the same kind and form a part of every kind of matter. The number of electrons in the atoms of the different elements was determined by applying a theory of the scattering of these rays by electrons which I had worked out from the valuable measurements by Barkla on the amount of radiation scattered by different gases, and it was found that this number was proportional to the atomic weight of the element and was of the same order as that number. This led me to construct a theory of the structure of the atom which had special reference to the chemical properties of the element, for it was shown that such a structure would give a variation of the chemical properties with the atomic weight quite similar to that expressed by Mendeléef's law. Another excursion I made into chemistry was to use the magnetic and electric deflexions of the positive rays to determine the chemical composition of the gas in which they were generated. It proved a useful method, and Dr. Aston has modified and improved it, so that it is now one of extraordinary delicacy and accuracy.

The number of those engaged in research grew very rapidly, and for many years before the War there were always thirty or more researches going on in the laboratory. This put a great strain upon the apparatus and upon the workshop. We have been very fortunate in our chief assistants, and the laboratory owes much to Mr. Sinclair, Mr. Bartlett, and Mr. Pye, and especially to Mr. Lincoln, who has been chief assistant since 1899. Personally, I am greatly indebted to the skill and care of Mr. E. Everett, who has been my private assistant for nearly forty years, and has given me most valuable and able assistance in my investigations.

It is impossible here to mention individually the research workers between 1895 and the beginning of the War: their number is too great. Suffice it to say that three of them have gained the Nobel prize, twenty-two have been elected fellows of the Royal Society, and more than fifty have become university professors of physics. When the War began the usual work of the laboratory stopped. The research workers went either to the front or to laboratories formed for developing and testing methods likely to be of use to the fighting services. Our own workshop was employed in making gauges, and all the research at the laboratory was war-work. Soon after the end of the War my duties as master of Trinity obliged me to resign the professorship. The University was fortunate enough to induce Sir Ernest Rutherford to take the post. At the same time, with great kindness and consideration, they gave me a position which enabled me to retain my connexion with the laboratory which for more than forty years had given me unrivalled opportunities for doing the work I liked best, which has brought me many cherished friends and has created rich memories of kind acts, good fellowship, and goodwill.

James Clerk Maxwell.

By Sir JOSEPH LARMOR, F.R.S.

THE course of evolution of the career of a man of genius is always an interesting, and should be an instructive, study. In the case of Clerk Maxwell the materials are ample, thanks to the care of his two biographers, one of them a classical scholar with Greek predilections, the other his intimate and sympathetic scientific assistant. Their account leaves an impression of the absence of any formal education outside his home life. His father was his chief early friend; his real intellectual initiation was identical with his amusements, mainly concerned with dynamical contrivances such as spinning tops, and extending into explorations in practical hydrodynamics by feats of swimming in the bath. Perhaps the Scottish atmosphere of practical engineering, fanned into interests of pure science by men such as Robison, Kelvin, Rankine, explained the origin of this mental bent; but it does not account for the literary grace and charm of his writing.

Another main formative influence was the Scottish Calvinist theology, in its more humane and devotional aspect, supported by intimate conversance with the phrasing and poetry of the English Bible, which has so often been the well-spring of distinction in literary expression. This Puritan trend, while it treasured the historical formularies of the Scottish Church, was in him far from sectional; it ramified into enjoyment of literature, such as the poetry of Milton, and also the descriptive and evolutional poetry of Tennyson, whose influence was still dominant in the Cambridge of his undergraduate time.

Another phase of Maxwell's thought was made public in the biography, in some occasional papers of metaphysical and religious import, which had been read to a private Cambridge society. At the time of publication, as one remembers, the interest of them was regarded as mainly personal, being outside the analytic severity of the British psychology of that period. But they were discovered by Prof. Höffding of Copenhagen, and have come back to us, in the English translation of his works, as among the competent pronouncements on the philosophy of Nature.

It is Maxwell's main achievement that he unified physical science, by connecting light and radiation with electricity so as to form one interlocked systematic whole. This advance made the Cavendish Laboratory, from its foundation in 1872, the focus of electrical pure science which it has remained ever since. It must have been remarked by students of his writings how abruptly he left off the development of this electrodynamic and optical theme in the last six years of his life, after the publication of the treatise on "Electricity and Magnetism" early in 1873. For example, no question ever occurred as to the dynamical riddle, then prominent in physical speculation, which was presented by the Fresnel laws of refraction of optical waves: though when Helmholtz, and Lorentz, and J. J. Thomson entered into the study of his theory, they at once recognised independently that the form, at any rate, of these laws was immediately involved, while FitzGerald, going deeper, had already connected the electric theory intimately with the entire *corpus* of the optical theories of MacCullagh.

One reason may have been Maxwell's preoccupation with fundamental objections of fact, operating against his views. His dynamical mentor, Lord Kelvin, to whom he largely owed the impulse to the elucidation through kinematic models of deep-seated physical phenomena, had here failed him, and to the end of his days held to the view that the foundations of the electric theory of light were unintelligible, by which he seems to have meant not sufficiently disentangled dynamically, his own fundamental developments on latent momenta notwithstanding. The wide discrepancy between the index of refraction of a substance and the square root of its dielectric inductive constant was such a threshold difficulty of fact, and loomed important in those days when the influence of dispersion was not very fully recognised. The first fundamental service of the illustrious Boltzmann to the theory of his master in research was the measurement of the dielectric constants of gases, resulting in confirmation of Maxwell's law for such simple forms of matter: while the experimental work of Hopkinson on organic compound substances also brought out significant correlations.

FIG. 3.—JAMES CLERK MAXWELL, Director 1871–79.

Curiously enough, it is now recognised universally, on the initiative of the late Lord Rayleigh, that Maxwell himself had been the pioneer in clear-cut dynamical atomic views of dispersion. He had formulated a fully illustrative scheme, in the guise of a rather detailed examination question in the Cambridge Mathematical Tripos of 1869. This must have implied that the floating dynamical instincts of that time were adequate, at any rate in the view of himself and his co-examiners, for a rapid apprehension and verification of the ideas involved; while the collections of Maxwell's tripos problems, that were treasured and utilised in Cambridge teaching, must have made them not unfamiliar to expert optical students.

There is a cognate question, why in his expositions Maxwell made so slight use of moving electric charges as the originators of the electrodynamic fields which, after the example of Faraday, were his main concern. For the atomic constitution of electricity had been fully established by Faraday's own laws of electrolysis, as Helmholtz afterwards emphasised. The idea of electrons acting on one another through a law of attraction at a distance had been long before placed by W. Weber at the foundation of the reasoned exposition of his fundamental electric measurements. Maxwell himself hovers around the phrase 'atoms of electricity.' He even sets in special prominence the remark of Gauss that what was needed was, above all, some notion of how influence was propagated in time between the sources of the electric manifestations. Gauss confesses that he had not been able to find it; while Maxwell implies that it now lies exposed in his equations of the electrodynamic field. Yet he did not pursue this path. Hertz found no difficulty, in 1887, in determining, on Maxwell's principles, the radiation from a vibrating source such as an oscillating electron. Indeed FitzGerald had already, five years earlier, incidentally determined the nature and amount of the radiation from a rapidly alternating current circuit, that is, from a magnetic vibrator; and on the same occasion he pointed out that a spark from an electric accumulator, discharged through a small resistance, ought to produce electric waves as short as ten metres or less. He thus was well qualified to expound Hertz's epoch-making detection of the Maxwellian waves in free space, in an address to the British Association soon after it was announced.

In other respects also Maxwell had largely confined himself to the medium of electric propagation as a whole, as modified by its content of material atoms and ions in bulk. He had measured, incidentally, with the assistance of Hockin ("Treatise" § 798; more fully in the Memoir[1] of 1864), the transparency of metallic foils such as gold-leaf, and found that it implied effective electric conductance far smaller than the steady value as determined on a bridge. The natural reason for this discrepancy he indicated: also the reason why electrolytes are not opaque; but there he left it, for indeed the relevant experimental knowledge was as yet a blank, while other subjects were pressing on him. We may perhaps add to this the distractions involved in the creation of the Cavendish Laboratory, and the laborious editing and expansion of the Cavendish electrical manuscripts.

In the same context ("Treatise," § 792; more fully in the Memoir of 1864, § 107) he propounds very briefly his law of radiation pressure, now fundamental in physics and astronomy; he tries to estimate the magnitude of the pressure for sunlight, coming to grief in detail over the arithmetic. He perhaps reached the law more by physical instinct than by demonstration. If we imagine a limited train of electric waves reflected back at a perfect conductor, a current sheet is induced on it, and the Amperean force on that sheet constitutes a pressure of the train on the conductor. But the train was originally isolated in space, and will again be isolated with only its direction reversed; thus there is no other source for the pressure exerted by it, on the electrons of the current, but a change, here a reversal, of momentum associated somehow with the radiation. This is the idea developed directly in experiment by Poynting, that a train of radiation is a carrier of momentum. Maxwell associated the pressure with his quadratic stress, in origin purely formal, in the electrodynamic field. Close scrutiny, first in time by Lorentz, reveals, however, an outstanding motional part of the mechanical force which is not absorbed into that stress-tensor, significant though it was; and Poincaré and Abraham remarked that it was just such as could arise from a distribution of momentum in the field, thus without vitiating the stress-representation. The complete forcive on the material content of any region is then expressed as the resultant of this formal electromagnetic stress over its boundary, when there is such stress there, together with the rate of communication of this aethereal momentum throughout its volume: if there is no material content these will balance.

It is, then, the transfer of this postulated momentum of radiation that constitutes radiation pressure on material bodies. The transfer is effected, as above, through Amperean force on the current of electrons. Reversing the argument, radiation pressure, as experimentally confirmed by Lebedew, Nichols and Hull, Poynting, implies momentum in radiation; and its existence to the requisite amount implies a reality, of some kind scarcely yet fully explored, in the Maxwellian quadratic field-stress. An intimate knowledge of the structure of the electron which sustains the stress ought to involve both its own dynamical nature and the structure of the field in which it subsists.

It is not necessary to follow up Maxwell's share in the practical settlement of the scheme of electric units, the essential preliminary to the present electrotechnic age; in that Kelvin was the leader. But we may note his early memoir in determination of the nature of subjective colour, in which he verified with precision the surmise of Young, following on Newton, while Maxwell himself was followed by Helmholtz and by Rayleigh, that all the gorgeous play of colour that is a main glory of our natural world arises from combinations of only three independent elements.

It has been already remarked that Maxwell added little in later life to the essentials of the electric theory. The first edition of the "Treatise" had obviously been thrown hurriedly into the press, in a series of rather disjointed fragments which taxed the ingenuity of dynamical interpretation of his British disciples for years; some additions for the early chapters of a second edition were mainly developments of the usual standard knowledge. But he could still be drawn on the subject. Compare his answer in keen and humorous verse to Tait ("Life," p. 684 (1877), already however in "Treatise" (1873), § 577), who proposed apparently to repeat with a spinning excited dielectric disc Rowland's then recent experiment of the magnetic convection effect of a charged rotating disc, as was afterwards carried through by Röntgen. He wanted Tait to spin instead a copper coil with galvanometer in circuit, suddenly to arrest its motion, and note if

[1] The abstract of this memoir (*Roy. Soc. Proc.* **13**, pp. 531-536), accidentally omitted in the "Scientific Papers," is an interesting general exposition of the author's ideas, largely in line with modern points of view

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