all at a maximum in about 90% sulphuric acid and salt-formation could not be supposed to account for this in the latter instance since further proton uptake by the ammonium ion was not credible. No simple explanation of the maximum was therefore apparent.

Another question examined was the possibility that the nitric acidium ion $H_2NO_3^+$ or molecular nitric acid might be the main agents in nitrations in the less concentrated acids. In order to test this point Williams made use of Westheimer and Kharasch's method, extending its application to 65—85% sulphuric acid by the use of several other indicators which had measurable ionisation in this region. Ionisations of this type (and of nitric acid to the nitronium ion) were shown to be related to an acidity function J_0 :

$$J_0 = -\mathrm{p}K_{NO_2{\cdot}OH} - \log([NO_2^+]/[NO_2{\cdot}OH])$$

This function is connected with the Hammett acidity function, H_0, by the relation $J_0 = H_0 + \log a_{H_2O}$ (cf. Gold and Hawes, *J.*, 1951, 2102). The experiments showed that nitration of the trimethyl-*p*-tolylammonium ion is very rapid in acids containing less than 85% of H_2SO_4 and that neither molecular HNO_3 nor the nitric acidium ion could be principally involved, but that the NO_2^+ ion must be formed in this region (although not detected spectroscopically) and that this is in fact the effective nitrating agent.

Another subject investigated for the Ministry of Supply was the reversible *N*-nitration of guanidine in sulphuric acid. The reaction was observed and measured in both directions in acids of concentration 71·5—83% and correlation with the ionisation of indicators again showed that the nitronium ion was the nitrating agent and not molecular nitric acid or the nitric acidium ion.

More recently the esterification of alcohols by nitric acid in presence of sulphuric acid had been studied, a reaction which is in fact an *O*-nitration. The reaction is complicated because the alcohol reacts with both sulphuric and nitric acid, in each case reversibly. A primary alcohol which resisted any other action of the nitric acid was found in dinitrobenzyl alcohol and an examination of the kinetics was begun.

Gwyn Williams had many interests among which were music, literature, mountaineering, and chess. He was bilingual in Welsh and English. He also spoke and read French, German, and Russian, and made abstracts of Russian papers for the Bureau of Abstracts.

His work for European student relief and for refugee scholars in the years before the War revealed his deep human sympathy. He was keenly interested in the work of Coleg Harlech, a residential college for adult education of which he was treasurer. He had in 1935 conducted there an experimental course in the history and methods of science which subsequently became a regular course at Harlech.

Williams served his college as a member of its Council and its finance committee. He was for a number of years Secretary of the Board of Studies in Chemistry of the University and latterly a member of the Senate.

He was an unassuming man, an assiduous and scholarly worker, and a wise and staunch friend. It is a tragedy that his work should have been cut short so abruptly. Having had some symptoms of heart trouble he went into the hospital of St. John and St. Elizabeth, St. John's Wood, for examination, but died suddenly on the 6th April, 1955.

G. M. BENNETT.