OBITUARY NOTICES.

JAMES BADDILEY.

1885—1951.

JAMES BADDILEY, for many years director of research for the Dyestuffs Division of Imperial Chemical Industries Limited, died on September 26th, 1951, after an operation. Born on July 15th, 1885, at Blythe, Yorkshire, he was the eldest of a family of three sons and a daughter, his father being a well-known farmer and breeder of shire horses in the Worksop district. Baddiley received his early education at the Bradford Grammar School, proceeding in 1903 with a West Riding County scholarship to the Yorkshire College, Leeds, which became in the following year Leeds University. Here he was one of the first pupils of A. G. Green, who had been appointed to the Chair of Colour Chemistry and Dyeing the same year, and his success in Green's department determined his future career. Graduating in 1907 with first-class honours in Colour Chemistry, he was awarded the Le Blanc medal and also gained silver medals and first-class certificates in the examinations of the City and Guilds Institute in both Coal Tar Products and Dyeing.

Green, with an established reputation for successful technical research and discoveries in the field of dyes, was now building a more academic research school at the University and it was natural that so promising a pupil as Baddiley should be invited to join him. This he did, and for the next two years he collaborated in investigations concerned with stilbene derivatives and the cotton dyes derived from them. His part was to study the action of alkali hydroxides on derivatives of *p*-nitrotoluene having substitutents in the *ortho*-position, in the course of which he prepared and examined many new derivatives of 4 : 4′-dinitrostilbene. The results of this work were published in a joint paper (Green and Baddiley, *J.*, 1908, **93**, 1721).

Leaving the University in 1909, Baddiley joined the firm of Levinstein Ltd. at Blackley, Manchester, where he was destined to remain for the whole of his industrial life. The first recruit to join this firm from Green's school at Leeds, he started as private assistant to Dr. Herbert Levinstein. Speaking of Baddiley at this time, Dr. Levinstein says: "He was a neat manipulator, a most pleasant person of good manners, good tempered, and easy to get on with. I was delighted with him and we did a good deal of work together with equal satisfaction, I think, to both of us." This description fitted well the man as we knew him later. He had an extraordinary fund of patience, whether in the progress of a research or the development of a policy and would never be rushed into a course until he was sure of his ground. He could at the same time be obstinate in his opinions when he believed he was right.

The years from the time he joined Levinstein's to 1914 were, as it transpired, a training period for his life's work, in which he was to play a leading part in the renaissance of the dyestuff industry in Britain. At Levinstein's, during the period of eclipse of the British industry, competition with the German firms had always been kept alive and the lamp of research had never been extinguished even if it had burnt low. Baddiley's work was mainly concerned with cotton dyes. At this time the use of derivatives of J acid (2-amino-5-naphthol-7-sulphonic acid) as components for azo-dyes, to confer cotton substantivity, was being developed in Germany and the identification by Baddiley of a Bayer product called Benzo Fast Red 8BL as a dye formed by coupling diazotised aminoazobenzenesulphonic acid with benzoyl-J acid led Levinstein and Baddiley to investigate this field. They obtained a number of patents for the manufacture of derivatives of J acid and their use in azo-dyes (B.P. 11,877; 15,068; 15,070; 26,577/1910; 12,281/1911) and built up a successful range for manufacture. A new black dye for velvet, Vulcan Black G, was invented and much work was done in developing azo-dyes of a new type, to be after-treated on the fibre with formaldehyde to render them faster to washing. These dyes had resorcinol or *m*-aminophenol as end component which rendered them reactive to formaldehyde so that a more complex and less soluble compound was produced in the fibre (B.P. 27,525; 28,296/1912; 1435; 1436; 25,547; 28,569/1913; 8569/1914). This early work must have given Baddiley his life-long interest in cotton substantivity and its relation to molecular size and solubility, a problem which at a later stage in his career recurred in connection with the dyeing of artificial fibres.

On the outbreak of war in 1914 it soon became most unpleasantly clear how far the British textile industry and other colour-using industries had become dependent on foreign, especially German, dyes. Prices of such stocks of dyes and intermediates as were in the country rose to fantastic heights. The small dye-making firms, of which Levinstein's was one of the foremost, had forthwith to manufacture as fast as they could such dyes as were within their range and at the same time to do their best to discover as quickly as possible how to make a vast number of intermediates and dyes which had only been obtainable from abroad. Ivan Levinstein and his son at once started to build up a research department around Baddiley, with the assistance of A. G. Green who resigned his chair at Leeds and went to Manchester to found a school of dye research at the College of Technology whilst at the same time acting as adviser to Levinstein's. By early 1917, when the writer joined Baddiley, he had a staff of about 15 chemists engaged in working out processes for the manufacture of a wide range of benzene and naphthalene intermediates and of azo-, triarylmethane, and rhodamine dyes. Most of the staff were trying to keep several lines of research going at the same time, for the call for more manufacture was insistent. Baddiley, always ahead of his team in knowledge, experience, and wisdom, was there at hand to guide and advise. When the war came to an end the staff rapidly increased in numbers and for a time its supervision was divided between Baddiley and Green, the former being responsible for immediate technological problems, the latter for longer-range projects. In practice, however, there was little difference between the work and outlook of the two divisions and, in course of time, after Green's retirement, Baddiley became entirely responsible for all research. The amalgamation of Levinstein's with the Government-sponsored firm of British Dyes Ltd. and later the absorption of Scottish Dyes Ltd. into the British Dyestuffs Corporation brought Baddiley added responsibilities and more scope. Following the end of the war were several very difficult years, in which the national necessity for building up an efficient dyestuff industry was hard to reconcile with the need of the consumers, especially the textile industry, for a full range of dyes at the lowest possible prices. That the purely technical side of this obstinate problem was solved was largely due to the untiring work of Baddiley backed by his loyal research department, encouraged administratively by Dr. C. J. T. Cronshaw and supported by the then chief colourist, the late R. S. Horsfall, who was unfailing in his efforts to raise the standard of the Company's products. The technical progress of the industry made the satisfactory political solution of the dyestuffs problem possible. When the great chemical combine, Imperial Chemical Industries Limited, was formed in 1926, the British Dyestuffs Corporation was included and Baddiley's position as head of the Research Department of the Dyestuffs Group (afterwards Dyestuffs Division) was confirmed. In 1930 he became a Delegate Director of the Group and Joint Technical Manager, and he remained director in charge of research until his retirement in 1947.

Baddiley had few equals in his wide knowledge of the chemistry and technology of dyestuffs, backed as it was by a sound knowledge of organic chemistry and appreciation of its possibilities, and supported by a retentive memory. Necessarily most of his energy had to be devoted for many years to what became known as " imitative " work, the production of the equivalents of competitive continental dyes, but when the back of this work had been broken his department was able to devote more and more of its time and man-power to speculative and fundamental work on dyes and to other fields of applied organic chemistry. He always kept pace with these expanding interests and used his powers to great effect in developing chemicals for use as dyeing assistants and later in directing research in the fields of plastics, pest control products, and finally medicinal chemicals. His personal contributions were important and his name is associated with about fifty patents in the period 1915—1930. He paid particular attention to the problems of dyeing artificial fibres, including viscose (regenerated cellulose) and acetate rayon. It was at one time difficult to dye viscose rayon in level shades with direct cotton dyes, partly, no doubt, because of a certain lack of uniformity in the manufactured fibre. Associating this behaviour with a greater avidity of regenerated cellulose, compared with cotton, for direct dyes, Baddiley and his colleagues, P. Chorley and R. Brightman, invented a range of dyes having lower affinity for cotton but which yet dyed viscose satisfactorily, more slowly but in level shades. Many of these dyes contained gamma acid (2-amino-6-naphthol-8-sulphonic acid), in place of the isomeric J acid as a component (see B.P. 270,446 and additions, B.P. 277,756, 281,410, etc.). From these inventions came the Icyl range of dyes for viscose.

Baddiley's most noteworthy contribution to the dyeing of artificial fibres related, however, to the dyeing of acetate rayon. When a satisfactory cellulose acetate fibre had been produced by Dr. H. Dreyfus it was found that only a very few of the existing textile colouring matters would dye it and these few provided a hopelessly restricted range which could not be used together and had poor fastness properties. In general, solubility of a dye in water was incompatible with affinity for cellulose acetate. Baddiley has attributed the suggestion to colour the rayon by means of aqueous dispersions of insoluble, weakly basic coloured substances to two of his colleagues, Horsfall and Sanderson. This suggestion led at first to the discovery only of a few yellow and orange dyes, but the outlook for the fibre was entirely changed when Baddiley with the late A. Sheperdson discovered that amino-derivatives of anthraquinone when applied from a dispersion had a strong affinity for acetate rayon, giving deep dyeings of a high degree of fastness. This basic discovery immediately opened a wide field, since it is possible by devices well known to the chemist to synthesise compounds from anthraquinone, containing one, two, or more simple or substituted amino-groups, of almost any colour in the spectrum. From this discovery came the important Duranol range of acetate dyes (B.P. 211,720). The fundamental value of this discovery was publicly recognised in 1939 by the Society of Dyers and Colourists by the award to Baddiley for this and his other great services to the dyestuff industry of its highest honour, the Perkin medal. He thus joined the ranks of a very distinguished company which includes such eminent chemists as Graebe, Liebermann, A. von Baeyer, and R. E. Schmidt amongst foreigners and A. G. Green and C. F. Cross amongst fellow-countrymen. On the occasion of the presentation of the medal Baddiley delivered a fascinating address on " Post-war Development in the Dyestuffs Industry " on which he could speak with the full authority of one who had taken a leading part therein (*J. Soc. Dyers and Col.*, 1939, **55**, 236).

In his earlier years Baddiley was a great walker and lover of the countryside and later a keen motorist, but he allowed few outside interests to interfere with his devotion to chemistry. Family connections ensured that he had a lively interest in the problems of agriculture and in the applications of chemistry to their solution. His intimate knowledge of the history and development of the German dye industry convinced him that the finest nursery for an organic chemist, whatever his ultimate destination, was a dyestuffs laboratory and he was happy to see successful research in other fields emanating from the laboratories under his charge. After his retirement he continued to attend research discussions at Blackley, maintaining his chemical interests to the end.

Baddiley took an enlightened view of the interdependence of pure and applied science and their need for one another's help. Under his régime the doors at Blackley were wide open to academic scientists, to numbers of whom he became a familiar figure. But for this he would have been little known to the chemical world outside the industry, for he was of a retiring disposition and shunned any kind of publicity. He always retained an affection for the University of Leeds, becoming in 1944 a member of its Textile and Dyeing Advisory Committee and in 1945 a life member of the University Court.

He married in 1915 Ivy Logan Cato and had four children, two sons and two daughters. One of his sons became a farmer, the other has already made his mark in the chemical profession, of which his father was so bright an ornament and so devoted a member.

E. H. RODD.

SAMUEL SUGDEN *

1892—1950

SAMUEL SUGDEN was born in Leeds, Yorkshire, in 1892; he was the eldest son of a Samuel Sugden who was connected with wool trade journals.

He gained his secondary education at Batley Grammar School, where apparently he developed a very definite interest in science though, so far as can be ascertained, this did not come from any family bias or tradition. He won a Royal Scholarship, offered by the Board of Education, which took him to the Royal College of Science in London in October 1912. He had already passed the Intermediate Examination, so he was able to take the Final one in July 1914. On the result, he was awarded the Associateship of the Royal College of Science in chemistry, with First-Class Honours, and also the Governors' Prize for the best student in practical chemistry and the Frank Hatton Prize for proficiency in advanced chemistry. He showed himself to have not only outstanding ability but also very definite personality. He gave a memorable impression of energy and of extreme enthusiasm for the latest developments in science, not only by his liveliness in discussion but by his activity in doing such things as building wireless sets, a hobby which, in those days, was a most exciting form of pioneering.

* Reprinted, by permission, from *Obituary Notices of Fellows of the Royal Society*, Vol. 7.

Sugden enjoyed his time at the Royal College of Science, though he had to tolerate a full share of chaff and banter, for he was a Yorkshireman with rough edges, and he was small of stature. Fortunately he had a very cheerful disposition and was not easily ruffled, so he took all this with good grace, and responded with gusto but without acrimony. He showed little interest in any form of sport, but he had one Yorkshire heritage—a love of choral music. This was, perhaps, accentuated by his having been brought up as a Wesleyan Methodist and being accustomed, therefore, to a high standard of congregational singing.

It was understood at the College that he had intended to stay there to do research in inorganic chemistry; but the outbreak of war in August 1914 appears to have caused him to change his plans. He left the College on October 6th, and first took employment as a chemist with a cement-manufacturing firm at Snodland, Kent. However, his strong sense of duty soon caused him to consider enlisting, so he made inquiries about obtaining a commission. In this he was unsuccessful, presumably because of his lack of inches; and in September 1915 he joined the Royal Army Medical Corps as a private soldier. He was soon disillusioned, for he was set to do bacteriological testing; and although his scientific training fitted him for this, the task quickly became a dull routine.

In June 1916 he was released to join the Research Department of the Royal Arsenal, Woolwich, as a chemist to work under the late Sir Robert Robertson, F.R.S. There he was much happier once he had recovered the ability to think, which process of convalescence, he said to a colleague, took fully three months. Although it is not possible to associate his name with any major discovery of that war, his work was of considerable importance in promoting the satisfactory performance of many munitions. It bore on the physical changes which took place in certain explosives during storage, and the devising of precautions essential during manufacture and filling to ensure correct functioning. He also developed methods for determining the specific surface of fine powders used as ingredient of certain explosives; and he developed smoke shells for ranging. In the latter connexion a colleague tells a story of how Sugden ingeniously contrived to make a pitch filling pulverize on explosion, by adding powdered charcoal, but how, in his instructions to the Ordnance Factory, he unfortunately omitted to specify that the charcoal should be powdered, so he received an urgent message that the charcoal sticks would not dissolve.

Out of the group of about a hundred scientists he found plenty of kindred spirits who would join him in lively discussions on any scientific topic that happened to arise. Nevertheless, he decided that the restraints of a Government scientific department were such that he would be happier in the academic world; so at the end of the war, as soon as he could, he sought a post; and on April 30th, 1919, he resigned from the Research Department to become a lecturer at Birkbeck College. Even during the war he actively pursued his interest in pure science, for in February 1917 he was elected a Fellow of the Chemical Society; and in 1918 he was awarded the external B.Sc. Degree of the University of London with First-Class Honours in Chemistry.

He undertook his new duties with great vigour, enthusiasm, and joy. He had found his vocation at last; and he very quickly made his mark, as will appear later. In passing it may be mentioned that he completed his academic qualifications by taking his M.Sc. Degree in 1921, and his D.Sc. Degree in 1924.

For a number of years he lived a cheerful, self-sufficient bachelor's life, running a small flat, and managing his domestic affairs very well. Quite suddenly, however, he surprised his friends by announcing his intention to marry; and in 1926 he married Eleanor, daughter of the late Thomas Dunlop of Glasgow.

He steadily accumulated advancement and honours. In 1928 he was appointed Reader in Physical Chenistry at Birkbeck College; and in the same year he became a Member of the Council of the Chemical Society. In 1932 he was appointed Professor of Physical Chemistry at the College and, again in the same year, he was elected an Honorary Secretary of the Chemical Society. In 1934 he was elected a Fellow of the Royal Society. In 1937, on the retirement of Professor F. G. Donnan, F.R.S., he was appointed a Professor of Chemistry at University College, London.

First at Birkbeck College, then at University College, Sugden established active schools of research, and also did much for the teaching of physical chemistry. He had wide scientific interests, independence, and originality, a gift for thorough analysis and for lucid exposition, great powers of self-discipline and concentration, and a very happy touch in personal relations. These qualities made him not only an excellent researcher and teacher, but also a good administrator in academic affairs.

Sugden was very sociable. He especially enjoyed entertaining his friends by his own fireside; and they found his company and that of his wife most refreshing. Not only did he have an impishness and a sly wit which were very attractive, but he had also a repertoire of stories appropriate to a wide variety of occasions. Sugden retained a certain amount of North Country cockiness; and to hear him engaging in a boasting match with one of his scientific colleagues was a most interesting experience. He was a redoubtable opponent for, if his adversary paused for breath, or for effect, Sugden, being small and nimble-witted, would quickly seize the initiative and talk him to a standstill. To younger scientists he was the kindest of persons : he never posed or tried to dominate, but unobtrusively made suggestions which were always shrewd and pertinent. He had definite views on politics; in his early days he was well to the Left, though subsequently he moved some way to the Right; but he was too interested in science to be a crusader.

The outbreak of the second war in 1939 brought an abrupt end to the comfortable phase in Sugden's life; indeed, as events turned out, it effectively ended his scientific career. In accordance with a previous arrangement he immediately joined the Ministry of Supply, and went to work at the Chemical Defence Experimental Station at Porton, near Salisbury. Although he had had no previous contact with this aspect of military science he threw himself into the work with so much vigour and good sense that he soon made substantial contributions. At first he was in charge of one particular practical problem, the elucidation of factors affecting the stability of certain war gases; but later he played a prominent part in the military assay of war gases, and particularly of new substances. Still later, he was for a time superintendent of the whole research group at Porton. At no time, however, did he allow himself to become an arm-chair scientist; he retained a very lively interest in what was actually being done in the laboratories and in the Chemical Defence factories.

Moved by a feeling of desperate urgency, Sugden spared no effort to get things done as well or as fast as he thought they should be. It is not easy or simple to communicate this sense to a somewhat rigid organization : one result of his attempts was that he incurred some personal unpopularity, and began to acquire a reputation for impatience which had not previously been associated with him. During this period also he had cause to be anxious for his wife's health and safety. Despite these personal trials, his enthusiasm never flagged; and he drove himself regardless of possible consequences. In 1942, feeling that chemical warfare would not be employed and that he could be more useful in another department, Sugden sought and obtained a transfer to the Armament Research Department. He was appointed Superintendent of Explosives Research on October 12th, 1942. Here again, however, he ran into difficulties; so in June 1943 he resigned, and soon after became Scientific Advisor to the U.S.A. 8th Army Air Force in Great Britain.

It speaks much for Sugden's devotion to his subject that at this time, despite all the cares and duties which distracted him, he accepted the invitation to give the Liversidge Lecture to the Chemical Society; and on April 15th, 1943 he delivered a most interesting and stimulating talk on magnetochemistry. He had also continued to serve the Society by being a Vice-President from 1939 to 1942, as he did again later from 1946 to 1949.

In November 1943 he had a quite severe attack of influenza, and he returned to work before his doctor approved. Soon after, he had what proved to be a severe breakdown in health, from which he did not properly recover until the summer of 1944. He returned to his academic duties in September 1944, and soon was helping to cope with the difficult post-war problems of restoration which faced University College. Hardly had relatively normal conditions been restored, however, when it became clear that he was unlikely to reap much reward for his hard work. In 1948 he developed symptoms of angina pectoris. He continued his normal life as best he could, but, though the smile and the friendliness remained, the energy and most of the fire had gone for ever. At first he affected to make light of his complaint, but soon he could no longer do this. He faced the position bravely and with resignation, as did also his wife. He died on October 20th, 1950, aged only fifty-eight years. He is survived by his widow; there were no children.

After going to Birkbeck College, Sugden lost little time in starting research. He appears at this period to have explored more than one possible field, for his first paper " On reduction by metals in acid solutions, Part I ' (*Trans.*, 1921, **119**, 233) is about a topic entirely different from anything he worked on later, and it is in fact the last of its kind. His second paper (*ibid.*, p. 1483) is the first of the series on surface tension and derived properties which made his name. It deals with experimental methods of measuring this property, as do also the third and the fifth (*Trans.*, 1922, **121**, 858; 1924, **125**, 27); and they illustrate very well many of the good qualities of Sugden's work, his grasp of the literature and the background, his thoroughness, exactness, and careful attention to detail both of theory and in practical work, and the general good sense which enabled him to see what was important and what was not. There is no clue in these papers to the motive which led him to study surface tension; but one of his friends tells how one day he found Sugden, long before he went to Birkbeck College, measuring the surface tension of a reddish liquid which, Sugden explained, was molten T.N.T.; so this property may have interested him for some time. At all events, in his published work Sugden first dealt very carefully with the methods of measurement (*Trans.*, 1921, **119**, 1483; 1922, **121**, 858; 1924, **125**, 32); then in 1924 (*Trans.*, **125**, 32) he considered relations between surface tension (γ) and temperature or some other temperature-sensitive functions, including particularly the McLeod relation between surface tension and density. Very soon after, in the same year, he considered and rejected, on new evidence, the Langmuir and Harkins theory that surface tension is determined largely by the orientation of molecules in the surface (*ibid.*, p. 1168); and so he cleared the way for the formulation of the *parachor* (*ibid.*, p. 1177), *i.e.*, the function

$$P = \frac{M\gamma^{1/4}}{D - d}$$

where M is the molecular weight of the substance, γ is the surface tension of it as a liquid, and D and d are the densities, at the temperature of measurement of γ, of the liquid and the saturated vapour respectively. It is a function which, in effect, permits molecular volumes to be compared at the same internal pressure. It shows remarkable additivity, but there are several structural constants which have made possible attempts to answer structural problems which had been left unanswered or in doubt by the classical methods of organic and inorganic chemistry. From the three-year gap between his first paper on surface tension and the discovery of the parachor it seems unlikely that Sugden had at first a clear idea of where he was going, but rather that he was feeling his way and hoping that something would turn up. As soon as something did, he exploited it very ably.

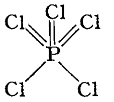
From 1924 to 1932 he gave the greater part of his energy to applying the function, to considering its significance, or to searching for other useful molecular–volume functions. He published twenty-two papers on these topics (*J.*, 1924—1932). The best idea of the work is obtained from his book "The Parachor and Valency," published in 1930, which surveys all the papers save the last few, and adds considerably to them. It is an admirable work in many ways.

All the initial applications turned upon two constitutional constants, one for the degree of unsaturation and the other for the presence of a semipolar double bond, or dative link. The degree of unsaturation was defined as x/n, where x is the number of latent valencies, or the number of hydrogen atoms needed to give saturation, and n is the number of atoms in the ring—which may be two or more. This constitutional constant is then $23{\cdot}2x/n$. Thus a double bond gives an increment of 23·2; but a six-membered ring gives one of only 7·7. The constant for the semipolar double bond is $-1{\cdot}6$.

These constants could not be predicted theoretically any more than could those for the atoms; but the unsaturation expression was based on experimental values for rings which had been established by standard organic chemical arguments; and the semipolar double bond value was derived from compounds which were supposed to have this bond in order to conserve the valency group of eight postulated by G. N. Lewis, *e.g.*, nitro-compounds.

On this basis Sugden was able to choose between alternative formulæ for several organic compounds, which differed according to whether there were multiple links or rings, or chains; *e.g.*, he showed that paraldehyde has a ring rather than a chain structure, that benzoquinone is a monocyclic diketone rather than a trans-annular peroxide, and that benzil is a chain and not a ring compound; he supported Ott's and Pfeiffer's views (*Ber.*, 1922, **55**, 413) about the structures of the two forms of phthaloyl chloride. He also examined some examples of ring–chain tautomerism, and found qualitative support for Ingold and Shoppee's conclusions (*J.*, 1928, 365, 1662, 1868) regarding the phorones, though there were anomalies which prevented a quantitative analysis of the problem from being made.

He showed that the observed parachors are consistent with Lowry's hypothesis that when the higher valencies of elements such as phosphorus or sulphur are developed by the attachment of oxygen, the new bonds are semipolar double bonds and not ordinary double bonds. This agreed with current theory and with current interpretation of the stereochemical evidence, but it was regarded as a striking and valuable confirmation thereof. Sugden developed his



thesis much farther, however, for he concluded on similar grounds that whenever the co-ordination number exceeds four, some of the atoms or groups are attached by one-electron or " singlet " links, as, for example, in phosphorus pentachloride, following an earlier suggestion by Prideaux; and also that in chelate rings the links between the metal atom and what is nowadays called the ligand are singlet ones. He proposed singlet linkages in a number of other complex compounds, and developed a coherent theory that they were formed (*a*) whenever the valency group would otherwise exceed eight, or (*b*) because duplet bonds would give anomalous charge distributions, as, for example, in the aluminium derivative acetylacetone wherein the dative links, if each were supposed to give a transfer of one electron charge, would give a negative charge of three units to the metal atom; and he concluded that a singlet link is more stable when it joins two elements which differ markedly in their electron affinity. His views were criticized by Sidgwick (" Electronic Theory of Valency," Oxford, 1927, Chapter VII), who pointed out that many of the parachor results could be explained on duplet formulæ if it be supposed that there is a negative parachor value corresponding to the expansion of a valency group beyond an octet.

Sugden argued (with Waloff, *J.*, 1932, 1492), however, that the only consistent alternative would be to assume that the sharing of each extra electron (*i.e.*, over and above the number for which the normal parachor constants are derived) would cause a contraction of 11·6 units; and he showed that there is no such change when the number of electrons shared by an atom is altered, as in some series of mercury, thallium, and boron compounds. It is certainly true that the singlet link hypothesis provides a remarkably consistent interpretation of parachor values, though inspection of some of the numerical data shows that there are minor discrepancies; and it formally solves the difficulty of the improbable charges in the duplet formulation of complex compounds. Anomalies were found, however; *e.g.*, it was shown that some of the thallium compounds on which his argument rested are actually associated to definite degrees in solution (Sidgwick and Sutton, *J.*, 1930, 1461), and that some non-associated *o*-substituted phenols which might be supposed to have a chelate ring involving a hydrogen bond, show an anomaly which does not correspond to singlet linkages (Sidgwick and Bayliss, *J.*, 1930, 2027). Furthermore, considerable doubts were expressed as to whether such a singlet link would have the strength, compared with that of duplet links, which the chemical facts require. This last criticism was made very pointedly in 1931 by Pauling (*J. Amer. Chem. Soc.*, 1931, **53**, 3229) who, in the course of a systematic application of wave mechanics to the problems of chemical binding, concluded that singlet electron links would be only about half or two-thirds as strong as duplet links, and that they were likely to be formed only between atoms of equal or nearly equal electron affinity. Sugden's theories on this matter were thereafter largely discounted, and he himself eventually abandoned them, though he retained a sentimental regard for the singlet link.

In looking back at this old controversy one sees that it came at the end of an era when structural investigations by physicochemical means as we know them now were all but impossible. After the molecular weight of a substance had been determined, the only further steps which could be taken were to measure its boiling point and its solubility, and perhaps to determine its molecular refractivity; and then to speculate on the basis of a very primitive theory. The discovery of a new property which might give information about structure was therefore an important event; and it had the very valuable indirect effect of prompting chemists to search for other such properties. In retrospect it is clear that the parachor was not a satisfactory property for the analysis of structure because its value as a function thereof could not be predicted with certainty on a basis of physical theory. This meant, and still does, that predictions were based essentially on numerical consistency and on bold assumptions of analogy, which bases proved ambiguous or inadequate. The parachor led, in fact, to the wrong conclusion about the structure of the azides; and it did not give any clue to the mystery of the stability of benzene, for the observed parachor agreed well with that for a single Kekulé structure. Despite this, those who were working in the field at the time remember vividly how stimulating was Sugden's pioneer work, and know how much it did to help bring in the new era.

After 1930 there developed rapidly several other methods of examination which gave more direct information about molecular structure, and Sugden soon turned his attention to some of them. He first interested himself in the relation between magnetism and electronic structure. In this work he used experimental techniques and theoretical methods which had been developed by others, but he showed all his usual liveliness and perceptiveness in the choice of topics. He applied measurements of the paramagnetic moments of molecules to various ends. Thus he used them to investigate the electronic arrangement within metallic ions, either simple or complex, and in neutral co-ordination compounds, and correlated this with the stereochemical arrangement of the co-ordinated groups [(with Morgan) *Nature*, 1931, **128**, 31; *J.*, 1932, 161, 246; (with Barkworth) *Nature*, 1937, **139**, 374; (with Nettleton) *Proc. Roy. Soc.*, 1939, *A*, **173**, 313; *J.*, 1943, 328]. He also showed that certain organic molecules are paramagnetic and therefore are free radicals [(with Kenyon) *J.*, 1932, 161; *Trans. Faraday Soc.*, 1934, **30**, 18; with Allen, *J.*, 1936, 440] but that hypophosphoric acid is not paramagnetic and therefore is $H_4P_2O_6$ and not H_2PO_3 [(with Bell) *J.*, 1933, 48].

In connexion with the first topic, he prepared two isomeric nickel benzylmethylglyoximes [*J.*, 1932, 246; (with Cavell) *J.*, 1935, 621], which indicated the possibility of *cis–trans*-isomerism, and therefore that the four nickel valencies are directed to the corners of a square. This possibility had been predicted by Pauling and, furthermore, according to his theory both of them should be diamagnetic. Sugden showed that in fact they are. He went on to examine a number of cobalt complexes, and showed that the quenching of the orbital component is far from complete and that it depends in some way upon the nature of the ligand. In his Liversidge lecture (*J.*, 1943, 328) he gave a most excellent critical discussion of such matters. Among his last papers was a series on the magnetic susceptibility of the rare-earth elements [*J.*, 1949, (with Hilal) 135 and (with Tailby) 136, 137, 139].

Soon after he became actively interested in magnetic measurements Sugden started work also on dielectric measurements (*J.*, 1933, 768; *Nature*, 1934, **133**, 415). He showed that the electric dipole moments of substituted nickel glyoximes give some support to the hypothesis that they exist as *cis–trans*-isomers (with Cavell, *loc. cit.*); but his major contributions in this field came from measurements of electric moments in the gas phase. In the course of a series of systematic investigations [(with Groves) *J.*, 1934, 1094; 1935, 971; 1937, 158, 1779, 1782, 1992; also *Trans. Faraday Soc.*, 1934, **30**, 734] he gave careful attention to the question of the magnitude of atom polarizations and to the best way of allowing for them. He became convinced, also, of the importance of making a more complete and general allowance than hitherto for the alteration of the moments of whole molecules by inductions between the component moments; so he set about this difficult task, and thus obtained a new set of bond moments. By means of these he was able, for example, to confirm the existence of anomalies in benzene derivatives which had been previously noticed and correlated with the " electron drifts " postulated by organic chemists in their theories of reaction.

Sugden's other major scientific interest was the use of radioactive tracer elements in chemistry. He did some useful work on the technique of concentrating the radio-bromine formed by neutron bombardment [(with Le Roux and Lu), *Nature*, 1939, **143**, 517; (with Lu) *J.*, 1939, 1273], and then proceeded to use this element for studying the changes in the parameters of the reaction $RBr + Br^{*-} \longrightarrow RBr^{*} + Br^{-}$, with variations of R and of solvent [*J.*, 1939, (with Le Roux) 1279, (with Elliott) 1836; (with Le Roux, Lu, and Thomson) 1945, 586; (with Evans) 1949, 270]. This work was interrupted by the 1939—1945 war, but even so he obtained a number of results which began to form a pattern and which revealed the surprising fact that the slowness of the reaction of bromo-2 : 4-dinitrobenzene, relative to that of alkyl bromides, is due not to a high activation energy but to a low effective collision area.

Besides his major book, " The Parachor and Valency," Sugden translated, from the German, Alfred Stock's book on " The Structure of Atoms " (1923), and, in conjunction with the late Professor T. Martin Lowry, F.R.S., he wrote the well-known textbook " A Classbook in Physical Chemistry "(1929).

In appraising Sugden's work, it must be remembered that his working life, as an academic scientist, was little more than the twenty years of the last peace, for after the war he had hardly been able to do more than start research again before he was stricken by illness. In that time he was remarkably productive. He was, indeed, one of the most galvanic figures among English chemists. He was very versatile; and, because of his originality and thoroughness, he added much to every subject he touched.

He will be remembered by his many friends not only for his intellectual gifts, his kindness, and his personal courage, but for his zest. He really enjoyed learning and research; and he had the power of communicating that joy to others.

L. E. SUTTON.