there was any result; for that would give "the electric current's true direction," or alternatively let it "be your boast to prove . . . that there is no Electric Fluid." This test for sensible inertia of the electrons, as we would now say, has been carried through to success in America only the other day.

Plenty of electric discussion was, however, going on, especially in the years just after Maxwell's death. It was regarded as a great improvement when Heaviside and Hertz, nearly simultaneously, got rid of his vector potentials as being mere mathematical figments, though with him of heuristic dynamical origin; yet the two resulting circuital relations, as Kelvin called them, had already been formulated long before by Maxwell himself, as a concession to a demand for the essential outcome of his theory, in concise form freed from tentative dynamical implications. But these circuital equations are concerned only with the smoothed-out electrodynamic fields. Ultimate dynamical theory, going back to the sources of the field, has not yet been able to do without their vector potentials. Nowadays the circle has indeed gone round full tilt; we have been familiarised with the point of view that the electric and magnetic fields, so tangible in the world of engineering, are in theory only two partial aspects of one six-vector, itself the (Hamiltonian) gradient of a fourfold vector potential that alone is fundamental in Nature, as presented to our minds.

Maxwell's other main contribution to science, equally monumental, lay in the domain of the molecular kinetic theory of gases; it provided the more severe mathematical occupation of his later years. He had taken over the subject in his early days from Joule and Clausius and their predecessors, in the form of a rough *aperçu* of the phenomena of a crowd of independent moving molecules: he converted it into an exact theory, thereby creating the science of statistical dynamics which dominates modern molecular physics. The root principle of that science is Maxwell's law of distribution of velocity, or of other quality, in a multitude of molecules which has attained a steady state. As results there came to him exact dynamical theories of friction and diffusion and conduction of heat in gases, and analytical developments for rarefied gas arising out of the phenomena revealed by the Crookes radiometer. Here also Boltzmann was in readiness to follow up this train of research. From their memoirs in general statistical dynamics, the law of equipartition of energy among the various modes of molecular freedom stood out as a cardinal result. The problem of how the inadmissible consequences of this law are to be evaded has opened up new regions of physics, practical as well as speculative, which at present tend to dominate the whole field.

A characteristic illustration of his genius was the early enforcement of the averaging character of the processes of thermodynamics, by appeal to the possible achievement of ideal minute intelligences, named Maxwell's demons by Kelvin, who could by merely guiding or sifting interference upset the fundamental principles of that science; this arresting quip carried the new doctrine of the statistical character of natural law for molecular structures into regions of thought where abstract dynamical argument could scarcely have penetrated. His lucid expositions in formal thermodynamics need only be mentioned; in them he appeared mainly as the simplifier of the fundamental advances achieved in the American work of Willard Gibbs, the foundation of modern physical chemistry.

Maxwell spent his summers on his small estate of Glenlair in Galloway, among his own people, living as a Scottish laird. Doubtless it was there, among the solitudes of the hills, that illumination mainly came. In the autumn he usually attended the British Association, where, as one used to hear, his gaiety and humour were looked forward to as enhancing the value of the annual scientific discussions, then at their prime. The writer recalls that, returning to Cambridge as an undergraduate one October, a man of the type of a country farmer came into his compartment of the train at Dalbeattie, remained silent for a time, then remarked with emphasis, as something that concerned the world to know, to this effect: Clerk Maxwell has been taken away, mortally stricken; he will never come home again. He died in 1879 at only forty-eight years of age.

Lord Rayleigh.

By Sir Arthur Schuster, F.R.S.

MAXWELL'S health began to fail in the early part of 1879. Troubled by his wife's illness, which weighed heavily upon him, his accustomed good spirits had left him; but there were no signs that he himself was suffering from a mortal disease until his return from the summer holidays, when we were shocked to hear that he had only a few weeks to live. His death was a calamity which might have been fatal to the continued prosperity of the Cavendish Laboratory had Lord Rayleigh not consented to accept the professorship. His hesitation and the pressure put upon him by those who had the interest of the laboratory at heart, are set out in the 'Life' of the father written by the son.

After his election, Rayleigh lost no time in making plans for the conduct of his new duties. I am sorry not to have preserved a letter I received from him asking whether I had any suggestions to make, as I had gained some experience in its working during the two years I had spent in the laboratory. Others received similar requests, but I doubt whether the new director of the laboratory received any useful hints on essential matters.

On his own initiative Rayleigh adopted a definite and novel policy, its essential point being the fostering of a spirit of community among the advanced students. To make a beginning, he desired to identify the laboratory as a whole with some research in which a combination of workers was necessary. The question of electrical units had, at that time, gained practical importance and the subject seemed suitable for a combined attack. There was the additional reason that Maxwell had been connected with the original determination of the Ohm fathered by the British Association, and that the principal piece of apparatus, the rotating coil, was preserved in the laboratory.

Different methods of procedure having led to values which differed more than was desirable, Rayleigh decided to repeat the former work, using the same coil, but paying more attention to what others might have considered minor details. The method by which the rotational velocity of the coil was kept constant was simple and effective, and the same applies to the very ingenious method devised to compare the frequency of the tuning-fork, to which the rotation was referred, with the rate of a standard pendulum clock. When sufficient experience had been gained with the original coil, a new one was constructed giving a result which was decidedly more accurate than any obtained up to that date. Rayleigh was disappointed in his original desire, no volunteers besides myself offering to help, until Mrs. Sidgwick joined us, and the later history of the research is well known.

FIG 4.—LORD RAYLEIGH, Director 1879-84.

I may, perhaps, here refer to a personal recollection which has always remained in my mind. During the early stages of the investigation of the Ohm, Lord Rayleigh made the remark to me that this was the first piece of work he had undertaken that required great accuracy of measurement, and he added that he was not at all sure that he would really like it. I replied that I felt sure that the satisfaction of getting another decimal place in a physical constant was a pleasure which would grow on him more and more. Later events, I think, justified my remark.

Persevering in his desire to form a kind of research community within the laboratory, Rayleigh introduced an innovation that may seem trivial, but has been effective in more than one laboratory since. A tea-interval was introduced, during which the different workers could meet and join in informal discussion on scientific problems. Tea was served in a room in which other experiments were kept going, and those who had the privilege of attending these informal scientific meetings will more especially remember the water jets breaking into drops, and the effects of electricity on the appearance presented by them. There were also the spinning colour discs in which we could compare our colour senses.

A good example of Rayleigh's method of work was shown by the little appliance made with card-board, sealing-wax, simple glass lenses, and a prism or two, by which he could determine the relative amount of red and green required by different persons to produce the yellow of a sodium flame. By comparing the results of different observers it was found that, while most persons agreed fairly well in their estimate, there are anomalies which run in families. Lord Balfour and two of his brothers, for example, required a considerably 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