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For this list of papers and many of the biographical details I am most indebted to Prof. A. J. McConnell, of Trinity College, Dublin.

RALPH HOWARD FOWLER

E. A. MILNE*.

Sir Ralph Fowler, Plummer Professor of Mathematical Physics at Cambridge, died on 28 July, 1944, at the age of fifty-five, as the result of an illness which first attacked him in 1938. Though he struggled valiantly, too valiantly, against it, its ravages prematurely aged him and he was carried off at what might have been the height of his powers. Pure mathematician, applied mathematician in such differing branches as ballistics and quantum theory, statistical mechanics and physical chemistry, proficient at cricket and more than proficient at golf, a man of affairs as well as a pure scientist, and a faithful servant to more than one government department, he was distinguished for the versatility and quickness of his mind, for his power of immediately coming to close grips with any problem presented to him, for his rapidity of decision and his sagacity of judgment, and above all for the part he played in developing the school of mathematical physics at Cambridge. Fowler was a big man, both physically and in the world of science; he was a hard hitter, in every sense of that phrase; he was a man of winning charm, who inspired devoted friendships; and his friends in both official and non-official circles had foreseen a future for him even more full than the career which was so tragically closed.

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* Abridged from the *Obituary notices* of the Royal Society.

Ralph Howard Fowler was born on 17 January, 1889, at Fedsden, Roydon, Essex, the eldest son of Howard and Ena Fowler. His father, Howard Fowler, was the second son of William Fowler, of Moor Hall, Harlow, Essex; he had been educated at Clifton and at New College, Oxford, where he was an exhibitioner, and he had played cricket for Oxford University for three years, and Rugby football for three or four seasons, also representing England at Rugby. Ralph evidently inherited his general athletic ability from his father.

In 1902, at the age of thirteen and a half, he was elected to a scholarship at Winchester. He remained there till 1908. He became Prefect of Hall (as the Head of the School is called), and played in the cricket match against Eton in his last year.

In December, 1906, Ralph was elected to a Major Scholarship at Trinity College, Cambridge, and he went up to Trinity in the Michaelmas Term of 1908. Here he read mathematics, taking a first class in the Mathematical Tripos, Part I, in 1909, and becoming a wrangler in Part II in 1911, with a mark of distinction in Schedule B. He became B.A. in 1911, M.A. in 1915. He was awarded a Rayleigh Prize for Mathematics in 1913.

He obtained his blue for golf (or, to be accurate, in those days a half-blue) by playing in the Cambridge four against Oxford in 1912, having also played in many matches in 1911.

After taking his degree he began to research on problems of pure mathematics. He was attracted to problems of the behaviour of solutions of second-order differential equations, namely of the form "near infinity" of real, continuous solutions of such equations. An early paper in the *Quarterly Journal of Mathematics*, edited by J. W. L. Glaisher, dealt with cubic transformations of Riemann's P-function, and seems to have been inspired by work of E. W. Barnes, the present Bishop of Birmingham, whose lectures on differential equations Fowler must have attended as an undergraduate. Further papers appeared, in the *Quarterly Journal* and in the *Proceedings of the London Mathematical Society*, on the classification of the asymptotic behaviour of the solutions; and there was a paper on the set of points $(\lambda_n \theta)$, where θ is an irrational, inspired by work of Hardy and Littlewood. Fowler was subsequently to make great use of his early experience in these fields, firstly in work done in collaboration with C. N. H. Lock, also on approximate solutions of certain differential equations, secondly in work done in 1930 in connexion with investigations on stellar structure by the present writer, who was studying solutions of Emden's differential equation

$$\frac{1}{\xi^2}\frac{d}{d\xi}\left(\xi^2\frac{d\theta}{d\xi}\right) = -\theta^n.$$

The origin of this work is perhaps worth relating in detail. Eddington had shown that the equilibrium of a gaseous star was given by the above equation with $n = 3$, and had deduced, *from equilibrium considerations only*, a formula for the luminosity of the star in terms of the mass. I was puzzled as to what happened when the rate of generation of energy did not have this particular value. Clearly, if the star were generating a great deal too much energy, it would blow up by radiation pressure. What happened when it generated too little, or only slightly too much? The answer came into my mind as a sudden intuition: the star must cease to be gaseous and *Emden's differential equation must have other solutions.* I at once wrote to N. Fairclough, asking him to undertake some new numerical integrations with non-Emden boundary conditions, integrating from the outside inwards; and discussed the matter with T. G. Cowling. The results of both these investigators arrived at the same time and gave the hint to the hitherto incompletely known behaviour of the non-Emden solutions in their dependence on boundary conditions. I communicated Fairclough's numerical quadratures to Fowler, who immediately saw their theoretical significance. They gave him the clue which had been missing in his post-graduate researches, and he at once devised a beautiful and at the same time rigorous method of handling and classifying the solutions of Emden's equation for different exponents n and for all types of boundary conditions. The results were published by the Royal Astronomical Society in its *Monthly Notices*, and had a considerable influence in helping to classify the possible configurations of gaseous and partly gaseous stellar configurations. Later Fowler extended the work to more general equations, and published the resulting theorems in the *Quarterly Journal* in its revived Oxford form.

To return to his career: he was elected, as a result of this work in pure mathematics, to a Trinity Fellowship in October, 1914. By then the first German war had broken out, and Fowler obtained a commission in the Royal Marine Artillery.

Before passing on to Fowler's war career, I may refer to his remaining contribution to pure mathematics, his tract on "The elementary differential geometry of plane curves", which was published as a *Cambridge Tract in Mathematics* (No. 20) in 1920, and reached a second edition in 1929. This put into modern rigorous form ground usually covered in French *cours d'analyse* in their chapters on geometry, and contained some original work in what might be thought to be a well-trodden field.

Fowler took part in the ill-fated Gallipoli campaign as a gunner officer in the Marines, and was badly wounded in the shoulder. Whilst he was convalescing in 1916 (he was then a First Lieutenant, R.M.A.), A. V. Hill, then a Captain in the Cambridgeshires, who had just ceased to be a Fellow of Trinity and become a Fellow of King's, was inventing, in conjunction with Horace Darwin, of the Cambridge Scientific Instrument Company, the instrument later known as the Darwin-Hill Mirror Position Finder—an instrument originally designed to record the flight of aeroplanes. Hill proposed to Fowler that the two of them should combine to test this instrument in the field, and thus began R. H. Fowler's interest in mathematical physics. This association with A. V. Hill was indirectly to alter the whole course of Fowler's interests and studies.

Hill, Fowler and I were in theory the servants of the Munitions Inventions Department of the then Ministry of Munitions, but Hill saw to it that he had largely a free hand, not greatly impeded by bureaucratic interference. We were soon joined at Northolt by W. Hartree and A. C. Hawkins. During this Northolt period, Hill and Fowler wrote a famous secret pamphlet, "M.I.D. 2", on the working and errors of the mirror position finder. The instrument soon came to be used for testing readings of height finders, and Hill's party moved to the National Physical Laboratory at Teddington sometime in May, 1916. There they were joined by W. Hartree's son, D. R. Hartree, and H. W. Richmond.

It now occurred to Hill that the principal role of the instrument would be to make accurate observations on high-angle shell-bursts produced by firing anti-aircraft guns. J. E. Littlewood, then a Second Lieutenant in the gunners attached to the Ballistic Office at Woolwich, had devised a rapid and powerful method of calculating high-angle trajectories avoiding the painfully long process of "small arc" integration, by determining first the time-height relation for "vertical fire" and using this, combined with a use of Siacci-formulae for fire on the flat, to determine trajectories at any angle. But no one knew whether projectiles really followed Littlewood's trajectories or not. Hill induced the then Ordnance Committee to let him take his mirrors and party to Gorleston-on-Sea, to observe some range-table firings from a 6-inch A.A. gun mounted on a monitor on the river there. After some delays due to unsatisfactory weather, adequate observations were obtained, and to the astonishment and joy of all concerned, the observed positions of the shell-bursts fell exactly on Littlewood's trajectories, at the correct time-markings, within the very small errors of observation.

Hill next persuaded the Ordnance Committee to stage extensive trials of various calibres of A.A. guns at Eastney, Portsmouth, under the general supervision of the Experimental Department of H.M.S. *Excellent*; and the party moved in August, 1916, to H.M.S. *Excellent's* headquarters on Whale Island, where they remained until the close of the war. The gun-trials became more comprehensive, including fuse-trials, and a larger staff was recruited. There came to join Hill and Fowler in due course the mathematicians G. T. Bennett, T. L. Wren, R. A. Herman, C. E. Haselfoot, D. R. Hartree's brother, C. W. Hartree, J. G. Crowther, W. E. H. Berwick, W. R. Dean, S. Pollard, the physicist C. Mayes, and many others. The party was known as the Anti-Aircraft Experimental Section of the M.I.D., and Fowler became its Assistant Director, resident at Portsmouth, while Hill, as Director, though frequently visiting Portsmouth, made contacts in London with service needs. Hill became a brevet Major, Fowler a Captain, R.M.A. The two fitted like hand and glove. Hill was the inspirer of most of the researches that were conducted, whilst Fowler executed them in the field. It was a most friendly community—sometimes bullied by Fowler (though he always assuaged its feelings afterwards), and then released from its tension by a visit from Hill. But what is significant to record in a short biography of Fowler is the important scientific influence which Hill had on him, and he in turn on the other members of the section. At directing research, both were superb; to behold the way they set about a new problem, often in a new terrain with new material, to see the way they made inferences and came to conclusions and sound judgments and to take part in it all, was far better training than most universities can offer to aspirants in research.

After the war proper was over in 1918, Fowler, with the aid of E. G. Gallop, C. N. H. Lock and H. W. Richmond, while still connected with Portsmouth, designed an elaborate series of experiments on firing projectiles through "jump cards", and determining the spatial orientation of the projectile, as it passed through them, from the holes it made; the object was to deduce the aerodynamic forces and couples (apart from direct air-resistance) on the projectile as it progressed and spun. The results were eventually published in two papers in the *Philosophical Transactions of the Royal Society*, and by-products of them elsewhere. The *Philosophical Transactions* papers became classical, and have had a marked influence on research in ballistics during this present war, both here and in Canada and the U.S.A. Another piece of war-time research resulted in a joint paper (with the present writer) in the *Proceedings of the Royal Society*, on "Siren harmonics and a pure-tone siren".

It was always tremendous "fun" to collaborate with Fowler. Usually it was not he (to be quite just) who produced the first original idea; but he tossed it back at lightning speed, and you had to be very agile to field it properly. He had a power of throwing himself unrestrainedly and wholeheartedly into other people's interests which is rarely exhibited; once a thing had been expounded to him and been subjected to his searching cross-examination, he could argue on level terms about it with the leading experts on it, and usually contribute to the discussion some material consideration that they had overlooked. His presence always helped one to see what was the really important aspect of the matter under consideration, and one came away refreshed and with new heart. He was the ideal director of a research institution—as was to be recognized later.

His work at Whale Island during 1916–1919 comprised many things besides spinning shells and fuses, and the arranging of high-angle shell trials. He personally took the greatest share in the labour of writing reports, in his own tirelessly professional style; he took a considerable part in the labour of numerical calculation; he did much of the actual observing of shell-bursts; his was the personality behind the friendly co-operation with the naval officers of H.M.S. *Excellent*; he helped to inaugurate the inspection of A.A. shooting in the various coastal A.A. Defence Commands; and he took a part in the work on A.A. sound-locators. He visited France more than once, to make contact with the men in the field. He interested himself in upper-air meteorology, which was important for the analysis of firing trials, both with respect to the wind-structure (pilot balloon ascents and motion of shell-bursts) and temperature-structure (use of the Dines meteorograph, attached to the wing-strut of an aeroplane). For his work in A.A. research, Fowler was awarded the O.B.E.

In April, 1919, he relinquished his commission and resumed residence at Trinity College, Cambridge, as a Fellow. He did not, however, cease his connexion with ballistics, for some time later he was made an Associate Member of the re-named Ordnance Board, and the war of 1939 found him again in that capacity.

Once back in Cambridge he plunged into the mathematical life of the place. As remarked, his first self-imposed task was to prepare for publication his material on plane curves. That done, he began to study the newer problems of mathematical physics. These had begun to attract him before he left Portsmouth, for I owe to Fowler my first acquaintance with Jeans' *Dynamical theory of gases* and Eddington's Physical Society *Report* on Einstein's general relativity. Though now over thirty years of age, Fowler had the eager mind of a learner, and together we attended E. Cunningham's 9 a.m. lectures on special relativity, and other lectures. But the great influence in his life was now Rutherford, who had returned to Cambridge as Cavendish Professor in 1919. A wonderful friendship sprang up between the two, and it extended to all Rutherford's pupils and co-workers; I say *wonderful* because *a priori* it was rather unlikely that there should exist a strong sympathy between Rutherford, with his depth and search for simplicity and his impatience with abstract mathematical theory, and Fowler, with his breadth and search for generality and his insistence on mathematical rigour. But Rutherford's robust and rugged genius appealed to Fowler, and Fowler's amazing versatility and power of becoming absorbed in problems put to him by others appealed to Rutherford.

Fowler was appointed a College Lecturer in Mathematics at Trinity in 1920. He was soon working on quantum problems, and an early paper in the *Proceedings of the Royal Society* applied an extension of Fourier's integral theorem to quanta.

Then again Fowler became friendly with F. W. Aston, at the time the latter developed his mass spectrograph, and he collaborated with him in a mathematical attack on the problems of focusing beams of charged particles presented by this instrument. He also published in the *Philosophical Magazine* a paper on the classical kinetic theory of gases.

In 1921 Fowler married Eileen, only daughter of Sir Ernest and Lady Rutherford. They had four children. Eileen died shortly after the birth of the fourth, in December, 1930. After her death the children were brought up, along with her own children, by Mrs. Phyllida Cook, most devotedly, and the two households dwelt together in Cromwell House, Trumpington, where the Hartrees had at one time lived.

Up till now Fowler had been, as it were, looking round for problems that were worthy of him. He now (1922) began his serious life-work, by collaborating with C. G. Darwin in papers on the partition of energy, published in the *Philosophical Magazine* and in the *Proceedings of the Cambridge Philosophical Society*. These were inspired by some work by Ehrenfest and Trkal on statistical mechanics. Previously writers had concentrated on evaluating the *most probable* state of an assembly of similar systems interacting with one another. Darwin and Fowler replaced this by the direct calculation of the *average* state of the assembly, determined according to the calculus of probabilities, and they calculated also the magnitude of the *fluctuations* from the average state. They also replaced the customary use of Stirling's Theorem in approximating to factorials by an exact expression in terms of a contour integral, which they evaluated by the method of steepest descents. Appealing to Bohr's Correspondence Principle and Ehrenfest's Adiabatic Hypothesis, they assigned equal *a priori* "weight" to each permissible state in each quantized degree of freedom. They constructed "partition functions" of a variable z, namely the function $f(z)$ equal to $\Sigma p_r z^{\epsilon_r}$, where ϵ_r is the energy of a state r, of *a priori* weight p_r, the partition function for N such similar systems being $[f(z)]^N$. A certain parameter θ which turned up in the subsequent analysis was shown by comparison with thermodynamics to be connected with the absolute temperature T by the relation $\theta = e^{-1/kT}$, k being identified as Boltzmann's constant. The authors began by applying this method to the simplest possible systems—Planck vibrators—and passed to molecules in space, making the transition to classical mechanics by passing from sums to integrals. They showed that their partition function was analogous to the *Zustandsumme* of Planck, $\Psi = S - E/T$, where S is the entropy and E the energy. They applied the method also to radiation in an enclosure, and derived Planck's Law; and to the internal energy of a crystal.

More particularly, Fowler alone applied their method to assemblies in dissociative equilibrium, both to chemical dissociation and the high-temperature dissociation of atoms at low pressures into ions and electrons. Those were the early days of the theory of high-temperature ionization: out of S. Chapman's and F. A. Lindemann's controversy about the origin of magnetic storms had come the first suggestion, by Lindemann, of the natural ionization of a gas, and independently M. N. Saha had published his celebrated ionization theory of the high level chromosphere and the stellar spectral sequence. Theoretical physicists were still a little sceptical about the application of the methods of physical chemistry—the Nernst heat theorem and the van't Hoff reaction isotherm and isochore—to the separation of electrons from atoms; indeed, it was only after the acceptance of the Bohr theory of spectra and atomic constitution that such progress became possible at all. There was accordingly a great field open for the formulae and calculations to be justified by the methods of statistical mechanics, and Fowler jumped in, with his mastery of his new technique, and filled the lacuna. He effectively established the validity of the dissociation formula for high-temperature ionization. This was in 1922–1923.

Early in 1923 Fowler and I published in the *Monthly Notices of the Royal Astronomical Society* a joint paper entitled "The intensities of absorption lines in stellar spectra, and the temperatures and pressures in the reversing layers of stars", which had considerable influence in astrophysical theory. For example, it influenced all the early work of Miss Cecilia H. Payne at Harvard. As I have often been asked what were our respective shares in this paper, I propose to describe its genesis.

As I had written on the application of Saha's ionization theory to stellar spectra, showing that the total free-electron pressure had to be taken into account, and as Fowler had just established the ionization formula rigorously, and as there were clearly going to be further applications, we decided to collaborate. Saha had concentrated on the marginal appearances and disappearances of absorption lines in the stellar sequence, assuming an order of magnitude for the pressure in a stellar atmosphere and calculating the temperature where increasing ionization, for example, inhibited further absorption of the line in question owing to the loss of the series electron. As Fowler and I were one day discussing this, it suddenly occurred to me that the *maximum* in the intensity of the Balmer lines of hydrogen, for example, was readily explained by the consideration that at the lower temperatures there were too few excited atoms to give appreciable absorption, whilst at the higher temperatures there were too few neutral atoms left to give any absorption; the actual intensity must depend on the product of two factors, the excitation factor and the ionization factor, of which the first factor, the fraction excited, must increase from almost zero, while the second factor, the fraction un-ionized, must decrease to almost zero. Thus the product, being zero at the two limits of temperature, low and high, must have at least one maximum in between, and probably only one. I rapidly expounded this to Fowler, and we said we would work it out for our joint paper. That evening I did a hasty order of magnitude calculation of the effect and found that to agree with a temperature of 10,000° for the stars of type A0, where the Balmer lines have their maximum, a pressure of the order of 10^{-4} atmosphere was required. This was very exciting, because standard determinations of pressures in stellar atmospheres from line shifts and line widths had been supposed to indicate a pressure of the order of one atmosphere, or more, and I had begun on other grounds to disbelieve this. I walked round to Fowler's house in the Chesterton Road, and, finding him out, dropped a short note in his letter-box telling him of this result. The next two or three days I was busy over other matters, and when I next saw Fowler he told me he had read my note and had worked out an exact and elegant formula for the pressure at which the lines of a subordinate series of a neutral atom should have their maximum. I then assembled the astrophysical data at the Solar Physics Observatory, and produced a draft of the paper we eventually read to the Royal Astronomical Society.

I think this little story typical of much of Fowler's work done in collaboration. The project was often his, the original idea of the paper was someone else's, but he so quickly digested it, transformed it, brought his technique to bear on it and developed it, that he became a full partner in the investigation. And I must add that, without Fowler's tremendous drive, many such investigations would never have been brought to the stage of publication.

He and I published a further paper on the maxima of absorption lines in stellar spectra in 1924, extending the work to the successive spectra of ionized silicon (which had been sorted out by A. Fowler) and of ionized carbon. The maxima of these fixed the temperature scale for the *B*- and some *O*-stars, following earlier work of H. H. Plaskett. Fowler published a third paper alone in 1925. In 1925 too he collaborated with E. A. Guggenheim in applying the ionization theory in detail to steller interiors.

In 1924 I was in constant discussion with him on the *mechanism* of ionization in large assemblies, as opposed to the mere determination of the *degree* of ionization. We had both been impressed by Klein and Rosseland's isolation of the phenomenon known as "collisions of the second kind", in which the reverse of excitation by collision occurred. In a paper in the *Philosophical Magazine* I applied this idea to *photo-electric ionization*, and this was immediately followed by a paper by Fowler on the application of the same idea to *ionization by collision*. Briefly, if in a steady state some ionizations occur as a result of collisions, then there must be as many *three-body* encounters which result in the *re-attachment* of an electron to an ion, to balance them. We then recognized the complete generality of the "principle of detailed balancing"; the phrase was, I think, due to Fowler, though the importance of the principle was being simultaneously recognized elsewhere.

For an essay collecting together his work in statistical mechanics, Fowler was awarded the Adams Prize of the University of Cambridge in 1923–1924. The material of this essay was later published by Fowler as a splendid volume, *Statistical mechanics* (Cambridge, 1929), which reached a second (revised) edition, though shorn of its astrophysical applications, in 1936. This is a highly technical work, not easy to read, but a mine of information on all the thermodynamic properties of the various phases of matter and its environment of radiation—gases (perfect and imperfect), solutions, crystals, conductors, surface films and liquids. Its professional competence is amazing, and it is almost fatiguingly exhaustive. It and its successor, *Statistical thermo-dynamics* (1939), with E. A. Guggenheim, should long remain standard treatises.

In 1926 Fowler published the most original paper of his lifetime, a paper read before the Royal Astronomical Society entitled "On dense matter", wherein he showed that the material in white dwarf stars must consist of a gas in what is called the *degenerate state, i.e.* a gas obeying the quantum statistics discovered independently by P. A. M. Dirac and E. Fermi. It had been recognized by astronomers (chiefly by A. S. Eddington) that a star like the companion of Sirius, with its typical stellar mass, high effective temperature and low absolute luminosity, must have a very small surface area, hence a small radius, and therefore a high density—incredibly high, by terrestrial standards, being of the order of one ton per cubic inch. Either a fundamental explanation had to be found, or there was something seriously amiss with the bases of astrophysical measurement. Fowler found the explanation: the atoms, ionized to almost bare nuclei, were pressed close together, and the free electrons formed a degenerate gas—as Fowler put it, "like a gigantic molecule in its lowest quantum state", About the same time L. H. Thomas recognized that the electrons in an ordinary atom of high atomic number could be described as a degenerate gas, and obtained an equation for its equilibrium of Emden's type.

Fowler did not go into elaborate calculations of the equilibrium of white dwarfs in this paper, though he later shared in the discussion of the polytropes "$n = \frac{3}{2}$" which were subsequently shown to describe them, in the paper on Emden's equation already described. Justly famous as this paper on dense matter afterwards became, it was always a slight source of chagrin to Fowler in that he did not also see at the same time a field for the application of Fermi-Dirac statistics to the assembly of free electrons in the interior of a *conductor*. It was left to Sommerfeld to revive the theory of electronic conduction by showing that the electrons in a solid metal conductor were not attached to particular atoms, but constituted a gas at an apparent very low temperature. A gas becomes degenerate when ρ/T^3 is large, and this may occur in a white dwarf star (where ρ is very high compared with T) or in a conductor where T (apparent) is very small compared with ρ; and Fowler once told me " he could have kicked himself" for not seeing this before Sommerfeld, when he had the idea under his thumb.

Differential equations, differential geometry, ballistics and aerodynamics, statistical mechanics, stellar atmospheres, stellar interiors, degenerate gases —these by no means exhaust the range of Fowler's interests even before he became professor. He had papers in the *Philosophical Magazine* in 1925–1926 on the summation rules for the intensities of spectral lines and in band spectra; he wrote papers on the statistical theory of strong electrolytes, and became such an acknowledged expert on questions of physical chemistry that he eventually became a member of the editorial board of the newly-formed *Journal of Chemical Physics*; he studied the thermionic and photo-electric emission of electrons from metals—hot, cold, clean and coated; he worked at ferro-magnetism and magnetostriction. But he did not confine his attention to his own researches. He was of immense assistance to the Cambridge schools of experimental and theoretical physics, and took up quarters at the Cavendish Laboratory. It was therefore inevitable that when the university wished to make an election to the newly-instituted Plummer Chair of Theoretical Physics, it should turn to Fowler; and he was duly elected Professor in January, 1932.

His stream of papers continued unchecked. His now even closer association with Rutherford—he had examined α-particle problems in his earlier researches—led to his interesting himself in the absorption of γ-rays in elements of high atomic number; he wrote papers on various aspects of adsorption; he considered semi-conductors; he examined questions of surface tension; he delivered the Bakerian Lecture of 1935 on the specific heats of crystals; he expounded the theory of the separation of the isotopes of hydrogen by electrolysis. An excellent example of his well-informed style is afforded by his Liversidge Lecture of 1934 on the heavy isotope of hydrogen, wherein, on a subject not specifically his own, he assembled all the significant facts and figures relating to this new discovery—acting under superior orders, as he said. One may guess shrewdly that "supreme headquarters" consisted principally of Rutherford, who knew how to give Fowler rope for his activities. The lecture was published in the *Proceedings of the Cambridge Philosophical Society*.

He had been elected a Fellow of the Royal Society in 1925. He became a Fellow of Winchester in 1933. In 1936 he was awarded one of the Royal Medals. In 1938 he was appointed to the Directorship of the National Physical Laboratory, in succession to Sir Lawrence Bragg, who had succeeded Rutherford in the Cavendish Chair at Cambridge. No more competent person could have been chosen, and it was a tribute to Fowler's practical ability which he had handsomely earned; but that cannot lessen one's astonishment at such a complete transformation of the original pure mathematician into the physicist, engineer and administrator that the head of the National Physical Laboratory must be. It must be remembered that, apart from his early war experience of 1916–1919, he had never performed a physical experiment in his life.

However, fate decided that he should not take up the appointment. In the summer of 1938 he received the first attack of the illness to which he eventually succumbed, and on medical advice he resigned the Directorship before the Plummer Chair had been declared vacant. Consequently he was re-elected to the Plummer Chair.

In 1939 came the war. Fowler at once picked up the threads of his association with the Ordnance Board, and at considerable personal inconvenience often came up from Cambridge to attend its meetings at the place to which it was evacuated. In spite of the growing threat to his health, he was able as ever to master the complicated agenda of the Board meetings at a glance, was a tower of strength when difficult questions were referred to him, and could make members of the Board who had held high administrative positions in government research departments tremble at his words. Soon, greater opportunities were given him. He was chosen to establish scientific liaison between research on war problems in service departments in this country and similar research in corresponding departments in Canada. One heard of his being here, there and everywhere. He was already familiar with Canada and the United States through numerous visits in peace time, having attended the Toronto meeting of the British Association in 1924 and on that occasion crossed to the Pacific, and having occupied lecturing posts at Princeton and Madison, Wisc. Of the success, the very notable success, of his mission to Canada there could be no doubt; cabinet ministers referred to him in their speeches. He extended his mission to Washington, and, that completed, returned to England. For these services he was created a knight in 1942.

This self-sacrificing exertion, it is probable, shortened his life. But his activities did not slacken. Back in England he soon became a full-time member of a research section at the Admiralty. The last piece of collaboration I undertook with him, at his request, was an investigation of the relative destructive power of armour-piercing and high-explosive projectiles, when directed against a modern battleship. He was being consulted by the Ordnance Board on questions of shell-ballistics up to within a few weeks of his death. But the full inner story of his last months at the Admiralty is not known to the writer, and in any case would have to remain secret.

It is difficult to estimate the loss to British science caused by the premature death of one so accomplished and experienced as R. H. Fowler. Chairmanships of important committees had come to him; he had been editor (with P. Kapitza) of the Oxford series of monographs on physics; but the presidencies of important scientific societies, which would naturally have fallen to him, were still to come. His constructive, original work was perhaps completed. But the ripe comments on contemporary movements in science, the summaries of work by other men and in other lands, the wise direction of post-war scientific activities, his part in the revival of international scientific relationships, all of which he would have contributed from the unequalled width of his experience, we must sadly go without. A position of great and growing influence was already his; what it would have become we can only conjecture. We know only that we grieve for a prince among men.