Centenary of J. J. Thomson

George Thomson

When Joseph John Thomson was born on 18 December 1856 in a Manchester suburb, physics was curiously different from what it now is. This is not merely a statement of the obvious fact that in the intervening century so much has been discovered to which he and his pupils contributed a substantial share but of a difference in outlook. In those days physics was taught as a branch of mathematics. The first teaching laboratory in England, the Clarendon at Oxford, was not started till 10 years later. Certain university professors did indeed do experimental research, notably Sir William Thomson, afterward Lord Kelvin (no relation to J. J.), at Glasgow and Stokes at Cambridge, but even in Scotland, which was in this respect more advanced than England, practical work was limited to lecture experiments. Faraday was still working in London at the Royal Institution in the laboratory that had been founded 50 years before by the expatriate American Count Rumford, and Henry was in Washington at the Smithsonian Institution.

"J. J." was of the first generation to be taught physics as a practical subject. He entered the newly founded Owen's College, now the University of Manchester, at the early age of 14 and took a course of engineering for 3 years, changing then to physics and mathematics, because his father had died and his mother could not afford the heavy premium that an apprentice engineer was then expected to pay. But he did not remain long at physics; after 2 years he went up to Cambridge with a scholarship and read mathematics, as was then the custom. Although, after taking his degree, he started some experimental research in the Cavendish Laboratory where Lord Rayleigh had just succeeded Maxwell, it was his mathematical work that first brought him reputation, and he continued to produce mathematical and experimental work concurrently throughout his long life. I have heard him say that mathematical research was more enjoyable than experimental. With an experiment, one spent a lot of rather worrying effort overcoming a series of practical difficulties to get the thing to work; when it did work, one took a few readings and all was over.

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Theoretical Work

In those days Maxwell's theory was new, and most physicists found it very difficult, especially the idea of electric displacement, which it must be admitted was not made very clear. J. J. set to work to be the interpreter of Maxwell, and it is characteristic that he did so largely by introducing a more picturesque physical conception than that of Maxwell, developing Faraday's idea of tubes of force. However, this came a little later. In his first paper (1881) he used the technique of Maxwell to derive the field around a moving charge. This paper contains, I believe, the first hint of a connection between energy and mass. It showed that an electrified sphere would have its effective mass increased by the mere fact of electrification. He also found an expression for the force on the moving charge in the presence of a magnetic field, a result which some 10 years later, with a different numerical constant, came to figure as the "Lorentz term." Incidentally, he also found the value for the magnetic field due to the moving charge.

At the same time that he did his electromagnetic work, he also produced for his fellowship thesis at Trinity College a treatment of thermodynamics by the method of Lagrange equations, and a little later he won the Adams prize with a long essay on vortex motion, which interested him because of its possible connection with atomic theory, since the intricacies of vortices had appealed to both Kelvin and Helmholtz as possible models for the atoms of chemistry. It is odd that this long-obsolete theory should in fact have led J. J. to his first work on electric discharge. It seemed to him that the theory might indicate that chemical dissociation would be necessarily associated with electric charge and that the conductivity of gases was an example of this. His first experimental paper in the new field was published with Threlfall in 1886, and for the rest of his life he was seldom without an experiment on the conductivity of gases or the phenomena shown by the discharge of electricity in them. For some years these experiments did not lead to anything of prime importance, and before reviewing the great events associated with the discovery of the electron one may follow through with the development of his theoretical work. At the end of 1884 he succeeded Rayleigh as Cavendish professor at Cambridge.

In 1891 he published a paper in which for the first time the idea of momentum in electromagnetic radiation was put forward and given a mathematical expression. Thomson showed that it was in the direction of the vector which Poynting had established a few years before.

Two results that he found after the discovery of the electron belong more properly here. In his Silliman lectures at Yale in 1903, he carried the idea of the connection between mass and energy a bit further forward and showed that the electrostatic energy in a unit volume was equal to the kinetic energy of the mass which (so he calculated) was bound by the tubes of force, moving with the velocity of light. This was one stage further toward the relationship $E = mc^2$ which was formulated by Einstein in 1905. In the same set of lectures, he put forward the view that the wavefront of light is not uniform but consists of specks of great intensity separated by considerable intervals where the intensity is very small. His evidence for this was the strange behavior of x-rays in ionizing the gas through which they passed, although the absorption in the gas could be small. Certain molecules of the gas were, so it seemed, picked out for special treatment. This is, of course, one example of the photoelectric paradox that caused so much trouble in the 1920's, and which was formulated, rather than explained, by Einstein's postulate made in 1905 that the energy of radiation proceeds in quanta of magnitude $h\nu$.

The Electron

Let us now turn back to his work on the electron, by which he is best known to the present generation. There were two quite different lines of approach that led at the end of the 19th century to the discovery of what we now call the electron—the need in electrical theory for charge to be regarded as being done up in separate bundles, multiples of a unit rather than continuous, and the discovery of elementary particles carrying a charge.

The first evidence for a unit charge comes from Faraday's experiments on electrolysis, and it is curious that these experiments were not taken more seriously at the time that they were made as proving that electricity is made up of units. Some of the earlier theories of electricity, such as that of Weber, did indeed treat electricity as existing in separate charges, but the magnitude of the charge did not come directly into the theory or play any very important part in it. Johnstone Stoney in 1874 had called attention to the importance of the charge carried on a monovalent atom, and in 1881 he estimated its value at 3×10^{-11} electrostatic units (the true value is 4.8×10^{-10} electrostatic units). The name *electron* is also due to Johnstone Stoney, although it does not seem to occur before 1891.

In one sense the electron was named before it was discovered. Maxwell's theory has nothing to say about electrons, but the development of it, which Lorentz made early in the 1890's, required the conception of discrete charges. This theory found its first experimental support in 1896, when Zeeman discovered the effect that goes by his name. Zeeman was a pupil of Lorentz, and Lorentz at once put forward an explanation of the results in the form which is familiar to most. In Zeeman's early experiment the magnetic field was not strong enough to resolve the lines, fortunately for him, for the cases he examined showed the "anomalous" effect, but he made an estimate of e/m, the ratio of the charge to the mass of the "electrons" responsible, which he found to be of the order 10^7, 1000 times that for the hydrogen ion. Curiously enough, in his first paper he gives the sign of the charge as positive instead of negative. It is a matter of interest that the last experiment of Faraday's life was an unsuccessful attempt to detect precisely this effect, and it was as a result of reading about this experiment, in a sketch of Faraday's life written by Maxwell, that Zeeman was led to try the experiment again with more modern equipment.

The other line of attack is a quite different one. It comes from experiments on the electric discharge which go back to the middle of the 19th century, when cathode rays were discovered by Plücker, but here also the year 1896 is critical. In November 1895 Roentgen discovered x-rays. This apparently almost accidental discovery has perhaps had more direct effect on physics than any since that of Galvani. J. J. was one of several who discovered almost simultaneously early in 1896 the ionization produced by x-rays, and Rutherford (who had just come to Cambridge as a young research student from New Zealand) joined him in working on it.

Now cathode rays had been a source of great interest to physicists for a long time, and there was a controversy about their nature in which physicists were divided on roughly national lines. Germany favored the view that they were some form of electromagnetic radiation, while France and Britain believed that they were corpuscular. The strongest evidence for the theory that they were electromagnetic radiation was the discovery by Lenard that the rays could be sent through a thin sheet of metal into the atmosphere, and to do so must have penetrated through many atoms without losing their original direction. This seems familiar enough to us, but to the physicists of those days it seemed impossible that anything material, even the hydrogen atom, could be small enough to do this. The strongest evidence for their being corpuscular was their deflection in a magnetic field. Perrin had shown that when a collector was placed opposite the rays it received a negative charge, and that when the rays were deflected away by a magnetic field the charge ceased. J. J. showed that the charge followed around with the rays when these were deflected, and so was very closely associated with them. There was, however, a serious difficulty. Hertz had shown that they were not deflected by an electric field, as moving charged particles surely must be.

In April 1897 J. J. gave a lecture at the Royal Institution in which he described how he had determined the ratio e/m for the cathode rays. This was done by measuring with a thermopile the heat conveyed by the rays and also by determining their charge and their deflection by a magnetic field. A little earlier, in February, J. J. had shown that the deflection of the cathode rays in a magnetic field was independent of the gas, provided that the voltage on the tube was kept the same. He now showed that the value of e/m was of the order of 1000 times greater than that for hydrogen in electrolysis, and, further and more important, that it was the same for air, hydrogen, and carbon dioxide in the tube, and whether the electrodes were iron, aluminum, or platinum. In this lecture, and in the more detailed paper published in October of the same year, J. J. stressed the universal character of the cathode rays and gave reasons for supposing that they were universal constituents of matter. In the latter paper, he described his well-known method by which e/m is determined by comparing electric and magnetic deflections and measuring the latter. His e/m tube is the direct ancestor of all cathode-ray oscillographs. But Hertz had shown that the rays were not deflected by an electric field. The explanation is quite simple, once you know it. In the tubes used by Hertz there was a considerable pressure of gas, and as this became ionized the ions moved up to the plates between which the electric field was supposed to have been generated and to a large extent neutralized it. J. J. was able to show that if the pressure was sufficiently reduced (a matter of considerable labor in those days), an electrostatic deflection of the rays gradually appeared.

In discussing his results, J. J. rejected the idea that in some way the mass of the electron represented an electromagnetic contribution to the mass of atoms, which, it might be vaguely conjectured, could have been shaken off the atoms by the rapid motion. He preferred instead Prout's hypothesis brought up to date and supposed that all atoms were made from common constituents. He was partly influenced in his view by Lenard's experiments showing that the cathode rays were absorbed by matter roughly in proportion to the mass penetrated, which was to be expected if their absorption was due to collision with a constituent present in all matter in proportion to the density. Lenard's experiments also convinced him that their mass was very small. In this same paper he put forward a model atom based partly on the American physicist Mayer's experiments with floating magnets controlled by a strong central magnetic pole.

These were not the only measurements of e/m made about this time. Kauffmann in July of the same year published a value based on the assumption that the energy acquired by the cathode ray was equal to that acquired by a charged particle falling through the potential difference of the tube. This involved the assumption that no appreciable amount of energy was lost to the gas, an assumption at the time not fully justified. Indeed, not many years before, Schuster had used the same principle to give an upper limit but had contented himself when he calculated that a *lower* limit, which was obtained by assuming that the velocity of the particles was the same as that of the mean square of the atoms of the gas in the tube, gave a result that was compatible with the rays' themselves being atoms.

Wiechert early in the year had published in the rather obscure journal of the Königsberg Scientific and Economic Society a rather similar investigation. He rightly regarded Kauffmann's assumption as giving an upper limit to e/m, which he found to be 4000 times greater than for hydrogen in electrolysis. He supposed—it is not quite clear why—that the cathode fall of potential gave a lower limit to the energy, in his case leading to a value 400 times that for hydrogen. He then developed a method invented the year before by Des Coudres which was theoretically sound; it involved measuring the velocity of the cathode ray against the oscillations of an electric system of the Hertz kind. He was just able to show first that e/m was more than 200 times that of hydrogen and then that it was more than 2000 times, with an indication that this was not far from the true value. This early work was not exactly a measurement, but it certainly indicated that e/m was much larger than it was for hydrogen. Wiechert assumed without proof that e is the same for cathode rays as for hydrogen and explicitly stated that cathode rays were not ordinary atoms. He did not apparently consider them universal constituents of matter but rather as "atoms of electricity."

Although the value of e/m was 1000 times larger than that for a hydrogen atom, this was not direct proof that the difference was due to small mass. It might conceivably, though improbably, have been due to a much larger charge. Improbably, because it was unlikely that this large charge would be the same for different gases and for various electrodes, as J. J. had shown it to be. Lorentz, in commenting on Zeeman's discovery in 1896, had suggested that the electric atoms concerned in it had mass of about 1/1000 of that of the hydrogen atom. However, measurements of the charge on various ions were being made. Townsend in the Cavendish Laboratory had measured the charge on some gaseous ions evolved during electrolysis, probably as a result of splashing. He got a value near that then assumed for the charge on a monovalent atom in electrolysis but found that the charge on hydrogen was from one-third to two-thirds that on oxygen. Later in 1898, J. J. measured the charge on the ions that he and Rutherford had shown were produced by x-rays in gases. He used a method based on C. T. R. Wilson's work on clouds. Drops were produced by expansion, their size was determined from their rate of fall, and their number was determined from the size and the mass of water available to be deposited. Then e came from knowing the saturation current produced in the gas by the x-rays. The original value was 6.7×10^{-10} electrostatic units, but it was corrected 3 years later in a second experiment to 3.2×10^{-10}, because it was thought that in the first experiments the negative ions had a tendency to monopolize the vapor and that the whole of the positive ions had not been brought down.

Although it was interesting that this charge was of the order of that expected from the charge on a hydrogen atom in electrolysis, the x-ray ions had no direct connection with the cathode rays or, indeed, with Zeeman's electrons. But in 1899 J. J. made a determination of both e and e/m for the *same* kind of ions—namely, those produced by ultraviolet light. He showed that e/m for these ions was approximately the same as that for the cathode rays and that e was the same as for the ions from x-rays. He also showed in the same paper that the negative particles emitted from a hot wire had approximately the same e/m. This really completed the proof. Opposition to the idea of particles smaller than atoms did indeed continue, but it was merely the spasmodic dying kicks of the older physics, a matter of muscular contraction rather than brain. J. J. preferred to call his particles corpuscles, since, at first at any rate, their identity with the electrons of Lorentz and Zeeman was not proved, but the more specific and vivid name has won. I hope myself that it will continue to be used for the primitive negative particle. I have not that desire for symmetry which would make me feel that because we call one particle a positron we must call another a negatron. One is, after all, much commoner than the other.

The early experiments were rather crude, but values of e/m quickly became accurate. The value of e is more difficult to measure, and it was not till R. A. Millikan improved a method due to H. A. Wilson at the Cavendish Laboratory that a reasonably accurate measurement was arrived at. In fact, at the time of Millikan's work, the most accurate measurements were those made on alpha particles with the assumption that each carried a double charge.

I have mentioned J. J.'s early interest in Mayer's experiments. He continued to take this as his model of an atom, except that he replaced the central magnetic pole of the analogy by a uniform distribution of positive electricity throughout the atom. His preference for this model over the perhaps more obvious one (or so it seems now) of planets controlled by a central sun was not merely one of mathematical convenience. He proved that on Newtonian dynamics, and no one had then suggested anything else, such a system of orbits would be unstable if the planets repelled one another, and even if they were not, that they must radiate, by Maxwell's equations, large amounts of energy. The nuclear atom of Rutherford requires the hypothesis of Bohr or something similar to make it tenable, and that, of course, was then a long time in the future.

J. J. spent a good deal of effort on his model. He examined the equilibrium conditions of atoms with large numbers of electrons and showed that an arrangement of rotating concentric rings would be stable and would lead to a qualitative explanation of the periodic table. He also proved that a ring of more than a small number of electrons would radiate extremely slowly when rotating, another reason for preferring his to the planetary model.

Beta rays were quickly shown by Becquerel to be electrons, and the variation of their mass with velocity found by Kauffmann was the first experimental proof of the electromagnetic mass that J. J. had predicted in 1881. Delta rays were the discovery of J. J. (1904), one of his few incursions into radioactivity; they were named by him.

A more far-reaching piece of theory was the attempt to explain conduction in metals by means of electrons. The idea goes back to Weber, but J. J.'s theory, first put forward in 1881 and elaborated in 1900 and 1907 after the experimental work on free electrons, was the first that stressed the exclusive part played by the negative electricity. It regarded the electron as not entirely free, but operating in something of the fashion that the charged atoms in solution did in the Grotthus chain. Here again, of course, was a field where no satisfactory explanation could be produced until the quantum theory had been discovered. J. J.'s reason for retaining from his early theory the rather artificial idea of the Grotthus chain was to avoid the difficulty of the specific heats, which it does up to a point. About the same time O. W. Richardson, working in the Cavendish Laboratory, showed how the thermionic emission of electrons could be explained on a kinetic theory and a formula could be deduced, in part at least, from thermodynamical reasoning.

One of the most interesting questions was the number of electrons in an atom. At first it seemed reasonable to suppose this to be of the order of thousands, the ratio of the masses. Some 10 years after the discovery of free electrons, J. J. published a paper in which by three independent methods he reached the conclusion that the number of corpuscles, as he still called them, in an atom, was not far from the chemical atomic weight. The first method was the dispersion of light in gaseous atoms in which a calculation based on a dispersion formula resembling that of Sellmeier showed that hydrogen has about 1 corpuscle per atom, assuming that it is the corpuscles that move.

The second method is the well-known one based on the formula for the scattering of electromagnetic waves by charged particles, which he had calculated, and the experiments of Barkla on the scattering of x-rays. As we now know, the good result of this method was rather lucky. Long waves will not work because the individual electrons scatter in phase, and for short waves the quantum effects become prominent. However it gave a value for air of about 25 corpuscles per molecule, less than twice too large.The last method was based on the diminution of velocity of beta rays going through a gas. Attributing this to energy transferred to the corpuscles in individual collisions, he showed from Rutherford's measurements that the number and atomic weight are of the same order. Here there is a logarithmic factor that had to be guessed.

Positive Rays

In 1905 J. J. turned from the study of cathode rays to that of positive rays, in a conscious effort to find the positive constituent of matter. Positive rays had been studied for some time by Wien under their German name *Canalstrahlen*. One may recall that they are formed when a hole is made in the cathode of a discharge tube and then appear as a luminosity in the gas in the region behind the hole. Wien had shown that their e/m was appropriate to that of atoms, but his apparatus did not allow him to resolve the rays coming from different kinds of atoms. J. J. at first thought that he had got a universal constituent, but better experiments and, in particular, the technique that enabled the region beyond the cathode where the rays were observed to be obtained at a much lower pressure than the discharge in which the rays were produced, enabled him to show that the rays depended on the gas in the tube and, in fact, consisted for the most part of charged atoms and molecules of the kinds present in the discharge.

Thomson's was the first mass spectrograph, although he did not use the term. The different atomic species appeared as parabolic arcs on a photographic plate that received the rays. I have vivid recollections of him sitting on a high stool in the laboratory measuring up the last photograph his assistant, Everett, had obtained, the plate clamped in a frame and the arcs measured by means of a needle point carried on two perpendicular slides. The most startling consequence of this work was to come after World War I at the hands of Aston, who had been working as J. J.'s assistant for the 2 or 3 years before 1914.

But there was really more to it than just isotopes. The sharp parabolas were in fact the first real proof that the atoms of ordinary elements are all the same and that atomic weight is an individual property and not merely the mean of a wide statistical variation. Of course, this is true only where isotopes are absent, and, of course, a method that could prove it in certain cases must necessarily discover the isotopes in the others. Actually the situation was a little harder than this would seem to indicate, because the positive rays showed not merely atoms but compounds, and not merely the ordinary valency compounds but almost every kind of fragment of the molecules in the gas; thus water shows OH as well as OH_2, and methane shows all the radicals CH, CH_2, and CH_3.

With the limited pumping facilities of the age, it was very difficult to get rid altogether of the hydrogen that had been absorbed in the electrodes, and, since the accuracy of measurement was only of the order of 1 percent at best, it was not always easy to be sure whether a parabola was due to a new atomic species or to an undiscovered hydride of something else. Hence, when neon was tested and two parabolas turned up with masses of about 20 and 22, it was difficult to be sure whether they represented two species of neon or neon and an unknown hydride, although J. J. inclined strongly to the former view, basing his belief on peculiarities of the parabolas which resembled those of atoms rather than those of molecules. After the war, as is well known, Aston, after an only partially successful attempt to separate the two kinds of neon by diffusion through pipe-clay, made a mass spectrograph of different design and superior resolving power. It enabled him to measure the masses precisely and show that their mean was the atomic weight of neon as determined by its density.

The positive rays had some interesting secondary consequences. They showed the existence of several previously unknown compounds, including, for example, a molecule of hydrogen with 3 atoms, the existence of which prevented the earlier discovery of heavy hydrogen by masking the mixed molecules DH, although J. J. had advanced the opinion that there were two kinds of "3" in his experiment.

Quite early in the course of these researches J. J. pointed out the merits of positive rays as a method of chemical analysis, stressing the small amount of material required. For a long time no application was made, but now the method, as is well known, has found an important place in technology, especially for the analysis of oils.

J. J. was an indomitably hard worker. An incomplete list of his papers numbers 187, mostly quite substantial. He wrote a textbook on electricity and one on the properties of matter, the last a part of a series which he and Poynting undertook together, although Poynting did most of the others. He also wrote his *Conduction of Electricity through Gases,* which passed through three editions, each much larger than the last. Then there were monographs such as the *Silliman Lectures* already quoted, *The Discharge of Electricity from Gases, Corpuscular Theory of Matter, Rays of Positive Electricity, Motion of Vortex Rings, Application of Dynamics to Physics and Chemistry,* and *The Electron in Chemistry,* the last an application of electron theory to chemistry rather on the lines and roughly contemporary with the work of Lewis and Langmuir, which he published after World War I.

But it may fairly be said that Thomson's greatest contribution to physics was not in his own work but in the work of the school that he founded in the Cavendish Laboratory, which trained a large number of the ablest physicists of his day of almost every civilized nation. Eight of his pupils have received the Nobel prize. A great part of the work of this school was devoted to the elucidation of the intricacies of electric conduction and discharge in gases, and many of J. J.'s papers, both theoretical and experimental, were on this theme. He was specially fascinated by the electrodeless discharge, which he was one of the first to study; a paper he wrote on it when he was over 70 is still quoted, and his work on recombination of ions is the basis of much modern theory.

Conclusion

In many ways J. J. was a man of paradox. Although he was a great experimentalist with a remarkable power of diagnosing the diseases of a piece of apparatus, he was very clumsy with his hands—except indeed in penmanship, his handwriting being both clear and forceful. In designing apparatus he always used the simplest means and had, I think, no great love of apparatus for its own sake. He would never, for example, have developed C .T. R. Wilson's cloud technique into a cloud chamber, though he used it to measure e. Again, although he was an accomplished mathematician with a mastery of the techniques of his day, he seldom used mathematics as an indication to where a theory should lead. The ideas were usually intuitive, based on visualization and analogies, not on mathematics. When occasionally he departed from this practice, as when he rejected the planetary atom for reasons of stability, the mathematics let him down. He was not afraid of making mistakes and put forward his fair share of ideas which failed. He was always intrigued with the conception of energy and of its different forms and made more than one attempt to reduce them all to the kinetic energy of imaginary particles, an idea which may perhaps yet prove to be true.

Outside the laboratory—or indeed in the laboratory during the break for tea which he instituted—he was a man of wide interests and usually preferred to discuss events or people rather than physics. There were few subjects, except music, which he could not discuss and fewer still in which he took no interest. When he became master of Trinity College, he enjoyed the college life, especially the contacts which it gave him with undergraduates, and it was by no means exclusively the intellectual ones with whom he was friends. Although he had never been particularly good at games, he was keenly interested in them, and of some at least was a shrewd critic.

He judged other physicists largely by the originality they displayed but was on the whole more appreciative of other men's experiments than of their theories. He was very reserved in speaking of those things about which he really cared but had warm feelings and strong beliefs. To him physics was something to be approached with the enthusiasm which he valued so highly in the young physicist, but also with a certain reverence, the reverence due to the infinite.

"A great discovery is not a terminus, but an avenue leading to regions hitherto unknown. We climb to the top of the peak and find that it reveals to us another higher than any we have yet seen, and so it goes on. The additions to our knowledge of physics made in a generation do not get smaller or less fundamental or less revolutionary, as one generation succeeds another. The sum of our knowledge is not like what mathematicians call a convergent series . . . where the study of a few terms may give the general properties of the whole. Physics corresponds rather to the other type of series called divergent, where the terms which are added one after another do not get smaller and smaller, and where the conclusions we draw from the few terms we know, cannot be trusted to be those we should draw if further knowledge were at our disposal."

G. Dahlberg, Human Geneticist

Gunnar Dahlberg, professor of human genetics and director of the Swedish State Institute of Human Genetics at the University of Uppsala, died on 25 July 1956, just a month before his 63rd birthday. He was educated in medicine at Uppsala and admitted to practice in 1920. His interest, however, was in research, and in 1921 he became research assistant in the newly established State Institute of Race Biology at Uppsala (Statens Rasbiologiska Institut) under Lundborg, whom he succeeded as professor and director in 1936. In 1945 a cerebral hemorrhage paralyzed his right side and left him with a permanent impairment of speech, but within 2 months he was back at work and continued until ill health caused his retirement in 1955.

Dahlberg's first major work was a monograph on *Twin Births and Twins from a Hereditary Point of View*. This was the dissertation for which he received the M.D. degree from Uppsala in 1926. This comprehensive work signalized the resumption of interest in twins under the influence of modern genetics, demography, and anthropometry. It contained detailed data in 191 pairs of monozygotes and 52 pairs of dizygotes, diagnosed by the polysomatic method, and a hypothesis of twinning as due to hereditary predisposition to double formation in the oocyte before the reduction division. The center of Dahlberg's interest, which dominated his subsequent work, was indicated in a single chapter on "Hereditary factors in populations," in which evolutionary forces such as selection and mutation on gene frequencies were evaluated by a simple calculus.

Dahlberg's chief contributions thereafter were concerned with theories of the genetical struture of human populations, culminating in his last book, *Mathematical Methods for Population Genetics* (1947), and his paper on "Genetics of human populations" in *Advances in Genetics,* volume 2, 1950. Of particular importance was his emphasis on the existence, within a total population, of partial populations, called isolates, within which random mating occurs. For Dahlberg [see *Human Biology,* **14**, 372 (1942)] the isolate concept underlies the concept of race and provides the rationale for a genetical theory of race formation. Since Dahlberg's influence helped to give a new form and direction to the study of human genetics, it may be well to quote his own statements [*Advances in Genetics,* **2**, 96 (1950)]:

"Human genetics have arisen from plant and animal genetics. In Germany this branch of research came to be influenced by ideas about race, as can be seen, for example, in the German term, *Rassenbiologie*. In that country it gradually came to enter into, and be influenced by, the Nazi ideology.

"In England, human genetics came to be linked with Galton's eugenics (the doctrine of the well-born), and was therefore extensively directed towards the investigations of families considered to be of particularly high quality.

"Thus, with human genetics focussed on population problems connected with politics as it was in England and Germany, no very important research was done. The main result from this point of view was subjective colored propaganda literature without any scientific value worth the name."

The change which came about in Dahlberg's scientific lifetime is illustated by the change in activity of his own institute from race biology to human genetics; Galton's Laboratory of Eugenics at University College, London, has become a laboratory of human genetics; the Eugenics Record Office at Cold Spring Harbor (founded 1910) has been absorbed by the department of genetics of the Carnegie Institution of Washington, and similar changes have occurred elsewhere. Dahlberg's influence was to direct human genetics into channels of research on evolutionary forces at work in human populations, and for this research he provided not only the strongly held and stated views quoted here but useful simple methods (sometimes called Mendelian algebra) and ideas to be tested by them. His final views on the essential problems of human genetics are in the concluding paragraph of the aforementioned paper: "But necessary though it may be to develop the theoretical-cum-mathematical side of the problems, the primary need is for empirical investigations of the processes taking place in human populations. We require both knowledge of the frequency of intermarriage, assortative mating, the fomation of isolates, etc., and also investigations of the actual frequency of individual characters in populations. We have, however, still very little possibility of comparing the make-up of a population at different junctures, or of comparing different populations at the same juncture. A great deal must be done to achieve an empirical foundation for the assessment of populations from the viewpoint of heredity. But this must be regarded as a very important task for human genetics to carry out" [*Advances in Genetics* **2**, 97 (1950)].