OBITUARY NOTICES.

EDWARD CHARLES CYRIL BALY.

1871—1948.

EDWARD CHARLES CYRIL BALY, who died on January 3rd, 1948, had very nearly attained the age of 77, having been born on February 9th, 1871. He was the son of Edward Ely Baly and Alice Mary, daughter of John Winterbottom. At the time of their marriage in 1870, Edward Ely Baly was a clerk in the Bank of England. He was appointed Deputy Chief Cashier in 1879, and the family then came to reside in the official residence at the Bank in Threadneedle Street, since the Chief Cashier refused to occupy it. Here the Baly family lived until the retirement of E. E. Baly in 1895.

In 1883, E. C. C. Baly went to Temple Grove School, East Sheen, and in 1885 obtained a junior Platt Scholarship at Aldenham School, where he won the senior Platt Scholarship in 1886. As he has himself related, he got an excellent training in Latin and Greek under the Head Master of Aldenham, John Kennedy, to whom he owed a deep debt of gratitude. Realising that his interests lay more in science than in classical scholarship, his father took him in 1888 to see Professor Ramsay at University College, London. The result was that he came to University College in 1889 and (at Ramsay's suggestion) studied for the Associateship of the Institute of Chemistry, which he obtained in 1892. Baly's father must have been a man of scientific tastes, for he possessed a 6-inch astronomical telescope with spectroscopic attachment, the result being that his son early gained some knowledge of astronomical spectroscopy. Becoming under Ramsay's influence a skilful glass-blower, he very soon acquired the technique of making and filling " vacuum " tubes with various gases and examining their emission spectra. Although the results were not published till 1893, it seems pretty certain that the young student for the A.I.C. was soon engaged in interesting research work ! Baly's ability as an investigator and teacher was recognised by Professor Ramsay. In 1895 he was asked by Ramsay to lecture on inorganic chemistry to junior students, and in 1898 he was appointed Lecturer in theoretical and practical chemistry to medical students. In 1899 he received the title of Assistant Professor, and in 1901 he was appointed a scientific assistant in connection with the practical examinations in chemistry and physics held by the University of London. For his work on the emission spectra of the rare gases, and especially his researches with numerous collaborators on the selective absorption spectra of organic compounds, Baly soon became recognised as an expert on spectroscopy, and established a spectroscopic laboratory at University College. He obtained the title of Lecturer in Spectroscopy in the University of London (1903–10), and gave evening lectures on that subject. His book on Spectroscopy first appeared in 1905 and was highly esteemed. A second edition was published in 1912, and a third edition (in 3 volumes) in 1924–27. His contributions to the advance of science were recognised (in 1909) by election to the Fellowship of the Royal Society. He was also elected a Fellow of University College.

The first time I saw Cyril Baly was in 1895 (or 1896) when I called in at University College on my way to Germany. What I saw was a young man with rather long hair, standing on the top of a step-ladder and pouring sodium (or potassium) amalgam into a glass funnel from which the amalgam dropped down a long glass tube up which streamed a current of ozonised oxygen. I was led to understand that he was trying to prepare metal " ozonides ". So far as I know he never published anything on this subject. It was not till 1899–1900, when I worked in Ramsay's laboratory as a research student, that I first got to know him and appreciate his kindness and his great skill as a glass-blower and constructor of apparatus. My efforts to measure the rates of effusion of argon, helium, and other gases through a minute hole in a platinum disc would have been doomed to failure had it not been for Baly's invaluable help.

In those days (and for many years afterwards) he was a skilled pianist and deeply interested in music, especially the works of Brahms. He owed much to his father, who was a brilliant pianist, and to his mother, who had a beautiful soprano voice. They taught him to play the piano and encouraged him to become proficient. At University College, Professor Arthur Platt and he founded in 1902 the Musical Society, which was a great addition to the amenities of the College, and had a most successful career under their joint care and supervision. There can be no doubt, I think, that an important part of Cyril Baly's temperament was essentially artistic and intuitive.

In 1902 he married Ellen Agnes, only daughter of Thomas Jago, physician and surgeon of

Barnsbury, London. Both father and daughter were musical people. This marriage has been a very happy one. They had four sons, all of whom, and their mother, are living.

In 1910 Cyril Baly was called to Liverpool University, succeeding Campbell Brown as Grant Professor of Inorganic Chemistry. At Liverpool he continued and developed the work on absorption spectra, chemical constitution, and reactivity, including also phosphorescence and fluorescence. Soon after 1920 he took up with great enthusiasm a study of photochemistry (" photochemical catalysis ") and photosynthesis. His last paper on this subject was published in 1939, whilst his whole work in this field was reviewed in his book on " Photosynthesis," published in 1940.

During the first world war, Baly was appointed one of the Deputy Inspectors of High Explosives working under the Chief Inspector, Lt.-Col. C. D. Crozier. His area included Fleetwood, Lytham, Liverpool, N. Wales, and Bristol. For his excellent work in this important department of the production of war munitions he was awarded the C.B.E. in 1918.

At Liverpool University, Baly proved to be a very inspiring and enthusiastic leader in both teaching and research. His lectures were highly appreciated by many generations of students, who were not only kept in touch with the modern advances of chemical science but also strongly attracted by the novelty of many of the lecturer's ideas, the good form and lucidity of his exposition, and the friendly and informal manner of its delivery. Baly knew how to make chemistry a live and exciting subject, one in which new and important discoveries and developments were constantly taking place. He had personally witnessed Ramsay's famous discoveries in the period 1894–1910, and could hold his Liverpool students spell-bound by the story of those great days. Moreover, his students knew that their professor was actively engaged in research, with the result that many of them became his enthusiastic collaborators.

During the twenty-seven years that Baly was Professor in Liverpool many important developments of the Chemistry Department took place. A Professorship in Organic Chemistry was instituted, and in order to accommodate this Department a new and important addition to the old chemistry building was made. The Chair of Organic Chemistry was held successively by R. Robinson, I. M. Heilbron, and A. Robertson (the present holder). There seems little doubt that Baly must have played an important part in these developments.

In 1937 he retired owing to the age limit, and came back to the London area, where he took a house in Wendover, Buckinghamshire. He soon took an active part in the work of the British Association of Chemists, acting as Honorary Registrar for a number of years. During these latter years he also became an examiner in chemistry to the Pharmaceutical Society. Towards the end of the war it became evident to his friends that his health was failing, for he seemed to have considerable difficulty in walking, and the once genial and talkative Cyril Baly became strangely silent.

As mentioned previously, Baly became early interested in the emission spectra of rarefied gases (under electric discharge), and was much intrigued by the fact that oxygen and nitrogen exhibited two spectra, one in the positive column and the other in the negative glow. His first published papers dealt with the separation and striation of rarefied gases under the influence of the electric discharge, and a possible explanation of the twofold spectra of oxygen and nitrogen. He became convinced that these different spectra were caused by " different entities in the chemical sense ". In fact, by using an induction coil giving a 6-inch spark in air, with platinum tubes as electrodes, and passing a slow stream of oxygen at atmospheric pressure into the glass tube and out through the hollow electrodes, he obtained (under certain specified conditions) samples of oxygen of markedly different densities—in one experiment 15·84 from the cathode and 16·12 from the anode (referred to $O = 16$). This result and others of a similar nature were not published, but were submitted to the Smithsonian Institution in a competition for the Hodgkin Prize. It is curious that such astonishing results were not published by Baly. Very many years afterwards he mentioned them in a public lecture, on which occasion Whytlaw-Gray suggested that they might have been due to a partial separation of isotopes.

During this earlier period at University College Baly published work on the emission spectrum of cyanogen, and on the emission spectra of neon, krypton, and xenon. These spectra of the then newly discovered " rare gases " were measured by means of a ten-foot concave grating, but the results were published before Kayser's criticism of the accuracy of grating measurements was known. Nevertheless, the mounting of this grating and its calibration represented a fine and difficult piece of work. Two other researches of this period may be mentioned : one (with Ramsay) dealing with the composition of the gaseous and liquid phases during the distillation of liquid air, and one (with F. G. Donnan) on the variation with temperature of the surface energies and densities of liquid oxygen, nitrogen, argon, and carbon monoxide.

In 1903 Baly turned his attention to a field of investigation in which the most important scientific work of his life was to be carried out, namely the selective (ultra-violet) absorption spectra of carbon (organic) compounds in the liquid state or in solution, and the relation of these spectra to their chemical constitution and chemical activity. As is well known, the first work in this field was due to Huntington and Hartley (W. N.) in 1879. Hartley devoted his life to it, and his work (and also that of J. J. Dobbie) had amply demonstrated the interest and importance of such measurements. In the session 1901–1902 I had given a few lectures on this subject to advanced students at University College, and suggested to Baly in 1903 that the investigation of such absorption spectra might be more fruitful than that of the emission spectra of gases. This suggestion was eagerly adopted, his first investigation (1904–05), in collaboration with C. H. Desch, dealing with acetylacetone and ethyl acetoacetate, and their metallic derivatives. It was found that the pure " ketonic " form was almost completely diactinic, whilst the pure " enolic " form showed only a small general absorption. When an absorption band was obtained, it was concluded that both forms were present in a state of " dynamical equilibrium ", the band being due to the " change of linking " accompanying the reversible change from one tautomeric modification to the other, and the production of the band being explainable by the conception of the atom as " a system of electrons ". It must be confessed that such a theory seems rather puzzling at first sight, because, if there really existed a rapid *kinetic* interchange between the enol and the keto-forms, as seems to be assumed, one might expect the electronic vibrations to make the solution self-radiating. What Baly and Desch meant, however, was, I think, that the presence of an absorption band was due to the existence of a corresponding *potential* period of (damped) vibration, in the sense of classical resonance. In other words, when an absorption band is formed, the state of affairs in the solution must be such that there exists a possibility of the enol-keto-change of linking, *i.e.*, a potential activity of this type. As a matter of fact, in later years Baly (in collaboration with F. O. Rice) somewhat changed his interpretation of these phenomena in view of the criticisms of Hantzsch and others, and adopted an explanation based on his " force-field " theory (see later).

In a paper published in collaboration with Professor J. Norman Collie (1905), the seven absorption bands of benzene were explained by the assumption that each band is due to a " separate make-and-break of valency links " between the carbon atoms of the ring, caused by " pulsations " of the whole ring. Here again we must assume that the " electronic vibrations " due to these various changes of valency-linking represent potentially existing periods of vibration of the system.

It may be said, I think, that Baly's view as regards the formation of absorption bands (in the ultra-violet or the visible parts of the spectrum) came to a fairly definite form in the papers published (1906) in collaboration with A. W. Stewart. These investigations concerned mainly the absorption bands exhibited in ethyl pyruvate and various organic compounds containing two ketonic groups, *e.g.*, α-diketones and *p*-benzoquinones. The view is expressed that the observed absorption bands originate in " some form of oscillation between the residual affinities of the oxygen atoms of the carbonyl groups ", and for this " oscillation " the name *isorropesis* (meaning " equipoise ") is proposed. This oscillation " results in the establishment of a *free period of vibration* (in the visible region of the spectrum) ". Hence the compounds are coloured. It is to be observed that this more general concept of isorropesis does not involve any actual wandering of an atom, but only a change from one valency distribution to another. In the case of ethyl pyruvate this was expressed by the formulation

In this connection, the authors (Baly and Stewart) say : " It is very hard to indicate what is meant by the aid of the usual structural formulæ, as they only indicate a static condition of the molecule, while what we wish to suggest is essentially a dynamic state. We wish to make it perfectly clear that we do not suppose these two forms necessarily to exist statically; but, owing to the defect of ordinary structural formulæ, it is impossible to write them otherwise if the usual symbols be employed. Our conception can best be comprehended if it be clearly borne in mind that the two formulæ are not intended to represent actual compounds, but merely two phases of the same compound. If this conception of phases be understood, it will be apparent that the change of linkage is continually going on, and that this change will affect the intra-atomic relations of the molecule ".

From these statements we may presume that the continual change of linkage occurs when the molecule is (classically) resonating owing to the absorption of light by the existing " free period ". I think that is what the authors meant. Referring to this concept of isorropesis, in his Obituary Notice of A. W. Stewart (this vol., p. 396), Professor S. Smiles regards it as " the concept of a state of balance existing in the valency distributions ", and expresses the opinion that it evidently contains a tentative step towards a theory of mesomerism. It is certainly true that Baly was attempting to explain a definite property, *i.e.*, selective absorption of light, by a concept denoting the " equipoise " between different valency distributions, but it must be confessed that his concept of " classical " resonance (very natural in 1906) has little relation to the modern concept of quantum-mechanical " resonance " associated with the name of L. Pauling.

During this period of collaboration with A. W. Stewart, Baly arrived at conclusions having an important bearing on his theory of colour in organic compounds. In order to start the isorropesis, he held that some influence must be present to disturb the residual affinities of the atoms concerned. Given the necessary influence, it followed that all the phenomena of visible colour are due to the oscillation between residual affinities on atoms or groups of atoms in juxtaposition. Moreover, any assumption that two compounds must be fundamentally different in constitution if one is coloured and the other not is quite untrustworthy. As a result, Baly attacked the " quinonoid " theory of colour associated with the name of H. E. Armstrong. Those who were present at the meetings of the Chemical Society in the years 1904–10, when Baly was expounding his theory of isorropesis and extending it to a great variety of organic compounds, will remember the scorn with which Professor Armstrong received the appearance at Burlington House of this strange Greek child from Gower Street.

Although the concept of isorropesis and " isorropic " absorption bands played a notable part in many of these investigations, Baly gradually developed another line of thought which led him to connect absorption spectra with chemical reactivity. This new theory originated in an investigation, in collaboration with Miss E. G. Marsden, on certain amino-aldehydes, and in work on the sulphonation of phenols in collaboration with F. O. Rice. In the former case it was found that if a small quantity of an alcoholic solution of hydrogen chloride be added to the alcoholic solution of any one of the amino-bases investigated, a new absorption band appears, but on addition of excess of acid the spectrum changes to that of the hydrochloride of the amino-base. According to Baly, the first small portion of acid produces a definite intermediate phase in the " opening-up " of the (" electromagnetic ") field of force of the amino-aldehyde molecule, the alcohol of the solution having already opened it up to a first degree or phase. This higher phase produced by the small quantity of acid then reacts with the excess of acid to form the definite salt (hydrochloride). The analogous phenomena observed in the sulphonation experiments were explained in a similar manner. From these results Baly drew the general conclusion that " in all chemical reactions the force-fields of the reactant molecules have to be opened up to definite stages before any reaction can take place, and that the changes in the absorption of light observed are due to the existence of these stages and not to any change in the primary structure of the molecule ".

This " force-field theory " led Baly to an interesting general theory of chemical reactivity. A molecule is unreactive if its field of force is " closed ". The opening-up of the field, and hence the reactivity of the molecule, can be effected (in definite stages) by the action of a solvent, by various chemical reagents, and by absorption of light. This activation of molecules by the opening-up of their force-fields might evidently afford an explanation of catalytic actions. Moreover, a " de-activation " of molecules, by the closing-up of their force-fields owing to the interlocking of the latter with the force-fields of new substitutent groups, might also occur. It was obvious to Baly that all such actions could be closely related to the presence or absence of absorption bands, and this led to a further vigorous development of his work in this field.

An interesting example of the application by Baly of the force-field theory is afforded by his criticism of some of Hantzsch's results. Hantzsch had obtained a number of differently coloured salts of diphenylvioluric acid, and had explained these differences by *different* structural formulæ. The following quotation will explain Baly's view. " The stages in the opening-up of the closed force-field of a complex acidic substance will depend on the strength or affinity of the base employed. Thus, the hydroxides of lithium, sodium, potassium, rubidium, and cæsium are in ascending order of magnitude as far as their affinity as bases is concerned. It follows that the effect of these may be different as regards the opening-up of a complex acidic force-field. This at once affords an explanation of Hantzsch's differently coloured diphenylviolurates, for the more electropositive the metal the higher the stage that will be called in to play, and hence the longer the wave-length of the light that will be selectively absorbed. There is no need for any difference in the structural formulæ of the salts, for they only differ in the condition of the force-field of the complex acid."

Another example of Baly's conception of the various phases of the opening-up of a molecular field of force, both by the action of solvent molecules and by light, is afforded by his paper, published in collaboration with R. Krulla (1912), and entitled " A theory of fluorescence ". The reasoning here is very tentative, but the views expressed are interesting since they emphasise the concept that when light opens up the field from one phase to another it expends free energy, *i.e.*, does work on the system, so that the higher phase of the field is richer in energy. This paper was followed by a long series of papers dealing with light-absorption and fluorescence, culminating in an elaborate paper entitled " A theory of absorption, fluorescence, and phosphorescence " (1915). In these papers (and particularly in the last one) Baly made use of the force-field theory and also of ideas derived from the theory of energy quanta. I think it is correct to say that he was led into quantum considerations by his supposition, based on quite inadequate evidence, that the " central " frequencies of his observed absorption bands in the ultra-violet are integral multiples of a " basic " frequency in the infra-red. In his important work of that period, Bjerrum had put forward the theory that the *rotational* frequencies of the long infra-red bands are integral multiples of a basic value, but Baly erred in assuming that the frequencies of the *atomic and electronic oscillations* (bands) are integral multiples of the same basic value. This extension of Bjerrum's theory was quite unjustified. It is probably correct to say that Baly's work of 1912–15 was an interesting, and, for its time, valuable contribution, but that it contained good and bad ideas mixed together in a confusing way, and an erroneous extension of Bjerrum's theory of 1912. One must remember, however, that these were very early days in the development of quantum theory, and that Baly was not a theoretical physicist. He made a gallant and in many ways pioneer attempt to solve a difficult and complicated problem.

In this brief sketch of the very great amount of original work carried out on absorption spectra in liquid systems by Baly and his numerous students and collaborators, I have not attempted to go into details of experimental methods or results. He made valuable improvements in the methods used by his predecessors, and his results are in general trustworthy so far as they go. Owing to the nature of the systems investigated and the technique mainly employed, the " absorption bands " as he observed them could only indicate rather general regions of selective absorption. It would not have been possible therefore to apply any exact *quantitative* theory to results of this type, even if such a theory had existed then. But Baly's theories of isorropesis and molecular fields of force, although they were necessarily of a rather qualitative and " elastic " character, enabled him to make many interesting deductions relating to chemical constitution, processes, and reactivity. Although he sometimes made mistakes of interpretation (and some of actual fact), he was an energetic and imaginative pioneer in the search for the hidden " dynamical " causes underlying the observed absorptions, rather than a seeker for mere analogies of a purely " statical " character between the observed spectra and " statical " structural formulæ. I think the future historians of chemistry will assign a very definite value to his prolonged and strenuous effort, even though the modern electronic and quantum-mechanical theories of chemical science have solved the type of problems he investigated in a different manner.

In 1920 Baly became deeply interested in the problem of the photosynthetic production of carbohydrates and other organic compounds (proteins and alkaloids) in the green leaves of living plants exposed to light. Many years previously, von Baeyer had suggested that the first stage is the formation of formaldehyde, which is then polymerised to hexoses, and this theory had been partly confirmed by the work of Willstätter and Stoll. In 1913 Benjamin Moore (in collaboration with Webster) had found that if an aqueous solution of carbon dioxide be exposed to direct sunlight for some days no formation of formaldehyde can be detected, but if colloidal ferric hydroxide or uranium hydroxide be present small quantities of formaldehyde are produced. It was, I think, this work of Moore and Webster which chiefly aroused Baly's interest, since such a photochemical reaction seemed difficult to explain. Hence, in collaboration with I. M. Heilbron and W. F. Barker, he tried the effect of short-wave ultra-violet light, because it was known that carbon dioxide absorbs light in the region of 2100 A. It was found that when a stream of carbon dioxide was passed through water contained in quartz tubes and irradiated by the light from a quartz mercury lamp, small quantities of formaldehyde could be detected in the solution. New results were obtained when certain " white " powders, such as aluminium hydroxide and zinc and magnesium carbonates, were kept suspended in the solution during the irradiation. When the resulting solutions were filtered and evaporated, 0·02 g. of a " gummy residue " was obtained, which on being heated with dilute hydrochloric acid gave " reactions for carbohydrates ". Baly attributed the success of these experiments to the adsorption of the carbonic acid on the surface of the powders, and concluded that this adsorption was necessary for the formation of carbohydrates by the action of light.

Meanwhile C. W. Porter and H. C. Ramsperger, in the University of California, had repeated the earlier experiments on the action of short ultra-violet light on an aqueous solution of carbon dioxide, and had found that, when stringent precautions are taken to avoid the entrance of any trace of impurity, no formaldehyde is produced. Hence they concluded that in the Liverpool experiments the formation of formaldehyde must have been due to the presence of some unknown impurity. To this criticism Baly had several answers. One was as follows : " There is no doubt that in this reaction the carbonic acid is converted directly into carbohydrates and that formaldehyde as such is not produced, and it seemed that the most probable explanation of the discrepancy between our results and those of Porter and Ramsperger was that the action of the ultra-violet light is to establish a photochemical equilibrium, $6H_2CO_3 \rightleftharpoons C_6H_{12}O_6 + 6O_2$, which reverts to carbonic acid again in the dark. In the presence of oxidisable impurities a small amount of carbohydrates will be formed, which will be photochemically decomposed to formaldehyde. This decomposition of all the carbohydrates by means of ultra-violet light is well known ". Another explanation of the discrepancy was : " The ' impurities ' mentioned by Porter and Ramsperger were not impurities in the ordinary sense, but all the suspended dust particles had been removed from the solution. This was not done in the experiments carried out by Heilbron, Barker, and myself, with the result that sufficient surface was present to give a minute amount of photosynthesized products ". It must be confessed that such explanations do not appear very satisfactory.

The research took a new turn when, as a result of a suggestion made by Dr. W. E. Stephen (one of Baly's collaborators), a *green* powder (nickel carbonate) was used instead of the previously mentioned white powders, and the light from an ordinary tungsten-filament lamp replaced that from the quartz mercury lamp. Glass vessels could now be used. It was the colour of chlorophyll that suggested the use of a green substance, but it was found that pink cobalt carbonate gave equally good results. The yield of " carbohydrate material " now obtained was better than that given by the white powders in ultra-violet light, the best yield being 0·075 g. in two hours. From the products a solid osazone was obtained. A particularly good form of nickel carbonate was prepared by an electrolytic method, since Baly had found that small amounts of alkalis (alkali carbonate) had a deleterious effect.

Being greatly impressed by these results, I asked Baly's permission to allow us at University College, London, to develop them further by employing different parts of the visible spectrum. This permission having been obtained, the research was undertaken by Dr. James Bell, who naturally began by repeating Baly's work, employing all his methods and precautions, and using besides his kieselguhr-supported catalysts the best form of nickel carbonate. The startling result was that Bell could find no trace of the formation of any organic matter whatsoever. An even more startling result was that when Baly heard of these negative results he tried to repeat his former work with the electrolytically prepared nickel carbonate and he also obtained negative results. Nor was he able to give any satisfactory explanation of the discrepancy.

As a result of this state of affairs, Baly now directed his attention almost wholly to the investigation of certain oxides supported on kieselguhr, and finally claimed to be able to prepare a photosynthetically active material by coating kieselguhr with an anhydrous layer of NiO containing ThO_2 in the molecular ratio of $1ThO_2 : 24NiO$, and that " when kieselguhr, which is completely coated with a trimolecular layer of these oxides, is irradiated in presence of water saturated with carbon dioxide, by daylight or the light of a tungsten filament lamp, there is photosynthesised a carbohydrate ".

In concluding this account of Baly's researches on " artificial " photosynthesis, reference may be made to his earlier papers on " The photosynthesis of nitrogen compounds from nitrates and carbon dioxide ", and " The photosynthesis of naturally occurring nitrogen compounds from carbon dioxide and ammonia ". Mention should also be made of his experiments on the action of ultra-violet light on a 40% solution of formaldehyde. The complex product of this action was investigated in the laboratory of Sir James Irvine at St. Andrews University, and was found to contain some hexoses, amongst much other material. As the solution during irradiation was prevented from becoming acid by the presence of magnesium or calcium carbonate, the possibility that the action was chiefly an exothermic auto-condensation in a slightly warmed alkaline solution cannot, perhaps, be excluded.

In this brief account of Baly's researches on photosynthesis I have tried to write as objectively as possible, as my personal contact with it was confined to the work of Bell; but I am bound to state that there exists a great deal of scepticism and doubt concerning many, if not all, of his claims. The general expert opinion may, perhaps, be summed up in the verdict " not proven ". In this connection may be cited the recent important book on " Photosynthesis " by E. I. Rabinowitch (Interscience Publishers Inc., New York, 1945), in which very many of the results claimed by Baly are strongly criticised in a detailed manner. On page 87 of this book it is stated that " practically all attempts to repeat Baly's experiments elsewhere have given negative results ". I understand that the criticisms of Rabinowitch are considered in the United States to be fair and just, and I think I am justified in stating that at least two eminent organic chemists in Great Britain share a good deal of his scepticism. It would seem that Baly was carried away by his enthusiasm and that his early results, incorrectly interpreted, apparently led to a veritable obsession which affected self-criticism and correct judgment. However that may be, he made a mighty effort to establish the existence of photosynthesis in inanimate systems, and, although the results he obtained appear to be still in dispute, it is quite possible that future and undisputable success may owe much to the stimulus derived from his investigations in this difficult field.

In concluding the account of Baly's scientific activities, it is possible only to make a brief reference to published work in various other fields of research, *e.g.*, the co-precipitation of sewage colloids with fine suspensates, the properties of sewage colloids, the mechanism of the activated sludge process of sewage disposal, the adsorption of the hydrous metallic oxides by kieselguhr, the reactivity of ammonia, the occurrence of helium and neon in vacuum tubes, high energy chemistry and vitamins, the kinetics of photosynthesis, the adsorption of gases and the equation of the liquid state, and the selective photochemical action of polarised light. As secretary to the British Association Committee entrusted with reports on absorption spectra, Baly was responsible for four valuable reports, published in 1916, 1920, 1922, and 1928. In 1928 he was President of Section B at the meeting of the British Association in Glasgow, his Presidential Address dealing with " Phosphorescence, Fluorescence, and Chemical Reaction ". For a number of years he published reviews of the progress of inorganic chemistry in the *Annual Reports.*

Baly went with the British Association to Canada (1924), South Africa (1927), and India (1937). After the meeting in Canada he undertook an extensive lecture tour in the United States. On the occasion of the Indian visit he received the honorary degree of D.Sc. of the University of Benares. Under the expert tuition of J. N. Collie he became a good rock climber, doing many climbs in Skye (some of which were new), and becoming a member of the Scottish Mountaineering Club. During his time as Professor at Liverpool he took an active part in the work of the Liverpool Section of the Society of Chemical Industry, becoming Chairman of the Section and at the same time a member of Council of the Society. He was elected an honorary member of the Society of Dyers and Colourists, to the West Riding Section of which he gave an annual lecture for 26 years.

Baly was an active and forceful person who made his influence felt in any society. Besides his knowledge of, and skill in, music, he was a most lively conversationalist, and one who understood and enjoyed the good things of life. His cheerful and optimistic temperament could enliven any company and endeared him to a host of friends. Self-confidence is a necessary virtue in life, and one may, perhaps, say (without any ill-feeling) that he was richly endowed with this virtue. Science cannot advance without the formation of hypotheses, and Baly possessed in good measure this important gift of imagination. It may be possible that, if he had a fault, it was due to the fact that his optimistic and self-confident mind could not always sufficiently control the wealth of his imagination. He was most certainly a highly skilled and resourceful experimenter. He was also a source of inspiration to many generations of research students and collaborators, who were strongly influenced by his immense energy and the activity of his mind.

F. G. DONNAN.

DOROTHY JORDAN LLOYD.*

1889—1946.

DOROTHY JORDAN LLOYD was born on May 1st, 1889, the daughter and grand-daughter of two prominent surgeons in Birmingham. It would appear natural that her early studies at Newnham were in zoology, passing over to physiological and then physicochemical studies of muscle. During the First World War, her scientific pursuits were diversified, and under the auspices of

---

* Reprinted by permission from *Nature*, 1947, **159**, 190.

the Medical Research Committee she was one of a team who studied substitutes for components of culture media for bacteria which were then in short supply. Then her brilliant short paper on the causes and prevention of ropiness in bread was most appropriate to war-time, and indicated the versatility of her mind. Under the stimulating influences of Gowland Hopkins and particularly of W. B. Hardy, she was led to the long-sustained studies of swelling in colloidal systems, which when carried over to the "structured" proteins in animal skins gained for her the firmly established international reputation as the authority on the chemistry of leather which she held at her death on November 21st, 1946. Thus, zoology in Part II of the Tripos, research in physiology followed by the D.Sc.(London), after some interval the Fellowship of the Institute of Chemistry in 1922, authorship of a standard work, "Chemistry of the Proteins" (first edit., 1926; second, with Ann Shore, 1938), and the directorship of the British Leather Manufacturers' Research Association (1927—1946) are the signposts of an exceptional career.

The output of Dr. Jordan Lloyd's published work both in pure and applied chemistry is impressive. Returning after the First World War to her main line of research, she published her earliest papers on the swelling of gelatin under the auspices of the Medical Research Committee; but most of her work on this subject, and indeed by far the greater part of her scientific publications, was published after she joined the staff of the British Leather Manufacturers' Research Association in 1921. Her early work followed on that of Procter by establishing the swelling curves of gelatin as affected by hydrogen-ion concentration and demonstrating the effect of salts on the swelling. Later she showed how lyotropic effects are superimposed on the swelling effects of the Gibbs–Donnan theory, and illustrated the temperature effects which that theory predicts. She emphasized the importance of the balance between osmotic pressure and cohesion in controlling the degree of swelling. This work was extended to a study of the swelling of protein fibres (collagen, silk, hair) in aqueous systems, in which not only the water uptake but also the changes in length and thickness of the fibres were followed; these changes are of importance in giving a more detailed picture of the effects of swelling on the individual fibres and on the interwoven structure of hides. These studies of fibres showed the effect of hydrogen-ion concentration on swelling, in so far as it produces salt formation by the protein, and also illustrated certain lyotropic effects. The weakening of the structure of the fibres which was observed in certain conditions was linked with the study of the molecular structure of fibrous proteins.

A number of Dr. Jordan Lloyd's papers deal with the closely related subjects of the chemical make-up of the fibrous proteins, and its bearing on their structure, water relationships, and swelling properties. She carried out experiments on the amount of water in gelatin which is "bound", that is, associated with specific groups in the protein molecules, and is to a considerable extent independent of osmotic effects. In addition to these studies of the molecular structure of fibrous proteins, she investigated the fibrous structure of hides and skins, and showed how the swelling properties of collagen fibres are affected by the sheath or network ("reticulin") which surrounds them and can be broken by strong swelling or weakened at very low and very high pH values.

The above work dealt with proteins in aqueous systems. In later developments Dr. Jordan Lloyd turned to a study of the effects of non-aqueous liquids (organic solvents and acids), using changes in volume, dimensions, and extensibility of fibrous proteins in these liquids, to throw light on the nature of the cross-bonds which hold the protein molecules to one another, and on the orientation of these molecules in the fibres.

Her keen sense of the applicability of this mass of scientific results to the processes of the industry was coupled with a resolution and an unfailing affability towards industrialists and their technicians which was an immense advantage to the Research Association. The long series of her lectures, well attended by its members and their technical personnel, and based on the work in progress in the Association's laboratories, were symptomatic of her hold on the imaginations of those in the industry, and illustrated her wide grasp of the application of science to it. In the industry the measurement of hydrogen-ion concentration and buffer index became usual, particularly in heavy-leather tanning; the use of the microscope and the correlation of photomicrographs with composition and use of tan liquors led to control of quality in leather in general and also in its production for special purposes—a control not hitherto attained; the disposition of the tanned fibres in leather is now related to its various measurable physical properties, and hence to quality and serviceability—such have been developed largely by her leadership and enthusiasm, which have contributed much to the gradual change of leather production from a chancy industrial art to a sequence of processes scientifically controlled.

Miss Jordan Lloyd was perhaps unique among the scientific women of her time—a vigorous and vivacious leader of her mixed team both in triumph and tribulation (for her laboratories were twice wrecked in the " blitz " on Bermondsey); a clear thinker and good speaker with a constructive mind; a manager with ability to delegate; a research worker herself who displayed an arresting interest in the investigations of many others; and with a demeanour and practical outlook in the tanneries which appealed to the employers and their technicians. She can have perhaps no more fitting epitaph—and one possibly no more welcome to her—than the impressive volumes now appearing which describe the results of twenty-five years of research for the British leather industry by the British Leather Manufacturers' Research Association under her inspiring leadership.

ROBERT H. PICKARD.