was appointed Professor, and he was serving in this appointment at the time of his death.

The bulk of Wright's work was devoted to ballistics, and he was responsible for the Service text-book and various pamphlets on the subject. He also found time, however, to write various notes and papers on such mathematical topics as the mutual gravitational potential of two similarly situated rectangular bodies [*Phil. Mag.* (7), 10 (1930), 110–127], and the conduction effect of a steady source of heat at one end of an infinite bar [*Phil. Mag.* (7), 12 (1931), 1015–1019].

Wright was of a cheerful disposition, popular alike with his students and with his colleagues; and he was wholeheartedly devoted to the instruction of army officers to fit them for technical staff appointments. His death is a severe loss to the Military College of Science and to his many friends.

ARTHUR STANLEY EDDINGTON

W. H. McCREA *and* G. TEMPLE.

Arthur Stanley Eddington was born at Kendal on 28 December, 1882. He was the second child and only son of Arthur H. Eddington, Headmaster of the Friends' School at Kendal. The father died a few years later and the family moved to Weston-super-Mare where Eddington went to the Bryn Melyn School. From there he proceeded to Owens College, now the University of Manchester, and studied under Horace Lamb and Arthur Schuster. In 1902 he went on from Manchester to Trinity College, Cambridge. He was Senior Wrangler in 1904, Smith's Prizeman in 1907, and was elected to a Fellowship of Trinity College in the latter year. After a short period of research in the Cavendish Laboratory, he was invited in 1906 by the Astronomer Royal, Sir William Christie, to become Chief Assistant at the Royal Observatory, Greenwich. His acceptance determined the trend of his whole subsequent career. In 1913 he returned to Cambridge to succeed Sir George Darwin as Plumian Professor of Astronomy, and in the following year he became also Director of the Cambridge Observatory in succession to Sir Robert Ball. He held these positions for the rest of his life.

Eddington died on 22 November, 1944, after an operation following a brief illness. His intellectual power was unabated till the end, and his friends had counted upon his still having many years of productive scientific activity in front of him. They had also looked forward to his being called upon to fill positions of ever-widening influence in the scientific world.

He was unmarried. He is survived by his only sister, Miss Winifred Eddington, who had managed the affairs of his household at the Observatory since his mother's death.

Eddington became a member of the London Mathematical Society in 1916 and served for two periods on the Council, being a Vice-President in 1927.

As a man, Eddington was, perhaps first and foremost, of a deeply religious nature. Religion meant for him a mental attitude and a balance of personality rather than a creed, for which he expressed himself as personally not feeling a want. He was careful, however, to distinguish between creed and beliefs. As a result not merely of his upbringing but also of his own deeply pondered views, he found a congenial environment in the Society of Friends, of which he was a life-long member. He was unaffectedly modest and retiring; yet he possessed a full measure of human sympathies; and, for one so apparently withdrawn from the world, some of his exhibitions of human understanding were almost startling. He could be fascinatingly entertaining in company in which he felt at home, but he preferred to take much of his relaxation away from his academic associates. He was an ardent spectator of professional football, and he was fond of bathing in the Granta and of touring the country on his bicycle.

At Greenwich, Eddington did his full share in organizing and in actually performing the routine observations. Amongst other things, he completely reconstructed the programme of observations of latitude variation. At Cambridge, as Director of the Observatory, his practice was to give encouragement to the work of others rather than to initiate programmes himself. His colleagues testify with gratitude to his continual readiness to discuss all aspects of their work and to the value of his advice and guidance. In the University, his courses of lectures as Professor were on the Dynamics of the Stellar System, the Internal Constitution of the Stars, the Theory of Relativity, the Theory of Protons and Electrons, the Combination of Observations, and Spherical Astronomy and the Derivation of Orbits. Though abounding in information, their style probably struck most of his hearers as a trifle prosaic by comparison with that of the lectures he delivered on special occasions. He examined several times for the Mathematical Tripos; he served for a time on the Council of the Senate; he was Chairman of the Solar Physics Committee, and Secretary of the Electors to the Isaac Newton Studentship. Otherwise he took no particularly prominent part in the administrative affairs of the University.

Eddington did not found a "school" at Cambridge in the sense of constantly having around him any considerable group of young research students and assistants working on problems related to his own current investigations. He did, of course, take charge of some Ph.D. students, though their number was comparatively small. But he founded "schools" in the much wider sense that through his lectures and writings he must be counted as having initially inspired a great part of the work done on relativity theory in English-speaking countries, and, as Prof. E. A. Milne has recently said, "if ever a man *created* a new field of research, Eddington created the field of stellar structure as a living study in physics". Scientists from all over the world, of all degrees of eminence, consulted him by letter, by personal visits to Cambridge, or during his own visits abroad. They all derived inspiration from the discussions they had with him, and conversely, Eddington himself has not infrequently acknowledged the way in which his own work was aided by such means.

Eddington delivered the Gifford Lectures in the University of Edinburgh in 1927, the Messenger Lectures in Cornell University in 1934, and the Tarner Lectures at Trinity College, Cambridge, in 1938, besides a large number of other special lectures and addresses. These undertakings provided him with opportunities to formulate his views on the wide implications, as he judged them to be, of current advances in physical science for other departments of thought, and to develop his conception of the philosophy of physical science itself. On their literary merits alone, these lectures must always hold a very high place; and they will probably play an increasingly significant part in the development of epistemology as Eddington's later scientific work comes to be more thoroughly explored. Indeed, much of the work of this "last period" still awaits the considered judgment of the scientific world, even though Eddington himself was apt at times to write as if its concepts had been generally accepted.

Eddington received many honours and distinctions. He was elected to the Royal Society in 1914 and was awarded a Royal Medal of the Society in 1928. He was President of the Royal Astronomical Society from 1921 to 1923, and received its Gold Medal in 1924. He was President of the Physical Society from 1930 to 1932. He had been President of the International Astronomical Union since 1938; but in consequence of the war the Congress of the Union over which he would have presided did not take place before his death. He was awarded the Bruce Medal of the Astronomical Society of the Pacific in 1924. He held honorary degrees of many universities at home and abroad, and he was an honorary member of several foreign societies and academies. In 1930 he was knighted and the Order of Merit was conferred on him in 1938.

This notice will not attempt to assess the ultimate value of Eddington's work. That this was very great is certain; just how great it was, no one will be fully competent to say for many years to come. His was

> "... a mind for ever
> Voyaging through strange seas of thought, alone".

In the course of his voyagings he claimed to have discovered many things, of whose existence we can have little doubt. But whether his own charting of the seas will be found wholly trustworthy, only future explorers will be able to tell.

In saying this, one has in mind chiefly that part of his work which deals with the foundations of physical theory and to which he himself undoubtedly attached most importance. His permanent reputation is already securely established in other fields, notably in astronomy and astrophysics. Few would question his position as the greatest amongst the creators of modern theoretical astrophysics. To his pioneering lead we owe our claim to have at the present time a broadly reliable conception of the physical state of almost all the known matter in the universe. Where he did not solve the fundamental problems of this subject himself, he so prepared the ground for a solution that it is difficult to name one such problem which had not been solved, at least in principle, before his death. Possibly the only known problems still to be solved are those which have to do with the origin of cosmic radiation. In brief, it is not far from the truth to say that during Eddington's lifetime the physical *structure* of the astronomical universe has been worked out. In ascribing a major share of this achievement to him, one does not belittle the vital contributions of many others, particularly those whose inspired observational work made it possible. The task now confronting astronomy is similarly to work out the *evolution* of the universe. Eddington and his contemporaries have indeed made notable progress to this end; but we could wish for another Eddington to direct the course of the big advances that appear to be impending.

Eddington's main work divides itself into three parts: that on stellar movements; that on astrophysics, with the internal constitution of the stars as its central theme; and that which starts with the theory of relativity and leads on, through the relations between relativity and quantum theory, to the foundations of physical science. One can imagine other good scientists doing the first part possibly in much the same way as Eddington; one can also imagine others doing the second part, though not in the same way; but one can imagine no one except Eddington doing the third. As dominating interests in his work, these three did happen to come chronologically in this order. But that should not be taken as thereby denoting an evolution of his mentality. It might be nearer the truth to say that he always possessed the three mentalities corresponding to the three parts of his work and that at any time in his life he could bring the appropriate one to bear upon a particular subject.

Astronomy and Astrophysics.

Stellar movements. A sketch of Eddington's work in this field has been given by Prof. W. M. Smart and we are grateful for his permission to quote it.

"In 1904 Kapteyn announced his discovery of the two star-streams. Hitherto it had been assumed that the individual space-velocities of the stars were entirely random. The observed motions, however, included the effects of the solar motion, and from the time of Sir William Herschel the main object of astronomers dealing with stellar proper motions consisted in deriving the constants of the solar motion, *i.e.* the direction in the sky towards which the sun was moving, and, when radial velocities became available, the sun's speed. It was only when Kapteyn began to investigate the hypothesis of random motion that he was led to the discovery that the stars could, apparently, be divided into two groups (or streams), for each of which the idea of the random distribution of velocities, in direction and magnitude, appeared to be applicable. If we consider a small area of the sky and construct a polar diagram showing the number of stars with proper motions in the different position angles from 0° to 360°, the curve would, in the absence of solar motion and on the hypothesis of random motions for all the stars concerned, be a circle, ideally. The effect of the solar motion is to transform the circle into an oval curve, the direction of the longest axis being directly related to the projection of the solar motion on the area of the sky considered. If this is done for different areas, the various directions of the longest axes of the oval curves, when plotted on a sphere, would all, in theory, converge to two antipodal points, one of which gives the direction in the sky (solar apex) towards which the sun is travelling. Kapteyn found that his polar curves were not the simple ovals as described, but were bi-lobed, suggesting that the observed stellar motions could be represented as the combined effect of two random-velocity distributions formed by the superposition of two simple oval curves, each oval having its own characteristics of shape, direction of the principal axis and number of stars involved. It was at this point in 1906 that Eddington came on the scene with a precise definition of the random distribution of velocities (assumed to be Maxwellian in character), a clear account of the mathematical consequences, and a convincing application to the proper motions of the Groombridge stars. There was now no doubt as to the main features of Kapteyn's discovery;

further, Eddington was able to derive the constants of each stream. In later papers he showed that the stars belonging to the two streams were intermingled in space, and in 1910 he analysed the proper motions of the stars in Boss's 'Preliminary General Catalogue', with results in most respects similar to those derived from the Groombridge stars. In 1907 Schwarzschild had shown that the observed features of star-streaming could equally well be represented in terms of a modified Maxwellian law embracing all the stars concerned—the so-called ellipsoidal hypothesis. Thereafter, investigators of the systematic motions of the stars have had alternative methods of dealing with the problem, the choice being generally dictated by the character of the particular research undertaken.

Eddington's first book, *Stellar movements and the structure of the universe*, appeared in 1914; it gave an account of his own researches, and those of others, on stellar kinematics. As a synthesis of contemporary knowledge in this new field of investigation it exercised a profound influence in astronomical research. It should not be forgotten that the last chapter of the book—" On the Dynamics of the Stellar System "—may be regarded as the beginning of a vast new subject, Stellar Dynamics, in which Eddington pointed the way for future developments. The apparent analogy of the stellar system with a gaseous system of molecules suggested by the kinetic theory of gases, was rejected and the fundamental law of stellar dynamics was stated to be:—the stars describe paths under the general attraction of the stellar system without interfering with one another. In the second of two papers in *Monthly Notices* (vol. 75, 1915) Eddington succeeded, on the basis of the ellipsoidal law of velocities, in solving the problem of a globular stellar cluster in a steady state, the solution specifying the stellar density and the distribution of velocities at any point; in a third paper (vol. 76, 1916) he attacked the more difficult problem of oblate systems, with its application to our own Galaxy clearly in view ".

Just how vast this new subject is can best be judged from Prof. Smart's own book, *Stellar dynamics* (Cambridge, 1938), and Dr. Chandrasekhar's *Principles of stellar dynamics* (Chicago, 1942). Both these books demonstrate also how much the subject owes to the foundations laid by Eddington.

It is worth while to quote the beginning of the preface to Eddington's first book; he wrote:

"The purpose of this monograph is to give an account of the present state of our knowledge of the structure of the sidereal universe. This branch of astronomy has become especially prominent during the last ten years; and many new facts have recently been brought to light. There is every reason to hope that the next few years will be equally fruitful;

and it may seem hazardous at the present stage to attempt a general discussion of our knowledge. Yet perhaps at a time like the present, when investigations are being actively prosecuted, a survey of the advance made may be especially helpful".

This extract well describes the sort of survey which it became Eddington's habit to produce at the analogous stage in the development of each of his major investigations. To call these surveys "especially helpful" is very much of an understatement; each has become an indispensable classic in its subject. And Eddington's happy choice of the stages at which to write them means that each is infused with the thrill of recent discovery. His first survey was written "so far as practicable . . . for the general scientific reader". The subsequent ones were more professional, but he supplemented them by delightfully written volumes for the general reader.

Calculus of observations. Much of Eddington's astronomical work naturally involved applications of the calculus of observations. He was recognized as an authority on this subject, and his treatment of it in his lectures at Cambridge had an important influence upon its development. Unfortunately he published no general account of these lectures. But he did expound some of his contributions in isolated papers; special mention may be made of the one which is possibly the most satisfactory existing formulation of the method of Least Squares [*Phys. Soc. Proc.*, 45 (1933), 271–282].

Stellar structure. This phase of Eddington's work started in 1916. Its inception is of much historical interest; for he did not of set purpose apply himself directly to its central problems. As he records, his "investigations originated in an attempt to discuss a problem of Cepheid variation". His examination of this problem, which is one concerning oscillations of a star about a steady state, raised questions regarding opacity, transport of energy by radiation and radiation pressure in a star, which were not answered by existing theories of stellar interiors. Thus he was led to examine anew the problem of the steady state itself. And, moreover, the route by which he approached it must have helped to suggest his first big advance, the realisation that the fundamental process of energy-transport in a star is radiative transfer. This suggestion had been propounded previously by R. A. Sampson in 1894; but in 1916 it was still generally believed that the transport takes place by convection. By a curious cycle of history Eddington was later to be one of the first to consider the possible significance of convective zones in a star which is mainly in radiative equilibrium, and more particularly in connection with the very same problem of Cepheid pulsation which had first led him to the discovery of the importance of radiative equilibrium.

The bulk of Eddington's researches on stellar structure are brought together in his book, *Internal constitution of the stars* (Cambridge, 1926). The fundamental problem is to discover the equilibrium state of a body of gravitating matter of given mass (and, if necessary, of given chemical composition) within which a given total amount of energy is being generated per unit time. There is great fascination in re-reading Eddington's treatment of it, bearing in mind the state of knowledge on the essential physical topics when the work was commenced nearly thirty years ago. The quantum theory of the atom was then in its infancy; this meant, in particular, that the fundamental processes of absorption of radiation and of thermal ionisation were very imperfectly understood. But Eddington succeeded, by an amazingly skilful balance between physical judgment and mathematical simplifications, in producing theoretical stellar models which themselves yielded useful results, when compared with observation, and at the same time could be adapted to the new results of atomic theory as they were being developed. His most astonishing feat by these methods was to derive the form of the dependence of opacity on density and temperature in effective agreement with the subsequent quantum theoretical calculations by H. A. Kramers.

When Eddington embarked upon the work, astrophysicists had virtually no idea of the physical state of the material inside a star like the sun, a "dwarf" star as it was called then, or a star of the "main sequence" in more modern terms, with a mean density of the order of one gramme per cubic centimetre. That was, of course, one of the chief things Eddington had to discover. He had one vital clue from which to start: the existence of "giant" stars of such low mean densities that it was legitimate to assume that their material behaves throughout like a perfect gas. So Eddington first set himself the task of developing a theory of gaseous stars. His expectation was that, if he could bring the calculated properties of such stars into agreement with the observed properties of giant stars, then the divergence between the properties of stars of the main sequence, calculated by the same theory, and their observed properties ought to provide evidence as to how the condition of their material departs from that of a perfect gas.

Now the assumption of a perfect gas, together with certain simplifying approximations, resulted in Eddington's "standard model of a star" (as it is now called) being a particular "Emden polytrope". This is a spherical gravitating distribution in which the relation between the total pressure P and density ρ at any distance r from the centre is of the form

$$P = \kappa\rho^{\gamma} \quad \text{(where } \kappa,\ \gamma \text{ are constants).}$$

Its properties are governed by the Emden equation

$$\frac{d^2u}{dz^2}+\frac{2}{z}\frac{du}{dz}+u^n=0,$$

where $\gamma = 1+1/n$, and r, ρ are proportional to z, u^n. These properties had been very extensively studied by R. Emden in his book, *Gaskugeln* (Leipzig, 1907). Thus the most essential mathematics of the subject was already available.

The central discovery which resulted from this part of Eddington's work was the now famous "mass-luminosity" relation. His theory predicted for a gaseous star a certain relation between its mass and luminosity with a small dependence upon the radius. When the resulting curve was made to fit the data for one typical giant star (Capella), by suitable evaluation of a certain parameter, then the points corresponding to the other giant stars for which there were adequate data also fell on the curve. Thus the primary objective of a satisfactory theory of gaseous stars was apparently attained. But not only so: Eddington found to his astonishment that the points for stars of the main sequence fitted just as well. Prof. Smart writes: "I well remember his excitement in February, 1924, when he came into the Observatory library to tell me that he had just completed the last of the calculations which finally established the general applicability of the mass-luminosity relation beyond all possibility of doubt".

The immediate inference was that the material in stars of the main sequence behaves like a perfect gas in spite of its high density. Eddington saw at once that the explanation lies in the drastic reduction in the size of the atoms composing this material consequent upon its high degree of ionisation at the temperatures he had computed. He and others were able to show that the material must in fact be expected on purely theoretical grounds to behave very closely like a perfect gas at even the greatest densities inside ordinary stars. The theory is not altogether simple because account has to be taken of the strong electric fields around the ions in the gas; Eddington succeeded in showing how this can be done.

It was the very success of the mass-luminosity law which then raised in precise form two new questions. First, it was found that the White Dwarfs, alone amongst known stars, did not conform to this law. These are stars of normal mass but of abnormally low luminosity and excessively high mean density (of the order of one ton per cubic inch). By examining the manner in which the mass-luminosity law failed for these stars, Eddington formulated the peculiar problems they present. These problems were brilliantly solved in 1926 by R. H. Fowler's demonstration that the free electrons in the material of such stars must form a "degenerate gas" in the sense of the then new quantum mechanics.

The second question arose from the fact that the agreement between calculation and observation depended on the value of the constant in the opacity formula. To secure agreement this value had to be taken to be ten times as large as that predicted by atomic theory for material of the assumed composition. Eddington showed at the time that the assumption of a high proportion of hydrogen in the stellar material would remove the difficulty. But he was not then prepared to accept this expedient, and the problem remained outstanding until 1932. By that time, however, the atomic data had been more securely established; also the cosmical abundance of hydrogen had been more definitely recognized. So, in that year, Eddington not only adopted this explanation of the difficulty, but also had sufficient confidence in all his data to use this as a method of computing the hydrogen content of the stars. Results in substantial agreement with his own were obtained independently by B. Strömgren.

We have still to mention the most formidable question of all—that of the source of stellar energy. Throughout the period of Eddington's researches on stellar structure the process of energy-generation was unknown, though there existed a growing conviction, emerging largely from his own discussion, that it must be "sub-atomic" in character. Eddington went boldly ahead using such hypothetical source-distributions (those of his "standard model" and "point-source model") as were suggested by compromises between mathematical convenience and physical plausibility. He believed that thus he could work out a sufficiently reliable picture of the physical conditions under which the actual process of energy-generation must operate. Vindication came in 1938. Laboratory data concerning the transmutation of atomic nuclei had been accumulating, and by then it was feasible to make an exhaustive examination of all *thermo-nuclear* processes which might occur inside a star. Similar conclusions from such an examination were reached simultaneously by H. Bethe and by C. von Weizsäcker. It must suffice here to state that one, and only one, process, that known as the "carbon-nitrogen cycle", is found adequate to account for the rate of energy-generation in the brighter stars of the main sequence including the sun, that it does so under just the physical conditions predicted by Eddington's theory (to within the degree of accuracy possible for the estimates), and that thereby also his computed abundance of hydrogen is confirmed as regards order of magnitude. A different thermonuclear process appears to give equally satisfactory results for fainter stars of the main sequence. The case of giant stars is somewhat less definite, but the progress already made is again consistent with Eddington's findings.

It must be recognized, of course, that our existing body of knowledge concerning stellar constitution is the work of many hands. Yet it may still be claimed that Eddington's fundamental contribution has not been superseded by later developments. Nevertheless, one is bound to record that some of his work has given rise to vigorous controversy. In its early stages critics argued that it must be doomed to fail just because the mode of energy-generation was unknown. But actually it is hard to see in what way the calculation of the steady state of a star would differ in principle from Eddington's when a known process is substituted for his hypothetical energy-sources. The position in regard to the problem of the *evolution* of a star is obviously different, and that is why this has to be regarded as the present preoccupation of theoretical astrophysics.

More recently, E. A. Milne* has linked a profound tribute to Eddington's work with certain penetrating criticisms, not of his methodology, but of his interpretation of the mathematics. One is bound to recognize the force of Milne's criticisms if they are considered in relation to dissected portions of Eddington's work. But one submits, with due respect, that the work ought not primarily to be viewed in dissected portions but in its grand entirety. One then sees it as an edifice resting on many supports: it will not collapse if certain of them prove to be insecure—especially if these particular ones are found to be only remnants of the scaffolding.

Other astrophysical investigations. Among Eddington's other contributions to astrophysics, he made important investigations in the theory of pulsating stars (the problem which had first attracted him to the subject) and of rotating stars. Though his chief work on stellar structure concerned that of the interior, he contributed too to the theory of the outer layers of stars. In work described in his Bakerian lecture of 1926, he also laid the foundations of the theory of interstellar matter.

Astronomical tests of relativity theory. Eddington's theoretical work in relativity is described below. But it was as a practical astronomer that he was responsible for carrying out for the first time one of the three crucial

* E. A. Milne, Presidential Address to the Royal Astronomical Society on "The natural philosophy of stellar structure", 1945 April 13, *Monthly Notices, Royal Astron. Soc.*, 105 (1945), 146–162.

observational tests of Einstein's theory. This was the test of the predicted deflection of light-rays in the gravitational field of the sun, a test which reduces in practice to observing the apparent displacement of stars seen very close to the sun during a total eclipse. F. W. Dyson, then Astronomer Royal, had pointed out that the eclipse of 29 May, 1919, would occur in a particularly favourable field of stars. Two expeditions were sent from this country, one to the Island of Principe (W. Africa) and one to Brazil, expressly to make the observations necessary to test the Einstein effect. Eddington accepted the invitation to lead the Principe expedition, and it is now a matter of scientific history that, as a result of the two sets of observations, Einstein's prediction was verified within the limits of their possible errors. Besides his more formal reports, Eddington has left an attractive sketch of this work in his *Space, time and gravitation* (Cambridge, 1920), Ch. VII.

As an astronomer, though in this case not as an observer, Eddington was also to a considerable extent responsible for the first really convincing application of another of the tests. Einstein had shown that lines in the spectrum of a star should exhibit a displacement towards the red, as compared with the same lines in laboratory spectra, by an amount proportional to M/R, when M, R are the star's mass and radius. The difficulty was to find a star for which the quantity M/R was known and for which it was also sufficiently large to render the effect appreciable. This was where Eddington's earlier interest in White Dwarf stars offered a solution. The faint companion of Sirius was one of only three such stars definitely known at the time; its mass and radius, estimated by standard methods, implied the stupendous density already mentioned. Eddington was one of the few who accepted the latter result, and he pointed out, moreover, that it was one which could be verified by measurement of the relativity red-shift. The difficult observations required were carried out by W. S. Adams at Mt. Wilson and, in Adams's own words, they were found to "confirm Eddington's prediction both as to the remarkable density of matter in White Dwarf stars and as to the test of generalized relativity afforded by them". Adams' cable giving the news reached Eddington while the Congress of the International Astronomical Union was assembled in Cambridge in 1925. He produced quite a considerable amount of excitement by announcing it at one of the meetings.

W. H. McC.

Mathematical physics and philosophy of physics. Since the year 1917, in which he received from de Sitter a copy of Einstein's famous paper on the general theory of relativity, Eddington devoted his energies with a steadily increasing intensity first to relativity, secondly to quantum theory, and lastly to general speculations on the philosophy of physics. Both in relativity and in quantum theory he viewed current expositions with a severely critical eye and did not hesitate to re-organize the entire theoretical structure with a view to obtaining better internal consistency and a more satisfactory agreement with his extremely individualistic philosophy.

Relativity theory. Eddington's investigations into relativity theory opened with the famous "Report" which he prepared for the Physical Society and which, for some time, was the only authoritative exposition of the subject in English. This has now been supplemented by his well-known work, *The mathematical theory of relativity*, which includes an account of later developments both by himself and by other workers, and which concludes with a prophetic passage referring to the great region of unexplored problems of the atomicity of matter and of energy.

Eddington's part in organizing the 1919 eclipse expedition to obtain observational evidence for Einstein's theory has been reported above. On the theoretical side, his investigations centred around Einstein's law of gravitation,

$$G_{\alpha\beta}-\tfrac{1}{2}Gg_{\alpha\beta}+\lambda g_{\alpha\beta}=-(8\pi\gamma/c^4)\,T_{\alpha\beta},$$

in which $g_{\alpha\beta}$ are the coefficients of the fundamental quadratic form for the "interval" ds in space-time, $G_{\alpha\beta}$ is the contracted Riemann tensor, $T_{\alpha\beta}$ is the momentum-energy tensor, while λ, γ and c are the cosmological constant, the constant of gravitation, and the speed of light in vacuo.

At the time when Einstein first put forward his theory, the most that could be said for this law was that in empty space it was the only law which

(1) was covariant for all transformations of the space-time coordinates, and

(2) involved only the tensor $g_{\alpha\beta}$ and its first and second derivatives (apart from the much too restrictive condition of the vanishing of the Riemann-Christoffel tensor, which leads to flat space-time).

The readiness with which this law was accepted was due to the fact that to a first approximation it agreed with Newton's law of gravitation in Euclidean space, while to a second approximation it successfully accounted for an outstanding small discrepancy between observation and planetary theory, and also successfully predicted gravitational influences on light.

The general form of Einstein's law, including the matter-tensor $T_{\alpha\beta}$, had also the great advantage that the curvature tensor on the left-hand side had a divergence which vanished identically. Eddington insisted strongly on this feature, and indeed at times he spoke as if a tensor with zero divergence was a satisfying substitute for gross matter. At the same time, he was among the first to prove that the Einstein tensor is by no means the only space-time tensor with this property; the property is, in fact, shared by any tensor obtained by Hamiltonian differentiation of an invariant integral over a region of space-time.

In the attempt to construct a more satisfying theoretical support for Einstein's law of gravitation, and if possible to incorporate electromagnetism, Eddington devised a wide generalization of Weyl's unified field theory, based on the concept of parallel displacement around a closed circuit. In this research he independently invented non-Riemannian geometry and obtained a remarkable interpretation of Einstein's equations in empty space as the "self-gauging" characteristic of an electron. The original object of this research—the inclusion of the electromagnetic field —was never attained in a really satisfactory way, and, with the subsequent development of Eddington's views on quantum theory, it was abandoned. But the new interpretation of Einstein's law remained as a continued inspiration in Eddington's later work in quantum theory.

Quantum theory. Eddington's researches in quantum theory (apart from an earlier discussion of Planck's radiation law) started with the study of Dirac's relativistic wave equation for an electron. This led him to formulate the theory that electric interactions were fundamentally a manifestation of the Pauli exclusion principle. In developing this principle he introduced the momentum operators corresponding to the ignorable coordinates θ which increase by π when two identical particles are interchanged. The enumeration of the number of "freedoms" in the space in which these operators were effective was a matter of some difficulty, but it was extremely encouraging that the answer seemed to be either 136 or 137, while the fine structure constant was about 1/137 and the "packing fraction" of the nucleus was about 136/137.

By a closer study of the 136 rotations in the space of the identity operators and of the 10 rotations in the space-time of relativity, Eddington developed a theory of the masses of the proton and electron which resulted in the remarkable quadratic equation,

$$10x^2-136x+1=0,$$

for these masses expressed in "natural" units. This equation yields a value of 1847.60 for the ratio of proton-mass to electron-mass, in startling agreement with experimental results. Subsequent refinements of this theory have yielded a result accurate to 1 part in 10,000. Further extensions of these methods—expounded in his *Relativity theory of protons and electrons*—seemed to offer an explanation of the curious 5/2 factor in the experimental value of the spin of the proton.

The theory of almost all of the subsequent investigations is the single particle viewed against the background of the whole universe—a situation recalling the self-gauging electron of relativity theory. Although Eddington's investigations here (and elsewhere) in quantum theory have been received with considerable reservations by mathematical physicists, there can be no doubt of the value and the correctness of his main contention that no problem of quantum theory is rightly formulated unless the "background" is specified and taken into account. The theory of this background—which he later came to call the "uranoid"—is a blend of Einstein's space-time and of Dirac's world of almost completely filled negative energy levels in positron theory. The enumeration of the stationary states in the uranoid led Eddington to propose a definite value for the total number of protons and electrons in the universe, and also to give precise values for such quasi-observational constants as the radius of an Einstein universe, its rate of expansion and the mean density of matter. In estimating these cosmological constants Eddington was as successful as he was with the physical constants connected with the masses and charges of elementary particles*.

All these researches were concerned with what Eddington called the "spin" extension of relativity. In 1943 he opened another line of investigation into the "statistical" relativity theory, in which he analysed the consequences of a systematic application of the principle of uncertainty both to the system observed and to the frame of reference. This theory is, perhaps, reported too briefly in his Dublin lectures to carry complete conviction, but some of the results are so striking that the reader cannot but wish them to be true. In particular there stand out the theory of the non-Coulombian interaction energy and the theory of the equivalence of space-time curvature and of probability effects in the uranoid†.

* Cf. E. T. Whittaker, "Eddington's theory of the constants of nature", *Math. Gazette*, 29 (1945), 137–144.

† A few hours before Eddington entered the nursing home on his last illness, he put the finishing touches to the manuscript of a new book on the whole subject. It combines the methods of "Relativity theory of protons and electrons" with those of his Dublin lectures. The results are apparently very remarkable and the book will be awaited with great interest.

Philosophy of physical sciences. Eddington's views on general philosophical questions related to physical science were given in his lectures on "The nature of the physical world" and on "The philosophy of physical science". His most devoted admirers must confess that both in these official utterances and in his private conversation he left us some very hard sayings. The idea which seemed to dominate his work was that a great part of physics was simply the mode of interpretation which the physicist had imposed on observation, and that this part of physics was, therefore, deducible by pure reason from our knowledge of the psychology of physicists, independently of any experiment. Indeed, at times he spoke as if nature was a wild welter of chance on which human reason imposed the laws of physics, much as a glass prism resolves white light into a regular spectrum of colours.

My personal opinion is that these professions of subjectivism or solipsism were intentional exaggerations, intended to direct attention to the new ideas advanced in his theories and to combat by over-emphasis the self-satisfied simplicity of current physical philosophy. The ideas which were really effective in his work were that the multiplicity of principles invoked in relativity and in quantum theory—such as gauge-variance, Lorentz invariance, uncertainty principle, exclusion principle, electric interaction—are reducible to a much smaller number of principles when they are systematically analysed; that, when they are developed completely and consistently, they determine quite definitely all the natural constants of the world—such as the number of dimensions of space-time, the number of particles in the uranoid, the relations between γ, c, e, h, m, M, etc., in the now familiar notation.

It would be premature and impertinent to conclude this brief and very inadequate notice with any attempt at a final judgment on Eddington's work. The man was ever greater than his work, but *that* was always stimulating and suggestive, opening up new perspectives of future progress. The detailed answers which he gave to the problems sketched above were avowedly incomplete, regarded as pieces of deductive reasoning, and were subject to continual slight revision. But it was in the formulation of the great series of questions which lay behind the magnificent series of his papers that he gave his best gift to his fellow-workers. These questions form, as it were, a great series of signposts leading to a Promised Land which perhaps he never quite attained in this life. We who come after him may, in time, see further than he did, but only because we are like pygmies standing on the shoulders of a giant.

G. T.

The writers are much indebted to the Editors of *The Observatory* for their permission to reproduce the notice by one of us (G. T.), and portions of that by Prof. Smart, both of which have already appeared in that journal. Their thanks are also due to Dr. H. A. Brück and to Prof. W. M. Smart for supplying some of the information which has been used.

List of books by A. S. Eddington.

In this list the books are grouped according to the order in which the subjects have been treated in the preceding notice. The more "popular" books in the second and third groups are marked with an asterisk (*). Some of the books have been translated into other languages for publication abroad.

Stellar movements and the structure of the universe (London, 1914).

The internal constitution of the stars (Cambridge, 1926).
**Stars and atoms* (Oxford, 1927).

Report on the relativity theory of gravitation (London: Physical Soc., 1918).
**Space, time and gravitation* (Cambridge, 1920).
The mathematical theory of relativity (Cambridge, 1923; second ed., 1924).
**The expanding universe* (Cambridge, 1933).
Relativity theory of protons and electrons (Cambridge, 1936).
The combination of relativity theory and quantum theory (Dublin: Inst. for Adv. Studies, 1943).
Fundamental theory (Cambridge, *in press*).

The nature of the physical world—Gifford Lectures, 1927 (Cambridge, 1928).
Science and the unseen world—Swarthmore Lecture, 1929 (London, 1929).
New pathways in science—Messenger Lectures, 1934 (Cambridge, 1935).
The philosophy of physical science—Tarner Lectures, 1938 (Cambridge, 1939).

A bibliography of Eddington's papers is not given here, but it may be of interest to recall that the following were published by this Society:—

"On sets of anticommuting matrices", *Journal London Math. Soc.*, 7 (1932), 58–68.
"On sets of anticommuting matrices; Part II: The factorization of E-numbers", *Journal London Math. Soc.*, 8 (1933), 142–152.
"On Dr. Sheppard's method of reduction of error by linear compounding", ***Proc. London Math. Soc.*** (2), 20 (1922), 213–221.