impossibilities of Rayleigh's formula. He next turned his attention to the thermodynamical aspect, and this illustrates at its best the groping process, for in fact thermodynamics does not contain the answer at all; and yet this track guided him to the right solution. He was led to study a certain thermodynamic function (the reciprocal of the second differential of the entropy with regard to the energy!), and was struck by the fact that, with Wien's formula, it was proportional to the energy itself—a fact that is really quite accidental. For Rayleigh's formula it was proportional to the square of the energy, and Planck was happily inspired to combine the two forms into one. The result gave him the true formula. It remained to find a theory to account for it.

In the course of his efforts to get this theory, Planck turned to the deeper meaning that is attributed to entropy on the kinetic theory, which connects it with probability, and once this was tried it gave the result more simply than might be expected. For in considerations of probability one is bound to work with discrete quantities, and not with continuous; and so one must adopt the idea of atoms of energy for the calculations, though with the ultimate intention of making them infinitely small. But this intention is frustrated, because the formula is obtained without going to the limit at all. This was how Planck arrived at his theoretical explanation of the radiation formula, and by comparison with experiment he was enabled to deduce two universal constants. From the first were obtained the earliest really good values for the charge of the electron and the associated constants. The second, he confesses, perplexed him a good deal, and indeed it would have been surprising if it had not. It was the quantum.

The remainder of the address is occupied with the later history of the quantum, and it is scarcely too much to say that this is simply the history of modern physics. Of all its applications, photoelectric effect, specific heats of solids at low temperatures, specific heats of gases, etc., he not unnaturally gives pride of place to Bohr's spectrum theory. The address concludes with some speculations as to what may be the solution of the almost impossible difficulties with which we are faced. The success of the quantum theory has been, and continues to be, so enormous that it often appears as if writers had forgotten that the whole present system of physics is based on a perfectly definite set of mutually contradictory axioms. So it is particularly interesting to hear some views on this question by one of the great authorities. Unfortunately, Planck does not seem nearer the solution than are the rest of us, for in one place he throws out the discouraging suggestion that the quantum theory is now in the state in which Römer left the theory of light, so that we may expect to have to wait a long time for the Maxwell of the subject to appear and reconcile the seemingly irreconcilable.

Obituary.

SPENCER PICKERING, F.R.S.

BY the death of Percival Spencer Umfreville Pickering at Harpenden on December 5 English science loses one of its most original and attractive personalities. His death was not unexpected; for more than a year his friends had known that Pickering was in a precarious condition, holding on to life by little more than his courage.

Pickering was born in 1858 of good family, and educated at Eton and Balliol. His earliest paper was published while still an undergraduate, and it is not uncharacteristic that it was polemical, directed against one of his dons, and concerned with a basic compound. But his real activity began with his appointment as professor of chemistry at Bedford College in 1881. He began to work upon the constitution of double and basic salts, and passed on to determinations of thermal phenomena accompanying the formation and solution of salts. Naturally enough, this work led to a general consideration of the process of solution, especially as it was about this time that the Van't Hoff theory of osmotic pressure and the dissociation into ions of salts in aqueous solutions was beginning to revolutionise the conceptions of chemists. Pickering would have none of this theory; his work lay at the other end of the scale among strong solutions and powerful electrolytes, and he saw solution as a process of association with formation of hydrates. In a voluminous paper published by the Chemical Society in 1889 he examined with an extraordinary wealth of detail the density, conductivity, heat of dissolution, heat capacity, and expansion of mixtures of sulphuric acid and water, demonstrated breaks of continuity in the graphs representing these properties, and isolated definite hydrates to match the breaks.

Controversy with the supporters of the ionic hypothesis grew intense, for Pickering was turning out an enormous volume of experimental work, and was standing for his hydrate theory almost *Athanasius contra mundum.* Between 1889 and 1896 he printed no fewer than fifty-six papers on solution with the Chemical Society alone, many of them of great length, and involving a vast number of exact determinations, all of which were done single-handed and without assistants. The tide was, however, running against Pickering; he and Ostwald were looking at different sides of the shield; but, though Pickering's work has since fallen into its proper place, at the time it lacked that pragmatic justification of leading to discovery which made the dissociation hypothesis so generally acceptable to the chemists of the day. In disgust Pickering forsook chemistry, the rapid flow of papers ending abruptly in 1896.

Pickering, however, as a younger man had, in order to regain his health, put himself to work as a labourer on the Experimental Farm at Rothamsted, and thus acquired an interest in the application of science to the problems of the cultivator. He had as early as 1894 designed a series of experiments upon the growth of fruit, and had persuaded the Duke of Bedford to set up a trial garden at Ridgemount in Bedfordshire. Results soon began to appear and to arouse dissent; not easily did the practical fruit grower, accustomed to old grass orchards, accept the doctrine of the injurious effect of grass upon tree growth. This subject occupied Pickering to the end of his days; the complexity of the problem grew with extended knowledge; but Pickering maintained his first explanation that the grass roots excrete something specifically poisonous to fruit trees. Much other ground was broken—the effects of pruning, methods of planting and preparing the soil for planting, manuring, insecticides—there is no part of the fruit grower's routine on which Pickering did not inaugurate investigation.

The conclusions published from year to year and gathered together into a final volume, "Science and Fruit Growing," in 1919, have been the occasion of much controversy. The unsuitability of soil and situation, and some defects in management in the early years, hindered their acceptance, but the Woburn trials will remain as the most substantial contribution of the last hundred years to the study of fruit-tree development, one full of stimulus to new workers. His work on spray fluids led Pickering back to chemistry and his earliest interests—basic salts; after a ten years' silence papers began to reappear on such questions as the basic copper salts of Bordeaux mixture, on emulsions (with his strange discovery of a method of solidifying paraffin), and on quadrivalent copper salts.

In his horticulture, as in his chemistry, Pickering was essentially the amateur of genius; he often seemed to be careless of, and even but moderately equipped with, the knowledge that was common form, academic or practical. But he had a disconcerting habit of making discoveries which contradicted that common form. Either from policy or from temperament, he never disguised these antagonisms; where another man might have looked round to find hints and anticipations in previous experience, Pickering would say roundly, "All men who have hitherto expressed opinions on this point have been entirely wrong," even in such a matter as the way to plant a fruit tree. He loved truth, and he pursued it all his life like an artist, for the interest it had to himself; there was also something of the artist's disdain in the way he presented it to the world.

Never in robust health, an accident that deprived him of the sight of an eye probably helped to keep him out of general society, nor had he any of the ordinary man's amusements. At one time he used to walk a great deal with his inseparable companion, his wife, but he seemed to get most pleasure out of the company of a few friends in his Harpenden garden, and it is there, among his fruit trees, or indoors at his piano, that one will remember Spencer Pickering, handsome, imperturbable, a fine and rare presence among men.

A. D. H.

WILLIAM ARTHUR HAWARD.

WILLIAM ARTHUR HAWARD, who accidentally met his death on Monday, December 6, whilst making some final experiments in an important investigation upon gaseous combustion under high initial pressures, upon which he had been engaged during the past two years as a Salters' research fellow in the Imperial College of Science and Technology, was passionately devoted to the cause of scientific research. There is every reason to believe that, had his career not been thus so tragically cut short, he would at no distant date have achieved great distinction as a scientific discoverer. Even during the research which he was completing at the time of his death he had, by most skilful experimental work, discovered a series of facts which pointed to an important new fundamental development in the science of combustion. Indeed, the actual experiment upon which he was engaged when the accident occurred was intended to test a new theory which had been suggested to account for some of his remarkable experimental results. In due course, when the results of his research are published, the importance of them to science will at once be apparent. He undoubtedly laid down his life in the cause of science.

The various stages in Haward's all too brief, but very distinguished, career were as follows: Entering the Royal College of Science in October, 1912, he took the associateship two years later, and also his London B.Sc. degree with first-class honours in chemistry. He thereupon commenced a course of post-graduate study and research in the department of chemical technology, under the direction of Prof. W. A. Bone. It was soon apparent that he was unusually gifted as an experimentalist, for he made some remarkable experiments upon certain aspects of surface combustion, which have yet to be published. During two of his summer vacations, in the years 1915 and 1916, he made investigations under the direction of Dr. R. V. Wheeler at the Eskmeals Home Office Experimental Station upon (1) the propagation of flame in mixtures of hydrogen and air, and (2) the uniform movement of flame in mixtures of acetylene and air, the results of which were embodied in two papers that were published in the joint names of himself and two others (who had assisted him) in the Trans. Chem. Soc. for 1916 and 1917.

In June, 1916, Haward was elected to a Beit research fellowship tenable at the Imperial College, but this was relinquished some six months later in order to join the chemical staff of H.M. Explosives Factory, Gretna, where he remained until shortly after the armistice. He then obtained a Salters' research fellowship, with which, at his