experiments than from their field observations. These are reasonably well known to sewage chemists, but have had little or no impact on other specialists whose co-operation is necessary for an appreciation of the facts revealed.

Points pertinent to the involuntary demonstrations at Belfast and Dublin are: (1) given organic matter, there is no difficulty in principle in inducing bacteria to produce hydrogen sulphide from sea water; (2) the chief value of sewage in this connexion is as a source of nitrogen and phosphate; these conduce to growth of weed, which provides a large bulk of organic matter *in situ*; (3) seaweed can contribute inorganic and organic sulphates in proportions together much greater than the proportion of sulphate in sea water; it probably also contains reducible organic sulphur; (4) no special cultivation of bacteria or seaweed is necessary; both will arise 'spontaneously' as elective cultures if appropriate nutrients and conditions are provided.

To point out these facts is not to suggest that an addition of sewage or of seaweed to sea water kept anaerobic will lead to production of hydrogen sulphide as easily as by adding acid to an alkaline sulphide. Work by Irvine and Gibson¹⁰, for example, did suggest that to produce hydrogen sulphide from sea water to which a fragment of decomposing mussel flesh had been added was almost as easy as that, though a few days incubation was required. There is, however, much chemical difference between any seaweed and the other forms of organic matter used by workers in the field of sulphate-reduction. Unless the right conditions are offered for sulphate-reduction by seaweed, anaerobic digestion of weed may lead to a methane fermentation or other type in which hydrogen sulphide is not prominent.

Without wholly accepting the view of Letts and Adeney that decomposition of seaweed proceeds in two stages, it will be clear that the seaweed must undergo a preliminary microbial decomposition (of unknown nature and extent) before it can serve as source of organic matter for reduction of its own sulphates and those of sea water; other factors such as temperature, species of weed, and the ratio of seaweed to sea water are important, and their elucidation must require considerable laboratory work.

Neither the series of occurrences at Belfast and Dublin nor the laboratory work performed hitherto has done much to indicate optima for commercial production of sulphur from sea water. What the estuarial phenomena of c. 1900 did was to show that hydrogen sulphide can be released from the sea to any desired extent. These repeated large-scale demonstrations are very little known in spite of their now topical interest.

The engineering difficulties in collecting the evolved gas appear formidable; but there seems to be no relevant biological or chemical problem that cannot be tackled by present technique, and the production of sulphur from sea water by biological means may well be the easiest to solve of the future problems of supply of raw materials to industry.

The ecological chain is at least as interesting as the connexion alleged by Darwin to exist between spinsters and red clover. Sewage is not essential, and might be replaced by, say, nitro-superphosphate. Thus a roundabout route would be opened for inorganic nitrogen to be converted (so to speak) into protein for human consumption, via sulphur, phosphate, clovers, and grazing animals. The

existing routes are also expensive in labour and in money, since they either depend upon mechanical cutting and processing in place of grazing, or upon expenditure to supplement or replace bacterial fixation of nitrogen from which most of our high-class protein-nitrogen comes²: this costs nothing except for earthy materials such as phosphate, potassium, calcium, and sulphur, which must in any event be provided. The spread of synthetic fixation of nitrogen has left the fundamentals of plant growth and protein production unaltered, and nothing could be more wrong than to imagine that agricultural application of 'synthetic nitrogen' has been a victory for the factory over biology. Putting industrial fixation of nitrogen from the air to aid sulphate-reducing bacteria in the sea may be of the type of things to come.

¹ Nicol, Hugh, *Chem. and Indust.*, 230 (1948).

² Nicol, Hugh, *Proc. Roy. Philos. Soc., Glasgow*, 74, 33 (1949).

³ Lawes, J. B., and Gilbert, J. H., *Phil. Trans. Roy. Soc.*, 192 B, 139 (1900).

⁴ Anon., *Nature*, 48, 323 (1893).

⁵ Zelinski, *Fortschr. russ. chem. phys. Gesell.*, 25, Heft 5 (1893); after Beijerinck (6).

⁶ Beijerinck, M. W., *Cbl. Bakt.*, II, 1, 49, 104 (1895); *Verz. Geschr. (Coll. Pap.)*, 3, 102 (1921).

⁷ Delden, A. H. van, *Cbl. Bakt.*, II, 11, 81, 113 (1904).

⁸ Baars, J. K., Diss., Delft (1930).

⁹ Letts, E. A., and Adeney, W. E., Roy. Comm. Sewage Disposal, Fifth Rept., App. VI (1908) (in House of Commons Comm. Repts., 56 (1908)).

¹⁰ Irvine, R., and Gibson, J., *Proc. Roy. Soc. Edinburgh*, 18, 54 (1892).

OBITUARY

Prof. Otto Meyerhof, For.Mem.R.S.

OTTO MEYERHOF, who was born at Hanover on April 10, 1884, died at Philadelphia on October 6, 1951.

He began his scientific career with the study of medicine and at first was interested in psychology and psychiatry. But, particularly under the influence of his friend, Otto Warburg, he turned to physiology and biochemistry; indeed, they collaborated in a paper on the part played by cell structure in metabolism—a paper describing for the first time the application of manometric methods to respiration measurements. After he had settled in Kiel (where he went in 1912) he began the magnificent series of papers on the biochemistry of contraction of intact, isolated muscle. Like Hopkins, he felt muscle to be the material *par excellence* for study of the correlation of function and metabolism in living tissue.

Setting out from the fundamental observations of Fletcher and Hopkins and their collaborators at Cambridge, Meyerhof made a wide and exact survey of the relations of lactic acid and glycogen content, oxygen uptake, and heat and tension production in a variety of conditions. He showed for the first time the close proportionality of production of tension and lactic acid formation in anaerobic contraction; but perhaps his most important findings were concerned with oxidative recovery. Here he showed that, of the lactic acid disappearing, the equivalent of some four-fifths reappears as glycogen while the oxygen uptake accounts for complete oxidation of the remainder. Furthermore, the heat production corresponds to only one-half of that to be expected from this oxidation, so that part of the energy is retained for synthetic purposes.

This series of papers was not published until after the end of the First World War and reached Great Britain only in the early months of 1921. The

excitement of their urgent translation in Hopkins' laboratory at Cambridge is a vivid memory, as well as the intense satisfaction derived from the flood of light they threw on problems continually discussed there.

Meyerhof went on for some time with his work on the whole muscle, especially in efforts to balance the observed heat production with that to be expected from the known chemical changes. Very accurate measurements gave always a deficit in the latter figure, and this was explained only after the discovery of creatinephosphate.

In 1926, two years after his move to the Kaiser Wilhelm Institute for Biology at Berlin-Dahlem, he began the work on enzymically active muscle extracts which, continued with several collaborators, was to prove so fruitful. Working with these extracts and with extracts from yeast, he followed up and extended the perspicacious but largely unproved suggestions of Gustav Embden, bringing to light new enzymic stages in carbohydrate breakdown and preparing hitherto unknown intermediates.

In 1927 the discovery of creatinephosphate and the observation (first by the Eggletons and by Fiske and SubbaRow, and then in his own laboratory) of its breakdown during contraction and resynthesis during recovery raised difficulties for the lactic acid theory of contraction. This was felt especially in two facts: first, that while creatinephosphate breakdown is a markedly endothermic reaction and while the breakdown was known to occur chiefly in the early stages of contraction, experiment so far had seemed to show proportionality between lactic acid production and heat formation; secondly, that the anaerobic resynthesis of creatinephosphate was not accompanied by heat absorption. Meyerhof at this time made an error of scientific judgment, rare for him, in his reluctance to admit the part played by creatinephosphate breakdown in contraction, apparently inclining towards the suggestion that the breakdown might not be happening *in vivo*, but was an artefact due to the treatment of the muscle in extraction. He failed, too, to attach importance to the findings of post-relaxation lactic acid formation by the Embden school. However, his experiments with Lipmann, published early in 1930, concerning the pH changes upon stimulation of intact muscle, left him in no doubt about the reality of creatinephosphate breakdown, and in the same year he was ready to welcome the results of Lundsgaard on iodoacetate-poisoned muscle with their clear implication that creatinephosphate breakdown is a stage nearer the muscle machinery than is lactic acid formation. He invited Lundsgaard to continue his work at Heidelberg, and the problems mentioned above were solved by combined work there.

Meanwhile Lohmann, in Meyerhof's laboratory, had made the discovery of adenosinetriphosphate in 1929, and during the 1930's Meyerhof's laboratory, together with others in Lwow, Cambridge and Berlin, cleared up the problems of the co-enzyme function of the adenylic compounds and laid the foundations for the understanding of the part played by these compounds in energy transfer. The unique contribution of the Meyerhof school here was the investigation of the heat of reaction of various stages in glycolysis and of the heat of hydrolysis of creatinephosphate, argininephosphate (a compound discovered in invertebrates by Meyerhof and Lohmann), adenosinetriphosphate and phosphopyruvic acid. Such studies led to the early realization of the important difference in nature between the phosphate

bond in these compounds and in the ordinary ester linkage.

Although he was during these years mainly concerned with isolated enzyme systems, Meyerhof still turned to the muscle as a whole, for example, in the work with Möhle and Hartmann on volume changes accompanying contraction.

In 1924 he had become head of the Department of Physiology in the Institute for Medical Research in Heidelberg and remained there until 1938. One of his research workers in the latter part of this period has described his daily round, discussing without any dictatorial attitude, but with complete grasp of detail in each case, the results of yesterday and the programme for to-day. This collaborator has written: "The daily round took him from worker to worker, Japanese, French, English, Italian, Spanish, American, German. It was a stimulating event and lingered on in one's mind. The thoughtfulness of the faces of the people in the room is strongly in my memory." Meyerhof himself worked in the laboratory daily and one striking characteristic was his prodigious memory for facts and figures. He loved to show a very generous hospitality to his colleagues.

It has been said of him that his intense and single-minded concentration on the pursuit of scientific truth shut him off as behind an intangible curtain from current events. This concentration made it possible for him to stay on in Heidelberg for five years after the rise of the Nazis, without loss of personal dignity and integrity. He even during this time accepted at least one new Jewish worker, refusing to consider the possibility of outside interference with the conduct of his laboratory. But by 1938 the situation had become impossible and Meyerhof, like other scientific men of Jewish origin, was obliged to leave Germany. He was welcomed in Paris and went on with his work at the Institut de Biologie Physico-Chimique until the invasion of 1940. With the help of the Rockefeller Foundation and of friends in America, he and his wife were able to reach the United States, and he was appointed research professor of physiological chemistry in the University of Pennsylvania. His reputation was, of course, international; he had shared the 1922 Nobel Prize for physiology with A. V. Hill, and among many other honours had been elected in 1937 a foreign member of the Royal Society. After his exile he did much to help in the re-establishment of displaced scholars less well known abroad.

From 1938, Meyerhof's scientific work was concerned mainly with enzyme studies, and he devoted much attention to questions of the free-energy content of phosphorylated compounds. He continued to break new ground, as in his study with H. Green of the transphosphorylating activity of intestinal phosphatase.

An aspect of Meyerhof's interests which remained less well known to his colleagues, at least in other countries, was his work on the history and philosophy of science. He was an authority on Goethe's scientific contribution; and in 1933 he published an excellent lecture on theoretical biology in which he adopted a point of view allied to the Whiteheadian doctrine of organism, in general agreement with the biological implications which Woodger had set forth a few years before. He had a great love, too, for painting and for literature.

Such were some of the achievements and activities of one of the greatest of biochemists.

DOROTHY NEEDHAM