fifteen years his lectures were the inspiration of a new generation of young lens designers. In 1927 he published the first part of his book, *Applied Optics and Optical Design*, which is the first and only textbook of lens design written with the needs of the complete beginner constantly in mind. It is not too much to say that this remarkable book has been a major contributory factor in the development of new optical-sighting devices during the present war, both in England and America, where it has been eagerly studied by physicists, engineers and astronomers who are unexpectedly obliged to design new devices employing optical systems. The second part of the book was never published, although a considerable part of the manuscript has been written and is in publishable form.

Conrady's interest in lenses began as early as 1893, as a by-product of his desire to possess a fair-sized astronomical telescope. He at once noticed that ordinary objectives invariably yield a smaller star image than simple geometrical theory would indicate, and within a few months he had independently discovered the Rayleigh limit of permissible aberration, based on the conception of differences in the optical path of various rays passing through the lens. He expanded this approach to cover all the aberrations, including chromatic, and published his findings in a series of papers in *Monthly Notices*, 1904–6. After he began formal teaching at the Imperial College he elaborated a more geometrical approach, which he also published in *Monthly Notices* in 1918 and 1919, but his earlier treatment has the greater appeal because of its essential simplicity and fundamental relation to the light-waves themselves. He was awarded the Traill-Taylor medal of the Royal Photographic Society in 1920, his address at that time being entitled *The Present State of Photographic Optics*.

He was elected a Fellow of the Society in 1902, and one of the greatest pleasures of his life was to attend the monthly meetings.

R. KINGSLAKE.

ARTHUR STANLEY EDDINGTON was born on 1882 December 28 at Kendal, Westmorland, the second child and only son of Arthur H. Eddington, the headmaster of the Friends' School in that town. At a very tender age his interest in numbers, a sign of the budding mathematician, began to reveal itself. On the death of his father the family moved to Weston-super-Mare, and there, at Bryn Melyn School, he received his early education. Proceeding to Owens College, Manchester, in 1898 October, he took the first course in physics during his first year. In the following session he entered on the Physics Honours course, and three years later took his degree at the head of the list of the whole Victoria University (then comprising Manchester, Liverpool and Leeds), in a particularly brilliant year. Most of the teaching in physics at that time was given by C. H. Lees, but Eddington also attended the lectures of Lamb and Schuster. He took his part in the social and athletic life of Dalton Hall (one of the Halls of Residence of Owens College) and was a regular member of the 2nd football eleven, occasionally playing for the first. Later, when at the Royal Observatory, he played hockey for the Observatory Hockey Club. Throughout his life he enjoyed cycling; G. C. Simpson relates how one day Eddington returned full of glee from a cycle ride, on which he had used a cyclometer for the first time, saying that while checking the cyclometer against the milestones, he had found himself putting on speed to get to the next stone before the cyclometer recorded the mile!

In 1902 he entered Trinity College, Cambridge, with an entrance scholarship in Natural Science. In the Mathematical Tripos of 1904 he was Senior Wrangler, and in the following year was placed in the first division of the first class of Part II of the Tripos. In 1907 he was Smith's Prizeman, and in the same year he was elected a Fellow of Trinity College. It is probable that he had intended to make physics his life-study, for he had spent the Michaelmas Term, after taking Part II of the Mathematical Tripos, working in the Cavendish Laboratory on the subject of thermionic emission. But in

1906 a vacancy occurred in the post of Chief Assistant at the Royal Observatory, Greenwich, caused by the appointment of F. W. Dyson to succeed Copeland as Astronomer Royal for Scotland. The post was offered by the Astronomer Royal, Sir William Christie, to Eddington and the offer was accepted. Thus Eddington came to Greenwich and astronomy became his first interest. Astronomy is under a great debt of gratitude to Christie for his judicious choice. Eddington would have made his mark in any field of work that captured his interest; fortunate indeed it was for astronomy that the opening at Greenwich presented itself when Eddington was ripe for it.

Eddington spent the next seven years at Greenwich, taking his share in the routine observations. He thus obtained experience in observational astronomy and a familiarity with its problems which were to stand him in good stead. Though his interests were always primarily in theoretical investigations, he was able to appraise the value of observations and to test theoretical conclusions by means of the data provided by observations. While at Greenwich he discussed the observations made with the Airy reflex tube for the determination of latitude variation. This instrument, though ingenious in principle, had given anomalous and disappointing results. As a result of Eddington's critical analysis, it was decided to discontinue these observations and to employ the Cookson floating zenith telescope, loaned by the Cambridge Observatory, for the determination of the variation of latitude and the constant of aberration. This telescope, designed by Bryan Cookson and used by him in a programme of observations at Cambridge, had not given results of the expected precision. Cookson's untimely death had prevented further investigations. Eddington changed the method of observation so that the star trails were photographed only in a small central region of each plate: Cookson had not used this method, though he had considered it as an alternative. A new programme of observation was planned and the preliminary results, discussed by Eddington shortly before he left Greenwich, proved to be highly satisfactory. Observations according to this programme were continued for more than twenty-five years.

Another observational problem with which he dealt was the mode of formation of the envelopes of Morehouse's Comet, based on the measurement of the excellent series of photographs of the comet obtained at Greenwich.

Eddington became interested, soon after he commenced work at Greenwich, in the problem of star-streaming. Kapteyn had announced, a few years previously, his discovery of the two star-streams. He had found that the motions of the stars, after eliminating the solar motion, could not be represented by the hypothesis of random motion, but that the stars tend to move in two favoured directions, the motions in each of these directions being distributed in a haphazard manner. This peculiarity of the individual motions was found to prevail throughout the heavens and was not confined to certain regions. The method of investigation used by Kapteyn was a graphical method based on the shape of the polar distribution curves of the proper motions of the stars in limited areas of the sky; by trial and error, a combination of the two streams was found which gave a satisfactory representation of the distribution of the observed proper motions.

In a paper published in 1907 Eddington treated Kapteyn's hypothesis by an elegant mathematical method, which was simple in practical application, and which enabled the constants of the two streams to be derived. He discussed first the proper motions of the stars in Groombridge's Catalogue, which had been derived a few years previously by Dyson and Thackeray in connection with their new reduction of this catalogue. Then, after the publication of Boss's *Preliminary General Catalogue*, he analysed the proper motions of the stars contained in it, which are distributed over the whole sky. Eddington's thorough and more precise method of discussion confirmed and amplified Kapteyn's conclusions. In 1914 he published his first book, *Stellar Movements and the Structure of the Universe*. This work contained an account of his own researches, but this was made subservient to the wider aim of giving an account of the many recent discoveries in sidereal astronomy and co-ordinating them to present, so far as was possible at the time, a coherent description of the stellar universe. Eddington did not limit himself to what had been definitely proved. In the preface he wrote: "There can be no harm in building hypotheses, and weaving explanations which seem best fitted to our present partial knowledge. These are not idle speculations if they help us, even temporarily, to grasp the relations of scattered facts, and to organize our knowledge". At that time, the opinion was being strongly held that the spiral nebulæ were members of the galactic system and not island universes. Eddington's comment on this was as follows: "If the spiral nebulæ are within the stellar system, we have no notion what their nature may be. That hypothesis leads to a full stop. . . . If, however, it is assumed that these nebulæ are external to the stellar system, that they are in fact systems co-equal with our own, we have at least an hypothesis which can be followed up, and may throw some light on the problems that have been before us. For this reason the 'island universe' theory is much to be preferred as a working hypothesis; and its consequences are so helpful as to suggest a distinct probability of its truth". This quotation illustrates Eddington's general attitude and also his intuition. The final chapter was a first attempt at the study of the dynamics of the stellar system; he showed that the apparent analogy of the stars with a gaseous system of molecules must be rejected and that the stars must be considered as describing paths under the general attraction of the stellar system without interfering with one another. In subsequent papers some further progress was made in the development of the new subject of stellar dynamics.

This book was a great stimulus to the further development of the subjects which it discussed. It raised a number of questions, some of which have since been answered, whilst to others no answer has yet been found. In each of the main fields in which Eddington worked he followed the same plan of publishing a connected account of the new advances, incorporating the work of others. Students and investigators of these fields are greatly indebted to him for the valuable assistance which was thus provided for them.

In 1913 Eddington was elected, at the age of thirty-one, to the Plumian Professorship of Astronomy at Cambridge, which had become vacant through the death of Sir George Darwin. In the following year, after the death of Sir Robert Ball, he was appointed Director of the Cambridge Observatory, where he resided until his death. Almost at once there commenced a long period of most fruitful activity. In 1916 appeared the first of an important series of papers on the radiative equilibrium of the stars, which broke new ground and completely altered the outlook of astronomers on many problems. In the earlier investigations of the distribution of temperature inside a star by Homer Lane, Ritter, Emden and others, it had been assumed that the transfer of heat within the star was brought about by means of convection currents. The theory of radiative equilibrium assumes that the heat is transferred by radiation, so that the temperature distribution is controlled by the flow of radiation. Convection currents may strive to establish a different distribution, but the transfer of heat by radiation is so much more rapid than by convection that radiative equilibrium must provide a very close approximation to the conditions prevailing in the interior of a star. The concept of radiative equilibrium seems to have been first adopted by Sampson in 1894, but the thermodynamics of radiation was not at that time sufficiently developed for the concept to be satisfactorily worked out. In 1906 Schwarzschild had applied the theory of radiative equilibrium to the Sun's atmosphere. Eddington was the first to apply it to the interior of a star. He derived the famous equation of radiative equilibrium which bears his name, and showed that it became integrable when a simple assumption was made. This led to the polytropic sphere with index $n=3$, known as Eddington's model. The theory requires an assumption about the mean molecular weight of the stellar material which Eddington at first assumed to be equal to that of iron. It was suggested to him independently by Newall, Jeans and Lindemann that in stellar conditions the atoms would be highly ionized and that the mean molecular weight must be much lower. In the second paper he therefore adopted a mean molecular weight of 2, which is approximately correct for all elements except hydrogen. He found that the luminosity of a giant star depended mainly on its mass, increasing rapidly with the mass. He pointed out that for the range of stellar masses between about one-tenth and a hundred times the Sun's mass, the radiation and gas pressures were comparable, which appeared to provide an explanation of the comparatively narrow range in observed stellar masses.

Eddington had supposed that the formulæ he had derived were applicable only to gaseous giant stars. In the interiors of stars of the main sequence, with their much higher mean densities, it was expected that there would be an appreciable departure from the perfect gas laws, so that the formulæ would cease to be applicable. On the current theory of evolution, a star began as a giant red star and passed successively through the two branches of the Hertzsprung-Russell diagram. A main sequence star of a given mass should therefore have a lower luminosity than a giant star of the same mass. In 1924 he set out to discover to what extent the dense stars deviated from the luminosities of gaseous stars of the same masses. A careful discussion of all the reliable determinations of stellar masses was made and the unexpected result was obtained that the formulæ of the theory predicted correctly the absolute magnitudes of all ordinary stars, irrespective of whether they were giants or main sequence stars. The explanation was at once suggested by Eddington—that the highly ionized matter in the interior of a star would behave practically like a perfect gas up to very high densities.

This discovery of the correlation between the masses and the luminosities of the stars, applicable equally to the giant and main sequence stars, was a result of outstanding importance. It necessitated a complete revision of currently accepted views of stellar evolution. The two branches of the Hertzsprung-Russell diagram must either represent loci of equilibrium points or, if there was evolution along them, it must be accompanied by appreciable loss of mass. Little was known at that time about atomic nuclear processes, and the alternative possibilities of the source of stellar energy being provided by the annihilation of matter or by the transmutation of hydrogen were discussed.

The white-dwarf stars did not satisfy the mass-luminosity relationship. At the transcendently high densities of these stars an appreciable departure from the perfect gas laws occurred. Eddington came to the revolutionary conclusion that the mean density of 61,000 of the companion of Sirius was not absurd and must be accepted. He pointed out that if this density were correct there should be an Einstein shift of its spectral lines of about 20 km. per sec., which was shortly afterwards confirmed by Adams at Mount Wilson. This provided strong confirmation of the conclusion that in ordinary dwarf stars matter may have the density of platinum and yet behave as a perfect gas, because it is still a long way from the maximum density.

In 1925 Eddington discussed the point-source model of a star. The predicted brightness of a star is decreased by concentrating the source of energy towards the centre; the extreme case is obtained when the concentration is complete, the source of energy being a point-source at the centre of the star. He was able to solve the problem by means of quadratures, and he showed that the influence of the internal density distribution upon the mass-luminosity relation was small. The numerical results obtained emphasized that the general internal conditions determine the surface conditions and not *vice versa*. The latter view, maintained by some investigators, was vigorously opposed by Eddington.

There remained one serious discordance in these investigations which troubled Eddington a great deal. The opacity of the stellar matter, inferred from the mass and luminosity of the star, proved to be ten times greater than that inferred from laboratory data as to absorption of X-rays, coupled with the thermodynamic theory of ionization. Eddington developed a theory of nuclear capture which gave full agreement with the astronomical observations; it was necessarily in conflict with laboratory experiment and was later discarded. Kramers's theory of electron capture, on the other hand, was in agreement with laboratory experiment; Eddington recognized that the discrepancy could be removed by assuming that the stars contain a large proportion of hydrogen, though he was not at first prepared to accept this explanation. But in 1932, after considering the guillotine factor in stellar opacity, he reconsidered this matter and, proceeding in the reverse direction, determined the hydrogen content for a star of known mass, radius, and luminosity, on the assumption of the validity of Kramers's theory. He pointed out that if stars of the same mass differed widely in their hydrogen content the deviations from the mass-luminosity law would be much wider than observed. For a star with the mass of the Sun the luminosity can range over seven magnitudes according to the proportion of hydrogen.

This series of investigations, with the exception of the last, were collected and discussed in detail in the important work entitled *The Internal Constitution of the Stars*, published in 1926. In spite of the great mass of investigations which have been stimulated in the years that have intervened by the pioneer work of Eddington, the book remains a classic of astronomy, which is invaluable to the student. Eddington's physical insight was so discerning that the main structure remains substantially sound after nearly twenty years.

In this book Eddington gives a digression on the different view-points of the mathematician and the physicist which is worth quoting, because it so exactly illustrates his own attitude towards the problems he discussed:

> "I consider that the chief aim of the physicist in discussing a theoretical problem is to obtain 'insight'—to see which of the numerous factors are particularly concerned in any effect and how they work together to give it. For this purpose a legitimate approximation is not just an unavoidable evil; it is a discernment that certain factors—certain complications of the problem—do not contribute appreciably to the result. We satisfy ourselves that they may be left aside; and the mechanism stands out more clearly, freed from these irrelevancies. This discernment is only a continuation of a task begun by the physicist before the mathematical premises of the problem could be stated; for in any natural problem the actual conditions are of extreme complexity and the first step is to select those which have an essential influence on the result—in short, to get hold of the right end of the stick. The correct use of this insight, whether before or after the mathematical problem has been formulated, is a faculty to be cultivated, not a vicious propensity to be hidden from the public eye. Needless to say the physicist must if challenged be prepared to defend the use of his discernment; but unless the defence involves some subtle point of difficulty it may well be left until the challenge is made.
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> "I suppose that the same kind of insight is useful to the mathematician as a tool; but he is careful to efface the tool marks from his finished products—his proofs. He is content with a rigorous but unilluminating demonstration that certain results follow from his premises, and he does not generally realize that the physicist demands something more than this. For the physicist has always to bear in mind a thousand and one other factors in the natural problem not formulated in the mathematical problem, and it is only by a demonstration which keeps in view the relative importance of the contributing causes that he can see whether he has been justified in neglecting these. As regards rigour, the physicist may well take risks in a mathematical deduction if these are no greater than the risks incurred in the mathematical formulation. As regards accuracy, the retention of absurdly minute terms in a physical equation is as clumsy in his eyes as the use of an extravagant number of decimal places in arithmetical computation.
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> "Having said this much on the one side we may turn to appreciate the luxury of a mathematical proof. If the results obtained do not agree with observation the fault must assuredly lie with the precision assumed. The mathematician's power of narrowing down the possibilities supplements the physicist's power of

picking out the probabilities. If space were unlimited we might try to duplicate investigations where necessary so as to satisfy both parties. But if one investigation must suffice, I do not think we should usually give way to the mathematician. Cases could be cited where physicists have been led astray through inattention to mathematical rigour; but these are rare compared with the mathematician's misadventures through lack of physical insight.

"The point to remember is that when we *prove* a result without understanding it—when it drops unforeseen out of a maze of mathematical formulæ—we have no grounds for hoping that it will apply except when the mathematical premises are rigorously fulfilled—that is to say, never, unless we happen to be dealing with something like æther to which 'perfection' can reasonably be attributed. But when we obtain by mathematical analysis an *understanding* of a result—when we discern which of the conditions are essentially contributing to it and which are relatively unimportant—we have obtained knowledge adapted to the fluid premises of a natural physical problem.

"I think the idea that the purpose of study is to arrive at a string of proofs of propositions is a little overdone even in pure mathematics. Our purpose in studying the physical world includes much that is not comprised in so narrow an ideal. We might indeed say that whereas for the mathematician insight is one of the tools and proof the finished product, for the physicist proof is one of the tools and insight the finished product. The tool must not usurp the place of the product even though we fully recognize that disastrous results may occur when the tool is badly handled".

The Internal Constitution of the Stars contained the results of three other related investigations. In 1918–19 Eddington had published two important papers dealing with the problem of the Cepheid variable stars, on the hypothesis that their light variations were caused by periodic pulsations. He was able to account for the period-luminosity relationship obeyed by these stars, which had been found by Miss Leavitt at Harvard from the study of Cepheid variables in the Magellanic Clouds. These papers have formed the basis of all later work on the subject. Eddington returned to the subject again in 1941 in an attempt to account for the phase retardation of approximately one-quarter period between the flow of heat in the main part of the interior of the star and the outflow of heat from the surface. He pointed out that the period is sensitive to changes of density and the luminosity to changes of mass; the period-luminosity relationship is, consequently, in effect a mass-density relation. This implies that for any given mass there must be a narrow range of density within which the star is pulsatorily unstable. The most probable transient modification was a sharp drop in the ratio of the specific heats when the predominant element was at the mid-stage of an ionization. This possibility he had previously considered but the predominant element was then presumed to be a heavy element, such as iron, and the assumption was found to lead to results not in accordance with observation. But the substitution of hydrogen as the most predominant element brings the critical region from the deep interior to near the surface. In the convective layer of the star, however, the equilibrium calculations of ionization and excitation break down. These papers were criticized by M. Schwarzschild, who showed that the deviation from ionization equilibrium in the hydrogen convection zone was less than Eddington had supposed. In reply, Eddington maintained that a simple explanation of the quarter-phase retardation could be provided if it were supposed that the convection zone was much deeper than had been previously assumed and that there is a large difference in the state of ionization of the upward and downward streams.

The second investigation, in 1926, was concerned with the reflection effect in eclipsing variables. If the components of an eclipsing variable are sufficiently close, the reflection by the faint star of the light of the bright star causes the hemispheres turned towards and away from the bright component to be unequal in brightness. This effect, in combination with the spherical distortion of the two stars, gives rise to variations in light between eclipses.

Eddington's discussion was supplemented, at his suggestion, by Milne shortly afterwards, who inquired into the problem of the distribution of the reflected radiation in direction, *i.e.* of the law of darkening of the disk for reflected radiation. The discussions by Eddington and Milne have satisfactorily solved this problem.

The third investigation dealt with diffuse matter in interstellar space, which had formed the subject of the Bakerian Lecture of the Royal Society in 1926. This was an important contribution to the understanding of the nature of the stellar clouds. He found that ionization and capture form the main processes of interchange between radiant energy and atomic kinetic energy in diffuse gas, and that this tended to raise the temperature to the level of the effective temperatures of the stars, independently of the dilution of radiation. The relative abundance of sodium to calcium was found to be very much greater than on the Earth; the tremendous preponderance of hydrogen over all other elements was not realized at that time, and Struve later showed that the discordance was much reduced when it was assumed that hydrogen supplied the overwhelming majority of the free electrons. Eddington concluded that the stationary sodium and calcium lines in the spectra of early-type stars were produced by absorption by the interstellar cloud. He pointed out that electron scattering does not cause reddening, so that Shapley's conclusion that interstellar space is nearly transparent because there is no reddening of distant clusters was invalid; the dimming of distant stars by interstellar matter could not be accounted for unless it is assumed that interstellar space contains non-gaseous (meteoric) matter. This conclusion has since been amply confirmed.

Stars and Atoms, published in 1927, gave a fascinating and graphic description of the investigations of stellar interiors in a form intelligible to the general reader. Eddington's popular writings did great service to science in expounding to the layman the meaning and implications of the new developments in astronomy and physics. They were written in a clear and elegant prose, with many graphic analogies, and had a charm and individuality of their own. Many passages could be quoted in illustration, but one from *Stars and Atoms* picturing the conditions inside a star must suffice:

> "We can now form some kind of picture of the inside of a star—a hurly-burly of atoms, electrons and æther-waves. Dishevelled atoms tear along at a hundred miles a second, their normal array of electrons being torn from them in the scrimmage. The lost electrons are speeding a hundred times faster to find new resting places. Let us follow the progress of one of them. There is almost a collision as an electron approaches an atomic nucleus, but putting on speed it sweeps round in a sharp curve. Sometimes there is a side-slip at the curve, but the electron goes on with increased or reduced energy. After a thousand narrow shaves, all happening within a thousand-millionth of a second, the hectic career is ended by a worse side-slip than usual. The electron is fairly caught and attached to an atom. But barely has it taken up its place when an X-ray bursts into the atom. Sucking up the energy of the ray, the electron darts off again on its next adventure.
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> "I am afraid the knockabout comedy of modern atomic physics is not very tender towards our æsthetic ideals. The stately drama of stellar evolution turns out to be more like the hair-breadth escapades on the films. The music of the spheres has almost a suggestion of—jazz.
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> "And what is the result of all this bustle? Very little. The atoms and electrons for all their hurry never get anywhere: they only change places. The æther-waves are the only part of the population which accomplish anything permanent. Although apparently darting in all directions indiscriminately, they do on the average make a slow progress outwards. There is no outward progress of the atoms and electrons; gravitation sees to that. But slowly the encaged æther-waves leak outwards as through a sieve. An æther-wave hurries

from one atom to another, forwards, backwards, now absorbed, now flung out again in a new direction, losing its identity, but living again in its successor. With any luck it will in no unduly long time (ten thousand to ten million years, according to the mass of the star) find itself near the boundary. It changes at the lower temperature from X-rays to light-rays, being altered a little at each re-birth. At last it is so near the boundary that it can dart outside and travel forward in peace for a few hundred years. Perhaps it may in the end reach some distant world where an astronomer lies in wait to trap it in his telescope and extort from it the secrets of its birthplace".

Eddington was a man of wide reading, with a retentive memory for the apposite quotation. The chapters of several of his books are headed by quotations and in his popular writings quotations are frequent. He drew upon an astonishingly wide range of authors and his quotations are always most apt. Quotations from Shakespeare are frequent; one in the Preface to *The Internal Constitution of the Stars* may be recalled:

"The reader will judge for himself whether solid progress has been made. He may, like Shakespeare, take a view less optimistic than my own—

> The heaven's glorious Sun
> That will not be deep-searched with saucy looks;

but I hope he will not be so unkind as to continue the quotation—

> Small have continual plodders ever won
> Save base authority from others' books".

In his days at Greenwich, Eddington had been a member of a small private Society, called "The Elizabethans", which was closely connected with the Observatory. The members met monthly during the winter months in each other's homes to read one of Shakespeare's plays, the various characters being allotted to different members.

Concurrently with the investigation of stellar interiors, Eddington had also been occupied with the theory of generalized relativity. The First World War prevented the transmission of Einstein's papers to this country for some time after his discovery of the theory in 1915. Eddington had received a copy from de Sitter in 1917 and was the first Englishman to appreciate its importance and to accept it unreservedly. His *Report on the Relativity Theory of Gravitation*, prepared for the Physical Society in 1918, provided the first account of the new theory in the English language and did valuable service in bringing the theory to the notice of British men of science. Many were converted to it; but many others, because of its revolutionary conceptions and the employment in its mathematical development of the tensor calculus, with which physicists and applied mathematicians were not generally familiar, were inclined to suspend judgment. The theory had accounted for the unexplained motion of the perihelion of Mercury, but further observational confirmation was needed to convince the sceptics. It was pointed out by Sir Frank Dyson that the total eclipse of the Sun on 1919 May 29 would provide a particularly favourable opportunity for testing Einstein's prediction of the amount of the deflection of rays of light by the Sun. It was decided to proceed with the necessary preparations at Greenwich for two expeditions to Brazil and the island of Principe, in the hope that the war would come to an end in time for the expeditions to set out. Davidson and Crommelin were to be the members of one party, to make observations in Brazil, and because many astronomers were in the Forces or engaged on war work, Eddington agreed to lead the other party, taking with him Cottingham, the horologist, to make observations in Principe. The war fortunately ended in time for the expeditions to set out and both parties made successful observations. At Principe clouds interfered with the observations so that only two plates showed sufficient stars to enable a determination of the deflection to be made: the result from these plates was $1''\cdot61 \pm 0''\cdot30$. The conclusion from the results of the two expeditions was that Einstein's prediction of the amount of the deflection was confirmed, and greatly helped to secure a wide acceptance of the theory. Before the expeditions set out it was suggested that three results were possible: the deflection might prove to be in accordance with Einstein's prediction; it might be half this amount, as would be expected on Newtonian theory because light-waves carry momentum; or it might be nil. Eddington had so unreservedly accepted the theory that he was convinced that satisfactory observations could lead only to the first conclusion; in discussing these three possibilities, he remarked that the most difficult result to account for would be the second.

It may be mentioned here that the most satisfactory confirmation of Einstein's third prediction, the displacement of spectral lines, has since been provided by the measurement of the displacement of the spectral lines of the companion of Sirius, referred to above, which was closely tied up with Eddington's own work.

In *Space, Time and Gravitation* (1920) Eddington gave a non-mathematical account of the theory, to which he prefixed the very apposite quotation from "Paradise Lost":

Perhaps to move
His laughter at their quaint opinions wide
Hereafter, when they come to model heaven
And calculate the stars: how they will wield
The mighty frame: how build, unbuild, contrive
To save appearance.

In 1923 he followed this with *The Mathematical Theory of Relativity*, which included an account of his own original investigations. The most notable of these, published in 1921 in *Proc. Roy. Soc.*, introduced a new generalization of geometry, founded on the idea of parallel-transport, which he put forward not as the actual geometry of space and time but as the geometry of world-structure, the common basis of space and time and matter. Quite apart from the physical applications which were most prominent in Eddington's mind, this work has proved of fundamental significance for the purely mathematical theories of non-Riemannian geometry.

He himself attached great importance to an interpretation of General Relativity which he set forth in two short papers in the *Phil. Mag.* of 1921 November and 1922 January. Starting from the principle that there is no such thing as absolute length (so that to say a length is constant merely means that its ratio to some other length is constant) he inferred that what we call a metre at any place and in any direction is always a constant fraction of the radius of curvature of space-time associated with that place and direction. Hence the Riemann curvature at a point of space-time must be the same for all orientations, and must be the same for all points of space-time. But the conditions for this are

$$K_{pq} = \lambda g_{pq} \qquad (p, q = 0, 1, 2, 3),$$

where K_{pq} is the contracted curvature-tensor, g_{pq} is the metrical tensor and λ is a constant: and these are no other than Einstein's equations for the gravitational field in empty space. Thus Einstein's law asserts the homogeneity and isotropy of space-time as regards Riemann curvature; but the mystery of the homogeneity and isotropy disappear when we realize that it is not intrinsic in the external world but in the measurements we make of the world: the law simply expresses the fact that our survey of the world is made by instruments which are themselves part of the world. With this new insight came the conviction, which dominated the rest of his life, that the true foundation of physics must be in epistemology—the theory of knowledge.

The years from 1928 onwards were occupied chiefly in applying the epistemological principles to the creation of a new and highly original fundamental theory, by means of which he succeeded in deriving theoretically the exact numerical values of all those constants of nature which are pure numbers. There has been considerable misunderstanding about the character of this series of investigations; it has sometimes been supposed (erroneously) that Eddington claimed to have derived the whole of physics, or a large part of it, by pure ratiocination, without depending in any way on the results of observation and experiment. Such an idea is refuted at once by an examination of the papers themselves. The position may be illustrated by reference to the history of an older problem. The ancient Egyptians were acquainted with the fact that the ratio of the area of a circle to the square on its radius was independent of the size of the circle: and for this number, which we denote by π, they found, by actual measurement, the value $\frac{256}{81}(=3{\cdot}16\ldots)$. In the third century before Christ, Archimedes showed that the number can be found to any desired degree of accuracy by pure theory, without the necessity for making measurements. For this purpose he assumed the axioms and propositions of geometry as they had been set forth in the preceding generation by Euclid: so that what Archimedes did was to assume the *qualitative* part of geometry and to deduce a *quantitative* aspect of it, namely the number π.

Now Eddington is simply the modern Archimedes. He regarded himself as at liberty to borrow anything in *qualitative* physics—he did in fact assume the identity of mass and energy, the theory of the energy-tensor and the interpretation of its elements, the Pauli exclusion principle and other propositions of the most advanced physical theory—but he did *not* assume any *number* determined *empirically*: and he deduced the *quantitative* propositions of physics, *i.e.* the exact values of the pure numbers that are constants of nature—the numbers that are analogous to the number π in geometry.

One can imagine the protests of those contemporaries of Archimedes who had been accustomed to find π by measuring circles, when he took the bread out of their mouths by his mathematical evaluation: and a similar outcry was raised by certain experimental physicists against Eddington, who, it must be admitted, sometimes played into their hands by using language which was liable to be misunderstood. For instance, he obtained for "the number of particles in the universe" a value which, his critics complained, could never be checked by observation; actually a study of his papers shows that the number N thus referred to is carefully defined and has a value which can be determined independently of Eddington's work by a combination of orthodox relativity-theory and astronomical measurements. The definition depends on the concept of an *Einstein world*; that is, a uniformly curved space filled with matter of uniform density in which the mutual gravitational attraction of its particles is exactly balanced by their mutual repulsion due to the cosmical term in General Relativity; N is then simply the number of particles in an Einstein world formed of hydrogen.

Eddington's original method (1931) of finding N theoretically was heuristic in character and has been superseded by his later work, but it is of great interest as revealing the psychological process of discovery. The idea was that in the ordinary Dirac wave-equation for an electron moving in the electrostatic field due to a fixed electron, the term which involves the mass of the electron is really due to the existence of all the other particles in the world; whence he showed that it must have the form $\sqrt{N}/R$, where R is the radius of the Einstein world. Thus he obtained the equation

$$\sqrt{N}/R=(mc^2)/e^2,$$

where m is the mass and e the charge of an electron, and c is the velocity of light. Combining this with the equation

$$\kappa\frac{Nm_p}{c^2}=\tfrac{1}{2}\pi R$$

(where κ is the Newtonian constant of gravitation and m_p the mass of a proton), which is derived from the theory of the Einstein world, it is clear that N and R can both be determined. Substituting the value of R in the theoretical expression for the rate of cosmical recession, Eddington obtained for the latter quantity the value 527·8 km. per sec. per megaparsec., which agreed fairly well with the observed recession. Later he found more accurate ways of calculating the recession from the constants of atomic physics.

A connected account of the series of investigations in which the theory had been developed from 1928 was given in 1936 in his book *Relativity Theory of Protons and Electrons*: it was based almost wholly on what he called the "spin extension" of relativity theory. After this he approached the same problems by a different method, which he called the "statistical extension" of relativity theory: an account of this was given in his Dublin lectures, *The Combination of Relativity Theory and Quantum Theory*, in 1943. At the time of his death he was engaged in writing a book, the completed portion of which is now in course of publication, in which both methods were combined.

The qualitative physical theory which Eddington assumed as the basis of his researches was not in all respects identical with that most widely accepted at the present time; for instance, he refused to believe that the meson observed in cosmic rays is identical with the theoretical meson introduced by Yukawa to explain the forces between the heavy particles in the nucleus. His theory of the nucleus, again, was distinctive: from general principles he derived two different types of binding between a proton and an electron, one the ordinary binding by a Coulomb force, and the other a "co-spin" binding which can unite the proton and electron into a neutron: the nucleus he conceived not as an aggregate of protons and neutrons, but as an aggregate of protons and electrons, the latter being united by co-spin binding not to individual protons, but to the whole aggregate of them.

He went further than any other investigators in getting rid of the old ideas of space, time and particles as the basis of physics: thus in his work on the hydrogen atom, instead of picturing it as an electron circulating round a proton like a planet round the Sun, he devised a totally different representation, analysing it into an "extracule" and "intracule", which correspond roughly to the symmetric and skew parts of the wave function.

Just as Newton's discovery of gravitation was founded on recognizing that the force causing an apple to fall from a tree was identical with the force restraining the Moon in her orbit, and just as Einstein's discovery of general relativity was founded on recognizing that the gravitational properties of mass are identical with its inertial properties, so Eddington's theory is founded on a belief in the essential identity of forces which in current physics are regarded as distinct—*e.g.* he identifies gravitation with the exclusion principle, and Coulomb-interaction with interchange-energy.

In formulating the quantitative results of the theory, it is to be remembered that experimental measures are expressed in the three traditional units gram-centimetre-second, which have no relation to any theory, and it was therefore necessary for Eddington to select three measured quantities to be used as conversion constants: he chose the velocity of light, the Rydberg constant and the Faraday constant. Combining these with his theoretical determinations of the purely numerical constants of nature, he obtained values for the charge of the electron, the Planck constant, the constant of gravitation, the speed of recession of the galaxies, the forces between the protons in the nucleus, the masses of the electron, the proton, the neutron, and the cosmic-ray meson, the mass-defects of deuterium and helium, the separation-constant of isobaric doublets, the lifetime of the cosmic-ray meson, and the magnetic moments of the hydrogen atom and the neutron. In practically all cases the values so found agree with the observed values within the margin of observational error. It was not without justification that in his latest writings, in which his work is presented as a unified whole, he adopted for it the name *Fundamental Theory*.

Eddington's views on general philosophical questions related to physical science were given in *The Nature of the Physical World* (1928), being the Gifford Lectures for 1927; *New Pathways in Science* (1935), being the Messenger Lectures at Cornell for 1934; and the *Philosophy of Physical Science*, being the Tarner lectures at Trinity

College, Cambridge, in 1938. In these books he dealt with the new developments in science—the theory of relativity, quantum theory, the principle of indeterminacy, the expansion of the universe, etc.—and with their effect on philosophical thought. The books were essentially concerned with the question, What kind of knowledge does science give us? He showed that in dealing with the universe, science is confined to investigating its structure; it can tell us nothing of the nature of that which possesses that structure. It was not so much the particular form that scientific theories have now taken that is important, for they may in time give way to some fuller realization of the world, as the movement of thought behind them changes. Whatever changes may come, it will never be possible to go back to the old outlook. Mention may also be made of a small popular book, *The Expanding Universe* (1933), which gave an account of the phenomena to be expected in a finite expanding spherical universe, of the type first suggested by Einstein and later developed by the Abbé Lemaître. These lucid books did more than any other books to make the intelligent layman aware of the new trends in science and of their philosophical implications. They did not escape considerable criticism from philosophers, but Eddington was always a fighter for his ideas and was a redoubtable opponent in argument: he hit back vigorously. The philosophers, for the most part, lacked the scientific knowledge to appreciate his point of view. There is little doubt that the philosophical implications of recent developments in scientific thought will compel philosophers to reconsider many of the views that have been held in the past.

Eddington was elected a Fellow of the Royal Society in 1914 and was awarded its Royal Medal in 1928. He was elected a Fellow of the Royal Astronomical Society in 1906, was its President during 1921–23, a period which included its centenary celebrations, and its Foreign Secretary from 1933; he was awarded its Gold Medal in 1924. He was awarded the Bruce Gold Medal of the Astronomical Society of the Pacific in 1924. He was President of the Physical Society during 1930–32. He received honorary doctorates from twelve universities, and was honorary member, foreign member or foreign associate of many learned societies in Europe and America. He was created a Knight Bachelor in 1930 and received the Order of Merit in 1938. He had friends in many parts of the world, for he had travelled and lectured widely, attending overseas meetings of the British Association, Congresses of the International Astronomical Union, and paying frequent visits to the Continent and to America. He was a true ambassador of science and contributed greatly to the cause of international co-operation in science. At the last General Assembly of the International Astronomical Union in 1938 he was elected President; his guidance at a time when the Union will have complex and difficult problems to face will be sadly missed.

Though Eddington was primarily an astronomer, much of his work was intimately bound up with the most modern developments in physics. In these days of specialization in science it is given to few to have so wide a range of interests and to make contributions of outstanding merit in such diverse fields as he did. He combined to a unique degree an appreciation of the significance of new developments with great powers of mathematical analysis and keen physical insight. A gifted expositor of the newest trends in physics, he was able to describe the most abstruse theories in clear and simple language. His death on 1944 November 22, at the comparatively early age of sixty-one, was a calamity for science; he was still in the zenith of his intellectual powers and his work was not finished. Many in Great Britain and abroad mourn the passing of a friend; their sense of loss is shared by many others in all parts of the world who have admired his achievements and have received instruction and stimulus from his writings. But his work will endure and his name will be enrolled amongst the greatest men of science whom this country has produced.

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