OBITUARY NOTICES.

LUCY HIGGINBOTHAM.

BORN, NOVEMBER 6TH, 1896; DIED, NOVEMBER 22ND, 1927.

LUCY HIGGINBOTHAM was educated at Penrhos College, Rhos-on-Sea, and the Manchester High School for Girls. She entered Manchester University in the autumn of 1915, and took the Honours course in chemistry, graduating with First Class Honours in 1919 and gaining the Leblanc Medal for the Chemistry of Colouring Matters. The following year was spent in work on derivatives of coumarone which earned her the degree of M.Sc., and was published in the *Transactions* jointly with Dr. Henry Stephen (J., 1920, **117**, 1534). For the next two years she acted as research assistant to Professor Arthur Lapworth, F.R.S., and contributed with him to the *Transactions* of the Society five papers dealing with the reactions of ethyl ethylidenemalonate and related substances (J., 1922, **121**, 49, 2823; 1923, **123**, 1325, 1618; 1924, **125**, 2339). In September 1922 she joined the staff of the British Cotton Industry Research Association, and was elected to the Fellowship of the Society in December of the same year. For the remainder of her active career she was engaged in researches on the minor constituents of cotton, in particular the complex mixture of substances present in cotton wax, and their behaviour during the processes concerned in the bleaching and finishing of cotton, the results being contained in a series of papers in the *Textile Institute Journal* (1924, **15**, T75, T120, T337; 1926, **17**, T233; 1927, **18**, T283), and in the *Memoirs* of the Shirley Institute. Early in 1926, illness necessitated a rest of several months. She returned to work again, but, after a relapse, went to Berne on the advice of her medical adviser for an operation, and died there on November 22nd, 1927. As a research worker she possessed unusual energy and a pronounced *flair* for the rapid exploration of a field of inquiry; outside her work, she was keenly interested in athletics, particularly tennis, golf, and motor cycling. She had many of the best characteristics of her native county, and her frank outspoken genial personality endeared her to her colleagues at the University and the Shirley Institute.

R. G. FARGHER.

EDWIN ROY WATSON.

BORN, JULY 20TH, 1880: DIED, NOVEMBER 10TH, 1926.

EDWIN ROY WATSON, the second son of William Watson, was born on July 20th, 1880, and received his early education at the Nottingham High School (1891—1899), where his career throughout was marked by high distinction. Although in 1897 he obtained an open science scholarship tenable at Emmanuel College, Oxford, this was not utilised, and he first graduated as an external science student at the London University in 1900. As the result of a second scholarship award, he became a student at Jesus College, Cambridge, in 1899, subsequently taking a First Class in Part I of the Natural Science Tripos (1901), and a second class in Part II (1903). From 1902—1904 he was mainly engaged in post-graduate research at Cambridge, under the direction of Dr. S. Ruhemann, although during this period he also took part in a temperature survey of Loch Ness, then in progress under Sir John Murray's direction. In 1904 he was appointed Professor of Chemistry to the Sidpur Engineering College, Bengal, where he remained for two years, then proceeding to Dacca College in a similar capacity. In 1921 he became Principal of the Government Technological Institute at Cawnpore, and with this institution he was still connected at the time of his death.

Watson was characterised throughout his career by thoroughness and tenacity of purpose. Being an excellent chemist and an able lecturer, and possessing an almost untiring energy, his influence with his students was most marked, and as a teacher at Dacca he will long be held in affectionate remembrance.

At the outset of his work at Dacca the College possessed no laboratory worthy of the name, and although on its removal to Ramna, and under his guidance, this defect was remedied, for a long period many conveniences were lacking and the conditions essential for good work were far from ideal. Even in these circumstances, however, he was able to prepare the students for their degree examination in science. But Watson was not satisfied with the mere routine teaching of science; he was at heart an investigator, and by untiring effort he was gradually successful in creating an atmosphere of research amongst his more advanced students. This was no mean task to undertake amidst the enervating influence of the climate and the many distractions of Indian life, but with this object he was at work both early and late, without regard to his personal convenience. The zeal for work of this character which he created is still in evidence at Dacca, and there can be no doubt that his influence has played no small part in the development of the research spirit which is now becoming prominent throughout India.

As a true teacher Watson sought to know his students and to learn their difficulties. He freely mixed with them, gained their confidence by means of his characteristically simple manner, and indeed encouraged those engaged upon research to visit at his home.

When appointed at the newly established Institute at Cawnpore, Watson at once devoted himself to its thorough organisation. This establishment, which was founded as an aid to the industries of India, not only undertakes the investigation of native products, but acts as a teaching centre for those post-graduate students who desire to take up technology as a career. Though here again Watson has left an excellent record of work, and his zeal for the welfare of the Institute was always clearly evident, it became apparent to his intimate friends that he was not so happy at Cawnpore as he had been at Dacca, and that he much regretted the cessation of his former opportunities for work in the domain of pure science.

Watson was keenly interested in the formation of the Indian Chemical Society, of which he became a Vice-President, and to him, without doubt, its inception was largely due. At the meeting of the Eleventh Indian Science Congress he was appointed President of its Chemical Section, and in his address pleaded for the better organisation of research work, the freer intercourse between manufacturer and pure chemist, and a more general discussion of chemical problems the solution of which was urgently required.

Watson's work at Cambridge, which was embodied in three papers, two carried out with Dr. S. Ruhemann, and the third independently, was concerned with the chemistry of the β-diketones (J., 1904, **85**, 456, 1170, 1319). His first investigation carried out in India (J., 1906, **89**, 578) was of a very different type and consisted of a study of the decomposition products of silver peroxynitrate under the influence of boiling water.

In 1912 Watson's attention was directed to the *Cydnus Indicus*, an insect which at Dacca is attracted by artificial light, and is notorious for its strong and unpleasant odour. This arises from the excretion by the insect of a volatile oily fluid, and Watson ascertained that it was due to the presence therein of *cyclo*heptanecarboxylic acid (of which $1\frac{1}{2}\%$ was present in the insect), though a trace of a second substance ($C_{11}H_{20}O_2$?) could also be isolated. The odour is so intense that the oil excreted by one insect, which averages 0·000005 g., is sufficient to render its presence apparent.

With the exception of these earlier researches Watson's scientific work deals almost entirely with the vexed question of the origin of colour in dyes, and the effect of structure on colour intensity, problems which fascinated him throughout his career. His many contributions on this subject, which are of first class importance, are too well known to require discussion here, and it is certain that his name will long be remembered in this connexion. In 1913, while in England on leave, he occupied himself at the Clothworkers' Research Laboratory, University of Leeds, with an investigation of flavonol colouring matters, which secured for him the Doctorate of Science in the University of London in 1914.

During 1916 to 1918, Watson was again on leave in England, and throughout this period he was engaged on investigations mainly of a technical character on behalf of the British Dyestuffs Ltd. at the University of Leeds. Whilst there, he gave a course of lectures on " Colour and Constitution " to works chemists, and these he employed later as the basis of his well-known monograph having that title (Longmans Green & Co., 1918). This volume, in which the various aspects of the subject are critically considered, indicates the author's complete mastery of his subject.

His work at Leeds, concerned as it was with processes of dye manufacture, was mainly of a confidential character, although later on he was enabled to communicate to the Society (J., 1921, **119**, 1211) the continuation of his earlier investigation on the preparation of dyes from phenanthrene (*J. Soc. Chem. Ind.*, 1915, **34**, 1136). Though his attempts to prepare phenanthrene analogues of the anthraquinone mordant and vat dyestuffs were unsuccessful, certain new azine derivatives of this hydrocarbon were obtained. His final papers (Mazumber and Watson, J., 1920, **117**, 830; Watson and Dutt, J., 1922, **121**, 1939, 2414), which were concerned with the " Constitution of Yellow Sulphide Dyes," and " Attempts to prepare Red Sulphide Dyes," give evidence of patient work and are of much interest.

Whilst Principal of the Cawnpore Institute, Watson was responsible for some twenty communications, of a technical character, to the *Journal of the Indian Chemical Society*, which are connected with the utilisation of Indian natural products. Amongst these may be cited a description of a satisfactory process for the refinement of the " Neem " oil derived from *Melia azadirachta*, which, though available in considerable amount, possesses an objectionable odour and an intensely bitter taste; methods for the preparation of T.N.T. from Assam and Burma petroleums; and trials indicating that Himalayan oak bark (*Quercus incana*) can be employed commercially as a source of tannin extract.

Watson was an ideal manipulator: he was precise and tidy at his work, and although somewhat deliberate in his methods was by no means slow in attaining his results. Of a modest and retiring disposition, possessing a somewhat nervous manner, and being absorbed in his work, he appeared at first sight somewhat difficult of approach, but this was in reality not the case, and to those who sought to know him he exhibited a delightful personality. Straightforward, and out-spoken when necessity arose, his sense of justice was extremely keen, and he was a rigid disciplinarian. In the circumstances of his life abroad, his intercourse with scientific equals was of infrequent occurrence, a disadvantage which he keenly felt, and although this at one time almost decided him to seek an appointment at home, on consideration it seemed unwise to abandon the advantages to which his long stay in India would shortly entitle him. In view of the parlous condition of chemical research in India when he arrived there in 1904, it is remarkable that he was able to accomplish what he did, no mean achievement indeed had he possessed the advantages of a similar position at an English University.

Watson's life in India was simple in the extreme, his main recreation being painting in water colours. Of this he was an excellent exponent, and he exhibited, with success, at Simla, Nana Tal, and other fine-art exhibitions.

During the last two years of his stay at Cawnpore, Watson became subject to fits of great depression, and eventually, as the result of a nervous breakdown, he obtained leave of absence from India in April 1926. Failing to obtain relief by a visit to Switzerland, he returned to London, where his sudden death occurred. He had hoped to investigate further laccaic acid, the colouring matter of lac dye, and for this purpose had arranged for laboratory accommodation at the Imperial College of Science. A letter on this subject to the author of this notice was one of the last, if not the last, he wrote.

Watson married Lily Mary Hurlston of Leamington Spa in October 1908, and had two children who died in infancy.

A. G. PERKIN.

HORACE BROWN.

1848—1925.

Horace Brown was born at Burton-on-Trent, July 20th, 1848. He died February 6th, 1925. He was elected a Fellow of the Chemical Society in 1871. He first served on the Council 1883—7; then as Vice-President 1894—7, 1904—7, 1913; as Treasurer 1903—4; as Foreign Secretary 1907—13. For reasons of health, he was unable to accept the Presidency. He was awarded the Longstaff Medal in 1894.

I have told the history of the Brown family and not a little of Horace's in my Adrian Brown Memorial Lecture to the Institute of Brewing (1921). He himself gave to this Institute a very full account of his career, in his "Reminiscences of Fifty Years Experiences of the Application of Scientific Practice to Brewing Practice (1918)."

He attended the local Grammar School from the age of nine years onwards, exchanging this for Atherstone Grammar School during 1864. He left school when 15½ years of age. He was well drilled in Latin and Mathematics and even learnt some Greek; years afterwards he polished up his Latin for the London Matriculation, with the result that, in his declining days, he again read Virgil with interest.

Through his classical training at school—and home practice I believe—he learnt to have regard for words and to express himself clearly, with simple elegance: it was never in his nature to be either careless or florid. His real education was out of school. The father, long manager of the Burton Bank, was a man of unusual ability and a naturalist (coleopterist) of repute; he was also both botanist and geologist. He had a large circle of scientific friends, some of whom occasionally came to stay at the bank. The boy was thus early brought into contact with the scientific mind and grew up in an ordered, intellectual atmosphere. In their walks abroad with the father, the boys (Horace and Adrian) became trained field workers and close observers. Later on, Adrian developed more as botanist and fisherman, whilst Horace became more specially interested in geology. An elder sister was a good pianist and Horace had musical proclivities, which she helped him to develop. He was a fair 'cello player and a great lover of Beethoven but in music was never a modernist and so never acquired Wagner.

When about twelve, of his own accord, he began to pay serious attention to astronomy. Next he toyed with electricity. On coming into possession of a microscope, as a gift from his father, he soon became an ardent systematic student of natural objects, especially of pond life. At fifteen, he was fascinated by Tyndall's "Heat as a Mode of Motion" and also began to take up chemistry experimentally; he seems to have soon found his métier here and to have devoted all his school leisure to the study. In the course of a couple of years, he worked through the qualitative Fresenius in the laboratory he improvised in a storeroom. At this time, he learnt to know Peter Griess, who undoubtedly had great influence in forming the bent of his boyish mind, helping him as he did to make his work systematic.

We met, at the Royal College of Chemistry, in the spring of 1865, just before Hofmann left for Germany. We became friends at once and remained so, without ripple of disturbance, up to his death in 1925. On entry he was far better prepared than are most students at the end of their first year—and through his own efforts. I well remember how we romped through the course. We were in the first bunch of Frankland's students and were used by him as immediate tests of the system which he put forward shortly afterwards in his "Lecture Notes." We became past masters in the art of constructing "imaginary" constitutional formulæ. Consequently, structural chemistry grew into a habit with me and I am satisfied that the opportunity put me years ahead in my outlook; the task did not make the same appeal to Brown: he was never much interested in "futures."

Brown's career at the College was a short one, as at the end of the year, when barely 17½ years old, he accepted an appointment as Junior Brewer in Worthington and Co.'s brewery, next door to his home. We do well, in these pampered days of educational spoon-feeding and examinations, when boys are kept at school up to nineteen years of age and deprived of all opportunity of developing individuality and learning the ways of the world, to reflect on this fact: to realise that the man who ultimately took the Copley Medal had less than six months' systematic academic training in chemistry or, for that matter, in any other branch of science. Fortunately, he was not taught but learnt nearly everything and by his own unaided efforts, the "urge" to do something being upon him. Probably, his success was not so much due to great intellectual power as to his unwearied, determined habits of study, to his breadth of outlook and the sanity of his judgment. In the widest sense, he was a born naturalist. Finally, he became the scientific amateur of high distinction—if not the only representative Chemistry has had in this remarkable English species, certainly to be placed in the front rank.

As Junior Brewer, his working time was twelve hours a day.

There was no regular Saturday half-holiday and he was expected to watch things on Sunday. None the less, he continued his private scientific studies—first in an old glass-house, in his father's garden, adjoining the brewery; then in a small laboratory which his father built specially for him. He worked under Griess's direction and occupied himself in preparing and studying the base from nitrobenzonitrile. The only way in which he was *allowed* to use his scientific knowledge in the brewery was in determining original gravities of beers. At the close of the brewing season, in May 1868, he spent a few weeks in Frankland's laboratory, studying the combustion method of water analysis which Frankland and I had just introduced. On his return, he installed the necessary apparatus in a small room which received the courtesy title *Laboratory*. He then set to work to analyse the well waters used in his own and other breweries and also the Trent and the parish pumps. The verdict of "sewage" which he returned in many cases led to the cleaning up of the water supply of the district. During several years he both thought and talked "sewage mostly." Later on (about 1883), in connexion with this work, he was led for a time to take up geology seriously and eventually contributed a valuable memoir on the Permian rocks of the Leicestershire Coal Field to the Geological Society (1889).

Three or four years after entry into Worthington's, when he had mastered brewing practice, mindful of his youthful experience, he turned to the microscope. His immediate object was to discover the cause of "caskiness" in beer. He ultimately traced the trouble to the growth of the blue mould, *Penicillium glaucum*. He was thus led to study yeasts, more particularly as at this time (1870) he became acquainted with Pasteur's *Études sur le Vin*; he at once saw the application to beer of the principles laid down by Pasteur for wine. He had already developed the method of ascertaining the "fate" of beers in advance by "forcing," prior to the appearance (1876) of Pasteur's second great work *Études sur la Bière*, which was to revolutionise brewing practice.

Such was the progress he made, that in 1873, at the age of 25, he became Manager of the whole of the manufacturing department of Worthington's Brewery, a post which he occupied until 1889, when he became a Managing Director, on incorporation of the firm as a limited liability company. He retired in 1893 and left Burton for London, where he engaged in scientific work up to the year 1917. He married in 1874.

A few months after Brown became chemist to Worthington's, his former teacher at the Royal College of Chemistry, Cornelius O'Sullivan, who had accompanied Hofmann to Berlin to act as his lecture assistant, was appointed chemist to Bass & Co. As is well known, one of his first acts was to study the hydrolysis of starch under the influence of malt diastase. He rediscovered maltose, which chemists had forgotten. He also showed that starch was resolved into maltose and a dextrin, in proportions which varied according as the diastase had been more or less heated. His results were a great revelation to the brewing industry. A few years after Brown became Manager, he secured an assistant, John Heron, with whom, about 1877, he began a revision of O'Sullivan's work. The inquiry was continued during many years (until 1899), in conjunction with Morris, Millar and others. The results, although not identical with O'Sullivan's, were of the same order. Various other workers have met with no better fate—starch is still an unsolved riddle. To-day, we know little more in principle than O'Sullivan taught us—though there is reason to think that we may be near to a correct interpretation. Probably, several enzymes are concerned in the malting process; we are assured also that "starch" is a complex material. That so much energy should have been spent and wasted in solving the starch problem is little short of deplorable—and but proof that we are far from having systematised our methods of attack. Brown probably ought to have known better than to undertake such a task : had he known more chemistry, he would scarcely have attempted it, at least until he had laid simpler and solid foundations.

About 1887, with an effort, he cut himself adrift from geological work, in order to take up the study of the malting process, then in an empirical state. Discovering how ill-equipped he was for the inquiry, he first set to work to give himself a solid grounding in physiological botany generally. This he obtained, he tells us, in a few years time, by hard work during his leisure hours. The final result was the memoir presented to this Society in 1890, by Morris and himself, *Researches on the Germination of some of the Gramineæ*. This, from the beginning, took rank as a classic and his work placed the malting industry upon a sound scientific basis.

His genius was even more fully displayed in the inquiry he then undertook, which he brought under the notice of the Society (in conjunction with Morris) in 1893, in the memoir *A Contribution to the Chemistry and Physiology of Foliage Leaves*. This again took rank as a classic. One of the most striking conclusions arrived at was, that the first visible product of assimilation in plants, under the influence of chlorophyll, was not starch but cane-sugar.

After his retirement from Burton, he continued the study of the assimilation process, particularly the absorption of atmospheric carbon dioxide by green leaves. Much of the work was done at

Kew, in the Jodrell laboratory, in conjunction with F. Escombe. They were able to show, for the first time, how it was possible to account for the sufficiently rapid ingress of carbon dioxide from the atmosphere to supply the leaf on the assumption that interchange took place only through the stomatal openings. Here again he laid solid foundations and captured the sympathy of all vegetable physiologists. The work was followed by a series of studies of the fundamental phenomena of gaseous and liquid diffusion.

These brief references may suffice to indicate the character and range of his activity but are no measure of its intensity. A full discussion of his work will be given in my Horace Brown Lecture to the Institute of Brewing.

As a man, he attracted all with whom he was in contact by his appearance and engaging courtesy of manner. In person strikingly handsome, in his later years especially, his face stamped with intellectuality, gifted with a most alluring persuasive voice and mode of address, in outlook a man of high refinement but great simplicity, appreciative of all forms of culture, he yet displayed no special leanings towards artistry other than his love of music : his tendency, indeed, was rather matter of fact. Without angles, he was never assertive, although very firm in his opinions. Although an engaging conversationalist, he was always peculiarly reticent in speaking of himself and his own doings, even secretive. Almost entirely self-trained in science, he had an extraordinary faculty for getting up a subject and a high sense of values. Whatever he did, he did thoroughly. Although he had no great depth of knowledge, he had great feeling, especially for vegetable physiology. Probably this is why—apart from his work on starch—he was so successful in selecting and solving fundamental problems.

In 1916, at the meeting of the Institute of Brewing to which he communicated his Reminiscences, in proposing the vote of thanks, I spoke of him as " An original inquirer, to the manner born : the methods he used," I said, " were the reflexion of his own personality, of exceptional finish, penetrant and always philosophical. . . . He had always shown an instinctive sympathy with vital phenomena, a penetrative appreciation of the physiological significance of chemical and physical phenomena such as is very rarely met with in the chemist."

In fact, he had the universal mind and could not be better described than he is, by implication, in the following passage, in a recent work on literary criticism, by Mr. Herbert Read :—

" It has been a common saying, since Pope first said it, that a little knowledge is a dangerous thing but far more dangerous is the knowledge which, though not little, is limited. It is idle to think that any good can come of a specialisation that is not linked to some wider ethos or that is not subordinate to general wisdom and this applies not only to the scientist whom we regard, rather rashly, as the only specialist but equally to the critic and the poet. It may be said, that the rare occurrence of a universal mind, as in Aristotle, Leibniz or Goethe, is the definite result of a *lusus naturæ*, of chance, in fact . . . universality is a quality possessed by all the rarer spirits of any age. . . . It does not mean the possession of all knowledge or even necessarily of any knowledge at all. It does imply a capacity to receive all knowledge and events with equanimity and unprejudiced percipience and to build up a positive attitude on this clear and secure perceptual basis."

HENRY E. ARMSTRONG.

SIR JAMES DEWAR.

1842—1923.

JAMES DEWAR was born 20th September, 1842. He died 27th March, 1923. Elected a Fellow of the Chemical Society, December 1st, 1870, he first served upon the Council 1874—77. He was Vice-President, 1880—83, 1886—88, 1896—97, 1899—1921. He was President, 1897—99.

The writer has attempted to picture the man—an impossible task—in a Friday evening Lecture to the Members of the Royal Institution, delivered on 18th January, 1924.* He had previously summarised his work at low temperatures and his work generally, as Fullerian Professor, up to 1917, in two essays, one on *The Charcoal Vacuum Septenate* (1909), the other on *The High Vacuum Septenate* (1917), both published in the Proceedings of the Royal Institution. During his lifetime, the spectroscopic work he did with his colleague, Prof. Liveing, was published in a volume of their Collected Papers (1915). The papers describing his chemical and physical work have been collected and published in two large volumes, like that on spectroscopy, printed at the Cambridge University Press (1927).

Dewar was educated at the Dollar Academy (1855—58), where he was distinguished in Mathematics. He entered as a first year Arts Student at Edinburgh University in 1859 and was on the register during the two following years. He first came under the influence of David Forbes, the distinguished physicist, also a geologist of repute. When Forbes left Edinburgh to become Principal of St. Andrews, he gave Dewar the chemical case he carried with him

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* Published separately, in advance of the Proceedings of the Institution for the year, by Ernest Benn, Ltd., London.

in his European travels. When he left Edinburgh for Cambridge, Dewar gave it to his nephew, Dr. T. W. Dewar, who has presented it (1923) to the Royal Society of Edinburgh. Dewar was a distinguished student in Tait's class.

When Lyon Playfair, the Professor of Chemistry, was able to take on an assistant, he engaged Dewar. On Playfair's retirement, in 1868, Dewar became assistant to Crum Brown; he taught the Medical laboratory class. In 1869, he was appointed Lecturer and later Professor of Chemistry in the Royal Veterinary College. In 1873, he also became assistant chemist to the Highland and Agricultural Society of Scotland. In 1875, he was appointed Jacksonian Professor in Cambridge. In 1877, he became Fullerian Professor of Chemistry in the Royal Institution. He held both chairs up to his death.

He began to publish in 1867. In Edinburgh, his work was mainly in organic chemistry. In Cambridge, he devoted himself particularly to spectroscopic inquiry but also did fine chemical work. In London, high temperature and spectroscopic studies first engaged his attention. From 1884 onwards, he devoted himself to the development of methods of reducing the difficultly coercible gases, nitrogen, oxygen and hydrogen to the liquid and solid states and to the study of low temperature phenomena in general. He all but succeeded in liquefying helium but was beaten in the race by the superior resources of the Onnes laboratory. In his last years, he found solace in the soap film. Characteristic of all his work was the elegance and simplicity of his methods. More particularly by the introduction of the vacuum flask, coated internally with silver to reduce radiation losses (*the Dewar Flask*), he made possible the use of liquid air and liquefied gases generally as refrigerating agents. To him, in fact, is due the introduction of liquefied gases as weapons of research at very low temperatures. Later, by his discovery of the marvellous absorbing power of properly prepared charcoal at low temperatures, he greatly improved the methods of producing very low vacua. The immensity of the service he thus rendered, both to philosophical inquiry and to industrial progress, although admitted, is not sufficiently recognised—especially in Germany, where his inventions were quietly appropriated with little if any acknowledgment.

The student of the Dewar Papers has a rich store before him. He will find something of interest and of value upon almost every page, if not in almost every paragraph, if he have imagination and reflect upon the lessons that may be read upon and between the lines. We are carried back to the days when the flood-tide of organic chemistry was but beginning to rise: the curtain goes up shortly after Kekulé had propounded his views upon the constitution of benzene (1865), with Crum Brown and Kekulé as prompters. Thus, in the opening lines of the first chapter of the first book, attention is directed to the importance of *oxidation*, as a means of determining structure, in the following terms :—

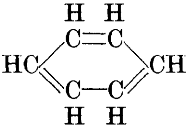
" The syntheses and oxidation analyses of organic compounds have so confirmed each other in many cases, that chemists are enabled to judge of the structure of a compound from the oxidation products. Many chemists have used the oxidation method in special cases, the bodies operated upon belonging principally to the fatty series ; but until Fittig and Beilstein published their memoirs on the aromatic compounds, it was never applied to the systematic study of a hydrocarbon and its derivatives. But although the syntheses and oxidation analyses of the derivatives of benzol confirmed each other, still the structure of the original nucleus (benzol) remained unexplained. Kekulé's original and elegant speculations on the structure of benzol and its derivatives induced me to try the effect of oxidising agents on benzol, with the view of eliciting whether the carbon atoms would separate in the way theory pointed out. The carbon atoms in benzol may be supposed to be arranged in a closed chain, where the carbon affinities are bound two and one alternately. Now, if we examine the formula graphically, it is evident there are three symmetrical groupings, C_2H_2 (acetylene), in benzol. We would, therefore, expect the carbons to separate in twos and produce the corresponding oxidised product, $C_2H_2O_4$, oxalic acid."

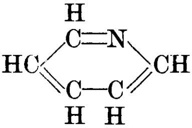
Failing to oxidise benzene with permanganate, Dewar took phenol. He obtained oxalic acid and thought that possibly either fumaric or mellitic acid (!) was formed. He ends with the remark—" The application of permanganate of potash as an oxidising agent in organic chemistry promises interesting results." The special feature of interest in the paper, however, is the description that is given of " a simple mechanical arrangement adapted to illustrate structure in the non-saturated hydrocarbons. This little device is the mechanical representative of Dr. C. Brown's well-known graphic notation." An elaborate diagram is given showing 7 different ways of associating 6 carbon atoms, among them being the well-known arrangement often referred to as Dewar's benzene formula, although he in no way specially calls attention to it. In the closing words : " Naphthalene and many other hydrocarbons give elegant symmetrical diagrams when represented as a closed circuit," we have a clear forecast of the utility of Kekulé's conception.

In the summer of the year (1867) in which he published this, his first paper, Dewar spent some time in Kekulé's laboratory at

Ghent. Under Kekulé's direction, he there applied permanganate to the oxidation of coal tar bases, particularly picoline. The work was not completed and published until 1870. The paper contains the following noteworthy passage.

"Pyridine may be written graphically as benzol in which nitrogen functions in place of the triatomic residue CH''' and thus may be represented as a closed chain.





And, considering the stability and mode of formation of these bases, it is not at all improbable that they may be produced by the simultaneous action of acetylene and its derivatives on hydrocyanic acid; thus as three molecules of acetylene condense and form benzol, so may two molecules of acetylene and one of hydrocyanic acid condense and produce pyridine."

Someone has yet to discover means of effecting both these syntheses economically.

In Ghent, Dewar became very intimate with Kekulé's assistant Körner, to whom far more than to Kekulé we owe the early development of the position chemistry of benzene—to which Kekulé himself attached no particular importance, in no way foreseeing its great applications. The two men, both in the flush of youth, we know, were a wild pair—thoroughly congenial spirits. The association, short as it was, with such a master-worker was of the greatest value to Dewar; he preserved his affection for Körner to the last. We often talked of him together. Whilst Kekulé was the man of affairs, as one so strikingly handsome and eloquent was bound to be, a philosopher and dreamer, Körner was reserved and a laboratory worker but of incomparable skill. He gloried in preparing fine crystals. He it was who first saw the practical potentialities of the Kekulé hypothesis. Using the mere sketch Kekulé, as architect, had provided, he quietly put together a finished building of considerable size and suddenly unveiled this, in all its perfection, to our astonished gaze, choosing the language of his adopted country, Italy, for his description of the work. When Henry Watts asked me to abstract the memoir for him, I did not know a word of Italian. On turning over the pages, I was so attracted that, with dictionary and grammar bought for the purpose, I soon puzzled out the wonderful story. Such abstracting was indeed joy—I have never engaged in a more fascinating exercise. Why do not more young chemists learn to take an interest in languages in this way? Is reading a lost art—is Liebig's well-known advice to Kekulé altogether forgotten? Dewar was privy to Körner's work and ever worshipped his manipulative skill. The experience made him an organic chemist—although he soon ceased from regular active participation in organic inquiry. Seeing an army of locusts coming down upon the field, ever impatient of pot-boiling and uniformity, he gradually extended his operations into then almost unpeopled regions. He had most wonderful fingers and a sufficiently mathematical mind, full of physical lore, to find that ordinary laboratory work did not afford the variety and kind of occupation his soul desired. The vast potentialities of structural chemistry were then scarcely apparent. He had the foresight, which all but the few lack, to realise that he must train himself to work in any field.

In 1868, Dewar exhibited at the B.A. meeting "a model devised by Professor Kekulé to illustrate the structure of organic compounds. He explained the peculiarity of the model consisted in carbon, with its four atomicities, being represented by a small sphere, with four equal wire arms joining the centre of the sphere with the angle of an imaginary equilateral tetrahedron." *

We have here an independent record of Kekulé's unwitting anticipation of his pupil van 't Hoff's happy use of the tetrahedron as the model of the carbon atom—a conception now established in the realm of fact by Sir William Bragg's inspired study of the diamond.

Latent in Dewar was the spirit of physical inquiry. Not only had he wonderful fingers, he was an insatiate experimenter, one who loved just to try things, to see what would happen; moreover, he was in contact with the physicist Tait. Chemico-physical inquiry gave him opportunities which organic chemistry did not seem to offer. So it comes that, among his early contributions, some are semi-physical. In his lectures at the Royal Institution, he never failed to grip his audience, not by his eloquence nor by the clearness of his expositions—actually he was difficult to follow—but by the extraordinary display he made of experimental illustrations which fascinated by their elegance and originality. Faraday took pains but he handled simpler themes. Dewar took more than pains. The stock illustrations made no appeal to him but he developed lecture-demonstration to a fine art. In the course of sixty years I have listened to all the great lecturers on Friday evenings—others may have told their story more clearly, more convincingly; no one has ever approached him in the calculated perfection of his illustrations. He regarded the lecture as an artistic performance, to be staged with absolute forethought and utmost care, provided with original scenery and all necessary appointments, without count

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* Compare Japp: Kekulé Memorial Lecture.

of cost. He had the same ambition in the R.I. lectures that his friend Irving, the great actor, had on the Lyceum stage.

The accounts on record give but the faintest idea of the performances we witnessed. Only those who have seen the two men on their respective stages have any clear idea of the way in which their actions were adjusted to and supported by their surroundings. Neither was the mere professor: both men were artists through and through.

Dewar's artistry is already apparent in the fifth of his recorded compositions—that on the motion of a palladium plate during the formation of Graham's hydrogenium. Graham was then Master of the Mint—my fellow-student, Roberts Austen, was the assistant who carried out the hydrogenium experiments for him. Dewar, through Playfair, had known Graham in Glasgow and, of course, was attracted by his work. " During the course of an experimental exhibition of Graham's discovery, I noted several phenomena associated with the occlusion of hydrogen by palladium, when it is made the negative electrode during the electrolysis of water; and as they illustrate, in a new form, the results already arrived at by the Master of the Mint, with his permission, I am induced to bring them before the Society," are the words with which he explains the origin of his attention to the subject. The peculiar behaviour of palladium towards hydrogen is illustrated in the most perfect manner in the memoir.

From palladium we pass to chlorosulphonic acid (B.A. 1869, with Cranston). To me, this essay is specially interesting, as at the same time I was doing practically the same work at Leipzig, using sulphuric anhydride alone, whilst Dewar and Cranston used it in conjunction with hydrogen chloride. The object they had in view was to study the action of the sulphonic chloride, SO_3HCl, upon phenol, as a sulphonating agent. Finding that they had been anticipated by Engelhardt, they attempted to prepare Kolbe's $CCl_3{\cdot}SO_3H$ by sulphonating chloroform and instead obtained carbonyl chloride, hydrogen chloride and sulphur dioxide, the chloride acting as if it were sulphuric anhydride, as it often does.

They also found that when heated with carbon disulphide, it gave rise to carbonyl sulphide and sulphur dioxide. Commencing with the anhydride, SO_3, I had attempted to prepare sulphonic chlorides of the chlorinated methyls but instead obtained carbonyl chloride and $S_2O_5Cl_2$ from carbon tetrachloride and the same products as Dewar and Cranston from chloroform. I also prepared carbonyl sulphide from carbon disulphide and sulphuric anhydride. Dewar and Cranston remark " probably the reaction (with SO_3HCl) would succeed better by the action of SO_3."

At that time, not a few of us were all working in one direction. Engelhardt, as well as Dewar and Cranston, under the spell of Kekulé's hypothesis, were studying the formation of isomeric derivatives of benzene, I also had begun (1868), at Kolbe's suggestion, to work on phenolsulphonic acids. Desiring to have another string to my bow, after reading Robert Williamson's account of chlorosulphonic acid, being familiar with sulphonic anhydride, it occurred to me to attempt to prepare ethereal derivatives of the chloride from the chlorinated hydrocarbons. I began with ethyl chloride and was studying the compound $SO_3ClC_2H_5$ when a former student of Kolbe's, in the late summer of 1868, the Russian von Purgold, called on Kolbe, on his way home from Paris, with a paper in his pocket just published in the *Comptes rendus* on the same subject. So I turned to chloroform, carbon tetrachloride and disulphide.

An interesting note on the atomic volume of solid substances (1870) deserves notice here. It merely has to do with the alteration in atomic volume, supposedly of the oxygen, in various oxygen compounds. This is a problem which has often been discussed, hitherto with no satisfactory result. We may hope that with the aid of X-ray and crystallographic study, we shall, at no distant date, be able to arrive at more definite conclusions. Dewar specially directs attention to potassium chlorate, bromate and iodate. The volume of the oxygen in potassium chlorate, supposing the chloride to retain its volume, is 15, whereas it is only 7 in the bromate and 0 in the iodate, supposing the iodide to retain its original volume in the iodate. Supposing that oxygen be the dominant element—as it may well be—it is conceivable that bromine in the bromate and iodine in the iodate may be thought of as shrinking to the volume of the chlorine in the chloride—at least, as Barlow and Pope have suggested, that there may be a mutual adjustment, to allow of isomorphous relationship. In point of fact, whatever we assume, we know nothing of the structure of these substances—whether they be open or closed systems. In the age-long controversy on this subject, no definite evidence either way has been produced.

Dewar worked with his friend Gamgee the physiologist on cystine. He also carried out two physiological inquiries with his great friend McKendrick, another physiologist. The first of these, on the physiological action of light, I know was suggested by Dewar, after McKendrick had expressed the desire to work with him. The experiments were carried out mostly at night, as they involved the practice of vivisection. The inquiry seems to have led to a correspondence in the local press, re disappearance of cats from the district. Dewar's interest in the work lay on the physical side, I suspect. I never discovered that he had any special interest in biological inquiry. The research involved a large amount of very delicate and exact observation and is a brilliant inquiry. The second of their joint investigations was on the physiological action of the chinoline and pyridine bases.

During the period up to 1873, when he left Edinburgh for Cambridge, Dewar was chiefly occupied with organic problems, occasionally interspersed with physical. In these latter, however, he laid the foundations of two of his greatest achievements—the Dewar vacuum vessel and the use of charcoal as an absorbent. In Cambridge, he still dallied with organic chemistry but was soon attracted to spectroscopy. Lockyer had set us by the ears, by his outrageous statements on the subject of elementary decompositions at high temperatures. Dewar could not look on quietly under such circumstances and he entered the field against Lockyer. His immediate colleague was the late Prof. Liveing, a man held in the deepest respect at Cambridge and by all who were privileged to know him. He had been appointed to his Chair in almost primitive times, when chemistry was all but unknown as a subject of systematic study. In character, the two men stood at opposite poles—Dewar being fiery, impetuous, so brimming over with enthusiasm and feeling as to be almost uncontrollable; Liveing, calm, deliberate and highly judicial, never carried away by his feelings. That Liveing learnt to appreciate Dewar's great genius—I will even say to love him—and ever sought to balance his activity was proof of the clearness of his insight and of his magnanimity. He did what few elder men do. Recognising the ability of his young colleague, carried away I am sure by his pertinacious enthusiasm, he practically placed himself at his disposal as his assistant and developed an amazing ability and industry as a spectroscopist. Previously he had published nothing. From 1878 onwards, they produced a stream of memoirs now available in the volume of their collected papers. The work they did together at once took rank as trustworthy, at a time when much loose work was being done. Now that spectroscopy is no longer the empirical subject it was and spectra are subject to rational interpretation, it is to be hoped that the great mass of results they accumulated will prove to be of value.

As years went on, Dewar spent more and more of his time at the laboratory of the Royal Institution. Receiving no proper encouragement at Cambridge, once bitten with a love for low temperatures, he gradually surrounded himself in London with an engineering plant which to-day would rank as ridiculous in its simplicity. With it, however, he effected marvels. He did his work with the aid of a few chosen skilled assistants. In early days, he was greatly helped by the late Mr. Robert Lennox.

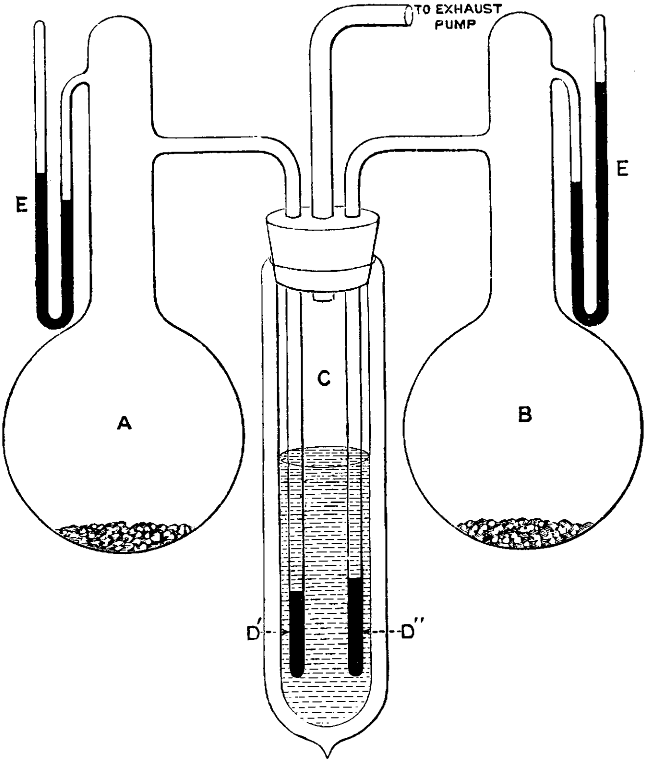
As Jacksonian Professor, he only lectured at Cambridge and had little occasion to deal with students. That he had an eye for special genius is clear, however, first in his selection of Liveing; then of Dr. Alexander Scott, to aid him in exact analytical inquiry; finally, of the late H. O. Jones, a chemist of unusual ability and great promise, who was associated with him in his work on nickel and iron carbonyls and in the study of carbonic sulphide, CS.

As a good example of the richness of the fare provided in the Collected Papers, the following is taken from his New Researches on Liquid Air :—(I. p. 458):

"*Samples of Air Liquefied in Sealed Flasks.* In a paper 'On the relative behaviour of chemically prepared and of atmospheric nitrogen,' communicated to the Chemical Society in December 1894, the plan of manipulating such samples was described. The arrangement shown in Fig. 4 illustrates how oxygen in A under 0·21 atmosphere pressure and nitrogen in B under 0·79 atmosphere can be compared as to the first appearance of liquefaction in each and finally as to their respective tensions when the temperature is as low as that of solid nitrogen. The flasks A and B have a capacity of more than a litre. Each has a manometer sealed on and in each phosphoric anhydride is inserted to secure dryness. A large vacuum vessel C holds the liquid air, which is gradually lowered in temperature by boiling under exhaustion. The moment liquefaction takes place, the tubes D′, D″ begin to show liquid. These tubes must be drawn fine at the end when accurate observations are being made. In the same manner two oxygen flasks were compared. One was filled with gas made from fused chlorate of potash, contained in a side tube sealed on to the flask. The other was treated in the same way, only the chlorate had a little peroxide of manganese added. The former gave perfectly clear blue liquid oxygen, the latter was turbid from solid chlorine. Two flasks of dry air that had stood over phosphoric anhydride were liquefied side by side, the only difference between the samples being that one was free from carbonic acid. The one gave a liquid that was perfectly clear, the other was turbid from the 0·04 per cent. of carbon dioxide.

The temperature was lowered by exhaustion until samples of liquid air from two flasks placed side by side as in Fig. 4 became solid. The flasks were then sealed off for the purpose of examining the composition of the air that had not been condensed. The one sample contained oxygen, 21·19 per cent. and the other 20·7 per cent. This is an additional proof to the one previously given that substantially, the oxygen and nitrogen in air liquefy simultaneously, even under gradually diminishing pressure and that in these experiments all the known constituents of air are condensed together. These results finally disprove the view expressed in "A System of Inorganic Chemistry" by Professor Ramsay where he says: "Air has been liquefied by cooling to −192° but as oxygen and nitrogen have not the same boiling point, the less volatile oxygen doubtless liquefies first." My old experiments showed that the substance now known as argon became solid before nitrogen but chemical nitrogen and air nitrogen, with its 0·1 per cent. of argon, behaved in substantially the same way on liquefaction.

Fig. 4.



Liquid Nitric Oxide.—Great interest attaches to the behaviour of nitric oxide at low temperatures. Professor Olszewski has examined the liquid and describes it as colourless. Samples of nitric oxide have been prepared in different ways. These have been transferred to liquefaction flasks, where they were left in contact with the anhydrous potash, sulphuric acid alone, a mixture of sulphate of aniline and sulphuric acid or phosphoric acid, for many days before use. Each of the samples, when cooled, gave a nearly white solid, melting into a *blue liquid*. The colour is more marked at the melting point than at the boiling point. Liquid nitric oxide is not magnetic; neither is the solid phosphorescent. Colour in the oxides of nitrogen evidently begins with the second oxide. Solid nitric oxide does not show any chemical action when placed in contact with liquid oxygen provided the tube containing it is completely immersed; but if the tube full of liquid oxygen is lifted into the air, almost instantly a violent explosion takes place."

In fact, the student of the Dewar papers will find them crowded with observations of historic and scientific value in an extraordinarily wide field of chemical and physical inquiry and nothing short of an original conspectus of the properties of matter at the very lowest temperatures, a mine of wealth awaiting exploitation. The lessons to be learnt from them are innumerable. A large part of the matter is put upon record, for the first time, in a way to make it available. In early years at the Royal Institution, Dewar was with difficulty persuaded to put his results upon paper. Much of his work was but briefly referred to in abstract accounts of his Royal Institution lectures. The accounts, therefore, are often not easy to understand and are obviously incomplete. Whatever the interpretation of the records, the two volumes make a most imposing show and probably it is not going too far to say, that no other single worker, except Faraday, has so varied and weighty a record of experimental achievement to his name.

In Sir James Dewar, as in Davy and Faraday, his predecessors at the Royal Institution, the value of genius, unhampered, unrestricted and in the main reliant upon its own efforts, stands displayed before the world in matchless perfection.

HENRY E. ARMSTRONG.