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Obituaries

HERBERT DINGLE

Herbert Dingle, who was President of the Royal Astronomical Society from 1951 to 1953, died in Hull on 1978 September 4 at the age of 88.

Dingle was born in London on 1890 August 2, but after the death of his father he was taken to Plymouth, where he spent most of his early life. He was obliged to leave school at the age of 14 and for the next 11 years he worked as a clerk. He continued his education by studying in his spare time. In 1915, at the age of 25, he won a Royal Scholarship for Physics at Imperial College of Science and Technology, London. He graduated in 1918, having already been appointed a Demonstrator in the Physics Department. He was a great admirer of the famous spectroscopist Alfred Fowler, the successor of Norman Lockyer at Imperial College, and under Fowler's influence he took up research in the same field. He became interested in both experimental spectroscopy and its astronomical applications. He was elected FRAS in 1922. In 1924 he published a textbook with the title *Modern Astrophysics*, which was one of the first on that subject. He was also part author of *The Splendour of the Heavens* (1923) and of *The Life and Work of Sir Norman Lockyer* (1929).

In 1927 Dingle went on his first total solar eclipse expedition to Colwyn Bay where he hoped to observe the Sun's spectrum, but he was prevented from doing this by cloudy skies. In 1932 he went to Montreal for the same purpose and with the same result. (The outbreak of the Second World War caused the eclipse expedition he was to have gone on in 1940 to be cancelled.) As described in his report, the Imperial College 1932 eclipse expedition led by him had two objects in view. One was to photograph and measure the bright line spectrum at the cusp of the partially eclipsed Sun up to within a few minutes of totality. (It had been observed at the eclipse of 1912 April 17 by Fowler and photographed by Newall.) The other was to photograph and measure the Fraunhofer spectrum near the limb of the Sun just before and just after totality when the diffused atmospheric light, being also limb light, could have no disturbing effect on the wavelengths obtained.

Dingle wrote much on spectroscopy and its astronomical applications and he did useful work in spectrochemical analysis. In particular, he did pioneer research on the spectra of ionized and doubly ionized fluorine. The availability of photographic plates sensitive to infrared radiation made it possible for him to study these spectra in that region. For spectra in the visual region the iron arc provided most of the standards. Since iron has many infrared lines, it seemed to be a natural source of standards in that region too. There was, however, a need for more accurate wavelengths of iron in the infrared and these Dingle supplied. He also worked on the spectrum of mercury.

Following the retirement of Fowler in 1935, Dingle became head of the spectroscopy section. In 1937 he was promoted to a chair in Natural Philosophy in the Physics Department but remained in charge of the section. Under Fowler Imperial College had been in the forefront of physics in the great days of line spectroscopy. It was no fault of Dingle's that his time as head of the section, from 1935 to 1946, coincided with a world-wide lull in the subject prior to its revival under the impact of nuclear physics in the 1950s.

Dingle's interests were not confined to spectroscopy and its astrophysical applications. As early as 1922 he wrote a useful book on relativity for the layman called *Relativity for All*. This revealed both his grasp of the subject, which in those days was generally regarded as highly esoteric, and also his powers of lucid exposition. In 1932 he spent a year as Rockefeller Foundation Scholar at the California Institute of Technology, where he had the opportunity to meet the theoretical cosmologist R.C.Tolman and also Einstein. He devoted much of his time there to relativistic cosmology. He calculated basic formulae for a fairly general type of metric which Tolman afterwards included in his well-known textbook *Relativity, Thermodynamics and Cosmology*.

Tolman suggested to Dingle that he should investigate the stability of the spatial homogeneity of the Universe. A long paper by Dingle bearing the title 'On isotropic models of the Universe with special reference to the stability of the homogeneous and static states' was published the following year in *Monthly Notices*. He believed that we have no reason to suppose that the part of the Universe which is observed is typical of the whole and hence that the Universe is homogeneous. He claimed that the assumption of isotropy has no greater empirical justification than that of the homogeneity he was discarding, but he felt obliged to make it for the problem to be tractable. He was critical of Eddington's suggestion that the condensation of the original uniform distribution of matter into galaxies was the disturbance which caused the homogeneous and isotropic Einstein Universe to expand. Dingle argued that the idea that such a condensation would take place simultaneously at an indefinitely large number of points, distributed throughout the Universe in such a way as to keep the density and pressure on the large scale perfectly homogeneous, was 'wildly improbable'. In his paper he concluded that an Einstein Universe subject to a local disturbance would contract. He obtained the metric of an isotropic Universe that was 'slightly inhomogeneous', and he found that the homogeneity of an expanding Universe is stable and that of a contracting Universe is unstable. Perhaps the main interest of his paper today is that it foreshadowed Dingle's rôle in the following years as a critic of the *a priori* claims made by some of the leading theoretical cosmologists, particularly Eddington and Milne.

Dingle was given to devastating and often witty criticism of those whom he believed were betraying the true spirit of science, and from the early 1920s he was actively concerned with the philosophical, as well as with the practical, aspects of physics. He developed a highly professional interest in the philosophy of science, and as a writer in that field he became very well known, particularly in the United States. This led to his second visit to that country, in 1936, when he was invited to give the Lowell Lectures at Harvard. His philosophy of science, as expounded in his books *Science and Human Experience* (1931) and *Through Science to Philosophy* (1937), the latter based on his Lowell Lectures, was in the now unfashionable tradition of British empiricism. He believed that the essential basis of science was the rational correlation of elementary experiences of the natural world which can be made objective. This led him to adopt a philosophy which had much in common with that of Ernst Mach and P.W.Bridgman's operationalism, but he laid particular emphasis on rationality. As he said in his James Scott Lecture on *The Nature of Scientific Philosophy* that he delivered in Edinburgh in 1948, 'To philosophize is ultimately to see all the elements of our experience in rational relations with one another'. He believed that 'if one's purpose is to achieve the full rationalisation of the whole of experience, the scientific philosophy, though still incomplete, is the only one that offers any reasonable prospect of success'.

During the Second World War, in the prolonged absence of Sir George Thomson from Imperial College, Dingle was in charge of the Physics Department. Soon after the war he was appointed Professor and Head of the Department of History and Philosophy of Science at University College London, a post he held for nine years until his retirement in 1955. To his genuine surprise, since he was no longer active in spectroscopy and astrophysics, he was elected President of the Royal Astronomical Society in 1951. He had already served as Secretary of the Society from 1929 to 1933, and for several years before he became President he was Chairman of the Library Committee. In 1952 he delivered a Presidential Address on the award of the Society's Gold Medal to Dr John Jackson. It was remarkable for the way in which Dingle used his literary gifts in presenting an extremely lively account of Jackson's contributions to fundamental astronomy. His Presidential Address the following year was devoted to a criticism of the views held by some theoretical cosmologists.

Dingle was for a time President of Commission 41 (History of Astronomy) of the International Astronomical Union. He was also active in the International Union for the History of Science and was for several years a Vice-President. He was one of the founders of the British Society for the History of Science and served as President from 1955 to 1957. In 1948 he took the initiative in founding the Philosophy of Science Group of that Society. Some years later this became the British Society for the Philosophy of Science. In 1950, with the support of the publishing firm Nelson, he founded *The British Journal for the Philosophy of Science*. In 1952 a volume comprising 19 of his essays in the history and philosophy of science was published with the title *The Scientific Adventure*.

The last 20 years of Dingle's life were dominated by his campaign against the special theory of relativity. Originally he had accepted the theory, particularly because he was sympathetic to the 'operational' philosophy that appeared to be associated with it. As already mentioned, he had written a popular book on the subject many years before. In 1940 he published a short textbook on it in Methuen's well-known series of monographs on physics. Dingle's doubts were first aroused in 1957 while reading Sir George Thomson's book *The Foreseeable Future.* He came across a passage dealing with the well-known relativistic 'clock paradox'. It suddenly occurred to him that the alleged time-difference between the two clocks concerned was inconsistent with the principle of relativity on which the argument was based. Dingle maintained that, if the two clocks were synchronized when they parted company, they would necessarily read the same time when they met again, assuming that they are of exactly similar construction. As a result, he became involved in a lively controversy in the correspondence columns of *Nature* with Professor W.H.McCrea which led to a great resurgence of interest in the 'paradox' and a spate of publications on it.

Later Dingle attacked the whole theory of special relativity because he believed that the concept of time dilatation should be rejected. He claimed that according to the theory two similar clocks A and B in uniform relative motion V work at different rates, but since the relation of each to the other is symmetrical it follows that, if A works faster than B, B must work faster than A. He argued that this is a contradiction and so the theory must be false. This argument was entirely concerned with uniform motion, whereas much of the controversy over the clock paradox had been concerned with the effect of acceleration and consequently with the question of whether the paradox could be adequately discussed within the framework of special relativity.

The essential point at issue was the way in which Dingle used the particular concept that he called 'rate of clock'. He exemplified this by taking A and B to be together at epoch zero according to each and introducing two other clocks H and N at rest relative to A and B, respectively, but not spatially coincident with them. He compared the times t_1, t_1' assigned to the epoch of B's momentary coincidence with H by the observers using the clocks A, B respectively, and the corresponding times t_2, t_2' assigned by them to the epoch of A's momentary coincidence with N. He argued that, since special relativity led to the formulae $t_1 = \beta t_1'$, $t_2' = \beta t_2$, where β is the reciprocal of $\sqrt{(1 - V^2/c^2)}$, a contradiction results. This is because in the former case the ratio of the 'rates' of clocks A and B is given by t_1/t_1' and in the latter case by t_2/t_2', and hence this ratio has two incompatible values.

The difficulty can be resolved by rejecting Dingle's assumption that ratios such as t_1/t_1' determine invariant relative 'rates' of clocks A and B. His implicit requirement that the epochs assigned to *any* event by A and B, respectively, should always be in the same ratio would imply that by a new choice of time unit for one of these clocks it could be arranged that the times assigned to any given event by A and B would be the same. Dingle's requirement is therefore equivalent to adopting the Newtonian concept of universal time, and this is incompatible with special relativity.

The incompatibility of Dingle's concept of clock-rates with the special theory of relativity convinced him that the latter must be rejected, whereas his critics came to the opposite conclusion. In his struggle with what he regarded as the 'relativistic establishment', described in his book *Science at the Crossroads* (1972), he maintained that his question concerning clock-rates had not been answered but had been evaded by his critics. He felt that this was due to a widespread belief that special relativity was above criticism and that truth was being sacrificed to dogma. He remained convinced of this