a compound isolated from the green bacterium *Chlorobium thiosulphatophilum* to + 340 mV. for a compound from *Rhodospirillum rubrum*. Duysens and Chance and Smith have shown by studies of difference spectra that a hæm pigment is relatively oxidized in light and reduced in dark in *Rhodospirillum rubrum*. Illumination of preparations of chromatophores isolated from the bacterium is accompanied by phosphorylation of adenosine phosphate, and it is probable that this is related to the oxidation of hæm pigment observed in light. This would indicate a close similarity in the photochemical activity of chloroplasts isolated from the green plant and chromatophores isolated from the photosynthetic bacteria.

The elucidation of the reactions involving carbon dioxide using isotopes and the comparative studies of photosynthesis in bacteria and green plants emphasize that the basic biochemical pattern is essentially similar to that of the dark metabolism of other organisms, both plant and animal. The unique feature of photosynthesis lies in the mechanism whereby light energy is used to drive the reaction sequence in the direction of net synthesis. An understanding of the mechanism of energy conversion is the major challenge for future research in photosynthesis and may contribute to the technology of energy conversion just as the study of the over-all process of photosynthesis has to the technology of food production.

C. P. WHITTINGHAM

A TRIBUTE TO FREDERICK SODDY*

By PROF. F. A. PANETH, F.R.S.

Director at the Max-Planck-Institut für Chemie, Mainz

THE first International Conference on Radioisotopes in Scientific Research seems an appropriate occasion upon which to consider our debt to Frederick Soddy. The duty to clarify his picture is specially incumbent on us, as it is the tragedy of his life that members of the younger generation may know him only as the person who adopted the term 'isotope' and, perhaps, as the author of provocative statements in economics and other fields far remote from science. The number of those who knew Soddy in his creative period is dwindling.

In fact, the whole science of radioactivity owes to Soddy infinitely more than the coining of the household word which is the unifying tie of this Conference.

First, I would like to recall that the fundamental theory of radioactive disintegration was developed by Rutherford and Soddy. From experiments carried out during 1901–3 at the University of Montreal by the physicist Rutherford and the chemist Soddy, they drew the conclusion that the emission of the Becquerel-rays (as they were called in those days) was accompanied by chemical transmutations ; in a vague way such a hypothesis had been considered before as one of several possibilities, but the French school in particular had favoured other explanations. Anybody who studies the original papers will easily recognize the decisive part the chemist must have played in this joint work ; some of the arrangements are, from the point of view of experimental chemistry, models of a scientific investigation. Nevertheless, in later books Soddy's name sometimes no longer appears in this context : to Rutherford is attributed the sole merit—illustrating the old truth, well known to students of the history of science, that great reputations tend to absorb the smaller ones—although Rutherford himself always gave full credit to his colleague.

Their ways, however, soon parted. Soddy's name immediately appeared again in connexion with an experiment of the greatest significance : the proof that, in the process of radioactive disintegration by α-rays, helium is evolved in sufficient quantities to be identified spectroscopically. This time the team was Ramsay and Soddy : the study of such small quantities of rare gases is so difficult that in those days Ramsay's laboratory in London was the only place where the necessary experience was available, but the radioactive technique—then quite new to Ramsay—was clearly Soddy's contribution. Anybody who knows from Ramsay's later publications on radioactive transmutation, after Soddy had left, how incapable he was of taking the precautions necessary in this field will credit Soddy with the clean and convincing conduct of experiments in which he later proved himself so much more adept than Ramsay.

So far, Soddy's name had been linked with those of senior and already very famous investigators. He came fully into his own during the ten-year tenure of a lectureship in physical chemistry at the University of Glasgow, where he devoted himself to radiochemistry proper, that is, to the study of the chemical behaviour of the radioactive substances. Such work had been going on in many laboratories, but the published results were frequently erroneous and no attempt had been made at a comprehensive survey. By far the clearest presentation of the new subject was given by Soddy in 1911, in a small volume called "The Chemistry of the Radioelements". Here, for the first time, the phenomenon of the chemical inseparability of those substances which to-day we call isotopic was stressed as something fundamentally new and important. More than that, a connexion was indicated between the position that a radioelement occupies in the disintegration series and in the periodic table ; the first part of the important 'radioactive displacement law' is already contained in this book, namely, the rule that emission of an α-particle means shifting down by two places in the periodic table. The second part of the law, namely, that emission of a β-particle leads to the production of an element one place higher in the table, was pronounced three years later, almost simultaneously, by Fajans and Soddy ; the experimental basis for this statement had in the meantime been safely laid, largely through the systematic efforts of Soddy's laboratory. In the course of various researches the inseparability of some radioelements from other elements had independently been found by Svedberg, by Boltwood, by Hahn and others, but it is Soddy's outstanding merit to have undertaken a special

* Address delivered in Paris on September 9 at the inaugural session of the International Conference on Radioisotopes in Scientific Research.

investigation in order to clarify this mysterious phenomenon ; here he was ably helped by a Scottish student with the name of A. Fleck, now Sir Alexander Fleck, chairman of Imperial Chemical Industries, Ltd. Their well-planned analytical work contributed decisively to the recognition of the validity of the displacement law and thereby to the understanding of the physical cause of isotopy. It was fitting that Soddy should christen this phenomenon, and that, in 1921, he should be awarded a Nobel Prize.

After these brilliant successes in his scientific efforts—I have not time to mention other excellent papers by Soddy—everybody would have expected him to become the leader of a strong British school of radiochemistry, in a position perhaps similar to that of Rutherford on the side of physics. Soddy's books ("The Interpretation of Radium" and others) were eagerly read ; they showed didactic and literary qualities of the highest order and seemed to add to his qualifications for a successful teaching career. But nothing of this sort happened. True, after a few years at Aberdeen, Soddy was called to Oxford as professor of inorganic and physical chemistry, but little scientific work emerged from his laboratory there, no school of radiochemistry was formed, and in 1936 he gave up his chair and retired into private life. How is this surprising eclipse of Soddy as a scientific worker to be explained ? To help us understand it, something must be said about his character.

Science was never the only concern in Soddy's life. He had a deep interest in social questions, and whenever he suspected that injustice was being done to a certain group, or that something was wrong in the social structure, he fought it in the most uncompromising way. I had an opportunity to see this side of his nature as early as 1913 when I had the privilege of learning the then current methods of gas microanalysis under his guidance in Glasgow. Scottish Sundays would have left me rather lonely if Soddy had not introduced me to the hospitable house of his parents-in-law, Dr. and Mrs. Beilby. Incidentally, I read recently in the *Proceedings of the Chemical Society* that on that house a tablet is to be fixed, recalling that there, about 1912, the name 'isotope' was invented. This date is certainly too early ; in the many discussions to which I had the pleasure of listening in the first half of 1913, this word was never used. According to Soddy's own story, it was a little later supplied to him by a school teacher, a lady to whom he had explained that the substances to be named occupied "the same place" in the periodic table. (The term made its first public appearance in Soddy's letter to *Nature* of December 4, 1913, p. 399.) It is very unfortunate that in recent years this clear scientific meaning has been obscured by the indiscriminate use of the word 'isotope' for any radioactive substance. If no relation to another substance occupying the same place is involved, the designation 'radioelement' or 'radionuclide' ought to be used. One substance in isolation can no more claim to be an isotope than an only child can be a brother.

I remember very clearly having heard Soddy's heated arguments, in Dr. Beilby's house at 11 University Gardens, in favour of home rule for Ireland and votes for women, the two main political topics of those days. Later he took a deep interest in political economy ; to the theory of money he devoted several publications in which he expressed very radical views. Objections from acknowledged experts in this field he used to brush aside with the remark that chemical experts did not accept his views on isotopes either when he first pronounced them. There was, however, the difference that in chemistry he could present such convincing facts that resistance was overcome within a few years. In economics he met with no such success, and he became more and more embittered about this failure.

Nearer home, he tried to introduce reforms into the structure of the University of Oxford ; he considered some aspects of the college system, as, for example, the independent teaching of physical chemistry in several small and poorly equipped college laboratories, as outmoded. He was a declared admirer of some features of the German university system. However, he was not good at waiting or satisfied with slow progress, but was always inclined to suspect unpardonable indolence or even ill-will. After he had left Oxford some developments went in the direction desired by him, just as votes were given to women and independence to Ireland. But Soddy did not wait. Never satisfied by a compromise, he manœuvred himself more and more into the position of an opponent and critic without influence ; finally he resigned, and Britain lost the hope many had certainly cherished that this great man and his pupils would establish a permanent school of radiochemistry.

After his retirement, Soddy had little contact with colleagues and followed the further development of the science he had helped so much to launch with growing distrust. He resented especially the dominant role physics assumed in radioactive research. Apart from the general trend—physics has invaded other branches of chemistry as well—it was Soddy's own premature retirement which accelerated this shifting of the centre of gravity. He had been the only man in Great Britain who could to a certain extent have kept the balance. Instead he was, in his later years, inclined to believe in dark scheming on the part of physicists who did not want to acknowledge the merits of chemists. But anybody who ever had the opportunity of knowing the generous character of Rutherford and his readiness to admire and enjoy good scientific work, wherever it was done, will be quite unable to accept Soddy's occasional bitter remarks. It is most regrettable that those unfounded grievances of an old and disgruntled Soddy have found their way into recent books.

The reasons for Soddy's tragic scientific isolation in later years are to be found in his own personality. He was gifted in many, perhaps too many, ways. He was such a good writer of English prose that it was all too easy for him to give his polemical essays the sting he wished. Once, when he had solved a problem of stereometry, it amused him to publish it in verse. An occasional excursion into the history of chemistry instigated him to the best vindication known to me of the strong claims of William Higgins, as opposed to those of John Dalton, to be the originator of the chemical atomic theory. His special gift for technical constructions made him for long periods exchange the laboratory for the workshop. There are many other examples which could be quoted to illustrate the richness and variety of his talents.

Rutherford once said in a lecture that he was naturally in favour of simple explanations in physics, being a simple person himself. Now, Frederick Soddy was not a simple person. He was a very complex personality, not easy to approach, living a rather solitary life—especially after the early death of his charming and devoted wife—but he was a great idealist, always ready to fight against what he deemed morally wrong, without any consideration of his own personal interests. Very kind and helpful to young and struggling people, he was suspicious of nearly everyone in authority and power. If he had lived, on September 2, 1957, he would have completed his eightieth year. It is fitting, at a conference to discuss the scientific applications of isotopes, to remember the man who was the first to see the general theoretical importance of what had been until then scattered observations. Let us honour the memory of a brilliant intellect, an experimenter second to none among the founders of radiochemistry, and an uncompromising champion of his ideals.

OPERATIONAL RESEARCH

IT is just about twenty years since the expression 'operational research' was coined to describe the activities of the Air Ministry research section studying the operational problems of radar. There was rapid progress during the first decade, mainly—and quite naturally—in military applications. The three British Armed Services all had groups in England, and there were many overseas sections. In the United States and Canada development was also rapid, and a tripartite operational research conference on defence matters was held shortly after the War; similar conferences have since been held at regular intervals, and have been extended in the military sphere to include nations of the British Commonwealth and the North Atlantic Treaty Organization.

Although work of the type now termed operational research has been carried out in industry for the best part of a hundred years, it is only since 1947 that there has been any formal description of this science. An Operational Research Society was founded in the United Kingdom in 1947—initially as a club with members drawn from personnel of the various military groups who had returned to civilian life. An American Society was formed in 1953 and a year or so later came the (American) Institute of Management Science. Societies or groups have since been formed in most of the major Western European countries, and also in India and Japan.

With this widespread interest, it is understandable that suggestions for an international meeting this year met with a ready response. Indeed, this highly successful conference (sponsored by the Operational Research Societies of Britain and America and the Institute of Management Science) for experienced workers in both the military and civil fields was over-subscribed from the start. The Conference was held at Oxford during September 2–6, and the two hundred and fifty members present represented twenty-one different nations, and it was felt that similar conferences should now be held at regular intervals.

At the inaugural meeting delegates were welcomed by Sir William Slater (president of the Operational Research Society). The eminent speakers who followed him avoided any attempt at formal definition of operational research, but spoke with great authority on its basic principles and growing importance in industry, in defence and in the economy of nations. The first four business sessions were devoted to the presentation of papers on methodology and applications, and the final session to development (that is, to the extent of growth of operational research) in the main countries represented. During the week, one session was spent in panel meetings, at which were discussed special aspects of operational research, for example, education and training, or its relation to management. There were a formal dinner and reception on the first night and also informal meetings during the week; these latter were prosecuted so vigorously that it is doubtful if any delegate got his or her full ration of sleep during the conference. After leaving Oxford, parties visited various British operational research centres. The proceedings of the conference are to be published at the end of the year and can be obtained from the *Operational Research Quarterly* (11 Park Lane, London, W.1).

The largest delegations were from Britain and the United States, and these accounted for twenty-four of the twenty-eight main papers. The conference brought out differences in the approach of these countries to operational research. Judging by the papers presented, interest in the United States is greater in development of mathematical techniques (seven papers) than in applications (three papers), though the methodological papers were usually based on real situations involving decision-making. This mathematical interest seems to stem from universities and research organizations. In the United States much operational research is conducted on a consulting basis by university departments, and this no doubt stimulates academic research. In the United Kingdom, however, interest is in applications (six papers) rather than methods (three papers). It is lamentable that so few British universities show any real interest in operational research. There are many individual workers known by their publications, but some of these are not members of the Operational Research Society and so were not eligible to attend this conference.

If papers are to be judged both by reading and by the interest aroused, then there were outstanding contributions in each session. On the morning of September 3, Mr. H. K. Weiss, of Northrop Aircraft, Inc., presented a paper on "Lanchester-type Models of Warfare". F. W. Lanchester's differential equations are well known from his classical work, "Aircraft in Warfare: The Dawn of the Fourth Arm" (Constable, London, 1916). In Weiss's paper historical data were used to establish the reasonableness of the Lanchester assumptions and then to expand the theory to take into account the movement of forces. It then deals with small combat forces, weapons of greatest effectiveness, and combat between heterogeneous forces. This work all leads to development of optimum tactics between heterogeneous forces. In the discussion it was acknowledged that the simplifying assumptions and unknown values of the relative effectiveness of weapons (killing-rate) cause serious difficulties. It is doubtful, for example, whether the effectiveness of a force is equal to (numerical strength) × (weapon effectiveness), for this assumes that a man who can and should shoot will in fact do so. Another complication is over-hitting: a casualty may be due to more than one