Rutherford came to Cambridge in October 1895. He already had a degree at Canterbury College, Christchurch, and two papers in the Trans. N.Z. Institute to his credit. He had just been appointed to an 1851 scholarship which made his coming financially possible. At that time J. J. Thomson was Cavendish professor of experimental physics. The chair and the laboratory were called after a great eighteenth-century physicist and eccentric, whose name would have been among the foremost in the history of electricity if he had published his discoveries. The laboratory had been built 25 years before with money provided by one of Cavendish’s kinsmen, the Duke of Devonshire.

Incidentally Rutherford created an administrative precedent in the University by being the first to take advantage of a new regulation which allowed someone with a degree from another university to take a degree after two instead of the usual three years and instead of passing examinations to submit a thesis which must be judged by a Committee to be ‘of distinction as a piece of original research’. He was only just the first for J. S. E. Townsend, afterwards the professor of physics at Oxford, registered later in the same day.

The year 1895 was a crucial year in physics, comparable to the year when Darwin’s Origin of species appeared. Nothing was ever the same again. It is worth taking a few minutes to consider the then state of physical science. It rested on what was to all appearances the unshakable rock of Newton’s laws of motion themselves a logical statement of Galileo’s original but vaguer ideas. To a nineteenth-century physicist ‘explanation’ meant mechanical explanation in terms of these laws. Maxwell, the first Cavendish professor, had put forward a theory of light in 1873 as part of a theory of electricity. It was long in gaining acceptance in spite of what seems now its obvious advantages. Partly the electrical theory was somewhat obscurely expressed; indeed J.J. first gained his reputation by his success in applying and expounding it, but more because it was felt, especially by the elder men, that Maxwell’s results, though no doubt interesting, did not constitute a proper mechanical explanation of light, however interesting and suggestive they might be.

Physics in those days was essentially a matter of continuous media. Differential equations ruled supreme and undisputed. Atoms were left to the chemists with a sniff. No one considered that they could have a structure. The idea of a natural unit of charge was distrusted. A man as great as Faraday shied away from it and avoided it even in cases where it seems to us that it was the obvious explanation. Maxwell, though he developed the kinetic theory, disliked the idea of charge in any
form and though he used it in his theory did so with the expressed hope that it
would eventually prove unnecessary. The ‘medium’ was all, charges only the still
imperfectly understood mechanism by which the ‘medium’ affected matter.

Experimentally, after a rather dead period in the seventies and eighties things
were waking up, especially in the fascinating field of the passage of electricity
through rarified gases. This goes back to the early eighteenth century, but improve­ments in air pumps about the middle of the nineteenth had given it a fresh impulse.
Especially the phenomenon of the cathode rays intrigued physicists and had led
to a fierce controversy as to their nature conducted almost on a national
basis, England and France against Germany, the first two favouring the view that
cathode rays are a stream of negatively charged particles, while most of the German
physicists thought them ‘electromagnetic phenomena in the aether’—possibly the
missing longitudinal waves, though this was admittedly only a guess still lacking
experimental support.

Then, particularly interesting to Rutherford, was the discovery Hertz had made
in 1888 that waves could be generated by electrical sparks, which confirmed the
predictions of Maxwell’s theories. This discovery was the starting-point of Ruther­ford’s two papers which seemed to promise for him a career in a still unrecognized
branch of electrical engineering.

He did indeed pursue this line for his first few months at the Cavendish, but
even before he had done enough to provide the material for his third and most
important paper on the subject, Röntgen discovered X-rays.

The importance of this discovery came largely from its dramatic character. The
idea of being able to see the bones of a living person naturally fired popular as well
as scientific imagination. Within a few weeks of the discovery it was being used for
medical purposes, probably the quickest application on record of a discovery made
from pure scientific curiosity. To a modern physicist X-rays are just one region of
the electromagnetic spectrum, but the scientific importance of the discovery was
threelfold. As a tool they were invaluable in the study of the conduction of electricity
in gases, for they gave a controllable means of making a gas conduct, and the
conduction of gases was to be the road to the understanding of the atomic structure
of matter. Then the experiments they made possible were later to pose more
strikingly than any others the fundamental contradictions with Newtonian ideas
that Planck’s quantum implies. Thirdly, they led to a search for similar radiations
elsewhere, and by an odd coincidence between the radioactivity of uranium and the
fluorescence of some of its salts Becquerel came to discover the former effect. Thus
Rutherford’s life’s work had its basis in Röntgen’s discovery. His time in Cam­bridge covers the transition of his interests from radio, through the conduction of
gases, on to radioactivity, which led him to the discovery of the nucleus and the
transmutation of the elements. His first paper on radioactivity, though published
after he had gone to Canada, describes work done at the Cavendish as is clear from
the acknowledgement at its end. One of my main reasons for choosing this part of
Rutherford’s career is that it shows the importance of versatility in the research
worker. He abandoned two very promising lines of research before he found the
field that really suited his genius. If he had not done so he would no doubt have
The Rutherford Lecture, 1964

had a very successful career, but it is fairly safe to say from hindsight that his place in the world of science would have been definitely lower than it is.

Rutherford's impressions of Cambridge are described in his letters to his fiancée, Mary Newton, later Lady Rutherford.

3 October 1895*

'Next day I had an appointment to go and see Thomson at Cambridge. . . . I went to the Lab. and saw Thomson and had a good long talk with him. He is very pleasant in conversation and is not fossilized at all. As regards appearances he is a medium sized man, dark and quite youthful still. Shaves very badly, and wears his hair rather long. His face is rather long and thin; has a good head and a couple of vertical furrows just above his nose. We discussed matters in general and research work, and he seemed pleased with what I was going to do. He asked me up to lunch to Scroope Terrace where I saw his wife, a tall, dark woman, rather sallow in complexion, but very talkative and affable. Stayed an hour or so after dinner, and then went back to Town again. I have forgotten to mention the great thing I saw—the only boy of the house—3½ years old—a sturdy youngster of Saxon appearance but the best little kid I have seen for looks and size [G.P.T.]. Prof. J.J. is very fond of him and played about with him during lunch while Mrs J.J. apologised for the informality. I like Mr & Mrs both very much. She tries to make me feel at home as much as possible, and he will talk about all sorts of subjects and not shop at all....

Mrs Thomson has been very kind and looked me out some lodgings with a widow and gave me directions where to find her. They also asked me up to dinner in the evening which was expressly not a dress affair....

I admire Thomson quite as much as I thought I would, which is saying a good deal. They have both been very kind to me, as you may judge from what I have written.'

8 December 1895†

...In my last letter I told you that J.J. had asked me to give an account of my work at the Physical Society. I am the first member of the Cavendish who has given an original paper before it, so I may consider the honour is greater than I can bear.... Instead of taking only part of the time, J.J. stuck the following announcement on the notice board "A method of measuring waves along wires and determination of their period, with experiments by E. Rutherford", and left me to fill up the whole time. The term "with experiments" rather knocked me sideways, but I went to work and rigged up a good few interesting experiments, which all came off very well. I had quite a distinguished audience including J.J. and Mrs J.J. and several other ladies—Sir G. Stokes—a good few lecturers and demonstrators besides the usual vulgar herd. I think the paper was quite a success and J.J. was pretty pleased, I think. No one but myself made any remarks, as it was rather beyond most of them. I really had to give a lecture and did not read anything at all.

* Eve, Rutherford, p. 15.
† Eve, p. 19.
My friends all reckoned I did very well indeed, so I suppose it may be considered a success. Mrs J.J. in speaking to me afterwards complimented me rather neatly in a way (which of course I took at a proper valuation, *cum grano salis*) that struck me as decidedly good, “You kept us ladies very interested indeed, and I am sure it was sufficiently deep for the more scientific members of the society”, or words to that effect...a very *a propos* way of conveying a neat compliment...

‘By the way I have not told you that I will be publishing some of my work before long. I spoke to J.J. about it and he said I had better send it to the Royal Society. As only the best papers, or at any rate the papers of eminent men, are chiefly found there, I have nothing to complain of. Townsend, my particular friend at present, is going up to his cousins at Yorkshire for Xmas—I don’t know now whether I have described him to you. Imagine a middle-sized man, very fair hair rather scanty on top, very fair moustache and a true Irish complexion and a merry blue eye rather good features and a very pleasant appearance altogether. He is a very fine mathematician and is a good deal of assistance to me in that way. I think it probable he and I will research together on abstruse subjects next term, for in some of the work I am doing, it is very difficult for me to do all without assistance.’

Rutherford started his work on radio by a back door in a way typical of how research in pure science leads to results of practical importance. The avowed object of both the New Zealand papers was to study the magnetization of iron and steel when the field producing the magnetization is reversed millions of times a second as it would be if caused by Hertzian waves. However, Rutherford did not at first use these waves, perhaps had no intention of doing so, but used instead the oscillations produced in a coil of wire when Leyden jars are discharged by a spark gap. This gives a calculable high frequency, and the magnetic field in the coil may or may not magnetize a piece of iron placed there. The effect was disputed, largely because the conditions are hard to make definite. J.J. had published some work on the subject, one reason perhaps why Rutherford came to Cambridge.

Rutherford showed that fine wires definitely could be magnetized by this method; he showed, by dissolving the surface in acid, that the effect was limited to a thin skin, and that the direction of magnetization reversed once even in this skin. Because of this surface effect, of the damping of the oscillation, and of the inherent non-linearity of ferromagnetism, experiments of this kind are hard to interpret quantitatively, but at least he showed that iron could respond to a magnetizing effect which changed direction as often as 500 000 000 times a second. Starting with a bundle of strongly magnetized needles he also showed that they lost most but not all of their magnetism when placed in the coil. This was the basis of his detector and of a commercial one used later by Marconi. He used 24 pieces of hard fine steel wire, each 1 cm long, and dipped in paraffin to prevent eddy currents. The change in magnetization was detected by a suspended magnet. This device was not in itself practical since no means of resetting it when it had recorded a single impulse had been provided. Rutherford had detected effects over a few feet before he came to Cambridge.
He continued work there, as is apparent from the following extract from a letter to Mary Newton*:

‘When I left off last time I had just been out on the Common trying to detect waves at long distance. The next day I tried and got an effect from the Lab. to Townsend’s lodgings, a distance of over half a mile through solid stone houses all the way. The professor is exceedingly interested in the results, and I am at present very useful when he is writing to various scientific pots as he can mention what his students are doing at the Lab. Some good startling effects with waves suit him down to the ground. The morning after my midnight excursion, Sir R. Ball came down to see how I got along. He called in the other day and told me I could make use of the Observatory which is about a mile out of town, if I wanted to try some more long distance experiments. It is a very good offer and I will probably go up there before long. I am at present trying to clear up some of the work I have in hand in order to get ready for publication.’

At this time Rutherford was considerably ahead of Marconi.

Later the work at Cambridge and some of that at Christchurch was published in a paper in the *Philosophical Transactions of the Royal Society*, on which Rutherford comments in a letter to Mary Newton, 12 August 1896:

‘...I have one piece of excellent news in regard to myself, viz. that I have received word that my paper goes in the *Phil. Trans. of the Royal Society*, which is an honour which befalls only the best papers that are sent in—so I congratulate myself on my success. I don’t suppose I will get copies for some time, but they are generally done up very well, and you may be certain I will send you one to put in your archives. The work the professor and I have done this long vac. will probably be published in the *Philosophical Magazine*. We will also give an account of it at the British Association Meeting at Liverpool’.

Almost immediately after Röntgen’s first paper several people, including J.J., discovered that the new rays made gases conduct. J.J. started on a systematic examination of this effect, at first with McClelland and then with Rutherford, who refers to the matter as follows in a letter dated 24 April 1896: ‘...I am working with the Professor this term on Röntgen Rays. I am a little fed up of my old subject and am glad of a change. I expect it will be a good thing for me to work with the Professor for a time. I have done one research to show I can work by myself. The Professor has an assistant Everett, a very smart manipulator and supposed to be the best glass-blower in England.’

The result was a paper of the first importance published in the *Philosophical Magazine* of November 1896 under their joint names‡. The paper contains the main fundamental ideas of the conduction through gases under moderate electric

* Eve, p. 29.

† There have been suggestions that signals were sent between the Observatory and the laboratory, a distance of about 1½ miles, but Rutherford in writing a note on his work for a history of the laboratory published in 1910 refers only to a distance of ‘about half a mile’. This agrees with a sentence in the *Phil. Trans. A*, 189, 1–24, 1897: ‘a small, but quite marked effect was obtained at Park Parade, a distance of over half a mile in the direct line’.

‡ It was rare for J.J. to publish a joint paper. It is clear from one of Rutherford’s letters that J.J. spent a good deal of time on the work.
fields, ionization, saturation of the current by the stronger fields, mobility of ions, recombination and measurements, or at least estimates, of these quantities and effects. Hints of some of these are contained in an earlier paper of J.J.'s with McClelland.

The idea that conduction in gases, like that in liquids, is due to ions was originally due to Giese well before the discovery of X-rays, when the main sources of conductivity were heat or a potential difference high enough to produce a luminous discharge. J.J. had accepted this view as probable, but before X-rays were available the character and properties of these ions was mostly a matter of guesswork. The conditions in a discharge are so complicated that no real progress could be made till it was possible to study separately the elementary processes involved. Even now it is one of the most difficult branches of physics. Ionization by heat is complicated by surface effects. The first real quantitative knowledge of this came from the work of O. W. Richardson at the Cavendish a few years later, made possible by the discovery of the electron. A side effect of Hertz's work was the discovery that ultra-violet light can sometimes discharge a negatively charged metal. This also was not well understood, Rutherford was to work on it before he left Cambridge.

This paper of 1896 proved that X-rays could produce charged particles from a neutral gas, 'ions' like those in liquids, but that unlike these the gaseous ions disappeared in a short time, re-uniting to reform the original gas according to the square law characteristic of a bimolecular reaction in chemistry. The symbol \( \alpha \) chosen for the coefficient which measures the effectiveness of this process has remained in use ever since. In consequence of recombination the current through the gas under an applied potential difference does not follow Ohm's law except for small potentials. For larger ones the current increases more slowly and its maximum or saturation value comes when every ion formed by the rays is removed to the electrode before recombination can destroy it. The ohmic current depends on the density of ions when the gas is in a steady state where ionization and recombination balance and also on the speed with which the ions are drawn through the gas by the applied potential gradient. That for small fields, at least, the speed is proportional to the field was proved by the current obeying Ohm's law in these conditions. Gaseous mobilities, as they are now called, were found to be larger by orders of magnitude than those in liquids, not surprisingly considering the difference in density of the medium if the charges on the ions are comparable.\(^*\) The estimate of mobilities in this first paper was extremely rough. Rutherford was to improve it. The paper estimated the proportion of ions to molecules in the steady state in hydrogen with the actual rays used at about 1 in 3 \( \times 10^{12} \).

It is worth mentioning that in those days the role of the electrodes on gaseous conductivity was not understood. J.J. had long emphasized the difficulty of getting the current in and out of the electrodes and had been led for this reason to study the electrodeless discharge produced by induction in bulbs containing no metal. He and Rutherford seem to have been surprised at the ease with which the ions gave

\(^*\) J.J. in a paper to the Chemistry section of the British Association in 1894 had calculated from kinetic theory data the mobility of a gaseous ion. In fact this considerably exceeds the actual mobility because of forces between the charges.
up their charge to the electrodes. The point is, of course, that while *extracting* a charged particle from a metal involves work to separate it from its electrical image in the metal surface, the same attraction ensures its return.

In the next two years three papers appeared in which Rutherford expanded the work of the one we have just noticed, making accurate measurements of mobilities and coefficients of recombination, studying the absorption of the rays in connexion with the ionization they produce and extending the whole to cover ionization due to ultra-violet radiation of metals.

During this period the discovery of the electron may be said to have taken place, starting with J.J.’s proof in 1897 that cathode rays are charged particles with a ratio of mass to charge of the order of one-thousandth that of the hydrogen atom in electrolysis, that this ratio is the same for cathodes of different materials discharging into different gases and that the particles are apparently universal constituents of matter. In the next year he measured the charge on the ions produced by X-rays using C. T. R. Wilson’s method of cloud condensation upon them and applying to it Townsend’s idea of using Stokes’s law to find the size of a small drop. The charge found was roughly what was expected for a hydrogen ion in electrolysis within the wide limits which were all that could then be placed. There is no doubt that J.J. was by then convinced that the charge on a cathode ray was the same as either of these, but the final proof came after Rutherford left for Montreal in September 1898. In 1899 J.J. showed that particles with the same \( m/e \) as cathode rays were emitted from hot wires and were also the origin of the ions caused by ultra-violet radiation which Rutherford had by then studied. Finally he was able to measure \( e \) for these latter and show that it was the same as for ions made by X-rays. Rutherford’s work for the rest of his time at Cambridge must be read with an understanding of this rapidly advancing knowledge.

The first of the three papers contains a study of air ionized and then electrified by removing ions of one sign by attraction to an electrode, and also the first rough measurement of mobility, there called ‘velocity’, using a blast of air; this corrected the estimate of J.J.T. and Rutherford which was hardly better than a guess. The term ‘ions’ is used as well as the phrase ‘conducting particles’ used in the joint paper. J.J. had indeed spoken of ‘ions’ in his paper of 1894, but here he was making a hypothesis rather than stating a belief in an experimental fact. Rutherford rejects a previous idea that the mobility of the negative ions is greater than that of the positive, one of his few errors. It was left for Zeleny to prove this. The most important novelty of the paper is the study of the absorption of X-rays in gases and its connexion with ionization which he showed to be general, not selective, i.e. the reduction in ionizing power by absorption in a gas is the same whether the gas in which the ionization is tested is the same as the absorber or not.* Gases which conduct well under X-ray are, however, strong absorbers.

In the second paper Rutherford studied recombination by two independent methods. To quote his own words:

‘(1) By blowing air at a known velocity along a tube and testing the conductivity at different distances from the point of action of the rays.

* This is not strictly true in all cases.
(2) By applying an electromotive force to the gas at definite intervals after the rays have ceased, and measuring the quantity of electricity that passed through the gas.

The intervals were determined and measured by the motion of a pendulum which broke various contacts in succession. This was a slight modification of a 'time apparatus' which he had used in New Zealand, though then the weight fell freely. He found recombination much affected by dust, and does not quote actual values of $\alpha/e$, perhaps for this reason.

He then proceeded to measure mobilities by two methods. The first of these was in essence a method suggested in the joint paper except that the time of decay to half value of the ionization, which has to be known in that method, was determined instead of being roughly estimated. This time is shown by the theory given in the joint paper to be equal to the number of ions in the steady state divided by the number produced per second, which is proportional to the saturation current. Both these were measured. The second is the one usually referred to in text-books as 'Rutherford's method', using a pendulum for timing. He adopted this method because Perrin had found that X-rays falling on a metal surface cause extra ionization near the surface. In the second method the electrode which collects the ions whose mobility is to be measured is shielded from the rays. Again he found no difference in mobility between positive and negative ions. He compares J.J.'s kinetic theory calculations of mobility with the much smaller values actually found and explains this by supposing the ions to be not single atoms or molecules, thus starting a controversy which lasted very many years. As so often happens, there is no single universal answer.

In the third paper sent for publication in February 1898 Rutherford studied the ionization produced by ultra-violet light and opened the way for the experiments of J.J. on electrons already referred to. In Rutherford's work, however, air was present at atmospheric pressure so the primary electrons attached themselves quickly to oxygen molecules and behaved much like negative ions due to X-rays. The cause of the conductivity was still quite uncertain, there was much talk of metallic dust, of particles of metal torn off by the radiation. Rutherford showed that the phenomenon was similar to that observed with X-rays, except of course that only negative ions were produced. He measured the mobility first by a blast method giving a value 1.5 \((cm/s)/(V/cm)\) not far from what he had found for X-rays in air. However, the blast was turbulent and the motion uncertain, also it was only practical to use it with air. He therefore devised a different and very beautiful method in which the ions are made to oscillate backwards and forwards, alternating in an electric field between two parallel plates, one the source of ions and the other the collector, which receives a charge or not, depending on whether the distance between the plates exceeds or not the amplitude of the oscillation. This was the first of innumerable devices dependent on the more or less complicated motion of ions or electrons to produce an effect; in a sense it was the first valve. Again the mobility was about the same, and the same whatever the metal. It varied inversely as the pressure down to 34 mm.

Rutherford was wise to abandon this kind of work for the new discovery of Becquerel that uranium produces radiations capable of affecting a photographic
plate. In fact experiments on ions at pressures comparable with that of the atmosphere have not led to very fundamental results apart from the determination of $e$. At these pressures collisions are so frequent and their consequences depend so much on the character of the colliding particles that impossibly high purities are required before one can make useful deductions. The big discoveries have been made by working at very low pressures where any one particle makes only a few collisions in the apparatus. Work such as Rutherford was doing became important in connexion with the propagation of electric waves in the upper atmosphere and in some astronomical phenomena, but was not the key to the more fundamental mysteries of matter.

The first reference in Rutherford's writings to the discovery of what was afterwards to be called radioactivity is in a paragraph at the end of the second of the above-mentioned papers in which he says that he was making experiments to find 'the velocities of ions in a gas conducting under the influence of the radiation given out by uranium and its salts'. The consequences of this investigation appeared in January 1899 in a long paper in the *Philosophical Magazine*. By then he was in Montreal but the paper is dated 1 September 1898 from the Cavendish, so its contents fall within our period. They form a logical extension of his earlier work to a new cause of ionization. They do not suggest that he was dissatisfied with the line he was following, rather the reverse, but the discovery of the dual character of this new radiation, the discovery of $\alpha$ and $\beta$ rays, as he called them, was more striking than anything he had done so far by himself. In the three papers just described he had shown his mastery of the art of experimentation by simple methods, but the ideas and the conclusions were essentially those of the joint paper. Incidentally, he now showed his superiority to a famous physicist by disproving Becquerel's conclusion that the rays could be refracted and polarized.

The early part of the paper recapulates 'the ionization theory of gases which was introduced to explain the electrical conduction produced by Röntgen radiation... and the results to which it leads'.

After disposing of Becquerel's conclusions on refraction and polarization Rutherford abandoned the photographic method used by the former in favour of his own electrical one. It had been realized from the start that X-rays are complex and that their penetrating power depends on the vacuum conditions in the bulb. It was not very surprising to find something rather similar again, but it took all the greater insight to realize that the two cases were not parallel, and that the two kinds of rays are fundamentally different. Most of the experiments were done with a layer of uranium compound exceeding the range of the $\alpha$ rays and the rays were not collimated so the absorption appeared exponential both for $\alpha$ and $\beta$. It was left for Bragg to discover ranges. Rutherford is careful to guard himself against the possibility of a third still more penetrating radiation, and the ionization due to the $\beta$ radiation was admittedly too weak for an accurate test of its assumed exponential absorption. He found about the same ionization for complete absorption of the rays by a number of gases. Actually his results somewhat overestimate the equality but it is marked, compared with the differences in absorption at equal pressure. He also showed that recombination followed the same type of law as for
Sir George Thomson

X-ray ions, and more specifically that the mobilities were the same, the same apparatus being used with the two sources. He quotes Zeleny’s result for the difference in mobilities of positive and negative ions and confirms it. By these and other experiments Rutherford satisfied himself that the ions, when they had been produced, behaved in the same way as those produced by X-rays and probably were identical, but in Rutherford’s words ‘the cause and origin of the radiation continuously emitted by uranium and its salts remains a mystery’, one which he was to solve in the following years.

Rutherford was fortunate that he came into physics at a moment when exciting discoveries were actually being made. Radio, X-rays, the electron, radioactivity began before his eyes and he was admirably placed at the Cavendish to take advantages of these great discoveries. He showed great and increasing experimental ability in his development of these openings. He showed his genius most in his choices of those aspects of them which had the greatest prospects for advance, guided by his superb flair for the oddities which in science are the surface signs of buried treasure.