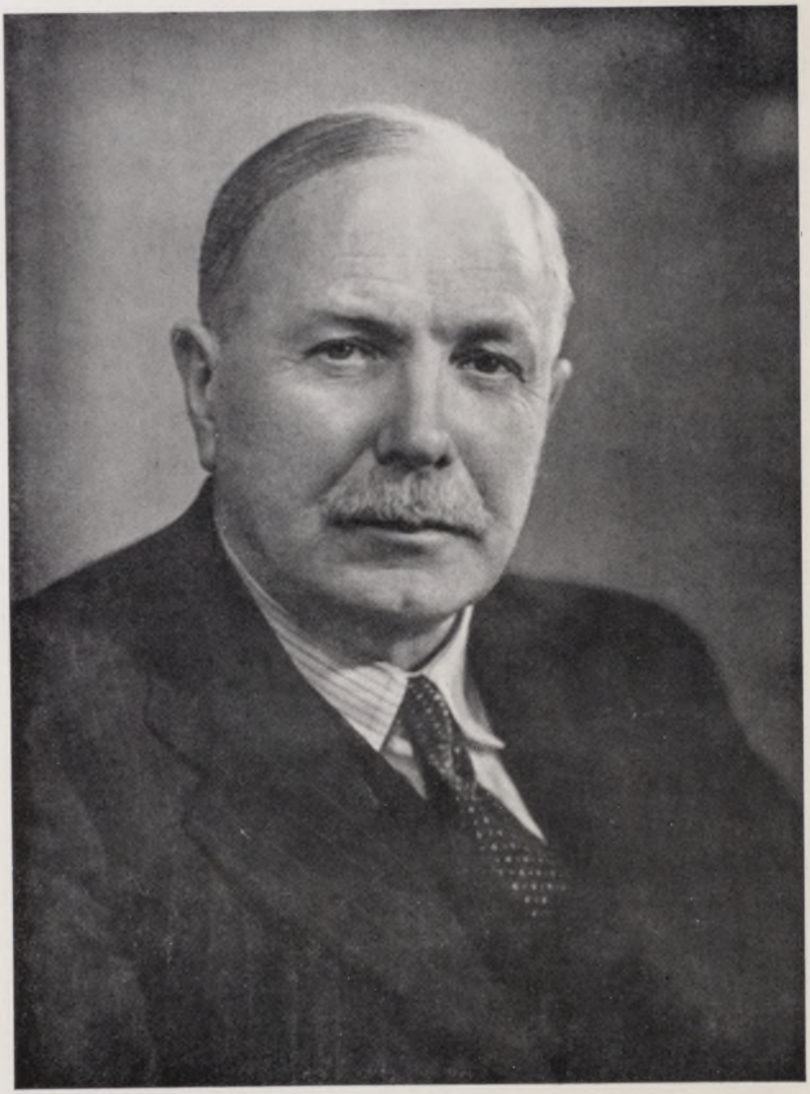

C G Darwin

CHARLES GALTON DARWIN

1887-1962

Charles Galton Darwin, born 19 December 1887, was the eldest son of Sir George Darwin, F.R.S., Plumian Professor of Astronomy at Cambridge, whose best known work was on the early history of the moon. His mother, whose maiden name was Maud du Puy, was an American lady. A grandson of the great scientist whose first name he bore, two of Darwin's uncles were Fellows of the Society and his ancestors included Erasmus Darwin, author of the *Loves of the plants* in verse, as well as of more conventional scientific writings, and the first Josiah Wedgwood. Among his cousins was Francis Galton who with Lord Kelvin was his godfather.

The life of his family when he was a child has been recorded by his elder sister Gwen Raverat in her admirable *Period piece* which describes inimitably their life interwoven with that of the other Darwin families then in Cambridge and to a lesser extent with a few other Cambridge children. One of the latter recalls Charles as 'a big cheerful energetic boy, humorous and scornful of nonsense'. He impressed his young contemporaries by discussing prime numbers and electricity with his father, he is also remembered as being pursued furiously by a sister round the garden with a fork!

Newnham Grange, which since Charles's death is to become Darwin College for postgraduate students, is a charming but rather rambling house on the banks of the branch of the Cam leading from Newnham Mill. There are bridges across from the garden leading to two islands; with a boat and a canoe and a tree house, it made an ideal home for a young and energetic family. Until he was about 10 years old, when his grandmother died, the family spent some time each year at Down House

Charles went to school for two years at the preparatory school of St Faith's, and then to Marlborough with a scholarship. There for two years he went up the school with the ordinary classical education. At classics he was passably good but in his own words 'had little interest or benefit from them and ended with contempt for them'. However, he retained enough Greek in later life to enable him to enliven his college chapel services by reading the New Testament in Greek from a copy conveniently provided in the Master's seat.

At about 16 he entered the Sixth form and specialized in mathematics with 'minor amounts of chemistry and physics and history', in which latter subject he once won a prize. His chief teacher in mathematics was Mr H. Savery. He learnt German in two visits of two months each in 1901 and

1906, during the second of which he was at Göttingen and saw Runge, who was surprised to find a schoolboy so advanced in mathematics.

In the same year he came up to Cambridge with a Major Scholarship to Trinity College. There he read for the Mathematical Tripos, his being the last year to take it in the old form in which the successful candidates were placed in order of merit. His numerous friends hoped that he would be the last Senior Wrangler but in fact he was bracketed fourth. The next year, 1910, he was classed I.2 in Part II.

The teaching in mathematics in those days in Cambridge was decidedly conventional, and though he records his debt to the 'invaluable drill' given by the coaching of R. A. Herman, he criticized severely in after life the deficiencies of the syllabus which was disconnected from the subjects then coming into importance. Late in life he wrote that he had never heard of relativity or the quantum theory before he left in 1910. I remember being surprised when he told me, shortly after taking his degree, that he thought the 'vena contracta' in hydrodynamics was a promising field of research. In fact the reformed tripos answered some at least of his criticisms. Darwin's work in after life bore the mark of Cambridge teaching but I think that it fitted his natural bent better perhaps than he realized. He shared the national characteristic of British science of thinking in terms of specific problems and arriving at broad theories by induction rather than by some *a priori* reasoning. Darwin all his life was an 'applied mathematician' rather than a theoretical physicist. His ideas were derived from experiments or from other men's work. He used his mathematics on them rather than to suggest them.

In 1910 he joined Rutherford in Manchester as Schuster Reader in Mathematical Physics. This was a post intended for post-graduate training in research but with a little lecturing, mainly thermodynamics and kinetic theory in Darwin's case. This was the period of the discovery of the nucleus.

Darwin's first work at Manchester was on a problem of the upper atmosphere suggested by Schuster. At the same time he did some experimental work with Marsden on the active deposit of thorium and then turned to the theory of the absorption and scattering of α-rays. He complains in a letter to his father that he was having to keep these three pieces of work going simultaneously as he had found an error in the former at the proof stage. The paper on α-rays was I believe the first theoretical paper to make use of Rutherford's new idea of the massive central charge, otherwise the treatment resembled that used by J. J. Thomson some years before for β-rays. It was, of course, entirely classical. Niels Bohr was to arrive in Manchester two years later.

After a paper on the orbits of relativistic electrons round a centre of force, Darwin turned from the nucleus to the diffraction of X-rays. Moseley was applying the methods of the Braggs and it was important to find a theory which would account better for the experimental measures of intensity than the simple assumption that each atom scattered as though the others were absent.

The first paper with Moseley appeared in the *Philosophical Magazine* in July 1913. It was mostly experimental, measuring the intensities of beams reflected from a crystal by the ionization they produced. However, the importance of temperature was realized, and a rather crude theory of the reflexion of white radiation was set out. This paper was followed by two others by Darwin alone in February and April 1914 and by a fourth which came out after the war, in May 1922. In these later papers Darwin put forward a sound theory of the diffraction of X-rays which still holds the field. They constitute a very remarkable achievement.

For the following account I am indebted to Sir Lawrence Bragg.

'In 1914 Darwin published two papers in the February and April numbers of the *Philosophical Magazine*, under the title "The theory of X-ray reflexion", which are landmarks in the history of X-ray analysis of crystals. He calculated the efficiency of X-ray reflexion by a perfect crystal, showing that over a very short angular range the superficial layers gave a complete reflexion, and he found that the calculated "integrated" reflexion, is far smaller than that which he and Moseley had observed. He rightly ascribed the discrepancy to the fact that a crystal is not ideally perfect but composed of a "mosaic" of blocks in slightly different orientations. Paradoxically, imperfection increases the intensity of reflexion, because the mosaic elements at depths beneath the crystal surface are not robbed of their chance to reflect by more superficial elements, since these are set at slightly different angles. The formulae which Darwin established have been the basis for interpreting quantitative measurements ever since.

'Before World War I there were no data to which they could be applied, and after the War they had almost been forgotten, so much so that Ewald worked out quite independently his "dynamical" theories of X-ray diffraction without realizing that much of the ground had been covered by Darwin. In August 1925 Ewald arranged a conference at Holzhausen on the Ammersee in Bavaria, to discuss the interpretation of X-ray diffraction, to which Darwin came with James and myself from England; Wyckoff from the U.S.A.; Brillouin from France; Waller from Sweden, Laue, Mark, Ott, Herzfeld and Ewald from Germany. We of course produced Darwin as our champion to show that the foundation stone had been well and truly laid by him in England before the War. Characteristically, Darwin had cheerfully thought it unnecessary to prepare himself for the conference, and it turned out that he had so completely forgotten the logic of his own papers that to our dismay he was unable to present his theory!

'X-ray crystallographers have always regarded this imaginative and original work of Darwin, produced at such an early stage of the subject, as one of his finest contributions to science.'

As a follow-up of the conference referred to above Bragg, Darwin and James published a paper in May 1926 which set out the results of the previous papers in orderly form and described experimental results showing that different crystals had varying degrees of 'disorder'.

In his second paper Darwin calculated the intensity of reflexion by a crystal, allowing for the effect of temperature and the refractive index of the rays, but showed that the calculated effect was much smaller than that observed if the crystals were perfect. In the third paper the discrepancy was shown to be due not, as Darwin had at first supposed, to the influence of the waves due to one plane of atoms on the atoms of other planes, though this effect indeed exists, but to the imperfection of the crystal in the way explained above by Bragg. In the post-war paper Darwin showed how his theory justified the method of calculation of atomic scattering from the intensity of crystal reflexions used by Bragg, James and Bosanquet. The papers impress one as the work of a mature and highly professional mathematician, the details are worked out punctiliously, the exposition of this very tricky subject is clear and the papers rank as classics. They are remarkable work for a man who had only done a few years of research.

In addition to this work on X-rays, Darwin before the outbreak of war had written in 1914 a straightforward paper on the collisions of alpha particles with light nuclei. This helped Rutherford in his work on the projection of hydrogen nuclei by alphas which in turn led to the discovery of artificial nuclear disintegration.

Darwin had been an officer in the Manchester University O.T.C.; when war came he was in camp and was sent to France almost with the first ship, but was kept at Boulogne on censorship and as R.T.O. After a year or more he was attached to the Royal Engineers for service with the units that W. L. Bragg was organizing for the detection of enemy guns by sound-ranging and was put in command of a section which spent most of its time in the Loos salient. He was awarded the M.C. Late in 1917 he was sent to the experimental station at Orfordness and attached to the R.A.F. for work on the noise of aeroplanes.

In 1919 Darwin was appointed Fellow and Lecturer of Christ's College, Cambridge, his grandfather's College. At this period Cambridge mathematical teaching was mostly in the hands of the Colleges. The old system of paid coaches lapsed shortly after the reform of the Tripos and the University only provided Professors whose lectures were usually intended for post-graduates. The Colleges were organized in groups, each of which provided a complete set of lectures for Part I and the compulsory papers of Part II of the Tripos. Most College lecturers were expected to do some supervision as well as lecturing. Darwin held his lectureship till 1922 when he went for a year to the Californian Institute of Technology as visiting Professor. He went out by way of South America, seeing many places his grandfather had visited from the *Beagle*.

During this Cambridge period Darwin's most important work was on statistics in collaboration with R. H. Fowler. Rather to his own surprise Darwin found himself contributing most of the mathematics to the partnership and Fowler, who at that time was newly converted from pure mathematics, most of the physics. By then Fowler was acting as mathematical consultant to the Cavendish where Rutherford was Professor.

In this group of papers the authors investigated the basis of classical* atomic statistics and their relation to thermodynamics. They showed the advantage of regarding the normal state of a gas as the average state rather than the most probable one, though in fact, of course, it is both. This simplifies the mathematics and leads directly to the 'partition function', a modified form of Planck's Zustandsomme, which they introduced. This has proved a useful concept and has been extensively used since. The new method lends itself to the use of contour integrals evaluated by the 'method of steepest descents', and to the calculation of fluctuations. Ordinary thermodynamics in terms of temperature and entropy, can, they showed, be deduced from their statistics without dubious assumptions. In 1922 he was elected a Fellow of our Society.

In 1924 Darwin published in the *Transactions of the Cambridge Philosophical Society* a long paper on the optical constants of matter. This was the first of a series of papers dealing with optical properties, especially those involving magnetic fields. He regarded scattering of light from a small portion of matter as the fundamental process, deriving the optical properties from the characteristics of this scattered wave and leaving aside for the time the question as to the mechanism by which the scattered wave is produced. Though this paper appeared after he had gone to Edinburgh it was probably mostly done in California.

In 1924 Darwin was appointed Tait Professor of Natural Philosophy in the University of Edinburgh. This was a new chair, autonomous but which had to be fitted in to the existing chairs of mathematics and natural philosophy, which gave a joint Honours Degree in arts.

In 1925 he married Katharine Pember, daughter of Francis William Pember, Warden of All Souls College. Her mother's father was Lord Davey, Lord of Appeal. She was herself a mathematician, as had been members of her mother's family. She continued some research after the marriage.

When Darwin came to Edinburgh Whittaker held the chair of mathematics and Barkla that of natural philosophy. Barkla by this time had committed himself, and to some extent his department, to the doctrine of the 'J phenomenon' and its study by an out-of-date technique which alone showed it. Darwin felt that his natural place was with the experimentalists, his rooms were in fact in the basement of the laboratory, and though Barkla was a delightful personality it must have been difficult for them to talk physics. However, though his lectures were in the morning, Darwin made a habit of coming for the laboratory tea-break, which was a period of discussion, at least as long as Harold Robinson was there. He also, of course, attended the physics colloquium at which he was a dominant figure.

* i.e. not Fermi–Dirac or Bose–Einstein, yet assuming energy distributed in quanta.

Darwin was an outstandingly good lecturer on a special subject. His lectures to undergraduates were carefully prepared and impressive. To the majority of his students he was a remote and tremendous figure, seen mostly in the lecture room, though those more closely associated with him record his generous help.

It may be asked why he never made any serious effort to establish a school of theoretical physics at Edinburgh. It might not have been easy to do this without creating a delicate situation of competition with Whittaker, who had keen interests in some branches of mathematical physics and already had a flourishing school, then too the old custom at the Scottish universities of the best mathematicians going on to Cambridge with a scholarship was not helpful from this point of view, but I think that the real reason was more personal. Darwin was more at ease with contemporaries or near contemporaries, than with younger men. He did not much enjoy the relationship of intimate leadership which some men find very attractive and which is almost essential to the success of such a school. It was a pity, for in other respects he was admirably suited to form one. He did, however, give courses of post-graduate lectures, often in the mathematics department, on various branches of theoretical physics. He also arranged for a fund (the Ritchie Fund) to be made available to invite distinguished scientists to deliver single lectures in Edinburgh, insisting that they should not be too specialized to be of value to undergraduates.

Darwin came to Edinburgh at a time when the quantum theory of atomic structure in the original form due to Bohr and Sommerfeld was dominant, when its first successes had been already obtained and the difficulties were beginning to grow. Darwin seems to have been led to his study of some of the problems in magneto-optics presented by this theory by his work on classical optics referred to above. He first published two short papers in the *Proceedings of the Royal Society* extending this to deal with magneto-optics in more detail, then came a paper in which he tried, with partial success, to relate the details of the Zeeman effect to a fairly generalized mechanical model using classical mechanics. The next stage was to apply the dispersion theory of Kramers and Heisenberg, a quantum theory based mostly on the correspondence principle, to the problem of the intensities of the components into which a spectral line is split by the Zeeman effect, and their connexion with the dispersive effect of the rotation of polarized light in a magnetic field. After a paper with Watson analyzing the experimental measurements of magnetic dispersion, Darwin moved on to apply the recently discovered wave-equation of Schrödinger to the problem of calculating the Zeeman effect for all strengths of field, which he reduced to the solution of algebraic equations. In a paper published next year (1928), also in the *Proceedings*, Katharine Darwin gave numerical solutions for some of the more important special cases.

In 1927, and apparently again in 1928, Darwin, who had been in touch with Bohr from Manchester days, visited Copenhagen and spent a memorable two months in Bohr's Institute. It was an exciting time, with the new quantum theory just getting into its stride. Heisenberg's 'uncertainty principle' was only a few months old, and the whole philosophy of physics was still waiting for the melting pot to cool enough to let it crystallize. Darwin took with enthusiasm to the new ideas, especially to Bohr's complementarity and came back a dedicated missionary. The stimulus of Bohr's ideas produced a group of papers which were, I consider, the most important of his life.

The first of these grew out of a letter to *Nature* in February 1927 which Darwin later discussed with Bohr on a visit to Copenhagen. The letter put forward the suggestion that 'the electron is to be taken as a wave of two components like light, not of one like sound'. The paper in the *Proceedings* developed this theme. He was able to work out wave equations which fitted the hydrogen spectrum. They were unsymmetrical, taking a different form according to the direction of space chosen as prime axis. Darwin tried to interpret them in terms of a vector but the vector was to some degree arbitrary. This was before Dirac's discovery of his electron with its four wave functions and Darwin was getting very hot, in fact his solution is an approximation to Dirac's. He broke off to discuss, in the best manner of the mathematical tripos, a series of examples of the behaviour of free electrons according to the Schrödinger wave mechanics, using these as illustrations of the 'uncertainty principle'. This is a most useful piece of work, helping one to a more intuitive idea of the way free electrons behave.

In February 1928 Dirac's first paper on his new relativistic electron appeared in the *Proceedings*. Darwin at once realized its significance and early in March sent to the Society a paper in which he translated Dirac's work from non-commutative algebra into the ordinary language of differential equations. He also showed that his own two equations were approximations to those derived from Dirac's theory. Then he went on to apply the theory to the problem of the hydrogen atom and determined the energy levels including their fine structure. It was a great achievement especially in the time. Possibly he had advance notice of Dirac's work but Dirac's paper was only communicated to the Society on 2 January 1928 and published on 1 February, while Darwin's paper was communicated on 6 March and appeared on 2 April, quick publication! Darwin's paper made the Dirac theory accessible to ordinary physicists and greatly accelerated its general acceptance. He followed this up with two papers which appeared simultaneously in the *Proceedings* of the same year, one on the magnetic moment of the new electron and the other on its diffraction. In the first he analyzed the magnetic field of a moving Dirac electron and showed the relation between the contributions of the current and of what can be regarded as the intrinsic magnetic moment of the electron. He also examined the relation between the polarization of an electron wave and that of a wave of light. In the second paper he worked out the simplest case of diffraction, namely, that by a line grating exerting periodic electric or magnetic forces, including the polarizing effects if any.

From consideration of Dirac electrons Darwin returned to the simpler non-relativistic Schrödinger ones and worked out in some detail the very important case of a collision between two electrons, a case which is easy to express in terms of particles, much less obvious in terms of waves. He then went on to consider some other examples of the uncertain principle especially as applied to the measurement of magnetic fields when, as Bohr had shown, the magnetic moment of the electron cannot be detected by direct measurement since it is masked by the magnetic field due to the electron's motion. In a paper published in 1932 Darwin solved similar problems for light. He showed among other results that the intrinsic angular momentum of a single photon due to its circular polarization, like the magnetic moment of the electron could not be separated from other effects.

One purely classical problem occupied a good deal of Darwin's effort at intervals over many years and may best be described here. It may be regarded as a natural extension of the admirable paper of 1924 in which he considers optical properties in general as consequences of the scattering of radiation, but the immediate stimulus was an uncertainty in the theory of the transmission of radio waves in the ionosphere. The problem is a somewhat technical one concerning the effective electric field acting on an electron in an ionized medium. Long ago in 1906 Lorentz had considered the same problem in connexion with the refractive index of light in his classic lectures at Columbia University later published as *The theory of electrons*. He had calculated the average force on an electron due to the polarization of the material around it, a force known rather unfortunately as the Lorentz term, unfortunately because there is another and better known Lorentz term, or force, on an electron due to its motion through a magnetic field, Lorentz derived the term for an electron forming part of an atom, and part of Darwin's first paper in the *Proceedings* of 1934 confirmed his result by a different method. The point at issue, however, was whether the force represented by the term did in fact act on a free electron as Hartree (who used Darwin's 1924 paper) had supposed, or should be omitted as was done by Appleton. It is one of those difficult and tricky problems in which all depends on which of several about equally plausible methods is chosen for taking an average. Darwin came to the conclusion that the Lorentz term should be omitted for free electrons, but admitted that his arguments were not absolutely conclusive. He returned to the matter in a paper in the *Proceedings* of 1943 and confirmed his conclusions by a calculation of the orbits of the electrons on which the force due to the term would act if it were proper to include it. The inclusion or otherwise of the term considerably modifies the mathematics of ionospheric calculations, but in view of uncertainties as to the exact state of the ionosphere it is not easy to make a clear-cut experimental text. I am informed by Mr Ratcliffe that the results show fairly conclusively (though still not as clearly as might be wished) that the term should be omitted, i.e. that Darwin was right. Part of the evidence in this direction comes from the curious radio echoes known as 'whistlers'.

In 1936 Darwin was elected Master of Christ's College and returned to Cambridge. His duties in this post were mostly administrative, but in fact he also gave a course of lectures for the Mathematical Triops on electromagnetism. He spent a good deal of time on projects for new buildings and partly because of this and partly because of the disturbance of a move only published a few semi-popular papers during his tenure of the Mastership. In 1938, partly at least because he foresaw the approach of war, he accepted appointment to the Directorship of the National Physical Laboratory in succession to Sir Lawrence Bragg, who had resigned earlier that year in order to take up the Cavendish Professorship of Physics in Cambridge.

The outbreak of the second world war in the autumn of 1939 meant that his first major task was to reorganize the staff and work at the N.P.L. to be of the maximum value to the country. Certain members of staff were seconded to other government posts and certain parts of the research programme had to be abandoned in favour of a mass of urgent short-term work of military importance. Especially valuable contributions were made by the N.P.L. to methods of dealing with unexploded bombs, to the design of armour plate, to the construction of the Mulberry harbour used in the invasion on France, to anti-aircraft defence and to the early development of radar. (Sir Robert Watson-Watt was head of the Radio Division of the N.P.L. when he wrote his definitive paper on radio direction finding or R.D.F., as it was called in those days.)

By 1941 the reorganization was complete (including the creation of a Light Division in 1940) and Darwin was seconded to Washington for a year as the first Director of the mission that became the British Central Scientific Office, set up in order to improve liaison between the scientific war effort of Britain and the United States. His work in Washington was very successful. Such a post requires considerable administrative ability but also a peculiar combination of qualities: wide scientific knowledge and sound judgement of what will work in war, initiative and especially tact to handle a great variety of people, each concerned with his own special interest. He was one of those concerned in liaison with the United States of America over the atomic bomb. He was told the details of the work of the MAUD Committee on the bomb and was sent a copy of its report. He was one of the very few scientists at this early date to realize that the use of an atomic bomb was a problem different in kind, as well as in explosive power, from conventional weapons. He wrote to Lord Hankey, the Minister then in charge of the Scientific Advisory Committee and asked 'whether the Prime Minister and American President would be willing to sanction the total destruction of Berlin and the country round when, if ever, they were told it could be accomplished at a single blow'.*

Shortly after his return, he was called on to give most of his time as the first scientific adviser to the War Office, so it was not until near the end of

* Information kindly given by Mrs Gowing, Historian of the U.K. Atomic Energy Authority.

the war that he was able to give his undivided attention to the laboratory.

That Darwin had foreseen the potential value of electronic computers in solving certain mathematical problems is clear from his creation of two new divisions, viz. Mathematics and Electronics in 1945 and 1946 respectively. The late A. M. Turing, F.R.S., a brilliant young mathematician on the staff of the N.P.L., was one of the pioneers in the logical design of such computers. The successful collaboration of these two new divisions produced in Pilot ACE the first electronic digital computer which was available to British industry for computational work. The Mathematics Division now has an outstanding reputation as a centre for research on numerical analysis, while the Electronics Division has developed into an Autonomics Division, devoted to problems such as self-adaptive control, the mechanical translation of languages and the use of cryotrons as computer elements.

In the post-war reorganization of the Department of Scientific and Industrial Research, Darwin played an important part in the creation of the Hydraulics Research Station at Wallingford and the Mechanical Engineering Research Laboratory (now the National Engineering Laboratory) at East Kilbride, since these were formed largely from the Engineering Division of the N.P.L. He was also instrumental in the formation of the Radio Research Station at Slough from the Radio Division of the N.P.L., although the final transfer of staff did not take place until several years after his retirement in 1949.

He continued to take a great interest in the N.P.L. and was a member of the Executive Committee from 1953 to 1959.

His staff at the N.P.L. seem to have felt awe as well as admiration for him, except for those who came fairly closely in contact with him and so were able to appreciate what a warm and sympathetic person was really there.

His unusually wide scientific interest, extending to almost every branch of physics, made him quick to see the value of the work being done.

The war and the N.P.L. left little time for research in mathematical physics, but this period still includes a few scientific papers. He wrote on the choice of the most probable values of *e*, *h*, and *m*, there was his second paper on the 'Lorentz term', to which reference has already been made, and a short note on the 'Diffuse reflexions of X-rays by crystals'. In addition there were lectures and addresses and a paper on Weber's function. After he retired Darwin wrote on conformal transformations and elliptic functions, a note showing that electron inertia could have no appreciable effect on terrestrial magnetism, one on a paradox in hydrodynamics showing that the passage of a solid results in a net displacement of liquid in the same direction as that in which the solid has moved, as well as a number of papers and addresses on population. He also wrote on the discovery of atomic number, and at the time of the Darwin Centenary on his grandfather. He contributed to the rather rambling discussion of the 'clock paradox' (what happens to an astronaut who shoots off into space and returns after many years), a letter to *Nature* remarkable for its lucidity and insight. Perhaps as a result of this he had papers in the *Proceedings*, one in 1958 and the other in 1961, on relativistic astronomy, which showed his really extraordinary mathematical ability in dealing with very varied problems, for general relativity was a field he had never studied before, requiring a mathematical technique new to him, though the actual problem resembled that of one of his earliest papers. It concerns the orbits of planets which in some circumstances can spiral into the sun. However, as he showed, this is not an event likely to be observed in nature as unless the 'sun' is either exceptionally massive or made of exceptionally dense material the planet will hit its surface before the effect can be seen.

Many men have strong desires for the social betterment of the world when they are young and lose them later on. In Darwin's case they were most obvious in the later middle age, for the last ten or twelve years of his life they affected him strongly and were indeed his chief concern. As befitted the godson of Galton they centred round genetics, and especially the principle of Malthus. He first set them forward in public in his Galton Lecture of 1939 before the Eugenic Society under the title of 'Positive eugenics'. In this lecture he studied the difficulties involved in people trying to improve themselves, or rather their successors in the world, and came to the conclusion that only a very general aim is possible, and that about the best that can be done is to encourage the more financially successful members of society to have more children.

In his book *The next million years* (1952) he considers the probable long term future of mankind and comes to more pessimistic conclusions. Man is a 'wild' animal and cannot improve himself as he has the domesticated animals because he can never know what he really wants for himself. He will quite soon have to face the Malthusian problem in an extreme form and there is no real way out, even with contraceptives, because those who are willing to use them will leave fewer descendants than those with a direct desire for children. In this book he perhaps took too little account of the probability that selection of human beings works more through the group than the individual though he points out that education in a sense provides inheritance of acquired characteristics. In his Rede Lecture (1958) he re-examined this point among others and came to the conclusion that it made little difference to his main thesis that in general man's natural increase would usually be kept in check by starvation. His book holds the reader by its originality and obvious sincerity in spite of the grimness of its conclusion. In a sense it is propaganda for a crusade, and so I feel sure Darwin felt it, but it is strange propaganda that insists repeatedly on the certainty of failure and deprecates as unlikely to succeed most of the more obvious palliatives. Nevertheless, it has been widely read and has obliged many people to think hard on uncongenial lines. This probably is all he expected, if so he had a considerable success.

During this period and indeed earlier, he was engaged in a number of activities on the administrative side of science, and with Lady Darwin travelled considerably, sometimes for pleasure, more often on a mission of some kind, though as they both enjoyed foreign countries they often went further afield than the mission demanded. After earlier visits such as those to South America and Copenhagen to which reference has already been made, he attended the sixth congress of the Physical Society in Moscow in 1928, visited the Universities of Stalingrad and Tiflis and returned by the Black Sea and Constantinople. In 1931 he gave the Lowell Lectures at Harvard, afterwards published as *The new conceptions of matter*. This takes the form of a popular account of the 'new' quantum theory, but goes quite deeply into the duality between waves and particles and by its clarity of expression can even now be helpful to physicists. During the same visit he spent a term as visiting professor at Princeton.

Darwin seems not to have made any other extended visits abroad till the winter of 1937/38 when he and his wife spent two months in India with the British Association, visiting widely.

After the war he again went to India in 1946/47 for a scientific congress at New Delhi, where he was asked to lay the foundation stone of the Indian National Physical Laboratory and given an Sc.D. In 1947 he visited the colleges of Iraq on behalf of UNESCO. Later he acted temporarily as head of the United Kingdom Delegation to the Atomic Energy Commission of the United Nations during part of the discussions held near New York in the hopes, by then faint, of bringing about an agreed international control.

In 1953, accompanied by Lady Darwin, he spent the whole winter in Thailand sent by UNESCO as scientific adviser to the government of that country to report to them on universities, libraries and scientific matters generally. This involved a lot of detailed work, but gave him the opportunity to get to know the leading people well and be really helpful.

In 1954 he attended the International Population Conference in Rome, and a smaller one in Nancy. The first especially was very important to him and is referred to in his Rede Lecture and other writings on the problem of population.

In 1956 he was Rutherford Lecturer to New Zealand and Australia. He and Lady Darwin went round the world and he was British Council Visitor in Ceylon, this was in the first half of the year. In October he went for an O.E.E.C. Conference to Vienna and in December to India and Pakistan primarily for a population conference at Delhi. In 1958 he attended the second Pugwash Conference at Lac Beauport, Quebec.

During 1959 he took part in the celebrations of the Darwin Centenary held in Philadelphia, by the American Philosophical Society of which he himself, his father, grandfather and great-grandfather had been members, and later in the year he attended the Darwin Centenary at Chicago.

In 1961 he and Lady Darwin again visited America. They made a long stay in Davis College, California, and he gave Charter Day addresses at several divisions of the University. They also visited the University of British Columbia. In 1962 he attended conferences at Salzburg and Munich.

Darwin was a Member of the University Grants Committee from 1943-1953, a double term. From 1941-1944 he was President of the Physical Society and from 1953-1959 that of the Eugenic Society. In 1958 when the British Association met in Cambridge he was President of Section A. Among other committees on which he served for some time were the Foreign Office German Committee, the Colonial Development Corporation, the B.B.C. Scientific Advisory Committee and the National Committee for Museums and Galleries. He was a Governor of Shrewsbury School. From his many scientific committees one may single out for mention the Royal Society's National Committee for Scientific Radio of which he was a prominent member for many years, and for which his work on the propagation of waves in the ionosphere gave him special authority. He died at Cambridge on 31 December 1962.

Perhaps Darwin's most permanent contribution to physics will be his early work on X-ray diffraction. Here he was early in the field and built a theory of those complicated phenomena which is likely to remain the basis for the reduction of experiments. It is a sound satisfactory piece of work of which anyone would be justly proud. His most exciting work was probably his use of the Dirac electron to derive for the first time the correct explanation of the fine structure of the hydrogen spectrum, which also showed his mastery of mathematical technique. But I am inclined to think that his most useful work was as an interpreter of the new quantum theory to experimental physicists. For this he was peculiarly well adapted both by nature and training. He had an exceptionally wide range of understanding and a most unusual capacity for seeing the essential idea in a maze of complicated mathematics or conflicting experiments. This capacity of seeing essentials helped him during the war and Sir Edward Appleton recalls with gratitude how quick he was to get the 'good enough' answer for practical use, for example in working out the effective radar-scattering area of disks, plane and curved. Tizard's remark that 'Charles was notably wise in dealing with things he knew nothing about' shows merely the limit to which this quality tended. He had had a severe mathematical training and his attitude to mathematics was strictly professional but he had also worked in a laboratory for some early impressionable years and understood the mentality of experimentalists. His devotion to Niels Bohr made him eager to interpret what might otherwise have seemed too delicate subtleties. The papers in which he worked out in detail some of the simpler consequences first of the Schrödinger and then of the Dirac waves are masterly and his translation of the latter into differential equations is an important permanent contribution to physics. I should like to record my great debt to him for the many ideas in physics he helped me to understand.

Charles Darwin retained something of the schoolboy throughout life. He was large, both physically and in manner, cheerful and definite, very good company, an ideal dinner companion and host. In his youth he was a member of the Trinity Lake Hunt, a group of Trinity undergraduates founded by Trevelyan which contained a considerable proportion of the best of the clever young Trinity men, including some who had stayed on and become dons. It was, and is, fairly strenuous, the members spending the day chasing one another over the hills of Cumberland, returning to a farm house in the evening. He also did some climbing in Skye and a contemporary records the confidence he inspired as an immovable anchor.

He was popular as an undergraduate, and became President of the light-hearted undergraduates' debating society at Trinity, known as the Magpie and Stump. At games he was only a moderate performer, though he played court tennis better than average. Golf was his best, and in the early 20's he was a member of the famous four that played on the Gog Magog course on Sunday mornings—Rutherford, Aston, R. H. Fowler and himself—but the standard of physics was higher than that of the golf! In his boyhood he had learned to play the piano, perhaps more unusual then than now, and though he gave up playing when about 15 kept a lively interest in music, especially the works of Beethoven.

He had definite opinions on many things, usually held with firmness, but in physics at least he kept an open mind and was prepared to change his views. Discussing physics with him was both stimulating and instructive. He had a wide knowledge of experiments as well, of course, as as of theory, and could put the gist of a mathematical argument across in conversation. He was keenly interested in social and political problems but with something of the impersonal attitude which is common in professional philanthropists, to whom people seem like the flags that denote battalions on a staff map. With his keen interest in the future of society he could have made a good socialist but never a good communist or indeed one at all, it had to be his line not the party's.

Charles was tolerant, and like many large men seemed easy-going. This may possibly have been true in some administrative matters, but certainly anyone reading his papers carefully must be impressed by the care he took to follow up paths which because they yielded nothing surprising hardly appear in the published work. He was very thorough in his physics.

Travelling was one of his chief pleasures. He was interested in the differences between kinds of peoples and their resemblances, much as his grandfather was in those between species. Nearer at home, his widespread family connexions meant much to him and he was proud of them.

Charles Darwin was a man of very varied abilities, good at many things, with many interests. He was far indeed from the conventional limited scientist, yet there was no doubt in the minds of his friends that, apart from his family, science to him was supreme.

He once said that to be in the forefront of the wave of scientific knowledge was the happiest destiny a man could have.

Darwin was made a Knight of the British Empire in 1942. He received honorary degrees from the following universities, in order of conferment: B.Sc. Bristol, D.Sc. Manchester, LL.D. St Andrews, Sc.D. Dublin, Sc.D. Delhi, LL.D. Edinburgh, D.L. Chicago, and D.L. California. He was an honorary Fellow of Christ's College and of Trinity College, Cambridge. He received the Royal Medal of the Royal Society in 1935 and was a Vice-President in 1939. He received the MacDugal Brisbane Prize from the Royal Society of Edinburgh. Besides the Rutherford Lecture given for the Royal Society he delivered the James Forest Lecture to the Institute of Civil Engineers, the Kelvin Lecture to the Electrical Engineers and the Galton Lecture to the Eugenics Society, the names associated with the last two being those of his godfathers.

He was a Foreign Member of the Hollandsche Matsch. Wet. of Haarlem, an Hon. Member of the French Physical Society and of the American Philosophical Society.

He is survived by Lady Darwin and five children. Of his children: Cecily is a crystallographer and lives in Philadelphia, with American husband and children; George is an electronic engineer; Henry is an international lawyer in the British Foreign Service; Francis is a zoologist on the staff of Birmingham University; and Edward a civil engineer.

I am greatly indebted to the following for help, especially in matters of fact, but the opinions expressed except for the actual quotations are, of course, my own: Sir Edward Appleton, Sir Lawrence Bragg, Sir James Butler, Mrs M. M. Gowing, Mr E. S. Hiscocks, Mr J. A. Ratcliffe, Dr R. Schlapp, Sir Gordon Sutherland.

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G. P. Thomson

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