OBITUARY NOTICES.

FRANCIS WILLIAM ASTON *

1877—1945.

Birmingham. 1877—1909.

HARD winds are blowing. The contemporaries of Lord Rutherford who helped him to lay the foundation of the nuclear sciences are following one by one their great leader. Francis William Aston, one of the most successful architects of that edifice, was born on September 1st, 1877, at Camomile Green, Harborne, Birmingham, as the third child and only surviving son of a family of seven. His father, William Aston, was a metal merchant of Harborne, second son and third child of George Aston, farmer and metal merchant of Birmingham. The family is presumably descended from a branch of the well-known family Aston of Tixall, Staffordshire. His mother was Fanny Charlotte Hollis, youngest daughter of Isaac Hollis, gunmaker of Birmingham, founder of Isaac Hollis & Sons, now the Birmingham Small Arms Company.

Francis William Aston was brought up on his father's small farm, now Tennal House, Harborne, then in the county of Staffordshire. He was fond of all animals, a passion which followed him all through his life. His earliest memories of a scientific nature were the formation and study of soap bubbles in the rickyard. His first research (carried out under conditions of extreme secrecy in a disused pigsty) was the action of sulphuric acid on pieces of zinc in an old blacking bottle.

His sister Helen relates that, as a child, he was passionately fond of mechanical toys and fireworks. Her first clear memories of him are those of his laboratory over the stable, where she and her sister Mary considered themselves much honoured to be admitted and allowed to watch his " experiments ", glass-blowing for the Sprengel pump, and the winding of miles of wire for his *X*-ray coil. He also made picric acid bombs cased in empty sparklet bulbs for the firework displays which were an annual domestic event and, for the same occasions, huge tissue-paper fire balloons of which the designing, making and sending off were very highly skilled affairs. The balloons had stamped addressed postcards affixed to them for the finder to send back, and these were sometimes returned from great distances.

Aston first attended at the establishment of the Misses Tonks at Harborne; in 1889 he went to Harborne Vicarage School, and in September 1891 to Malvern College, receiving first instruction in science from the Rev. Faber and Mr. Berridge. He was in the highest mathematical set and head of the school in science on leaving Malvern in 1893. He entered Mason College, which later became the University of Birmingham, in September 1893, where he studied chemistry under Tilden and Frankland, and physics under Poynting. Aston spent many hours in the Grand Library, reading accounts of the researches of Regnault and others in the copy of Deschanel's " Philosophy ". The carpenter's shop also saw a lot of him. He acquired skill in the use of tools which was of utmost value in his later life. During his stay at Mason College, a new chemical laboratory was erected in the construction of which he enthusiastically participated.

Aston passed London matriculation in February 1896 and, about the same time, fitted up a disused loft at his father's house as a laboratory and workshop where private research was pursued by him for many years. While working for higher examinations, he started to specialise in organic chemistry. It was in the chemical laboratories that, by spending most of his spare moments at the blowpipe, he acquired a skill in glass work which was later to become a decisive factor in turning his thoughts towards high-vacuum research.

In 1898 Aston was awarded the Forster Scholarship to work with Frankland on optical rotational powers of a complex tartaric derivative, the results of which were published in collaboration with Frankland in 1901. About that time, faced with the necessity of earning a living, he was persuaded to study fermentation chemistry in the new School of Brewing under Adrian Brown with a view to a brewery appointment which he obtained in 1900 at Messrs. W. Butler & Co., Wolverhampton, and which he terminated three years later. While in that appointment, he passed the examination for the Associateship of the Institute of Chemistry.

The discovery of *X*-rays profoundly excited Aston and became more responsible than any other event for the concentration of his interests more and more on the study of high-vacuum physics. During the time of his employment with W. Butler & Co., in his spare time he designed

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and made a new pattern of Sprengel pump and with it exhausted small focus tubes made from chemical test-tubes in his small workshop at home, where he also wound an induction coil capable of giving a 3-inch spark. Continuing his work at home with an automatic Toepler pump of his own design he discovered a form of irreversible discharge tube with which the discharge of an induction could be rectified. After having found a type of discharge in which the Crookes dark space could be accurately measured, he returned to Mason College which, in the meantime, had become Birmingham University, with a scholarship to take up research on this phenomenon. His first independent contribution to physics was a paper on " The length of the Crookes dark space " in the *Proceedings of the Royal Society* in 1905, which was followed by one on " The discovery of a new primary dark space " in 1907.

On the death of his father in 1908 he undertook his first round-the-world trip, visiting Ceylon, Burma, Java, Australia, the Pacific Islands, New Zealand, Hawaii, U.S.A., and Canada. Returning from his tour in 1909 he was accepted as lecturer in physics at Birmingham University but, after one term only, he accepted the invitation of Sir J. J. Thomson to work for him at the Cavendish Laboratory, and entered Trinity College in 1910.

Early Cambridge Years. 1910—1918.

It was a most fortunate decision which Sir J. J. Thomson took to invite the then thirty-three-years-old Aston to work under him at the Cavendish Laboratory. He did so at the suggestion of his great friend J. H. Poynting, Aston's teacher as Mason College, who early recognized Aston's great gifts. J. J. Thomson's decision proved to be not only most fortunate for Aston, but for the great physicist himself, for the Cavendish Laboratory, for Trinity College which Aston entered when coming to Cambridge, and for the speedy development of natural sciences which we have witnessed in the last thirty years.

After having elucidated the nature of cathode rays and discovered the electron, the unit of negative electricity, in 1906, J. J. Thomson turned his attention to the study of positive rays. In the intense field in front of the cathode of the discharge tube, the atoms are ionized. The negatively charged parts fly away from the cathode, forming cathode rays. There are also positive rays which travel towards the cathode. Owing to the very high field in front of the cathode, they pass right through it, if a hole is provided, and cause a glow in the gas.

Goldstein in 1886 observed that, if holes or channels were made in the cathode of a discharge tube, and the pressure was reduced to a fraction of a millimetre, then rays with luminous tracks could be seen, streaming through the holes into the space behind the cathode. These rays, called at first " canalstrahlen " or canal rays, though in some respects resembling the cathode rays in front of the cathode, were in other ways conspicuously different. They produced different colour in the gas along their course, a different kind of phosphorescence of the glass, and, what was more important, they were, by comparison, quite insensitive to magnetic deflexion. Wien in 1908 succeeded in deflecting them by means of very powerful magnetic forces and in showing that they carry a positive charge and have atomic dimensions.

J. J. Thomson was the first to succeed in obtaining a clear separation of the different kinds of atoms which might be present in these rays. The method was to use parallel fields, magnetic and electrostatic. These give crossed deflexions. The rays were received on a photographic plate, and co-ordinates measured on this gave the magnetic and electrostatic deflexions respectively. When carrying out these experiments at lowest possible gas pressures, so as to avoid secondary phenomena due to the particles acquiring or losing a charge while they were traversing the field, the picture on a fluorescent screen or photographic plate was found to be a series of parabolas with their common vertices at the point of zero deflexion and with their axes parallel to the direction of electrostatic deflexion. Each of these parabolas indicated one particular kind of atom or atomic group with a certain specific charge, and each point on the curve corresponded to a different velocity of the particle. In this way, a great variety of different atoms and atomic groupings were proved to be present in the discharge tube, and their nature could be identified by measurements of the co-ordinating picture, combined with the knowledge of the values of the electric fields. On this principle is based J. J. Thomson's parabola method in the development of which his assistant Aston had a great share.

The latter's great skill in mastering the high vacuum technique acquired during his Birmingham years proved to be of utmost importance in this work. It was Aston who introduced the use of large low-pressure discharge bulbs so necessary at that time for the production of clear parabolas. He designed the particular type of camera now standard in that method of analysis and by which the twin parabolas of neon was first photographed.

In 1905, J. J. Thomson was appointed Professor of Natural Philosophy at the Royal

Institution, on the resignation of Lord Rayleigh. The experiments for J. J. Thomson's Friday evening lectures in London were for the most part prepared by Everett and Kay and, later, by Everett and Aston. These lectures were for the most part on the subjects on which Thomson specialized.

In the time allowed for his work Aston proceeded with his research on the dark space and published in 1911 the first reliable determination of the distribution of potential in that region, inventing for use in the apparatus the now well-known stop-cock switch for moving objects in high vacua. He carried the subject into the field of rare gases in collaboration with H. E. Watson, a pupil of Ramsay, and, as he states, learned from him the technique of their manipulation. At this time, Thomson's attention was fully occupied with the investigation of a positive ray parabola of mass 3—now known to be triatomic hydrogen. Thus, it fell to Aston's lot to search for a proof that neon was not homogeneous. This he endeavoured to do by partial separation of its hypothetical constituents, using as a test their density measured by means of a quartz micro-balance specially designed by him for that purpose.

The principle upon which his micro-balance works is that, if a sealed vacuous quartz bulb is equipoised against a solid piece of quartz on a balance, the system can only be exactly balanced at any predetermined position if it is immersed in a fluid of an absolutely definite density. If the density is too high, the bulb will be buoyed up, if it is too low, it will sink. We can, therefore, compare the densities of a known and an unknown gas by introducing them successively into the balance case and determining the pressures at which the system is exactly balanced. The first method of separation, that of fractional distillation from charcoal cooled with liquid air, failed. The second method, diffusion through pipeclay, though extremely tedious, had more success, and in 1913 Aston announced that, starting from 100 c.c. of neon, after thousands of operations, two extreme fractions of 2 to 3 c.c. were obtained. The final densities which further purification failed to alter were 20·15 and 20·28 (oxygen = 32). This change in density is small, but it is much too marked to be ascribed to contamination or to experimental error. The results were announced at the meeting of the British Association at Birmingham in 1913 and, at the same time, the evidence afforded by positive ray photographs was discussed.

It was in 1913, at the close of Aston's period as assistant, that he was awarded the Clerk-Maxwell Studentship. He characterizes this period of his life in his appreciation of Sir J. J. Thomson in " The Times " (September 4th, 1940) as follows : " Working under him never lacked thrills. When results were coming out well, his boundless, indeed childlike, enthusiasm was contagious and occasionally embarrassing. Negatives just developed had actually to be hidden away for fear he would handle them while they were still wet. Yet, when hitches occurred, and the exasperating vagaries of an apparatus had reduced the man who had designed, built and worked with it to baffled despair, along would shuffle this remarkable being, who, after cogitating in a characteristic attitude over his funny old desk in the corner, and jotting down a few figures and formulæ in his tiny, tidy handwriting, on the back of somebody's fellowship thesis, or an old envelope, or even the laboratory cheque book, would produce a luminous suggestion, like a rabbit out of a hat, not only revealing the cause of the trouble, but also the means of a cure. This intuitive ability to comprehend the inner working of intricate apparatus without the trouble of handling it appeared to me then, and still appears to me now, as something verging on the miraculous, the hallmark of a great genius."

Discovery of the Complexity of Common Elements.

In 1906, Boltwood observed that his newly discovered element ionium was so similar to thorium that, if by chance their salts became mixed, it was impossible to separate them by any chemical process. Other chemical identities among the products of radioactivity were observed and the most painstaking and delicate method failed to affect or detect the slightest separation. The existence of " chemically practically unseparable " elements, later called isotopes, was made more and more understandable first by the discovery of the displacement laws and, then, by the gradually emerging knowledge of the decisive role of the charge of the nucleus in the determination of the chemical properties of the element and, finally, by the discovery of the neutron. Addition or removal of neutrons from the nucleus, if only the nuclear charge remains unchanged, does only slightly influence the chemical properties of the atom, hydrogen being an exception. This element has a unique position : it is the sole element met with, though only transitorily, as a naked nucleus in chemical reactions. The idea that isotopy is possibly a phenomenon not confined to elements produced by radioactive disintegration only was put forward at an early date. Soddy stated in 1910 that " these regularities may prove to be the beginning of some embracing generalization which will throw light not only on radioactive processes, but on elements in general and the periodic law. . . ." Chemical homogeneity is no longer a guarantee that any supposed element is not a mixture of several different atomic weights, or that any atomic weight is not merely a mean number. The generalization underlying his view was the law connecting radioactivity and chemical change. We find, furthermore, in the Presidential address of Crookes to the British Association in 1886 at Birmingham the following passage : " When we say the atomic weight of, for instance, calcium is 40, we really express the fact that, while the majority of calcium atoms have an actual atomic weight of 40, there are not a few which are represented by 39 or 41, a less number by 38 or 42, and so on."

However, it is a long way between suspecting the complexity of the common elements and proving it. To have brought this proof in a most striking way is one of Aston's great achievements.

Thomson's two separate neon parabola were an indication of a complexity of neon, though Thomson was reluctant to draw this conclusion. When a member of the audience of his Bakerian Lecture given on May 22nd, 1913, wrote to him, suggesting that the relationship between the two kinds of neons is analogous to that between radium D and lead, which are practically inseparable isotopes, the following answer was obtained :

" Holmleigh, West Road, Cambridge.—2 June 1913.—Many thanks for your letter. I do not think myself that it is impossible to separate the gas with atomic weight 22 from Neon (20) by fractionation : in fact F. W. Aston is at present engaged on this work. In W. Watson's experiments he was fractionating against a light gas helium and so threw away the lighter samples, the result was to increase the proportion of 22 to 20 beyond the normal. When we tested the gas Watson had used by the positive ray method we found a considerably larger percentage of 22 than in any other sample of neon we had."

While not adopting the view that the heavier constituent of neon was a compound NeH_2, which could have given the observed atomic weight within the limits of experimental error, Thomson was not convinced that this explanation was absolutely excluded. As Lord Rayleigh remarks in " The life of Sir J. J. Thomson ", the latter had always been haunted by this suspicion about hydrogen compounds and, for that reason, hesitated for a time to accept Aston's later results about isotopes of other elements.

What a serious menace the existence of hydrogen compounds could be for the mass-spectrographer is shown by the fact that, as late as in about 1930, when discussing during a stay in Switzerland the recent results obtained in the investigation of the composition of lead, Aston had misgivings that these results might partially be due to the presence of hydrogen compounds and intended to clear up this point after his return to Cambridge.

During the first world war Aston served as Technical Assistant at the Royal Aircraft Establishment at Farnborough and was crashed in an experimental aeroplane in 1914, but escaped unhurt. Here he made use of his chemical abilities in research on aeroplane dope and fabric. He also invented the special type of neon tube for short flashes.

Cambridge Years after the First World War.

When work was started again at the Cavendish, Aston continued for a time to experiment on separation of the neon isotopes by diffusion by means of an automatic apparatus. Although this apparatus performed the mechanical operations of diffusion many thousands of times in a satisfactory manner, the separation achieved was exceedingly poor—actually only half of that attained previously. He then realised that the most satisfactory proof of the existence of isotopes among the elements in general was only to be obtained by much more accurate analysis of positive rays. At the close of the war, while still at Farnborough, he started to examine theoretically the best methods of positive ray analysis, and by doing so hit upon the principle of the mass-spectrograph. This instrument by means of a sequence of electric and magnetic fields gives focused images of fine collimating slits, thus forming a spectrum dependent on mass alone.

When constructed at Cambridge in 1919 the mass-spectrograph was an immediate success; the isotopic nature of neon, chlorine, and other elements was demonstrated beyond doubt, and the whole-number rule was formulated. Chlorine was found to contain 35 and 37, and bromine of atomic weight almost exactly 80 and, hence, expected to be simple, gave two equally intense lines 79 and 81. Other elements were shown to be much more complex. Krypton, the first of these elements, had six isotopes, 78, 80, 82, 83, 84, 86; xenon and tin even more. Of the greatest theoretical importance was the fact that the weights of the atoms of all the elements measured, with the exception of hydrogen, were whole numbers to the accuracy of measurement. This " whole number rule " enabled the simple view then to be taken that atoms were built of two units, then thought to be protons and electrons, all the former and about half of the latter being bound together to form the nucleus. The difficulty in obtaining the necessary rays for analysis varies enormously from element to element. Two main devices were employed by Aston : the ordinary gas discharge which requires the element to be volatile or to form suitable volatile compounds, and the anode-ray discharge, in which the halide or other compound of the element is treated as an anode in a discharge at low pressure. The knowledge of the mechanism of the discharge in both methods is far from complete, so that working with them is still rather an art than a science. Aston often emphasised the important part the element of luck plays in this type of experiment. He thought he had been lucky, but this " luck " was not entirely fortuitous and certainly was well merited. He related also that the most beautiful mass-spectrum he ever obtained was to be seen on the first plate taken when analysing chlorine.

The results obtained by his first mass-spectrographs resulted in his election to a Fellowship at Trinity College; in the same year he was made Secretary to Cambridge Philosophical Society.

After more than fifty elements had been analysed by its aid, the first mass-spectrograph, now in the Science Museum at South Kensington, was replaced by a new one in 1925. The new instrument was designed primarily for measuring the minute variations of the masses of atoms from the whole-number rule, and had a resolving power ample for the heaviest elements. By its means many new isotopes were discovered. The technique of anode rays yields spectra almost free from the lines of compound molecules, and, for this reason, is particularly suitable for the identification of new isotopes. Aston applied this technique in his second mass-spectrograph in the analysis of the large group of the rare earth elements, which yielded some thirty new isotopes.

Using his first mass-spectrograph, Aston in 1920 found the mass of the hydrogen atom to be 1% greater than a whole number, possibly the greatest of all his important results. This measurement proved quite definitely the possibility of sub-atomic energy. It was already at that time reasonably certain that the particles forming four atoms of hydrogen are the same as those forming one atom of helium, so that, if we were able to transmute one into the other, nearly 1% of mass would be annihilated.

The importance of the accurate determinations of divergences from the whole-number rule induced Aston to design his second mass-spectrograph. This instrument was capable of an accuracy of 1 in 10,000. The atom of oxygen 16 was chosen as standard, and the percentage divergencies, expressed in parts per 10,000, called " packing fractions ", were determined for a large number of elements. These results were the topic of his Bakerian Lecture given in 1927.

In Aston's third instrument the accuracy of measurements approaches 1 in 100,000. This progress proved to be of greatest importance for the development of nuclear chemistry, as the equations of this science can only be founded upon accurate knowledge of the masses concerned. It is characteristic for Aston's far-reaching outlook that, as early as in his lectures at Cambridge arranged by the History of Science Committee in 1936, he emphasised his conviction that, armed with the knowledge of exact value of the masses concerned, the nuclear chemists will be able to synthesise elements just as ordinary chemists synthesise compounds, and it may be taken as certain that in some reactions sub-atomic energy will be liberated. He added that there are those among us who say that such research should be stopped by law, alleging that man's destructive powers are already large enough. So, no doubt, the more elderly and ape-like of our prehistoric ancestors objected to the innovation of cooked food and pointed out the grave dangers attending the use of the newly discovered agency, fire. Personally, I think there is no doubt that sub-atomic energy is available all around us and that one day man will release and control its almost infinite power. We cannot prevent him from doing so and can only hope that he will not use it exclusively in blowing up his next door neighbour.

Aston's general outlook in science was inclined to be non-mathematical, and he preferred models wherever possible. While he did not trouble to make detailed studies of the current theories, he grasped with a remarkable intuition the main points involved and especially how far he could avail himself of the result of these in his own work, and *vice versa*. His papers dealing with the theory of the mass-spectrograph were published together with R. H. Fowler.

Although since 1910 the scientific activities of Aston were concentrated in the Cavendish Laboratory, he took part and played a useful role in several eclipse expeditions organised by the Joint Permanent Eclipse Committee of the Royal Society and the Royal Astronomical Society. Being a keen observer, a brilliant photographer, and an enthusiastic traveller, he found much pleasure in participating in these expeditions.

In 1925 he was at Benkoeben in Sumatra. Here he took photographs of the corona through a 19-feet coronagraph and with a direct vision prism in front of the lens at second and third contact spectra of the flash. In 1932, at Memphri Magoy in Canada and, in 1936, at Kamishri in Hokkaido, Japan, where observations were prevented by clouds, he was to use a battery of Nicol prisms with Savart plates to study the direction of sky polarisation near the eclipsed sun. The object was to detect any changes in the direction from which scattered light entered from outside the shadow during the eclipse, and also to find evidence for polarised sky light scattered with the coronal beam by the earth's atmosphere, as found by Newall at Guelma in 1905. Aston did not go out with the main party and set up the eclipse camp. He used to arrive about a week before the eclipse and get familiar with the apparatus and attend the eclipse rehearsals. But his presence meant that one more instrument could be used for observation by a competent and expert physicist. He was always welcome on that account and for his personal charm. Aston intended to participate in the South Africa expedition planned for 1940 and in the Brazil expedition planned for 1947.

Travel, Sports, and Pastimes.

Being very fond of sea travel, in addition to the journeys undertaken to participate in eclipse expeditions and his first world tour on which he went in 1908, he undertook several large and numerous smaller trips, combining these mostly with scientific or sporting activities. Shortly after finishing writing the first edition of his " Isotopes " he revisited the United States in 1922, sailing in the Aquitania by invitation of the Franklin Institute, and lectured in many educational centres in the Eastern States. In 1929 he sailed in the Nestor with Rutherford and Fowler to South Africa to participate in the meeting of the British Association. He took part in the Indian Science Congress in 1937, and went a round trip to Australia and New Zealand on the Orient liner Otranto in 1938—1939; this was the last time he left England.

He undertook numerous trips to the continent, mostly to Switzerland, where he usually spent Christmas, and to Norway, where he spent Easter. His faithful companion on many of his journeys, his sister Helen, characterises him as a companion with the following words : " Not only was he a perfect travelling companion, with an amazing knowledge of every possible point of interest, geographical, astronomical, nautical, botanical, animal and personal to mention only a few, but he arranged the whole programme so perfectly, always with a view to what I should most enjoy, that every moment of the time was just a carefree delight, restful yet intensely exciting. His unselfish thoughtfulness for others (especially anyone who didn't seem to be having a very good time), and his unfailing interest in everyone and everything, from the Universe to the smallest trifles, and his witty and interesting conversation and reminiscences added to the enjoyment of every one, and tempted even the most retiring from their shells. He was particularly fond of deck games, which he played very well, especially quoits for which he won many prizes, and was as delighted with my successes as with his own. He was bound with the ties of strongest affection to all the members of the family, of which only I and my two sisters now survive.

" He was very fond of animals, especially cats and kittens, and would go to any amount of trouble to make their acquaintance, but he didn't like dogs of the barking kind, as it was a noise which he particularly detested, in common with various human noises, such as the sound of talking after he had retired for the night, so when travelling I always tried, if possible, to occupy the room between him and the conversationalists. Fortunately in his rooms at Trinity he was quite safe from such irritations, which, to any one of his very sensitive make-up, were a real trial."

Always a keen skater when ice was available in England, Aston started ski-ing at Engelberg in 1911 and had taken every available opportunity since of indulging in this, his favourite sport. He was a member of the S.C.G.B. and was most interested in cross-country touring. He gathered health and inspiration on his ski-ing trips which frequently both preceded and followed important discoveries. Deprived of winter sport during the first world war he started rock-climbing in 1915 and did many ascents in the Lake District, North Wales, and Skye in the company of well-known climbers.

Aston was a boy at school when the bicycle became fashionable, and between the ages of twenty and twenty-five devoted much time to cycling, touring England and Wales. He did many days' runs of over 150 miles in twenty-two hours. Having a thorough scientific knowledge of the petrol engine he was one of the earlier pioneers of motor-cycling, building his own machine in 1902 and attending on it the Gordon Bennett Race in Ireland in 1903. He was keen on lawn tennis, golf, swimming. Aston played lawn tennis regularly till middle age, being at his best between 1900 and 1910 during which period he won many prizes at open tournaments in England, Wales, and Ireland. Golf he started seriously in 1911, for many years playing round the Gogs at Cambridge with Rutherford, Fowler, and Taylor as a Sunday morning foursome. He learnt to swim in childhood, dived forty feet, and won prizes in swimming races. While in Honolulu in 1909 he learnt surf-riding, and considered it in many ways the finest sport in the world.

Aston was interested in most occupations calling for scientific skill, making harmonographs and other scientific toys. At an early age he built in his workshop a successful snapshot camera including a wooden shutter working to 1/50 sec. He was a brilliant photographer, and took thousands of ordinary and stereoscopic photos during travel, etc. He liked all games of cards requiring skill, and was a keen bridge-player. He was also an omnivorous reader with a leaning towards detective fiction of which Sherlock Holmes was always his first favourite. One of a musical family he had a very full musical life. He studied the piano at the age of seven, then the violin, finally taking to the cello. In Cambridge he played regularly in the concerts of the C.U.M.C. and C.U.M.S. : he was also musical critic of the " Cambridge Review ". When his health compelled him to give up all active exercises and active part in music he took up the collection of Chinese porcelain with tremendous enthusiasm, and acquired many very beautiful pieces from all over the world, which were an abiding joy to him up to his death.

Aston had a marked aptitude for finance, and in an unobtrusive way managed his investments so well that he increased very appreciably the estate inherited from his father. He regarded political activity as necessary, but unpleasant. By nature a strong individualist with conservative tendencies, he believed that all progress to be good and lasting must be made as gradually as possible.

Honours.

Aston was a recipient of numerous distinctions and awards. Beside those already mentioned, he was awarded the Hughes Medal in 1922 and the Nobel Prize in the same year. It was on an icy day, December 10th, 1922, that the Nobel Prize was handed over to Aston by King Gustav of Sweden, preceded by the playing of the Elégie of Jean Sibelius, so dear to him, by the orchestra of the Musical Academy. Professor Söderbaum, secretary of the Swedish Academy of Sciences, then addressed the laureate, stating among other things that Aston had succeeded in proving that a large number of fundamental elements which hitherto had been regarded as simple in reality are complexes of two or more isotopes. It was also Professor Söderbaum who proposed the toast to the prizemen at the Nobel banquet. Aston in his reply emphasised the importance of international goodwill and collaboration in modern science which the Nobel Foundation had done so much to promote. Svante Arrhenius, when proposing the toast of the laureates at the customary dinner given by him as the Director of the Nobel Institute, rightly emphasised that never before had the Nobel Prize been handed over to a group of such distinguished laureates which beside Aston included Bohr, Einstein, and Soddy. These Stockholm days were the greatest time in Aston's life, which is best shown by a remark made by his sister Helen, the faithful companion in many of his travels : " Among our many travels abroad the one which took pride of place for all time was, needless to say, when he went to Stockholm to receive the Nobel Prize. Even now it seems like a fairy story in which all good things came true, for him the pride of achievement, and for us reflected glory in a measure of which we had never dreamed. He was never tired of recalling all that we did in those wonderful days, and the kindness and hospitality showered on us all, and indeed Stockholm has been the city of our dreams ever since, set in a place apart from anywhere else in all our memories."

In 1923 Aston was elected to the Fellowship of the Institute of Chemistry and awarded the honorary degree of LL.D. by Birmingham University; the same year he was awarded the John Scott and the Paterno Medal. The year 1924 brought his presentation for the degree of Ph.D. at the Senate House and the election to the Council of the Royal Society. In 1925 he was made a member of the Athenæum Club and of the Russian Academy of Science; furthermore, he delivered that year the Presidential Address to Röntgen Society. The R. Accademia Nazionale dei Lincei made him a Foreign Member in 1926. In 1927 he was put on both the Council of the Royal Society and the Board of Governors of Birmingham University. In 1930 the degree of Hon.Ph.D. of the University of Freiburg was conferred upon him. He gave the Liversidge Lecture of the Chemical Society at Birmingham in 1932. Three years later he was made Chairman of the International Committee on Atoms of the Union Internationale de Chimie, drawing up the first " Table of Stable Isotopes " for 1936, and yearly " Reports " and " Tables " since. He also succeeded Barcroft as President of the Cambridge Philosophical Society in 1935. In the same year he was elected to the presidency of Section A at the Norwich Meeting of the British Association and to that of the Cambridge Philosophical Society. Aston's portrait was painted for the National Gallery in 1944, the last distinction received by him being the Duddell Medal, presented to him on May 23rd, 1945.

The Trinity Man.

Aston was born, educated, and trained in a very effective way for his future occupation at Birmingham, but it was in Cambridge that he carried out all his important work and supplied his marvellous contributions to the nuclear sciences. Trinity College was Aston's home for the last thirty-five years of his life. He considered himself fortunate and was proud and happy to be a Trinity man, attached to his college with the ties of admiration for its venerable past and splendid present. His attachment and gratitude were also shown when drawing up his will. After payment of death duties and legacies to members of his family and institutions, he directed the very appreciable capital to remain in the hands of Trinity College and ultimately to become theirs. The institutions which benefited under his will were the Cavendish Laboratory, the Royal Society, the Birmingham University, the British Association, the Cambridgeshire Preservation Society, the Cambridge Philosophical Society, the Fitzwilliam Museum, and Malvern College. All his scientific apparatus was left to the Cavendish Laboratory.

Aston's life was a chain of uninterrupted success; to him as to so few it was given to live in the way he most wanted. Whether designing or operating his mass-spectrograph, interpreting the results obtained, photographing the eclipse, gliding down the slopes of the Alps on his skis, trying to hit the bull's eye in deck games, or handling his money affairs, fate almost invariably favoured him. Was it luck? This question recalls a happening related by Professor Eve in his obituary notice on the late Lord Rutherford. On the occasion of one of his discoveries, Eve said to him : " You are a lucky man, Rutherford, always on the crest of the wave ". To which he laughingly replied : " Well, I made the wave, didn't I ? " and added soberly : " At least to some extent ".

Rutherford was the man Aston admired more than any one else. From 1919 he was a close friend of Rutherford and, as he often remarked, benefited to a degree not to be described from the continual help and encouragement of that great man till his death in 1937. Aston followed him eight years later. Death came to him with the merciful suddenness for which he always hoped.

G. HEVESY.

MARMADUKE BARROWCLIFF.

1883—1945.

MARMADUKE BARROWCLIFF was born on March 22nd, 1883. He was educated at Loughborough Grammar School and in 1899 entered the University College, Nottingham, and there studied chemistry under Professor F. S. Kipping and metallurgy and mathematics under W. H. Heaton. Three years later he joined the staff of the Indian Chemical Research Laboratories and carried out research work on the separation and constitution of natural products derived from plants. He then transferred to the research department of Burroughs Wellcome & Co., at Dartford, where he collaborated with the late F. L. Pyman on aromatic arsonic acids and other problems in organic chemical synthesis. In 1909 he left Burroughs Wellcome to take up an appointment as First Assistant in the Department of Agriculture in the Federated Malay States. Here he worked on soil surveys and also on the problem of coagulation of Hevea latex; his work led to a new method of coagulation for which patents were duly taken out. In 1915 he returned to England to work with F. H. Carr at Messrs. Boots Pure Drug Company, in Nottingham. Many problems arising out of the war engaged his attention, among which was a process for the manufacture of saccharin and a method for the preparation of granules for use in anti-gas masks : Barrowcliff was awarded the M.B.E. in 1920 for this work. He was with F. H. Carr the joint author of a book on synthetic drugs entitled " Organic Medicinal Chemicals ".

Barrowcliff joined the British Dyestuffs Corporation in 1919, and remained throughout the remainder of his working life with Imperial Chemical Industries Limited, Dyestuffs Division, in the formation of which the British Dyestuffs Corporation was one of the four merging companies. For some years he was head of the Operating Section, and in 1931 succeeded Mr. James Baddiley as Research Manager, a position which he occupied until his retirement in March, 1945. He became a Delegate Director of the Dyestuffs Division in January, 1939.

Barrowcliff himself set great store in chemistry and considered the title " chemist " as sufficiently meritorious and honourable, and himself sought no other description. Although he spent many arduous and contributory years in the dyestuffs industry he never became infected with its special appeal, since the service of chemistry in the science of medicine was to him the greatest appeal; and when in 1942 his company decided to broaden their activities into this field he threw himself, despite indifferent health, into his share of the burden with a zealous enthusiasm which may well have hastened his retirement on grounds of ill-health some years before it was due. Not even his retirement could evoke a degree of leisure for him; he took up voluntary and honorary work at Rothamsted on a programme of research which owed some sense of direction to his early days as a soil chemist in Malaya. He planned some work on penicillin, and took a course at the Tropical School of Medicine in London as a preliminary fitting-out expedition for the work he projected for himself.

He died suddenly at home in Harpenden on March 7th, 1945.

Barrowcliff was an intense person with a deep sense of loyalty, which could be directed towards institutions as well as persons. He was essentially shy, and was considered by some of his colleagues a little difficult and on occasion unyielding; but this was merely due to his shyness which made him reluctant to give his confidence, and was aided perhaps by a curious habit in writing of preferring an archaic word or phrase which conveyed to the recipient a very different meaning from the one he intended. An excellent judge of chemical skill and talent, he was especially successful in staffing his Department with able and skilful people. He had little use for the incompetent, and took no pains to disguise his views. At one time dancing and bridge claimed his leisure hours, but he was abidingly a skilful fisherman and loved equally fishing in the Trent or seeking trout in the higher reaches of the Ribble.

Barrowcliff, as would be expected of a pupil of Kipping, was an able and skilful organic chemist with a considerable experimental technique in his fingers, and had as much interest in the patient and detailed study of a well-known reaction as in a new synthesis. His early work on soil analysis was remembered, and he saw to it that his influence was used to ensure that analytical methods in organic chemistry were regarded as important developments.

C. J. T. CRONSHAW.

JOHN MASSON GULLAND.

1898—1947.

JOHN MASSON GULLAND was born at Edinburgh on October 14th, 1898, and fell a victim of the railway accident at Goswick, Northumberland, on October 26th, 1947. His father, G. Lovell Gulland, C.M.G., M.D., F.R.C.P., was Professor of Medicine at Edinburgh Univeristy from 1915 to 1928, and a brother of J. W. Gulland, M.P., who, for a considerable time, was prominent in Scottish politics and Chief Whip of the Liberal Party. His mother is the second daughter of David Masson, Professor of Rhetoric and English Literature at Edinburgh from 1865 to 1895, and Historiographer Royal for Scotland from 1893, and sister of the late Sir David Orme Masson, K.B.E., F.R.S., Professor of Chemistry in Melbourne, and of Rosaline Masson, the writer and novelist. John Masson Gulland was an only son, and his only sister married Dr. J. I. O. Masson, F.R.S., now Vice-Chancellor of the University of Sheffield.

Gulland carried off the school prizes for science at the Edinburgh Academy, and his science master remembers him as a boy of great charm, who won several trophies for shooting. In 1917, Gulland entered Edinburgh University, and, after the interruption of the first world war and service as 2nd/Lieut., Royal Engineers, with a Divisional Signals Company in France, he graduated in 1921, and was awarded a Carnegie Research Scholarship. Gulland's main interest was in organic chemistry, and he proceeded to study under Professor (later Sir) Robert Robinson at St. Andrews and at Manchester. Of the numerous problems under investigation in these laboratories, Gulland had some connection with the harmine work, but his main activities were devoted to the chemistry of the morphine bases. These experiences, very naturally, had a profound influence on Gulland's later work; he acquired a permanant enthusiasm for work on natural products, and frequently reverted to synthetical interests aroused during this early period. He took his Ph.D. at St. Andrews in 1925, and was awarded his D.Sc. at Edinburgh in 1929 for a thesis entitled "The Morphine and Aporphine Alkaloids". In 1924 Gulland was appointed University Demonstrator in Organic Chemistry under W. H. Perkin, Jun., at Oxford and married Miss R. M. I. Russell, M.A., LL.B. (Edinburgh), daughter of Sir James Russell, LL.D., of Edinburgh. Although Gulland collaborated with Perkin in the researches on strychnine and brucine, he did not find these subjects particularly attractive, and preferred problems connected with the chemistry of morphine. In 1926, Gulland became Lecturer in Chemistry, and was soon involved in the reorganisation of the courses and the teaching of organic chemistry to biological and medical students at Oxford. Gulland was most successful in this work; he took considerable trouble in the preparation of his lectures, which were delivered with much clarity and enthusiasm and were extremely easy for students to follow. These teaching duties, together with an association with Professor R. A. Peters, had an important influence on Gulland's career; it stimulated an interest in biochemical problems, and no doubt played an important part in his decision in 1931 to accept an appointment as Senior Biochemist at the Lister Institute of Preventive Medicine, and Reader in Biochemistry in the Univeristy of London. Here, the researches on the oxytocic hormone were pursued, and the important investigations on the nucleic acids were initiated.

Gulland was an excellent laboratory worker, neat and methodical, with a marked liking for colour reactions. His experiments were carefully considered beforehand, he kept detailed notes of his practical work, and it is to be regretted that the rapidly increasing non-laboratory interests occupied so much of his time from 1930 onwards. He was extremely critical of the work of others, frequently very fastidious in ensuring the accuracy of his own, and many of his scientific contributions are of special importance in repeatedly emphasising the necessity for revision and extension of earlier work. Dr. T. F. Macrae, who was Gulland's first assistant at the Lister Institute, described Gulland as an excellent supervisor of research, more than fair to his colleagues and encouraging them in their work, giving more credit to them than was often their due, and putting himself to some trouble to find suitable posts for them.

During his residence in London, Gulland found opportunities to display his interest and ability in organisation and administration; he served as one of the Honorary Secretaries of the Chemical Society (1933—1936), and as Secretary (1932—1934), and later (1935—1937) Recorder of Section B (Chemistry) of the British Association. In 1936, he was appointed to succeed F. S. Kipping, F.R.S., in the Sir Jesse Boot Chair of Chemistry at University College, Nottingham. He became Senior Gas Advisor to the North Midland Region of the Ministry of Home Security in 1939, and in 1943 he was seconded to the Ministry of Supply. The writer is indebted to Dr. F. Roffey for the following statement :

" In January, 1943, Gulland accepted an invitation to join the Ministry of Supply, and was appointed Assistant Director of Chemical Research and Development, a post in which he quickly made many new friends, and displayed a flair for dealing with a wide variety of problems, which were often complicated by the intricacies of the war time Government administrative machinery.

" This is not the place to disclose in detail the work he did, which ranged from the supervision of extra-mural researches on explosives intermediates to the retting of flax, but one major project came his way in connection with the production of weather-resisting fibre from alginic acid extracted from seaweed. This had been under investigation by various Government Departments and industrial firms since the earliest days of the war, and had ranged over a wide field with recurring promises of success that had never been realised. Gulland brought in a new outlook and immense enthusiasm, and in a remarkably short time had gathered together all the many loose ends, with the result that the main technical problems were solved by the early part of 1944.

" Unfortunately, by this stage of the war the Service requirement for which the work had originally been put in hand no longer held its earlier priority, and it was, therefore, not possible to apply the processes in large scale production for the war effort.

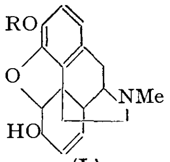
" Gulland's introduction to, and association with, the seaweed project had far reaching results, however, for it brought him into close contact with a number of people who had eyes on post-war developments in the Highlands of his beloved Scotland, and after he was released from the Ministry in August, 1944, at the urgent request of the University Authorities in Nottingham, he was able to follow up these new contacts, and played a prominent part in the founding of the Scottish Seaweed Association. Initially he acted as Ministry of Supply Assessor on the Advisory Committee of the Association, and subsequently was appointed a member of the Board of Management, Chairman of the General Purposes Committee, and Chairman of the Chemical Advisory Committee, all three of which appointments he held at the time of his death ".

In August, 1944, Gulland returned to his Chair at Nottingham and continued his nucleoprotein work, which lead to his election to the Fellowship of the Royal Society in 1945. He played an important part in the formation of the Lace Research Association in 1945, and in 1946 he became an independent member of the Board of Trade Working Party on the Lace Industry, and Chairman of the committee dealing with research, design, and welfare. In all these activities Gulland made numerous friends, who were impressed by the enormous enthusiasm with which he would attack a new and difficult problem, and by his desire to apply scientific knowledge and training in the widest and most practical way to improve the conditions for humanity.

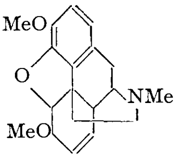
In October, 1947, Gulland resigned from his Chair at Nottingham to take up the Directorship of Research for the Institute of Brewing, and at the time of his death he was looking forward keenly to the wide opportunities of research which the new appointment offered.

Research Work.

Fortunate in arriving at St. Andrews in time to be the first collaborator with Robinson when views on the structure of the morphine bases were crystallising, Gulland was part-author in the publications which mark a new era in the chemistry of these alkaloids. This is not the occasion to discuss the details of this classical work, during which formulæ (I; R = H), (I; R = Me), (II), and (III) were derived for morphine, codeine, thebaine, and codeinone respectively. Although synthetical confirmation is still lacking, these formulæ have been widely accepted by chemists, and these publications constitute the basis of all subsequent work in this field. Much of Gulland's later work was influenced by these investigations, and several communications deal with developments. With Virden, an interesting attempt was made to utilise 2-nitro-3 : 4-dimethoxyphenylacetonitrile in the synthesis of aporphine bases of type (IV). For example, the base (V; R = OMe) was prepared from this nitrile and laudaline, but it could not be utilised, owing to its instability in the presence of acids. About the same time (1929) Avenarius and Pschorr announced a synthesis of apomorphine dimethyl ether (IV; R^1 = OMe; R^2 = H) from the base (V; R = H) by a series of reactions which involved acidic conditions sufficient to destroy the analogous base (V; R = OMe). Gulland and Virden were unable to repeat the work of Avenarius and Pschorr, and pointed out that, in view of the established instability of substances of type (V), the synthesis of apomorphine dimethyl ether requires confirmation.



(I.)



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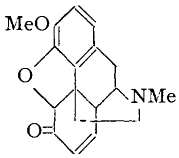
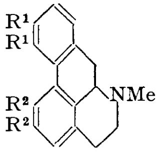
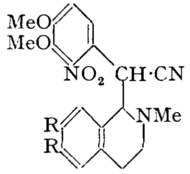


(III.)

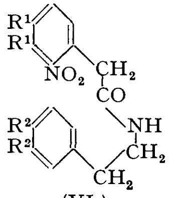
In 1925 the writer's researches were aiming at similar objectives, and in 1926 a collaboration with Gulland was arranged and a thorough examination of the methods of synthesis of aporphine bases was planned. Previous attempts to convert nitro-amides of type (VI) into *iso*quinoline bases had failed, but it was discovered that rapid cyclisation could be effected with phosphorus pentachloride in chloroform solution. Subsequent changes involving methiodide formation, reduction, diazotisation, and phenanthrene formation were effected by standard methods, and presented few difficulties. Numerous bases of the aporphine type (IV), including the methyl ethers of bulbocapnine (IV; R^1 = OMe; $R^2R^2 = CH_2O_2$), corytuberine (IV; $R^1 = R^2$ = OMe), and morphothebaine, were synthesised in the laboratories of Newcastle and Oxford. Resolution yielded optically active bases identical with those prepared from natural products. It was found, however, that ring-closure of amides of type (VI) required suitable activating groups and did not occur when R^2 = H, and attempts to synthesise *iso*thebaine and apomorphine dimethyl ethers were unsuccessful.



(IV.)

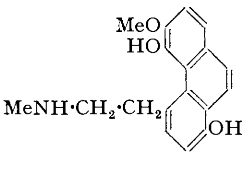


(V.)

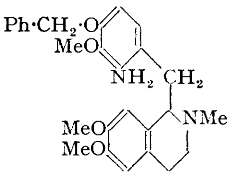


(VI.)

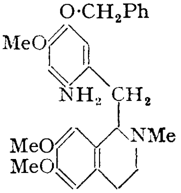
During these experiments, Gulland and Virden confirmed the structure (VII) for thebenine, a product of the action of dilute hydrochloric acid on thebaine or codeinone. Pschorr had previously converted thebenine by exhaustive methylation into 3 : 4 : 8-trimethoxy-5-vinylphenanthrene; Gulland and Virden reduced the vinyl group and showed that the dihydroderivative was identical with 3 : 4 : 8-trimethoxy-5-ethylphenanthrene, which was synthesised by an unambiguous method.



(VII.)



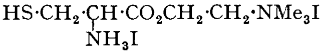
(VIII.)



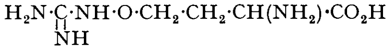
(IX.)

The synthesis of phenolic aporphines along the lines employed on the preparation of their ethers presents numerous difficulties, and four publications from the Oxford laboratories refer to this problem. Methods were developed for the preparation of 2-nitrohydroxymethoxyphenylacetic acids, and it was shown that protection of the phenolic groups as ethylcarbonato-derivatives was satisfactory at the cyclisation stage, but abnormal reduction of the methiodide was encountered. Protection as benzyloxy-groups was more promising, and the amines (VIII) and (IX) were prepared, but difficulties were experienced during the conversion into phenolic aporphines, and indefinite products were obtained. It is unfortunate that Gulland found it necessary to abandon the work at this stage.

About **1930**, Gulland was obviously tending in the pharmacological and biochemical directions. Investigations on the active constituents of pellitory root and of yew were followed by work with Professor R. A. Peters on the alleged antineuritic properties of certain quinolines and glyoxalines and on the reducing substances present in pigeons' blood. The biochemical interests developed with the appointment at the Lister Institute, where Gulland examined the spermicidal activity of quinones, quinols, and aromatic aldehydes, stressing the great activity of *p*-benzoquinones and ascribing the activity of aromatic aldehydes to their capacity of combining with amino-acids. An extensive investigation of the oxytocic principle of the posterior lobe of the pituitary gland was initiated. Preliminary adsorption experiments rapidly convinced Gulland that the isolation and purification of the oxytocic principle was unlikely, and he therefore concentrated on more indirect approaches. He studied the effect of nitrous acid, cyanide, reducing agents, sulphites, peroxides, chlorine, and hydroxylamine, and interpreted his results on the basis of the presence of a disulphide–sulphhydryl redox system. A study of the action of proteolytic enzymes supported the peptide nature of the factor, but dialysis experiments indicated a relatively simple molecule. These views were supported by the hydrolytic studies of contemporary workers, which led to the isolation of tyrosine, cystine, arginine, and choline, and Gulland attempted to prepare synthetic analogues of type (X); they were very hygroscopic, difficult to handle, and


$HS{\cdot}CH_2{\cdot}\underset{NH_3I}{CH}{\cdot}CO_2CH_2{\cdot}CH_2{\cdot}NMe_3I$

(X.)

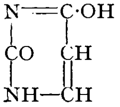
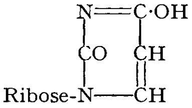
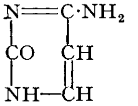
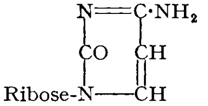
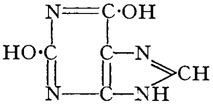
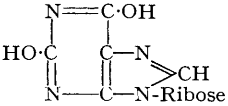
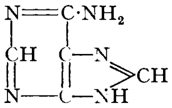
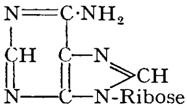
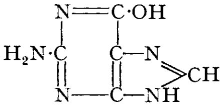
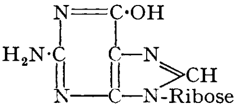

$H_2N{\cdot}\underset{\overset{\|}{NH}}{C}{\cdot}NH{\cdot}O{\cdot}CH_2{\cdot}CH_2{\cdot}CH(NH_2){\cdot}CO_2H$

(XI.)

had very slight oxytocic activity. Other investigations carried out about this period included an improved method for the isolation of β-hydroxyglutamic acid from protein hydrolysates, and an examination of canavanine, for which the unique hydroxylamine structure (XI) was suggested.

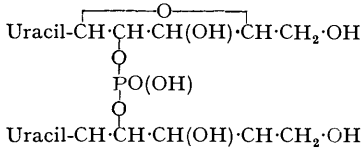
Shortly after arrival at the Lister Institute, the prolonged and important researches on the nucleic acids were initiated. Little previous work had been carried out in this country in this field, although Continental and American workers had been active. Gulland frequently drew attention to the lack of finality in earlier work, repeatedly stressed the desirability of revision or confirmation of existing views, and he was one of the first to appreciate the value of the application of physical methods, such as absorption spectra measurements and electrometric titrations, to the complex structural problems of this field. The experiments of Bredereck, Levene, and others had indicated that the pentose was attached to the nitrogen atom in position 3 in the pyrimidine nucleosides such as uridine and cytidine, and to the nitrogen atoms in either position 7 or 9 in the purine nucleosides such as xanthosine, adenosine, and guanosine.

These conclusions were confirmed and extended by a comparison of the ultra-violet absorption spectra of the nucleosides with the spectra of the *N*-methylated derivatives of the corresponding purines or pyrimidines. Strong evidence was thus obtained in favour of the 9-glycosidic structure for the naturally occurring ribosides and deoxyribosides of xanthine, adenine, guanine, and uric acid, and for the synthetic glucoside of adenine, and nucleotides such as muscle adenylic acid and adenylpyrophosphate.

Pyrimidine or Purine.	*Nucleoside.*	*Nucleotide.*
 Uracil.	 Uridine.	Uridine-3′ phosphate ≡ Uridylic acid.
 Cytosine.	 Cytidine.	Cytidine-3′ phosphate ≡ Cytidylic acid.
 Xanthine.	 Xanthosine.	
 Adenine.	 Adenosine.	Adenosine-3′ phosphate ≡ Yeast adenylic acid.
 Guanine.	 Guanosine.	Guanosine-3′ phosphate ≡ Guanylic acid.

A reinvestigation of the nature of the pentose sugar in yeast nucleic acid was undertaken, and the occurrence of D-ribose, suggested by earlier workers, was confirmed by novel methods. The nucleic acid was hydrolysed, and the pentose oxidised with bromine and barium hydroxide to the aldonic acid, which was identified by conversion into D-ribobenziminazole by reaction with *o*-phenylenediamine. When the oxidation was effected by alkaline hypoiodite, the D-ribonic acid was epimerised, and D-arabinobenziminazole was obtained.

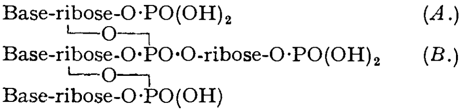
During experiments on the constitution of yeast nucleic acid, convenient methods for the partial synthesis of nucleotides were discovered. Jachimowicz (1934) showed that nucleosides were phosphorylated in position 5′ of the pentose by the action of phosphorus oxychloride in the presence of pyridine. These results were confirmed, but by employing phosphorus oxychloride in the presence of barium hydroxide, the nucleosides uridine, adenosine, and guanosine were converted into the corresponding 3′-phosphates, identical with uridylic, adenylic, and guanylic acid respectively. In later experiments, several 2′-phosphates were synthesised. Uridine-2′ phosphate was obtained by the action of diphenyl chlorophosphonate on 3 : 5-benzylideneuridine and removal of the phenyl and benzylidene groups by hydrolysis and reduction. 3 : 5-Benzylidenecytidine, phosphorylated with phosphorus oxychloride and hydrolysed with dilute acid, yielded cytidine-2′ phosphate, and diuridine-2′ : 2′ phosphate (XII) was prepared by the action of a limited quantity of diphenyl chlorophosphinate on 3 : 5-benzylidenecytidine, followed by successive hydrolyses with dilute alkali and acids. These synthetic 2′-, 3′-, and 5′-phosphates were quite stable under conditions which resulted in complete fission of yeast ribonucleic acid, and the results had important bearings on the mode of linkage of the nucleotides in the nucleic acid structure.



(XII.)

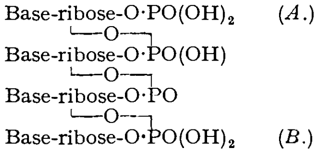
With the introduction of newer methods for determining molecular weights and improvements in titration technique, the tetranucleotide structures advanced by Feulgen (1918), Levene and Simms (1926), and Takahashi (1932) for yeast nucleic acid became untenable. Gulland carried out important work in this direction, and it is clear from various papers and reviews that he considered the evidence in favour of the existence of the tetranucleotide as a chemical entity was inadequate, although a polytetranucleotide structure was retained as a useful working hypothesis. Measurements made on the diffusion coefficients of yeast nucleic acid of different origins suggested that the molecular weights required from 8 to 18 tetranucleotide units, but, as deamination of the acid was effected without any marked reduction in molecular weight, it was concluded that the polynucleotides were not united by linkages between the phosphate and primary amino-groups.

Important results have been obtained from an examination of the titration curves of yeast nucleic acid, measured electrometrically using hydrogen or glass electrodes. The results indicated one secondary and three primary dissociations of phosphoric acid per tetranucleotide in the polynucleotide, and that the deaminated acid was similarly constituted. Mild hydrolysis of the polytetranucleotide resulted in a diminution in molecular weight by the rupture of linkages involving the liberation of additional secondary phosphoric acid groups which were recognised by the electrometric titration curves. These observations are inconsistent with all previously suggested formulæ for yeast ribonucleic acid in that they imply the presence of one completely esterified phosphate residue per tetranucleotide. Formulæ (XIII) and (XIV) are, however, in agreement with the data, provided that the groups (*A*) or (*B*) participate in the polytetranucleotide structure, by linkages which are of the same type as those between the component nucleotides.



$$\begin{array}{ll}\text{Base-ribose-O·PO(OH)}_2 & (A.)\\ \llcorner\text{—O—}\urcorner & \\ \text{Base-ribose-O·PO·O-ribose-O·PO(OH)}_2 & (B.)\\ \llcorner\text{—O—}\urcorner & \\ \text{Base-ribose-O·PO(OH)} & \end{array}$$

(XIII.)

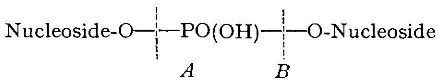


$$\begin{array}{ll}\text{Base-ribose-O·PO(OH)}_2 & (A.)\\ \llcorner\text{—O—}\urcorner & \\ \text{Base-ribose-O·PO(OH)} & \\ \llcorner\text{—O—}\urcorner & \\ \text{Base-ribose-O·PO} & \\ \llcorner\text{—O—}\urcorner & \\ \text{Base-ribose-O·PO(OH)}_2 & (B.)\end{array}$$

(XIV.)

In addition, evidence of the nature of the phosphate linkages in the nucleic acids has been obtained from a study of the behaviour of phosphoesterases. Takahashi (1932) had previously made experiments with phosphomonesterases, pyrophosphatases, and phosphodiesterases obtained from kidney and snake venom, but Klein and Rossi obtained conflicting results with phosphomono- and di-esterases from kidney, liver, intestine, and snake venom. Gulland and Miss Jackson showed that a non-specific monoesterase which readily attacked the nucleotides derived from yeast nucleic acid could be separated from the bones of sheep and guinea-pigs. Snake venoms varied in nature. Six out of twelve examined contained diesterases and non-specific monoesterases. Others, *e.g.*, Russell's viper venom, show a very slight non-specific monoesterase activity and marked diesterase and highly specific 5-nucleotidase activity; some 5-phosphates, including those of uridine and guanosine, were, however, unattacked by the venoms. In agreement with the observations of Takahashi, it was found that yeast nucleic acid was not dephosphorylated by the mono- or diesterases, and, whilst a mixture was effective, it was found, contrary to the previous work, that hydrolysis ceased when three out of every four phosphoric acid groups had been liberated.

Yeast nucleic acid was rapidly attacked by Russell's viper venom until 25% of the total phosphate was decomposed, and then a slow hydrolysis, corresponding to the monoesterase activity, was observed for several days, until about 50% hydrolysis was reached. At this stage, 1% sodium hydroxide (which does not hydrolyse the original nucleic acid into free phosphoric acid) effected further dephosphorylation, and it was suggested that the venom first attacked an alkali-stable phosphte group (*A*), liberating a nucleotide which was then attacked at the alkali-labile group (*B*) :



$$\text{Nucleoside-O—}\underset{A}{\vdots}\text{—PO(OH)—}\underset{B}{\vdots}\text{—O-Nucleoside}$$

Alkali-stable. Alkali-labile.

As the nucleotides obtained by the alkaline or the enzymatic hydrolysis of the nucleic acids are 3′-phosphates, these alkali-stable linkages are regarded as 3′-phosphate linkages, but the nature of the alkali-labile bonds represents an unsolved problem, as 2′-, 3′- and 5′-phosphates are all shown to be stable under conditions which lead to fission of the nucleic acids. This instability has been ascribed to a peculiar characteristic of the polynucleotide structure.

The extension of these methods to a preliminary investigation of deoxypentose nucleic acids is described in three papers. The deoxypentose nucleic acid was isolated as a tetrasodium salt from calf thymus, and analysis, combined with the results of electrometric titration, indicates that association of 4 atoms of phosphorus with 1 mol. of thymine, 1 mol. of guanine, 1·2 cytosine radicals, 0·8 adenine radicals, three amino- and two enolic hydroxyl groups, and four primary and not more, and probably less, than 0·25 secondary phosphoryl dissociations. A chain structure is advanced, and the hydrogen bonding, introduced to explain the macromolecular structure of the nucleic acid, is consistent with viscosity measurements and streaming birefringence observations. Unfortunately, the untimely death of Gulland has brought these important researches to an end.

Gulland's scientific interests concentrated upon problems of wide and general application to the welfare of mankind, and, provided this condition was fulfilled, he would apply his chemical knowledge and technique, undeterred by recorded failures in the literature; his ambition (no doubt he wished to live up to the great tradition he inherited) was to achieve something of outstanding importance. Consequently, Gulland's researches frequently involved problems of extreme difficulty, and it is not surprising that many remain uncompleted. In all these difficult fields, however, he was able to contribute something new, and in many his contribution was considerable.

Reference has been made to Gulland's work on behalf of the Chemical Society and the British Association, and the writer's thanks are due to Dr. H. J. T. Ellingham, Secretary of the Royal Institute of Chemistry, for the following account :

" First, as regards the Chemical Society. Sugden and Gulland were the two Honorary Secretaries at the time when I was brought in on the retirement of T. S. Moore, and it was my good fortune to serve with Gulland for about a year. One of his principal activities at this time was in connection with Abstracts. As a representative of the Chemical Society on what was then the Bureau of Chemical Abstracts, he was much concerned in negotiations with the Biochemical Society for extending the scope and usefulness of Abstracts A III. At the same time, an approach had been made to the Physiological Society, and Gulland paved the way to securing the co-operation of that Society in the work of the Bureau, which resulted subsequently in the Physiological Society agreeing to give up its own independent Abstracts and to join with the Chemical Society and the Society of Chemical Industry as a major partner in what became " British Chemical and Physiological Abstracts ". In these negotiations and in numerous general developments of Chemical Society affairs, Gulland's charm of manner and firm grasp of the essential requirements enabled him to secure willing co-operation from other Societies and individuals. It was difficult for anyone to resist an appeal by Gulland, for his honesty of purpose was so clear and his understanding of diverse points of view so complete that nobody could feel that they were being jockeyed into a false position. On the other hand, Gulland was not the man to be easily deflected from a worthy purpose, and he was firm with those who attempted to draw red herrings across the trail.

" As Honorary Secretary (1932—34) and subsequently Recorder (1935—37) of Section B (Chemistry) of the British Association for the Advancement of Science, Gulland found excellent scope for his organising capacity, wise judgment, tact, and social graces. A meeting of the British Association is in many respects a social event, and its success depends to a great extent on the personal qualities of the Section Officers. All who attended meetings at Blackpool and Nottingham during his Recordership have happy memories of the cheerful courtesy with which he shepherded members to their appropriate meetings, rescued them from difficulties, and saw to it that their legitimate needs were satisfied. At Nottingham, many enjoyed hospitality of Gulland and his very charming and capable wife, for it was their home town, and they laid themselves out to give a personal welcome to members of Section B."

Gulland was a great lover of Scotland and particularly of Edinburgh. He had a detailed knowledge of the Scottish Highlands and employed his skill in water-colours during holidays frequently spent there. His interest in the Scottish Seaweed Research Association was no doubt stimulated by a desire to assist his native country, and it was during the return from a meeting of this association that the disaster to the Scottish express led to the loss to science of a distinguished chemist, an outstanding personality and a man of very great charm.

The writer's thanks are due to Mrs. J. M. Gulland and to many colleagues for assistance in the compilation of this account. R. D. HAWORTH.