EINSTEIN'S UNITARY THEORY (1928–29).

Whereas Weyl and Eddington replaced Riemannian geometry by others still more unlike Euclidean, Einstein has now, in part, returned to more ordinary ideas. His geometry is one which possesses *distant-parallelism* as well as a Riemannian metric. To explain what is meant by distant-parallelism, we return to our two-dimensional analogy. Cover our hen's egg, or any other surface, with a network of 'curvilinear rectangles'. 'Parallel directions' are defined as those which make the same angles with corresponding sides of the local rectangles. This definition leaves the original choice of the network undefined, but we saw that on a sphere direction had to be defined by something, like a magnetic compass or a pole star, which was not a property of the sphere itself, and so in a certain sense undefined by its geometry alone.

Perhaps Einstein's parallel directions may be ultimately defined in terms of dynamics. He may even get back to the position of Newton, who conceived absolute rotation to be a real thing, which could be detected by seeing whether the surface of a fluid was a paraboloid of revolution or a plane. The behaviour of Foucault's pendulum and of gyroscopes certainly seem to furnish us with a dynamical definition of direction.

By using our sphere, we may even give some idea of the actual function that Einstein takes to measure what may be called electromagnetic potential. Suppose a boat has two short trips, each of one mile, one east and the other north. By sailing first one mile east and then one mile north, let us reach a point C. By sailing first north and then east we reach a different point C′, since the parallels of latitude get smaller as we go north (see Fig. 1). The distance CC′ represents Einstein's potential. This illustration is not exact, because on a sphere CC′ is very small compared with the distances AB, BC, whereas in Einstein's theory it is essential that it should not be so. To illustrate this we should have to suppose our sphere to have a crinkly surface.

If we now take the corresponding construction for three dimensions, the result is rather queer. If AB and DC are 'parallel' paths, the path from B 'parallel' to AD will not intersect DC. It is properties of this kind that Eddington finds unattractive, but they are essential to the electromagnetic part of the theory.

Of course the ultimate test of the theory must be by experiment. It may succeed in predicting some interaction between gravitation and electromagnetism which can be confirmed by observation. On the other hand, it may be only a 'graph' and so outside the ken of the ordinary physicist. Einstein's paper points out that so far there has not been time to examine the full consequence of his equations.

Even supposing the theory fully established, there are still fresh worlds for Einstein to conquer. The quantum theory remains outside his scheme. He made an attempt to deal with this so far back as 1923, but without any striking success. However, it has been suggested that the postulate of distant-parallelism will enable the unitary theory to take over Dirac's theory of a spinning electron almost unchanged.

The Detection of Helium.

THE natural facility with which the radioactive elements disintegrate has led on one hand to attempts to break down atoms artificially, and on the other to build them up from simpler particles. Rutherford succeeded in conveying the necessary energy to some of the less massive atoms and broke them down by bombardment with sufficiently energetic α-rays, atom by atom at comparatively rare intervals: the process of atom building is still not more than a dream, realised perhaps in the depths of space as Millikan has suggested in order to account for cosmic rays.

The production of gold from mercury, and many another attempted transmutation, have proved, to put it mildly, apparent rather than real changes. In the case of the experiments in which helium was supposedly formed in some way or another by an electric discharge, there has lurked for a long while a certain feeling of unsatisfactoriness. Prof. Paneth's recent work goes far to dispel this feeling (see *Zeits. f. phys. Chem.*, **134**, 353; 1928; and **1**, 170 and 253; 1928). The outcome is indeed satisfactory: those that found helium have reason to have got it; those that did not might well have found it, and been misled perhaps as to its origin.

Paneth and Peters show that helium is the only gas which at ordinary temperatures can diffuse through glass. At a pressure of 0·5 atmosphere 10^{-11} c.c. of helium will pass through a thickness of 0·5 mm. of soda glass per cm.^{2} per hour. The amount of helium that gets through from the air at ordinary pressure into an evacuated glass vessel (1 mm. wall thickness) is 10^5 times less, so that a glass apparatus is for all practical purposes 'tight' at ordinary temperatures. When warm the rate of diffusion through the glass is much greater (cf. Lo Surdo, *Atti R. Accad. Lincei*, **30**, 1, 85; 1921). A hard glass tube 1·5 mm. thick at 500° C. lets through 10^{-9} c.c. of helium from the air per cm.^{2} per hour. Helium, indeed, can be separated from neon and other gases by diffusion through hot glass. It is otherwise with palladium. Helium will not diffuse through palladium at a red heat. A mixture of helium and hydrogen can be separated completely by diffusion of the hydrogen through a palladium capillary; the quantity of helium that gets through is not even 10^{-12} of the quantity of hydrogen that passes. Helium and neon are found in the gases absorbed by glass which has been in contact with air, but the gas is considerably richer in helium than in neon. On the other hand, if there is a minute flaw in the glass or at a tap, causing a leak however small, the neon and helium found in the residual gases remain in the same proportion as they exist in the air, approximately 3 : 1. (It is noteworthy that Paneth and Peters found that good taps could be relied on not to leak if properly ground and greased; their apparatus was therefore not made tap-free. Twenty taps standing 48 hours had not leaked to the extent of more than 10^{-6} c.c. air, equivalent to about 10^{-10} c.c. Ne and He.) It can be shown that a vacuum tube which becomes heated by a discharge will contain afterwards traces of helium, if it is not protected from access of air externally, however great other precautions may have been taken to prevent ingress of air. A double wall is not even sufficient if both become warm. It is necessary to immerse the tube in water or in oil which cools and at the same time seals the glass. The presence or absence of helium in the residual gases is therefore mainly a question of the temperature of the walls of the tube and the sensitiveness of the method of detection.

Paneth gives 10^{-12} to 10^{-11} c.c. as the limiting volume of helium which can be detected spectroscopically. This means that in his apparatus the helium and the neon in about 10^{-5} c.c. of air can be detected—a limit about 100 times smaller than that given for the method used by Strutt (*Proc. Roy. Soc.*, A, **89**, 499; 1914). A careful study is made of the quantity of gas required to bring out the various spectral lines for the pure gases neon and helium and their 3 : 1 mixture obtained from atmospheric air. The spectra of the gases are examined using a capillary tube about 0·1 mm. bore; the fine capillary makes it unnecessary to use a slit with the spectroscope. Excitation is provided by external electrodes. The results enabled estimation of very minute quantities of the gases to be made without recourse to uncertain volume measurements in fine capillary tubes. For quantities at the limit of detection (10^{-10} c.c.) only the 5875 and 5015 lines of helium are visible and only the 5852 line of neon. The latter masks the 5875 helium lines in a mixture of the two gases and only 5015 remains visible. Paneth succeeded in this way in measuring the quantities of helium (about 10^{-8} c.c.) generated by only about 40 grams of thorium in 113 days, taking very special precautions to prevent contamination with helium from other sources. Even with every precaution a trace of neon was also detectable.

Either calcium or an electrically heated spiral of palladium were employed for removing large quantities of hydrogen from the gases under examination; for smaller quantities combustion with oxygen at the surface of palladium sponge was used. The gases were taken from place to place along with oxygen which was afterwards removed by absorption with cooled charcoal, the residual rare gases being 'run up' into the capillary tube for the spectroscopic test. Special precautions were taken to prevent any rare gases being present in the electrolytically generated hydrogen and oxygen used throughout the work; these latter were shown to contain less than about a millionth of a per cent of air. All parts of the apparatus with large glass surfaces and those subjected to heat were vacuum jacketted and then immersed in water.

Paneth and Peters have bombarded salts of potassium; they have run a heavy discharge through hydrogen between aluminium electrodes at pressures from 1 to 85 mm. and also between a palladium spiral electrode through which a large quantity of hydrogen was diffused, without obtaining any helium or neon other than traces from ascertained sources. They have tried a powerful silent discharge through hydrogen at 10 to 760 mm. pressure and they have passed a heavy discharge through paraffin, examining the hydrogen so generated. In all cases the results were negative, provided the glass was protected from transfusion by helium from the air. In spite of the stability of helium and the possibility of building it up from protons and electrons with evolution of 7×10^{11} cals per mol, these experiments show that even with a favourable high concentration of hydrogen, the amount of helium so formed is certainly less than 10^{-9} c.c. The same result applies to the production of helium by bombardment of water and of mercury with β and γ rays. To these experiences have to be added those of Allison and Harkins (*J.A.C.S.*, **46**, 814; 1924) in which very heavy discharges were employed, yet with no positive effects. Considering, too, that the sensitiveness of detection in Paneth and Peters' work is claimed to be 10^4 times greater than the volumes of helium and neon obtained in those experiments by other workers which have appeared to give positive results (*e.g.* production of helium from salts of potassium where the quantity found was between 10^{-5} and 10^{-6} c.c.), it is fairly definite that their source must be other than permitted by Paneth's arrangements and precautions.

One of these sources, when helium is alone found, is no doubt the diffusion of helium through heated glass (or quartz). It is interesting to note that this was also the conclusion of Masson in his experiments with the quartz mercury arc (*Proc. Roy. Soc.*, **91**, 30; 1915). It is noteworthy that Paneth found that glass which is exposed to air contains helium and less neon (50 cm.^{2} of glass holds more than 10^{-6} c.c. He). Hydrogen greatly assists the removal of these adsorbed gases. Oxygen, however, has practically no effect in 'washing' them out of the glass. Heating alone without washing with hydrogen is also comparatively ineffective. This fact seems also to explain some features of the earlier work. Prof. Paneth's work has gone a long way to clear up the unsatisfactory state in which this subject had been left. There is now no evidence for the formation of the rare gases by the discharge, but very definite reasons for their detection in the kind of experiments which were carried out (*e.g.* presence of He in X-ray tubes as found by Ramsay (Nature, **89**, 502; 1912)).

Passing from experimental work of a critical nature to that with a more constructive object, Paneth has utilised his methods of detection of minute quantities of helium in connexion with a variety of other problems (see *Zeit. anorg. Chem.*, **175**, 383; 1928; and *Zeit. f. Elektrochem.*, **34**, 645; 1928). Amongst them may be mentioned the origin of the abnormal helium content of sylvin and beryl, the quantity and origin of helium in gases of natural origin, and the helium content and age of meteorites. At Ahlen, in Westphalia, a source of natural gas has been found to provide about 40 m.^{3} per day containing 0·19 per cent helium, but this does not compare with the source at Calgary in Canada, 330,000 m.^{3} per day containing 0·33 per cent He, or with that at Petiola in Texas, 425,000 m.^{3} per day of 0·9 per cent. The ages of the various iron meteorites investigated are found to range from that of the Savic meteorite (8000 years perhaps) to the hoariness of the Nelson Co. meteorite, comparable to the age of the earth ($2\cdot6 \times 10^9$ years). It is thought that passage near the sun might account for the removal of helium from the Savik meteorite, making it appear more youthful than it is really likely to be.

Another interesting direction of Prof. Paneth's work was in the attempt to prepare helides after the manner in which he has so successfully made hydrides of various elements. No trace of the formation of helides of arsenic, antimony, lead, germanium, selenium, iodine, and chlorine was obtained. In the experiment with chlorine, the merest trace of the formation of a helide would have been detectable. It is considered that such helides as can be formed can only have a very fugitive existence, of the order of 10^{-8} second.

One might recall the words of Leonardo da Vinci in connexion with all this illuminating work: "Experience is never at fault; it is only our judgement that is in error in promising itself such results from experience as are not caused by our experiments". A. C. E.

Obituary.

GEORGE BIRTWISTLE.

GEORGE BIRTWISTLE was born at Burnley in 1877. Educated at Burnley Grammar School and Owens College, he won an open scholarship in mathematics at Pembroke College, Cambridge, in 1895. He was bracketted Senior Wrangler in 1899 and was placed in Class I., Division I., of the post-graduate part of the Mathematical Tripos in the following year. He was immediately elected to a fellowship and was responsible for the mathematical teaching in Pembroke until the time of his death. He had also served as assistant tutor and prætector of the college. He died very suddenly and unexpectedly on May 19.

It was as a teacher rather than as an investigator that Birtwistle was known, and as a teacher that he played a conspicuous part in Cambridge mathematics, especially during the last ten years. In certain respects his position was unique, for he was a link between the older theoretical physics and the new. Since the War, while continuing to lecture on classical mechanics, electrodynamics, and hydrodynamics, his interest in more recent developments, always strong, rapidly increased. He began to lecture on the older quantum theory, on thermodynamics (then just introduced into the schedule of elementary teaching), and finally on modern quantum mechanics. Each of these lecture courses ultimately grew into a book.

As a lecturer, Birtwistle was admirably clear and easy to follow. He set, in fact, a standard of exposition which made it very difficult for anyone to attract students to any duplicate course. His books are like his lectures—admirable expositions of those sections of the subject with which he deals, written in lecture-room style. He seldom attempts to go deeply into difficult points or to present the subject as a single logical whole. His aim is the lecturer's aim—to interest the student in the subject, especially in its more outstanding or exciting parts, and lead him on to other more systematic or abstruse expositions.

In all his lectures and in all three books, Birtwistle was successful in this aim, though naturally in varying degrees. Perhaps the least successful of his books was the last, on modern quantum mechanics. Here, owing to the novelty of the subject and the absence (when Birtwistle wrote) of other more systematic expositions (or indeed of any other exposition), the weakness of his deliberate method becomes more obvious. The book gives rather the impression of a collection of interesting isolated sketches. It stimulates the reader to ask for more, but to what other author is he to turn? With the coming of other books the weakness is already less felt and Birtwistle's book is gaining in value as a stimulating introduction. The staff of the Mathematical Faculty of Cambridge mourn the untimely loss of a valued friend and colleague.

DR. W. MARTIN.

DR. WILLIAM MARTIN, who died on May 24, was known to a very wide circle as an antiquary whose knowledge and insight enabled him to see almost everywhere in London vestiges of the life and activities of former times; but to many others he was known as an authoritative exponent of patent law, and he was an occasional contributor to our columns upon this subject.

Dr. Martin's antiquarian bent led him to treat patent law historically; but he was none the less alive to the conceptions which govern modern practice in this sphere. In his lectures and publications, notably his articles in the *Law Quarterly Review*, he worked out with great originality a systematic key to the immense body of decided cases with which he seemed to be familiar in every part. The law of treasure trove also attracted him; and in it he saw, contrary to the opinions of some antiquaries, means which could be utilised for the advantage of archæology as a check on the surreptitious disappearance into private collections of finds of general interest.

As an antiquary Dr. Martin was insistent on a strict separation of ascertained fact from the accretions of sentiment and fancy which too often obscure instead of illuminating the past. Nowhere was he more impatient of any looseness than in his