siderably larger proportion of green to get the yellow sensation. With a more elaborate apparatus designed by Rayleigh, I examined, on my return to Manchester, seventy-two persons and found among them four possessing the same peculiarity and one with an anomaly in the opposite direction. Among the four was a woman with two of her three sons. It is a curious coincidence that the colour sense of Clerk Maxwell was, and that of J. J. Thomson is, affected in the same way. An examination of the present director's sight is obviously indicated.

Though not belonging to the Cambridge period, Rayleigh's great work on the weighing of gases should be mentioned, because it was really planned at Cambridge. He frequently referred to the desirability of keeping a research going as a 'stand by,' that is, an investigation which presumably could be continued for a long time and dropped or taken up again as more urgent demands had to be satisfied or a slack time occurred. His choice fell on the weighing of gases, because he was always impressed by the probability of a unity of matter and the likelihood of the correctness in some form or other of Prout's law. Quotations from his address as president of Section A of the British Association giving his views on the subject will be found in the biography written by his son.

During Rayleigh's tenure of office at Cambridge a systematic and very successful instruction in laboratory work was introduced by Glazebrook and Shaw. For this and research purposes a substantial annual income was required, and Rayleigh raised a fund of 1500*l.*, to which he himself contributed one-third.

When he gave up the directorship of the Cavendish Laboratory, it was in a highly efficient state both as a teaching and research institution.

Sir J. J. Thomson, O.M., F.R.S.

By Sir Oliver Lodge, F.R.S.

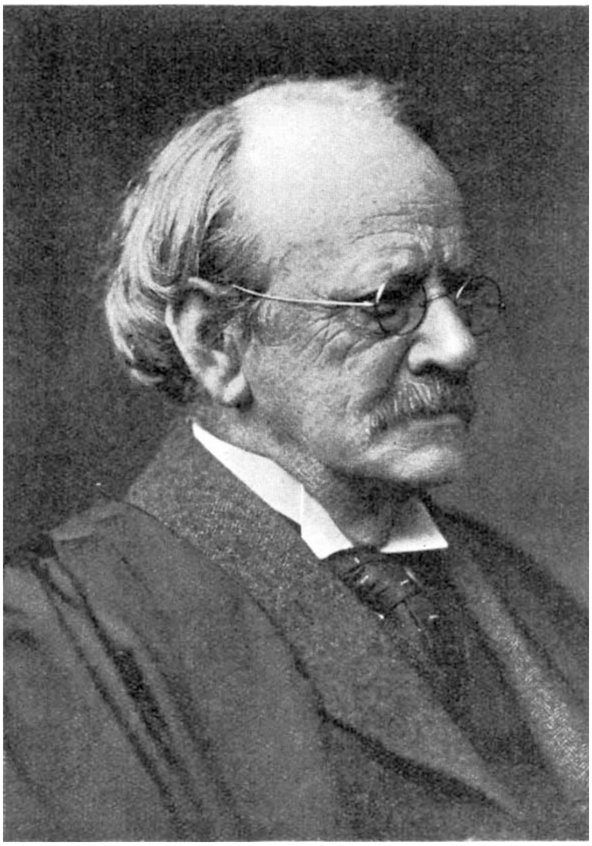
HOW much less the world would know if the Cavendish Laboratory had never existed; and how diminished would be the glory even of that laboratory if Sir J. J. Thomson had not been one of its directors! We used to think of him as one of the younger physicists; but now that his seventieth birthday is being celebrated that notion must be given up, even by his seniors. But, whether young or old, we have all venerated him for his brilliant achievements.

The discovery of the electron and the foundation of the electrical theory of matter cannot, any more than other fundamental discoveries, be attributed to any single man: these great advances are the outcome of the work of at least a generation. To them Helmholtz, Crookes, Johnstone Stoney, Sir Joseph Larmor, and in the electrolytic stage even Faraday, have all contributed; and, doubtless, in mentioning some names I am omitting others. But researches into the phenomena connected with the discharge of electricity through gases have been Sir J. J. Thomson's special field; and, as Clerk Maxwell hinted would happen, that branch of inquiry has thrown great light upon the nature of electricity itself. Very few before our time can have supposed that electricity was discontinuous. Maxwell's equations and Cavendish's experiments either postulate or appear to demonstrate continuity and incompressibility. That electricity was a fluid, comparable in any respect to a gas, seemed like a popular superstition. The discovery of a discontinuity in Nature must always have notable consequences; and though we may be willing to grant that ultimately every atomic character will be resolved into a deeper-seated and more fundamental continuity, yet for a long time it will be the business of science to absorb and work out the consequences of every discontinuity that is revealed.

John Dalton was the earliest to emphasise the chemical discontinuity of matter. J. J. Thomson is likewise the first to emphasise effectually the atomic character of electricity. His work is a happy combination of experimental and mathematical ability. He arranges ingenious experiments to display and dissect the phenomena; and at the same time most skilfully applies dynamics to analyse those phenomena in an illuminating and metrical manner. In his hands the magnetic deflexion of cathode rays observed by Crookes, coupled with certain other experiments by C. T. R. Wilson and J. S. Townsend, sufficed to determine the mass and the speed of the electric particles; and when electric deflexion of cathode rays was combined with magnetic deflexion, the determination could be made in a particularly neat and convincing manner. So that when Sir J. J. Thomson gave an account of his researches throughout the years 1897 and 1898, before Section A of the British Association meeting at Dover in 1899 (in the presence of a number of Continental physicists, many of whom had come over that day from Boulogne), the whole world rose to the conviction that a new era had dawned in electrical science. A foundation-stone was then laid for the innumerable researches which have gone on during the present century in every laboratory and library of the world.

The facts are so well known now that it is needless to elaborate them: within thirty years they have become almost ancient history. Naturally the renowned discoveries of Becquerel, Röntgen, and Madame Curie fell into line. The discovery of Zeeman, verifying that the radiating particle was not an atom but an electron, worked out initially by the genius of H. A. Lorentz, fell in also with a classical theory of radiation conforming to ideas promulgated by Hertz and Larmor and FitzGerald; though it is true that certain difficulties and puzzles afterwards arose, in which all physicists, including J. J. Thomson himself, are now deeply involved.

We used to think that Clerk Maxwell had explained to us the nature of light; and so no doubt to a great extent he had; but our theory of the emission and absorption of light has been singularly complicated by the action and reaction between electrons and radiation, to which O. W. Richardson, Barkla, and other experimenters, including Compton, have contributed so much information. Especially have both complication and illumination been enhanced by the discovery of that new discontinuity which now penetrates our treatment of every interaction between ether and matter—the outcome of singularly successful speculative reasoning by Max Planck at the beginning of the present century.



Photo] [*J. Palmer Clarke.*

FIG. 5.—SIR J. J. THOMSON, O.M., F.R.S., Director 1884–1919.

The accuracy with which both the charge and the mass of the electron are now known (largely through the ingenious measurements of Prof. Millikan), and the researches of spectroscopists all over the world, from Kayser and Runge, Schuster and Hicks, Balmer and Rydberg, down to Prof. A. Fowler—illuminated as they are by the discoveries of Rutherford and the brilliant constructions of Bohr—have raised an enduring monument, a sort of cathedral, glorifying the electrical theory of matter. To the completion of that splendid structure, the present generation, and probably many future generations, are contributing and will contribute. All this must be a great satisfaction to the Master of Trinity, who is still so happily engaged in his own researches that he has perhaps scarcely time to bethink himself of the beauty of the edifice which has been raised on his foundation-stone.

Of course J. J. Thomson has done many other things. At a very early stage he secured the Adams Prize for a theory of the behaviour of vortex rings, which may still one day be usefully drawn upon when the hydrodynamical structure of the ether, already begun long ago by Larmor, is seriously undertaken. Again, the electrical theory of matter would be nowhere if we did not know that an electric charge possessed inertia. The resolution of matter into energy, insisted on by the theory of relativity, has as one of its props that early paper of J. J. Thomson's in 1881 (forty-five years ago !), in which he calculated the extra mass conferred on a body by an electric charge; a suggestive idea to which both he and Heaviside contributed the further illuminating fact that that inertia would be increased by locomotion; a prediction which, when verified by Kaufmann, made an electrical or ether-field theory of matter inevitable.

Thomson did not limit himself to metrical determinations about negative corpuscles and cathode rays. He attacked also the positive or matter rays; and thereby developed a fundamental technique of positive-ray analysis, which speedily detected a variant of neon, and then in the hands of Aston defined atomic weights with altogether unexpected precision. This method, brilliantly and pertinaciously applied, has fully upheld the isotopic conception of Soddy (preceded as that was by the unorthodox speculation of Crookes at the Birmingham meeting of the British Association in 1896), and has thereby not only brought to our ken a host of new substances with similar chemical but different physical properties, but also has established in modified form the hypothesis of Prout, that atomic weights are really whole numbers, of which hydrogen is very nearly, though not quite, the common unit.

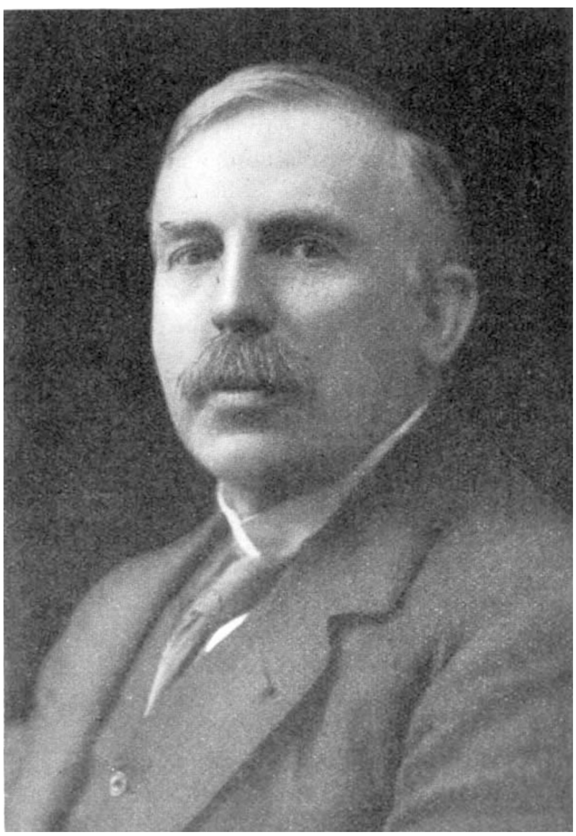
Readers who wish for a more detailed reminder of the steps which have led to these great advances may refer back to NATURE, vol. 91, p. 1, where Prof. Righi of Bologna contributes an article appreciating, from the Continental point of view, the work of Sir J. J. Thomson up to the date 1913.

Students and disciples all over the world could contribute far more details. His own son, the professor of physics at Aberdeen, is one of the brilliant products of the Cavendish Laboratory; and many prominent physicists, such as the present Lord Rayleigh, could testify with intimate knowledge of the work of that laboratory during Thomson's régime. They know the doubts and hesitations which had to be set at rest before the absolute uniformity of electronic charges could be confidently asserted. They know the persistent help given by Mr. Everett, his laboratory assistant for nearly forty years. They are acquainted with the incipient stages of many discoveries. But an older physicist esteems it a privilege to write this brief appreciation of the achievements of one who has worked with unexampled power in the borderland between chemistry and physics, who has introduced into that great science of chemistry revolutionary conceptions the end of which none of us can see, and who is still happily flourishing and active.

Sir Ernest Rutherford, O.M., P.R.S.

By Prof. NIELS BOHR, For.Mem.R.S., University, Copenhagen.

FOLLOWING the kind invitation of the editor to write a few words in appreciation of the work and influence of the present director of the Cavendish Laboratory, I presume that the readers of NATURE will not need any detailed exposition of his achievements. As, however, I am one of those who have had the good fortune to come into close personal and scientific contact with Sir Ernest Rutherford, it is a great pleasure to me to try to describe briefly how we, who are proud to count ourselves among his pupils, regard him.



Photo] [*J. Russell and Sons.*

FIG. 6.—SIR ERNEST RUTHERFORD, O.M., P.R.S., Director 1919–

My own acquaintance dates from the period when Rutherford, after years of ardent and successful collaboration with Sir J. J. Thomson in the Cavendish Laboratory, had left Cambridge, and —after his stay at McGill, where his work on radioactive substances had established his fame—in Manchester had founded a school for investigations in radioactivity. This centre attracted young scientists from all parts of the world. In the spring of 1912, on my first visit to Manchester, the whole laboratory was stirred by one of the great discoveries which in so full a measure have been the fruits of Rutherford's endeavours. Rutherford himself and his pupils were eagerly occupied with tracing out the consequences of his new view of the nuclear structure of the atom. It would give only a poor impression of our trust in his judgment for me to say that nobody in his laboratory felt the slightest doubt about the correctness and fundamental importance of this view, although naturally it was much contested at that time. I remember being told by Hevesy soon after my arrival the story circulating in the laboratory of how Rutherford, shortly before his discovery, in a conversation with Moseley expressed the opinion that after all the troublesome investigations of the preceding years—during which he had such faithful assistance from Geiger — one would have had quite a good notion of the behaviour of an α-ray, were it not for the return of a minute number of these rays from a material surface exposed to an α-ray bombardment. This effect, though to all appearances insignificant, was disturbing to Rutherford, as he felt it difficult to reconcile it with the general ideas of atomic structure