then favoured by physicists. Indeed, it was not the first, nor has it been the last time, that Rutherford's critical judgment and intuitive power have called forth a revolution in science by inducing him to throw himself with his unique energy into the study of a phenomenon, the importance of which would probably escape other investigators on account of the smallness and apparently spurious character of the effect. This confidence in his judgment, and our admiration for his powerful personality, was the basis of the inspiration felt by all in his laboratory, and made us all try our best to deserve the kind and untiring interest he took in the work of every one. However modest the result might be, an approving word from him was the greatest encouragement for which any of us could wish.

When the War broke out, the little community in his laboratory was dispersed. Having, however, then taken up a lecturing post in Manchester, I had the opportunity in the succeeding years of witnessing the undaunted spirit and never-failing cheerfulness of Rutherford even in the most difficult times. Although the study of the more practical physical problems arising in connexion with the defence of his country took up practically all his time and energy in those years, he could still towards the end of the War find leisure to prepare for, and finally accomplish, perhaps his greatest scientific achievement, the transmutation

of an element through the disintegration of the atomic nucleus by impact with α -rays; an achievement which may be said indeed to open up a new epoch in physical and chemical science.

Just at this time Rutherford was, on Thomson's retirement, offered the directorship of the Cavendish Laboratory as his unrivalled successor. I remember on a visit to Manchester during the Armistice hearing Rutherford speak with great pleasure and emotion about the prospect of his going to Cambridge, but expressing at the same time a fear that the many duties connected with this central position in the world of British physics would not leave him those opportunities for scientific research which he had understood so well how to utilise in Manchester. As everybody knows, the sequel has shown that this fear was unfounded. The powers of Rutherford have never manifested themselves more strikingly than in his leadership of the Cavendish Laboratory, the glorious traditions of which he has upheld in every way. Surrounded by a crowd of enthusiastic young men working under his guidance and inspiration, and followed by great expectations of scientists all over the world, he is in the middle of a vigorous campaign to deprive the atoms of their secrets by all the means which stand at the disposal of modern science.

The Cavendish Laboratory: 1876-1900.

By Sir RICHARD GLAZEBROOK, K.C.B., F.R.S.

IT is fifty years since I first entered the Cavendish Laboratory. Eight years previously physics, in the form of questions on heat, electricity, and magnetism, had become a part of the Mathematical Tripos, and in 1869 a syndicate had reported in favour of founding a special professorship to take charge of these subjects, providing him with a laboratory, a demonstrator, and apparatus to make his teaching practical. Two years later, in 1870, the Duke of Devonshire offered to provide the material part of the scheme "so soon as the University shall have in other respects completed its arrangements for teaching Experimental Physics."

Clerk Maxwell became professor on March 8, 1871, and the laboratory was handed over to the University by its donor, the Chancellor, on June 16, 1874, confident, as he said, "that within its walls researches will be carried out which will advance to no small extent the fame of our ancient University." A prophetic statement; for within those walls have worked Maxwell, Rayleigh, Thomson, and Rutherford, men whose names will ever be landmarks in the history of British physics.

To complete the history of the building, a large

ground-floor room for the practical instruction of the M.B. students was added in 1893, while in 1906 Lord Rayleigh offered to devote the greater part of the Nobel prize awarded to him to defraying much of the cost of a new wing; this was completed in 1908. In more recent years the erection of the new engineering laboratory has set free buildings and space adjoining the Cavendish, and a further important extension has been added to the laboratory.

To go back, however, some fifty years. In those days students came slowly. W. M. Hicks was the first. Wm. Garnett had become demonstrator on the opening of the laboratory. J. E. H. Gordon was the first to submit for publication the results of a research conducted at the Cavendish in a paper on the magnetic rotation of water, read before the Royal Society in 1875. When I became a student along with W. N. Shaw and Poynting in 1876, Chrystal and Saunder were at work on the verification of Ohm's law; Schuster joined a few months later. W. D. Niven had an investigation in progress, and soon afterwards Donald MacAlister carried out a modification of the well-known Cavendish experiment on the inverse square law of

electrical attractions. Poynting began his classical research on the determination of the constant of gravitation during this same period.

Experimental physics was a novel subject at Cambridge. In the Mathematical Tripos of 1876, Lord Rayleigh was an examiner and had set a question on the Wheatstone bridge method of measuring resistance, with an easy rider. I had read carefully much of Maxwell's "Treatise on Electricity and Magnetism," published in 1873, but, alas, had paid no attention to Chapter xi. on "The Measurement of

which I had to do this was the large pattern of Thomson's absolute attracted disc electrometer, a horrid monster which few students of to-day have ever seen even in a text-book, and fewer still have used.

However, I soon became absorbed in optical work. Stokes, in his well-known report on optical theories, had described a method of determining the form of the wave surface in a crystal and had applied the method to Iceland spar. He encouraged me to attempt the same for a biaxial crystal and lent me

FIG. 7.—General view of Sir Ernest Rutherford's research room. On the extreme left is the electromagnet and microscope used in experiments on the artificial disintegration of some of the elements by bombardment with α -rays. On the extreme right is part of the pillar used by Lord Rayleigh in his determination of the value of the ohm and the ampere. This room was occupied for more than twenty years by Sir J. J. Thomson, for his experimental researches.

Electrical Resistance." My coach had originally marked it with a large O—omit. It is true he had corrected this later, but I had no idea of what was meant by Wheatstone bridge. My annoyance was great when, on my first visit to the Cavendish, Maxwell himself explained it and I realised the simplicity of the question I had passed over.

In those days there were no organised classes in practical physics. Maxwell set us on to gain acquaintance with some instrument, to adjust it to get it to work and make some measurements. Among my earlier experiences I was told off to measure at regular intervals the E.M.F. of a battery of tray Daniell cells used by Chrystal and Saunder. The instrument with

the goniometer he had used. Schuster obtained for me from Hilger a suitable piece of aragonite and the work went on for some time in a dark room at the top of the Cavendish; eventuating in a paper published in the *Philosophical Transactions* for 1878, in which it was shown that Fresnel's wave surface was consistent with experimental results to a very high order of accuracy.

During the progress of the work Maxwell was a frequent visitor, interested in its progress, and ready to help. In the case of a serious difficulty he would remark that others had been asking him questions and he must take time. On one occasion I remember him explaining how thick the crust was in which their

questions had encased his brain, but the next day he turned up, accompanied as usual by his dog, with the remark, "If you will alter your arrangements in such and such a way you will find your difficulties disappear," and of course they did.

Those were happy days; there have been many since at the laboratory, but the study of a new subject was full of interest, and the gradual realisation of what experimental investigations with the view of the improvement of material knowledge involved, added much to the pleasure of life and fitted one more and more for the teaching work to come in the future.

In 1879 came a change; Maxwell's health gave way during the Easter term of that year, and after a painful illness he died on November 5. The professorship was continued—it was to terminate originally with the tenure of office of the first professor unless the University determined otherwise—and Lord Rayleigh was appointed on December 12, 1879. He had accepted the post for a limited period—five years—and began work early in 1880. He was anxious from the first to organise regular teaching in practical physics, and appointed Shaw and myself as demonstrators soon after his election. The notice of the work for the Michaelmas term, 1880, after announcing lectures by the professor, by Dr. Schuster on radiation, and by Shaw and myself, continues: "The laboratory will be open daily . . . for those who have had the necessary training. Courses of demonstrations will be given during the Lent term on heat and advanced electricity, and magnetism, and during the Easter term on light, electricity, and sound."

The scheme was something of an innovation in one important detail. Shaw and I were college lecturers, receiving nearly all our remuneration from our colleges; we were among the first to give college lectures in a university building. Hitherto there had been a marked distinction between the lectures of a professor and college teaching; the growth of the natural sciences brought a change. Various colleges still had their own classes and laboratories; at Trinity, Coutts Trotter lectured on physics in college. In physiology and comparative anatomy, however, Foster as Trinity prælector, Frank Balfour and Sedgwick as college lecturers, worked in university buildings. It was clearly out of the question for each college to have its own staff and laboratory in all the new subjects, and so the plan of giving university recognition to college lecturers was gradually evolved, a plan which this year the new statutes have made universal; the formal lectures in all subjects are now given by university officers.

The task we had undertaken was a novel one for us both. Shaw had worked at Freiburg with Warburg

and at Berlin under Helmholtz, but at neither place was there organised class teaching in practical physics. My experience had been gained solely as a research student under Maxwell. There were no text-books properly so called. We drew largely on Kohlrausch's "Physical Measurements" and Pickering's "Elements of Physical Manipulation." Pickering at the Massachusetts Institute of Technology had organised a method of instruction which we followed in great measure. There were, it is true, physical laboratories under Wm. Thomson (Lord Kelvin) at Glasgow, Carey Foster at University College, London, Adams at King's College, and Clifton at Oxford. But the time given to practical physics was short, the means of instruction scanty, and there was comparatively little endeavour to teach the principles of physics to a student by means of experiments which he performed himself.

Gradually the system developed; note-books descriptive of each experiment were written, and these in due time became Glazebrook and Shaw's "Practical Physics." The students were taught to describe in their own note-books their work, and enter up their observations; in the advanced class they were expected to read up the theory referred to and work to a high degree of accuracy. The material on which we drew for students was of a very high order. In my class on advanced electricity in 1881, I find the names among others of J. M. Dodds, S. L. Hart, W. B. Allcock, Edward Hopkinson, A. R. Forsyth, R. S. Heath, J. Ryan, and R. Threlfall.

Above all we had the wise counsel and cordial help of the professor: the course was planned and the syllabus arranged after careful counsel taken with him. We could go to him for advice in case of difficulty, otherwise we were free to teach as seemed best and to develop the work in the directions which seemed most desirable. In due time we had efficient helpers, Newall, Wilberforce, Searle, Skinner, Threlfall, Fitzpatrick, Callendar, Whetham, and McConnel, to name only a few, though some of these belong to a later period than that I am now describing.

It was, however, not merely the organisation of teaching which gave interest to the work. The laboratory began to be, to an extent as never before, a centre of new knowledge. A glance at Lord Rayleigh's collected works will show the importance of his output during the five years' tenure of the chair. The experiments of Rowland and Joule had led definitely to the result that the resistance coils constructed by Fleeming Jenkin and Maxwell in 1863 to represent the ohm (10^9 C.G.S. units of resistance) were in error. The Standards Committee of the British Association was reconstituted at Swansea in 1880, and Lord

Rayleigh at once began to take an active part in its work. His first series of experiments with the rotating coil of the original committee, carried out with Schuster, confirmed the fact that there was an error in their determination. Further experiments with a new rotating coil in 1882, and by the method of Lorenz, with the assistance of Mrs. Sidgwick, in 1883, led to definite values from which there has been little variation up to the present. Meanwhile he had arranged that I, as secretary of the B.A. Standards Committee, should undertake to test and issue certificates for resistance coils, and this work went on at the laboratory for the next fifteen years, being ultimately transferred to the National Physical Laboratory.

Throughout this period, and indeed until the end of his life, Sir William Thomson was a frequent visitor to the laboratory, enthusiastic always, keenly interested in the experiments in progress, and full of helpful encouragement for us all.

Work on the electrochemical equivalent of silver, and the E.M.F. of a Clark cell, followed quickly. At a later date Lord Rayleigh took an active part in designing the standard ampere balance for the Board of Trade. He acted as president of the International Electrical Conference held in London in 1908 at which the definitions of the International Ohm, Ampere, and Volt were agreed to by delegates representing Great Britain, certain of her colonies, and twenty-two other countries. The results then reached find their main support in his work¹ during the years 1881-1885.

Some of the apparatus used in these and in the earlier experiments of Maxwell is shown in Fig. 8,

¹ It may be of interest to compare the values for the ohm (109 C.G.S. units of resistance) expressed as the length of a column of mercury found at the Cavendish Laboratory in 1882 and 1883 with the most recent values.

1882 Rayleigh	106.26 cm.
1882 Glazebrook	106.25
1883 Rayleigh and Sidgwick	106.24
1913 F. E. Smith	106.245
1920 Grüneisen and Giebe	106.246

which is reproduced in "A History of the Cavendish Laboratory, 1871-1910," published by Messrs. Longmans, Green and Co., to which volume I have referred for much in this article. At about the same period the original resistance coils of the B.A. had been brought to the laboratory, and Fleming commenced his long series of electrical researches by a careful inter-comparison of these. The Darwins—George and Horace—were at work on the lunar disturbance of gravity. Schuster, in addition to assisting in the

electrical experiments, continued his investigations into spectroscopy and radiation. J. J. Thomson's paper—the first dated from the Cavendish Laboratory—"On some Electromagnetic Experiments with open Circuits," appeared in the *Philosophical Magazine* for July 1881. In the previous April his paper on the electric and magnetic effects produced by the motion of electrified bodies had been published; in it he demonstrated that a moving charge will act as though it possessed mass of amount $\frac{2}{3}e^2/a$, where e is the strength of the charge supposed to be treated as a small sphere of radius a . Other papers rapidly followed, while in 1883 a determination of the number of electrostatic units in the electromagnetic unit of electri-

city was printed in the *Philosophical Transactions of the Royal Society*, and a paper on a theory of the electric discharge in gases in the June number of the *Philosophical Magazine*.

Shortly before his death, Maxwell had called Shaw back from Berlin to undertake work for the Meteorological Council, and thus began, with a paper on the measurement of temperature by water vapour pressure (1883), followed by his report on hygrometric methods (*Phil. Trans.*, 1888), that long series of investigations which has gained him recognition as the leader of modern meteorologists.

My own work on the form of the wave surface of

FIG. 8.—Some classical apparatus. The coil on the table is the original spinning coil of Maxwell and Fleeming Jenkin used for the determination of the ohm (1864); it is now on loan at the Science Museum, South Kensington. The coil below is the spinning coil of the late Lord Rayleigh, with stroboscopic attachment, used for the determination of the ohm (1882). The coils on the right were made by Chrystal and used by Glazebrook in his determination of the ohm. On the right is Maxwell's apparatus for measuring the viscosity of air at different pressures. A phonic wheel of Rayleigh's stands to the left on the table.

biaxial and uniaxial crystals was continued in 1886 by McConnel in his paper on quartz. 'I was diverted from optical work by the duties which came to me on my appointment in 1882 as secretary of the Electrical Standards Committee. Chrystal, when at the laboratory, had constructed a ballistic galvanometer and wound a pair of large coils for a determination of the ohm by a modification of Kirchhoff's method. With the aid of two of the senior students, Dodds and Sargant, these were set up and a series of determinations made, with the result already given, practically identical with that obtained by Lord Rayleigh. These same coils were used by him for his Lorenz experiments, and the close concordance of the results is of importance as an error in the calculated value of the mean radius of the coils would affect them in opposite directions.

At the end of 1884 Lord Rayleigh resigned and J. J. Thomson became professor. The work of teaching continued on much the same lines. The new professor realised no less fully than his predecessor the value of practical physics not only as an engine of research but also as an instrument of education.

Thomson, assisted by Threlfall, at once began that series of experiments on the discharge of electricity through gases which has occupied him until now. Newall returned to Cambridge and shortly afterwards became demonstrator in succession to Shaw, who in 1887 was made tutor at Emmanuel College. About this time Newall, Threlfall, Cassie, Wilberforce, Fitzpatrick, Callendar, and Chree were at work on various problems, while Olearski and Natanson were the first of the long series of research students attracted to Cambridge by the growing fame of the professor.

An addition of some importance was made to the laboratory teaching in 1888, when practical classes for medical students were commenced. For some time previously a viva voce with instruments had been part of the first M.B. examination, and the introduction of more definite teaching suited for the examination became desirable; Fitzpatrick took charge of this. Meanwhile the other classes continued to increase, and as the teaching of practical physics spread to the schools it was necessary to devise fresh experiments of a somewhat more advanced character. Searle became demonstrator in 1891 and threw himself with great zeal into this work.

In 1895 a change of far-reaching importance was made in the University regulations. Up to that date research students from other universities had no recognised position in Cambridge; some few there had been, admitted to the laboratories by the courtesy of individual professors. From this time onwards graduates of other universities, and in some cases students who were not graduates, could enter the

University for purposes of research if they satisfied the proper authorities of their competence. They could obtain a degree at the end of two years if they submitted to the Board of Studies a thesis which in the opinion of the Board was "of distinction as a record of original research." In the October term after these regulations were passed, Rutherford, Townsend, and McClelland came as the first students; their number is now legion.

Rutherford's earliest work was on a detector of wireless waves. Hertz's experimental verification in 1887 of Maxwell's theory of electromagnetic waves had been received with great interest when first published. We knew the theory to be true—it was Maxwell's—and to have it verified by direct experiment had been no small satisfaction to Maxwell's pupils. I well remember some time in 1896 being taken by Rutherford into his room to see his magnetic detector at work receiving a message from, I think, the observatory.

Meanwhile Thomson had continued his work on the passage of electricity through gases, and the discovery of the Röntgen rays in 1895 gave him and the band of students he had collected a powerful method of investigating the subject. In 1897 came the discovery of the electron. Perrin in 1895, and Thomson two years later, had proved that the cathode rays of Crookes consisted of a stream of particles each carrying a negative charge. Thomson in 1897 measured the ratio of the mass of each particle to its charge and also the velocity with which the particles travel. The quantity, m/e , was found to be much smaller than would be the case if m represented the mass of the hydrogen atom and e its charge. Moreover, its value was independent of the nature of the gas in the tube, and he concluded that we have in the cathode rays a condition "in which all matter that is matter derived from different sources such as hydrogen, oxygen, etc., is of one and the same kind; this matter being the substance from which all the chemical elements are built up."

In the same year, 1897, Kaufmann and Wiechert determined independently the value of the ratio m/e , arriving at similar results. Schuster in 1884 had suggested that if the cathode rays consisted of charged particles it was possible to determine the ratio e/m by a method practically identical with that employed later by Kaufmann, and in 1890 published results of experiments which led to limits for the value of e/m for the cathode rays.

In the following year Thomson showed that the charge e carried by one of his corpuscles or electrons was the same as that of an atom of hydrogen, and from this it followed that the mass of the electron is about $1/1800$ of that of an atom of hydrogen. The

existence of the electron, the fundamental unit of negative electricity, became an accepted fact, and the long series of modern discoveries connected with the

New knowledge grew rapidly. Rutherford, Townsend, H. A. Wilson, Zeleny, Strutt, and others contributed. C. T. R. Wilson's paper on the condensation nuclei produced in gases by the action of various rays, published in the *Phil. Trans.* for 1899, proved of far-reaching importance. As time went on the influence of the Cavendish researches spread to other universities. Rutherford carried it across the Atlantic to Montreal, Townsend to Oxford, McClelland returned to Dublin, Langevin to Paris, while others came to take their place.

My days at Cambridge closed with my appointment as Principal of University College, Liverpool, toward the end of 1898. Formal connexion with the laboratory had ceased some two years earlier when I had undertaken administrative work for my College. The last experimental investigation I helped to carry out was undertaken along

names of Thomson, Rutherford, Bohr, and their pupils was the result.

Thomson and his pupils continued this work. McClelland examined the Lenard rays which appear on the opposite side of a thin sheet of metal on which the cathode rays impinge, and showed that they were identical in properties with cathode rays. Stokes had suggested that the X-rays of Röntgen consisted of pulses set up by the sudden stoppage of particles carried in the cathode rays. Thomson investigated the nature of the effects produced by such a sudden stoppage, proving that it would give rise to a thin pulse of very intense electric and magnetic force travelling out from the particle with the velocity of light. He later applied this theory to explain the secondary radiation produced when X-rays fall on matter. Langevin, the first of the foreign students to enter the laboratory under the new regulations, investigated the character of this secondary radiation and published his results in his thesis for the degree of Doctor of Science in Paris.

with Lodge, who spent a summer vacation in Cambridge, as an essay to be submitted for a prize offered by Elihu Thomson. The investigation in-

FIG. 9.—Apparatus used by Sir J. J. Thomson in his investigations. The upper tube is an early form of positive ray tube for visual examination of the parabolas. The lower, left hand, tube is one used to show the electrical deflexion of the cathode rays in 1897. By measuring the deflexion of a pencil of rays in an electric as well as in a magnetic field, the velocity and the ratio e/m for the electron were determined. On the right are examples of electrode-less discharge bulbs.

FIG. 10.—Apparatus for the analysis of positive rays by Sir J. J. Thomson's 'parabola' method. This was the first made in which the photographic plate could be lowered into position by a vacuum-tight winch. It was in this room, with a slightly modified apparatus, that the duplicate parabolas of neon were first photographed, in 1912.

cluded a determination of v by means of the measurement of the frequency of the oscillations in a circuit of known capacity and inductance, and was published in the Stokes Memorial volume

issued in 1899. Up to that time I had been closely connected with the Cavendish for nearly twenty-five years and have written of things I knew. I must conclude now and leave to others more intimately acquainted than myself with the progress of the last quarter of a century to tell in detail what that progress has been. Thomson continued his work until 1918, through the troubled period of the War, when the researches of the Cavendish assisted to the full in the solution of many problems. In that year Dr. Butler, the revered Master of Trinity, died, and Thomson

was chosen by the Crown as his successor. He resigned the Cavendish professorship with its charge of the laboratory, and the University created for him a professorship of physics which will cease with his tenure of the post. Rutherford followed as professor, and under his genial direction he and his 'boys' from day to day increase the sum of natural knowledge by their amazing results and add in no small degree to the fame and influence of the Cavendish Laboratory, thus verifying the forecast of its founder more than fifty years ago.

Research Work in the Cavendish Laboratory in 1900-1918.

By Prof. J. A. CROWTHER.

THE progress of science and the life of a laboratory have little connexion with the artificialities of chronology, and the beginning of the century does not coincide with any natural division in the work of the Cavendish Laboratory. If, for the sake of clearness, we must make a division in what was fundamentally a natural and continuous growth, our most convenient date will be the year 1903, and our obvious landmark the publication of the first edition of Thomson's monumental book, "Conduction of Electricity through Gases." Here we find for the first time a complete and logical account of the solid achievements of the previous eight or ten years of remarkable activity. Here also we find the foreshadowings of the ideas which were to direct and regulate a large part of the work of the succeeding years.

In the main the problems attacked in the years before 1903 were those of the mechanism of the conduction of electricity through gases. Some of these, it is true, were carried over into the succeeding period. Some, perhaps, have not yet been fully solved, but by 1903 the foundations of the subject had been well and truly laid. It had been proved, beyond doubt, that the conductivity imparted to gases by various agents, such as X-rays, the rays of radium, and ultra-violet light was due to the formation of charged particles or gaseous ions in the gas, and many of the properties of these ions had been determined. Rutherford had determined their mobility; Townsend had determined their rate of diffusion through various gases; C. T. R. Wilson had shown that they could serve as nuclei for the condensation of water in a supersaturated space. Thomson had discussed their motion, and shown that in all probability they consisted of single molecules of the gas in which they were formed.

In a closely allied subject Thomson had demonstrated experimentally the existence of the electron, or negative corpuscle as we then called it, and had determined its few simple properties. The measurement of the electronic charge actually falls within the present century, though

preliminary experiments had been made somewhat earlier. Determinations were made independently by Thomson and by H. A. Wilson, by somewhat different methods, and were published in 1903. During the same period Zeleny made new measurements of ionic mobilities, which when combined with further measurements on the diffusion of ions by Townsend established conclusively the identity of the charge on a gaseous ion with that on a monovalent ion in solution. These experiments mark the culmination of the first epoch.

When I commenced research work in 1905 the second epoch of activity was already well under way. With the exception of C. T. R. Wilson, who in the advanced laboratory was engaged in imparting to a new generation of students those experimental arts of which he is a master, the first group of workers had all dispersed. Their work, as collected in the pages of "Conduction through Gases," formed our text-book, and was the foundation on which we hoped to build.

It was a very happy, though crowded, community to which I had the honour of being admitted. The space available for research had not been extended since the laboratory was built, and what had been adequate for ten or twelve, the average number of research workers before 1903, was becoming a tight fit now that the numbers had risen to an average of twenty-five. A competent observer records that there was more physics to the square centimetre in the Cavendish than anywhere else in the world, and the resources of the laboratory in the way of apparatus were sometimes severely tested, though I cannot recall that they ever broke down. The oft-repeated legend that we worked entirely with sealing wax and string, though flattering to our technique, has little foundation in fact. The apparatus for the new experiments which we wished to make did not exist. It had to be invented and made on the spot. An adequate workshop, staffed with extraordinarily competent and helpful mechanics, with F. Lincoln as chief, carried us through the worst of our difficulties; while Everett, the professor's private