By Sir James Chadwick, F.R.S.

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It is a very great privilege to be invited to deliver the Rutherford Memorial Lecture, and especially to give it in this University, with which his name will be forever associated.

The first of these memorial lectures was given last year by Sir John Cockcroft in New Zealand, where Rutherford was born and received his education. It is most fitting that the second lecture should be delivered here, for McGill University gave Rutherford his first post, electing him to the Macdonald Chair of Physics when he was only 27 years old. He was particularly attracted to McGill, for, as he wrote at the time, ‘The physical laboratory is the best of its kind in the world’. He appreciated the opportunity which was thus offered to him to develop his own line of work and he seized it in his own remarkable way.

The story of his work in Montreal is of quite extraordinary interest. It is one of the most instructive examples in the history of science of the opening up of a new field by simple experiment and by penetrating reasoning and lucid interpretation. Rutherford laid bare the salient features of radioactivity, and he explained, in a beautiful and complete way, all the diverse and mysterious phenomena which had been observed at that time. More than that, he saw already that his work was leading to new conceptions about the nature of matter; the first steps towards his later discoveries of the nuclear structure of the atom and of the artificial transmutation of matter can be plainly seen in his writings of that time.

To appreciate Rutherford’s work one must look at it against the background of knowledge when he began. By the early 1890’s an impressive edifice of classical physics had been built on the foundations of Newton’s laws of motion, the laws of thermodynamics and the laws of electromagnetic induction. There was a feeling that all the interesting things had been discovered and that future work would consist in building more precisely on what was already established. There was at the same time a feeling of bafflement at the failure to understand the relations between the different properties of matter or to describe them in terms of fundamental concepts. The atomic theory of Dalton had been almost universally accepted for the description of chemical changes and relations but, as the great chemist Ostwald wrote, ‘the atomic theory is not a statement of anything found by investigation. It is not an established fact. It may or may not be literally true, but it furnishes a very convenient means of interpreting the facts of chemistry.’ The hypothesis of the atomic structure of matter provided an interesting description of the properties of gases; and, on the basis of the kinetic theory, rough estimates of the size and weight of individual atoms had been made. Nevertheless, ideas about the structure of matter and about atoms were nebulous to a degree. As Lord Salisbury said in his Presidential Address to the British Association in 1894:
'What the atom of each element is, whether it is a movement, or a thing, or a vortex, or a point having inertia, whether there is any limit to its divisibility, and, if so, how that limit is imposed, whether the long list of elements is final, or whether any of them have any common origin, all these questions remain surrounded by a darkness as profound as ever.'

There was speculation about the nature and origin of the chemical elements—a feeling, for which no logical reason could be given, that the existence of the elements and the evidence from their atomic weights concealed some simple truth, as 'that each elementary atom was only a greater or a smaller number of hydrogen atoms compacted by some strange machinery into one'. This feeling, almost too vague even to be called a conjecture, was quite unsupported by any experimental facts.

But the period of stagnation was over, physics was on the move. In November 1895 Röntgen announced the discovery of X-rays, and this led, by a fortunate association of ideas, to the discovery by Becquerel, only a few months later, of the emission of a penetrating radiation from uranium—the first manifestation of radioactivity. There followed the discovery of the electron by J. J. Thomson in the spring of 1897, and the revelation of something common to the structure of all elements and some intimate connexion between matter and electricity.

It was Rutherford’s good fortune to join the Cavendish Laboratory in 1895 just before some of these important discoveries were made. After finishing some experiments which he had begun in New Zealand on the detection of electric waves, he began work with J. J. Thomson on the ionization produced in gases by X-rays. The main features of the processes occurring in a conducting gas were soon made clear, and it was shown that the ionization current was a measure of the intensity of the X-radiation. Rutherford then went on, early in 1897, to use the ionization method to examine the radiations from uranium. This was the starting point of his work in radioactivity.

It will perhaps be of some interest to mention the kind of equipment with which Rutherford worked in these early days. There was of course little available to him which is now common even in the smallest laboratories—no efficient accumulators, no convenient, and very few inconvenient, instruments for measuring currents and voltages, no fast pumps for producing a vacuum. The voltage for his ionization chambers was provided by accumulator batteries of an elementary kind, formed by ordinary lead strips in large test-tubes embedded in paraffin wax. These accumulators had to be freshly charged each day. For measuring the ionization current he used a quadrant electrometer of the original Kelvin type. This was a clumsy instrument—it might have been made by a blacksmith—very different from the type used in later years. The quadrants were enormous; the large heavy needle was suspended by a silk fibre and its position was controlled by a small magnet attached to it. In order to obtain a reasonable sensitivity, the needle had to be raised to a high potential, generally by charging it with a spark or two from a Leyden jar. This instrument had other grotesque features, but I have said enough to suggest that it was both unreliable and difficult to work with.

By any standards, the electrical instruments of that time were crude in the extreme; like other prehistoric monsters they survive only in museums. The young
physicist of to-day would be appalled—and rightly—if he were asked to work with such apparatus; but it was with such apparatus that Rutherford performed the experiments that led to his great discovery of the transmutation of matter.

When Rutherford came to Montreal (1897) he took up, with R. B. Owens, the examination of thorium, which had recently been found to be radioactive. They soon observed a very strange phenomenon. While the radiation from other thorium compounds was constant in its intensity, as indeed Rutherford had found the radiations from uranium, the radiation from thorium oxide varied in a most capricious manner; its intensity was affected little if at all by raising the oxide to a high temperature, but opening the door of the work-room produced large changes. This irrational behaviour was soon traced to the effects of air currents. Most men would have put up a notice on the door of the room 'Keep this door closed'. But not Rutherford; the effect was so puzzling that he gave great attention to it for, as he wrote, 'it might possibly give some clue as to the cause and origin of the radiation emitted by these substances'—a remark of prophetic insight.

In later work Rutherford showed that all thorium compounds gave out a material which behaved like a gas and which was itself radioactive. He called it 'emanation'. By passing a current of air over thoria, he collected some 'emanation' in an ionization chamber and found, on stopping the air current, that the radioactivity decayed away according to a geometrical law with the time, diminishing to half-value in about 1 minute. This was the first observation of the exponential law of decay, found later to be characteristic of all radioactive substances. During these experiments, he noted another strange effect—that good insulators apparently failed to continue to insulate in the presence of thorium compounds. He showed that this was because all surfaces which came in contact for some time with the emanation became radioactive. This 'excited' activity, as he then called it, decayed after the removal of the emanation according to the same law as the emanation but with a much longer half-value period, 11 hours instead of 1 minute. A series of simple but effective experiments convinced him that this excited activity was due to some kind of matter produced either from the emanation or by its action.

Somewhat similar observations of an 'emanation' from radium and of 'excited' or 'induced' activity produced by it were made about the same time by other workers in this field, but ideas about these new effects were in a most confused state—except in the mind of Rutherford. He was coming to a conclusion—that one kind of matter could produce another kind—which, startling as it was and contrary to accepted modes of thought, seemed to him to be the only rational way of explaining the experimental facts.

To establish this conclusion beyond doubt he needed the assistance of a chemist. By good fortune Soddy, then only 23 years of age, arrived in the Department of Chemistry in the summer of 1900, and he joined Rutherford early in 1901 in his attack on these problems. Thus began that remarkable association which, in the short period of two years, clarified the mysteries of radioactivity and produced a revolution in scientific thought.
They showed that the ‘emanation’ from thorium was a distinct kind of matter, a chemically inert gas; it did not originate from thorium itself but from a new radioactive substance, which could be chemically separated from thorium. This substance, which they named thorium X, lost its activity according to a geometrical law with time, while the thorium from which it was separated regained its activity in an exactly complementary way, so that the sum of the two activities was constant. This process of production and decay was found to be a common property of the radioactive bodies. The law of decay of activity was in all cases an exponential law, with a different rate of change in each case, characteristic of the active material. They showed further that there was a more intimate connexion between the radioactivity and the changes that maintain it than had been supposed—that, in fact, the radioactivity, or radiation, was not a consequence of changes which had already taken place but was an accompaniment of the change of one radioactive body into another. This threw a light on the observed law of decay and gave it a definite meaning, that the proportional amount of radioactive matter of a given kind which changes in unit time is a constant.

Thus step by step, by experiment and by reasoning, they came with inescapable logic, though with much heart-searching, to the conclusion that radioactivity is a spontaneous disintegration of the atom, in which one kind of matter changes into another kind of quite different chemical and radioactive properties, with the simultaneous emission of energy in the form of ionizing radiations.

It is almost impossible for us to-day to realize how daring a theory this was and how difficult even for the most imaginative to accept. Not only was the vaguely conceived atom the very foundation of the scheme of interpretation, but the indivisible atom divided and one element changed into another. And the evidence on which this theory was based had been obtained by observations on quantities of matter so minute that they could not be seen or detected except by their radiations. It is not surprising that the theory met with severe criticism, even from workers in the same field. But Rutherford’s evidence was so complete and he dealt with the criticisms in such convincing, but courteous, a way that he overcame all resistance to this great change in ideas.

I have told this now old story of the steps leading to the disintegration theory because it brings out clearly some aspects of Rutherford’s genius—his acceptance of experimental facts, however strange and however difficult to reconcile with accepted ideas and theory—the genius for being astonished, the sureness of his judgement in singling out the essential points from a mass of information, and the power of his reasoning.

These qualities were backed by a stupendous capacity for work. Soddy, writing in later years about this period, said: ‘For more than two years scientific life became hectic to a degree rare in the lifetime of an individual, rare perhaps in the lifetime of an institution.’ At the same time that he was working with Soddy on the nature of radioactive change, Rutherford was investigating many other aspects of radioactivity, among them the nature of the radiations and their connexion with radioactive change.

In order to realize the full import of Rutherford’s work during this period and to
see it in its true perspective it is instructive to read his book *Radioactivity*, written during 1903 in the midst of this hectic work and published in 1904. In this book he made a complete review of the properties of radioactive bodies and of their radiations, as then known, and he gave a detailed and connected interpretation of the facts on the basis of the disintegration theory. The clear presentation of the evidence, the closeness of his reasoning, and the great scope of his thinking make, even to-day, an impact of overwhelming power on the reader and bring out most vividly that the formulation and application of the disintegration theory is one of the greatest achievements in the history of science.

It is particularly interesting, in the light of later developments, to look at some of the general considerations to which he was led from his theory and from his ideas about the origin of the radiations.

He had early recognized the importance of the $\alpha$-rays. He had shown that they were positively charged particles projected with great velocities, and that if they consisted of any known kind of matter they were either hydrogen or helium. In discussing their origin, he wrote: 'It is necessary to suppose that the atoms of the radio-elements are undergoing disintegration, in the course of which parts of the atom escape from the atomic system. It seems very improbable that the $\alpha$- and $\beta$-particles can suddenly acquire their enormous velocity of projection by the action of forces existing inside or outside the atom. For example, the $\alpha$-particle would have to travel from rest between two points differing in potential by 5-2 million volts in order to acquire the kinetic energy with which it escapes.' His idea at that time was that the $\alpha$-particle was already in rapid motion before its escape from the atom.

The current idea of the structure of the atom, based on the conceptions of J. J. Thomson and others, was that the atom contained a number of electrons in motion within a kind of atmosphere of positive electricity distributed over the whole volume of the atom. In such an atom there could be no electric fields of the magnitude required to impart such large energies to the $\alpha$-particles. Rutherford was, in a few years' time, to be confronted with facts which could not be explained on this idea of atomic structure, and his appreciation of this difficulty was to lead to a further revolutionary discovery, that of the nuclear structure of the atom.

He was much concerned to point out the magnitude of the energy associated with the radiations and to stress that, as this could only be a small part of the total internal energy of the atom, the energy latent in the radioactive atom must be enormous compared with that released in ordinary chemical reactions. He emphasized further that there was no reason to suppose that this enormous store of energy was possessed by the radio-elements alone, but that it was probable that atomic energy in general is of a similar order of magnitude; 'this store of energy had previously not been observed on account of the impossibility of breaking up into simpler forms the atoms of the elements by the action of the chemical or physical forces at our command.' And again, 'if it were ever found possible to control at will the rate of disintegration of the radio-elements, an enormous amount of energy could be obtained from a small quantity of matter'. This was
another foreshadowing of things to come, towards which Rutherford was to make
the first steps but which he himself was not to see.

By the time Rutherford left Montreal for Manchester in 1907 the transmutation
theory of radioactive changes had been established beyond all doubt. Rutherford’s
fame was secure. His early opinion that the physical laboratory of McGill
was the best of its kind in the world was now shared by many; he had made
it so.

I know that this university recognizes how much it owes to the reputation won
by Rutherford and to his example in developing a centre of research. On the
other hand, Rutherford owed much to McGill and to his colleagues. McGill
provided him with ‘the tools for the job’, and many of his colleagues, notably
John Cox, gave him understanding and support when he needed it. John Cox,
director of the laboratory and his senior in the department of physics, had indeed
been largely responsible for bringing him to Montreal. On meeting Rutherford in
Cambridge he had immediately recognized that he possessed exceptional qualities.
Cox smoothed Rutherford’s path in his early days in Montreal, shielded him from
too many administrative and teaching duties, and gave him wholehearted backing
in his work and in the upholding of his bold ideas. Rutherford and McGill owed
much to John Cox.

When Rutherford arrived in Manchester he found there a number of active
young men eager to work under his direction, but the laboratory, though well-
equipped for general purposes, lacked an adequate supply of radioactive material.
The defect was soon remedied by the loan of a quarter of a gram of radium from
the Vienna Academy of Sciences. This generous act put him for the first time in
command of sufficient material to carry out a number of investigations he had long
had in mind.

The most important for their bearing on modern physics were the experiments
on the scattering of α-particles—in which, strangely enough, Rutherford himself
took no active part.*

When, in 1906, Rutherford was measuring the deflexion of the α-rays due to
a magnetic field, he noticed that the presence of a small amount of air in his
apparatus distorted the path of the rays. A similar ‘scattering’ effect was observed
when a sheet of mica only one-thousandth of an inch thick was placed over the
source of the rays; most of the particles travelled along the expected path but
a few were deviated or ‘scattered’ through a few degrees. Small as this effect was,
it excited Rutherford’s interest, for he was well aware that no electric or magnetic
forces at his command could produce a change of two or three degrees in the
direction of motion of the rays in so short a distance as one-thousandth of
an inch. With his genius to be astonished and with his appreciation of magni-
tudes, he drew attention to this effect, and he pointed out that this ‘would
require over that distance an average transverse electric field of about 100 million
volts per centimetre’; and he concluded ‘such a result brings out clearly the

* At this period Rutherford’s eyes were troubling him and he found scintillation counting
rather tiring. In a letter to H. A. Bumstead he wrote: ‘Geiger is a demon at the work of
counting scintillations...I damned vigorously and retired after two minutes.’
fact that the atoms of matter must be the seat of very intense electrical forces'.

This significant comment marks a change in his thinking about the structure of the atom, and from that time he was anxious to investigate the scattering of \(\alpha\)-particles more closely.

The means of doing so came during the course of his experiments on the nature of the \(\alpha\)-particles. In order to obtain a decisive proof that the \(\alpha\)-particle was an atom of helium, he devised, with Geiger, a method of counting single \(\alpha\)-particles, making use of the phenomenon of ionization by collision to multiply many thousand times the ionization effect of a single particle, an exceedingly delicate operation with the facilities available at that time. Using this safe electrical method they were then able to show that the number of scintillations observed on a properly prepared screen of zinc sulphide crystals was equal to the number of \(\alpha\)-particles falling on it. He now had a simple and convenient method for counting \(\alpha\)-particles, and he at once turned his attention to the problem of scattering.

A rapid examination by Geiger showed that some particles were deflected through an appreciable angle in passing through a very thin leaf of gold, thus confirming in a very direct way Rutherford's earlier observation. Rutherford, with his usual eagerness to get a general picture of any new phenomenon before making measurements on details, suggested that Marsden, then a young student helping Geiger, should examine whether any particles were scattered through large angles from a metal surface—an effect similar to the 'diffuse reflexion' shown by \(\beta\)-particles. To his surprise, they were, even from the thinnest foils of metal. In Rutherford's words, spoken long afterwards, 'It was quite the most incredible event that has ever happened to me in my life. It was almost as incredible as if you had fired a 15 inch shell at a piece of tissue paper and it came back and hit you.' The difficulty of explaining how such an event could occur was emphasized by Geiger's further work on the scattering through small angles. All his experiments seemed to be consistent with the view that each atom of matter through which the \(\alpha\)-particles passed contributed a small share to the actual deflexion which was finally measured, which thus appeared to be the statistical sum of a large number of small deflexions. This picture was what was expected on the current conception of the structure of the atom, although, as I have said, Rutherford was already insisting that there must be much stronger electrical forces within the atom than was generally supposed. But the large deflexions observed by Geiger and Marsden did not fit into this picture at all; they should have been so exceedingly rare as to escape all observation.

Rutherford pondered on this strange fact for many months, until the winter of 1910–11, when, as Geiger related in a letter to me, 'one day, obviously in the best of spirits, he came into my room and told me that he now knew what the atom looked like and how the large deflexions were to be understood. On the very same day I began an experiment to test the relation expected by Rutherford between the number of particles and the angle of scattering.'

Those who knew Rutherford will recognize the typical Rutherford touch.

This was a crucial test, and when, after a few months, Geiger had confirmed the
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predicted relation, but not until then, Rutherford gave a public account of his nuclear theory of atomic structure.*

This nuclear theory is Rutherford's most dramatic contribution to physics and perhaps his greatest single contribution, for it has changed the whole face of physics and chemistry. The problem of the structure of the atom now divided itself naturally into two parts: (1) the constitution of the nucleus itself and (2) the arrangement and motions of the external electrons. It was clear that all the ordinary properties of matter must depend upon the electronic structure. Within a few years Bohr had shown, by applying quantum principles to the motion of the electrons, how the complicated relations of optical and X-ray spectra could be interpreted to reveal the electronic configuration of the atom, and how chemical behaviour could be described in terms of this configuration. Although Bohr's early ideas gave only a partial insight into the quantum conditions as we now know them and although they have been modified considerably in the course of time, this work remains one of the greatest triumphs of the human mind. It was the first bold step into the unfamiliar world of the atomic system of non-classical behaviour, of stationary states and transitions; and it opened the path which led finally to the discovery of the quantum mechanics which provides a complete interpretation of most of the ordinary physical and chemical properties of matter.

Rutherford's special interest lay in the new problem which his theory brought with it—the problem of the constitution of the nucleus. It was evident that the nucleus was the seat of the radioactive changes and that the explanation of the difference between one chemical element and another was to be sought in its structure. It was therefore to be expected that it must be a highly complicated system. Yet its dimensions are so minute—its radius is less than one-thousandth of that of the atom—that the density of matter in it is many billion times that of ordinary matter; the particles of which it is composed, whatever they might be, must be bound together very tightly. The question of the structure of the nucleus seemed almost impossible to attack experimentally and, as Rutherford said himself, it is 'a problem that might well be left to the next generation'.

But, characteristically, he proceeded to show the way. In his William Hale lectures in 1914, he said: 'There is no doubt that it will prove a very difficult task to bring about the transmutation of matter under ordinary terrestrial conditions... the building up of a new atom will require the addition to the atomic nucleus of hydrogen or of helium, or a combination of these nuclei... it is possible that the nucleus of an atom may be altered by direct collision of the nucleus with very swift electrons or atoms of helium such as are ejected from radioactive matter... under favourable conditions, these particles must pass very close to the nucleus and may either lead to a disruption or to a combination with it.'

Rutherford accordingly turned his attention to problems arising out of the collision of α-particles with atoms of light elements, which would be set in vigorous

* This was given at a meeting of the Manchester Literary and Philosophical Society on 7 March 1911. At the same meeting Professor Elliot Smith exhibited a cast of the Gibraltar skull, which to some of us young physicists seemed to be compounded of equal parts of plaster of Paris and imagination; and a fruit importer exhibited a rare snake found in a consignment of bananas from Jamaica.
motion by the impact and might be detectable by the scintillation method. A beginning on this work was made by Marsden in 1914 and some progress was made. He observed the projection of hydrogen nuclei by the impact of $\alpha$-particles, and he noted a strange effect when air was bombarded—the presence of projected hydrogen nuclei in greater numbers than he was able to explain as arising from hydrogen present as water vapour in the air or in other forms in materials exposed to the $\alpha$-particles.

The experiments were abandoned on account of the war, but Rutherford himself took them up again towards the end of 1917. He then succeeded in showing that the nitrogen nucleus could actually be disintegrated under the impact of the $\alpha$-particle with the emission of a hydrogen nucleus, or proton. This discovery was the first step towards a full understanding of the problem of nuclear structure, and it marks the opening up of a new field of work which has grown in an almost bewildering fashion since that time. It was the last work he published from Manchester, and it made a fitting end to his period there.

I come now to his Cambridge period and my own association with him. I had entered Manchester University in 1908 and I began research under his direction in 1910. But in 1913 I left to work under Geiger in Berlin and I did not return until the latter end of 1918. When Rutherford moved to Cambridge in the summer of 1919 he invited me to go with him to help to get radioactive work started there, and he helped to make it possible for me to do so. Thus began a close association with Rutherford which lasted until 1935 when I left Cambridge, a period of absorbing interest and inestimable benefit to me and a period which I cherish in my memory.

In the winter of 1920–1 he asked me to join him in a new attack on the disintegration problem; his reasons for this were that he expected difficulties in proving that any effects which might be found were not due to the presence of hydrogen, and he thought I was enough of a chemist for the purpose, and that I had made some improvements in the technique of counting scintillations; but also, I think, he wanted company to support the tedium of counting in the dark—and to lend an ear to his robust rendering of ‘Onward, Christian Soldiers’.

In the following two or three years we showed that nearly all the light elements up to potassium could be disintegrated by $\alpha$-particles. In all these cases a proton was emitted; we could find no evidence of the emission of any other kind of particle. It seemed probable, as Blackett’s beautiful photographs of the event in an expansion chamber showed later, that the process of disintegration consisted of the entry of the $\alpha$-particle into the bombarded nucleus, followed by the emission of a proton and the formation of a new nucleus, but our experiments could give no direct information on this matter. We naturally speculated about the properties of the new nucleus formed in this way, and Rutherford suggested, taking up an idea he had had in Montreal, that the new nucleus might be unstable and so radioactive. This idea was tested by Shenstone, who looked for the emission of heavy particles which might produce scintillations, and by Ahmad, who looked for the emission of any radiation by an ionization method. In neither experiment was any effect observed, although we now know, from the work of Joliot and Curie, that in some
cases the newly formed nuclei are indeed unstable and may emit electrons or positrons. The failure to detect this phenomenon was due to the inadequacy of the experimental techniques of that time.

This work and other work on the law of force close to the nucleus gave some insight into the problem of nuclear structure, but it fell far short of what was needed. The general conception of the nucleus was that it was built up from protons and electrons, with \( \alpha \)-particles probably as a sub-unit of structure; and there was evidence, from the \( \gamma \)-ray emission of radioactive bodies, that the particles were in quantum states, for these nuclei had systems of energy levels. From very general considerations, we formed a picture which corresponded to what was called later a ‘potential barrier’ and a ‘potential well’, but we had no clear ideas of how to explain it or even how to go about an explanation.

Further, the scintillation method had outlived its usefulness. It was simple, but it was too tedious and too subjective. We had indeed realized from the beginning of this work the great need for a reliable electrical method of counting, and in 1919–20 I had made some unsuccessful attempts to develop one, including the valve amplification method.

It began to seem that the problem of the new structure of the nucleus might indeed have to be left to the next generation, as Rutherford had once said and as many physicists continued to believe. For his faith in the promise of this work was shared by few men outside his own laboratory. I remember well various occasions on which he told me how he was being criticized for devoting his time to such fruitless efforts and especially for putting his men to work in this field rather than in more useful branches of physics. The latter criticism caused him anxiety, for he was always mindful of the welfare of his research students; but he was supported by the thought that, whatever the outcome of their work, they were receiving a thorough training in methods of research, which would serve them in any branch of physics. It was not that he had little interest in other aspects of physics. Indeed, there were always several different lines of work represented in the laboratory. But he held to his belief that the most important task in physics was to inquire about the ultimate particles of matter and how they combined together to form the chemical elements, and he felt that it was peculiarly his business to attack this problem, however difficult it appeared. Indeed, if he could not lead the way into the unknown, who could?

For a few years, Rutherford and his laboratory passed through a relatively quiet spell. Much interesting and important work was done, but it was work of consolidation rather than of discovery; in spite of many attempts the paths to new fields could not be found. During this period, however, a considerable advance in the technique of counting particles was achieved, by Greinacher in Basle with the valve-amplification method of counting \( \alpha \)-particles and protons and by Geiger and Müller in Berlin in counting \( \beta \)-particles. The Greinacher method was developed by Wynn-Williams and others into a reliable and accurate method of counting and recording, and it completely superseded the scintillation method. Then, in 1928, a new light was thrown on the problem of disintegration by the application of quantum mechanics to the nucleus by Gamow and by Gurney and Condon. This
theory enabled one for the first time to think quantitatively about disintegration processes and to see how artificial disintegration might be expected to vary with the type and energy of the bombarding particle. In this way Cockcroft came to the conclusion that protons accelerated by only 300,000 volts should have a fair chance of disintegrating the lighter elements. He and Walton were encouraged by Rutherford to proceed with the building of equipment to test this idea, and in 1932 they were successful in achieving for the first time a transmutation of matter by means wholly within the control of the experimenter.

Just before this event, I had discovered a new particle of nuclear structure, the neutron, a discovery which resolved some grave difficulties in the theory of nuclear structure and which in the course of a few years, through the work of Hahn, Joliot, Fermi and others, was to lead to the development of atomic energy. In his Bakerian Lecture in 1920 Rutherford had predicted, in words which have been so often quoted that I need not repeat them, the existence of this particle and described some of its properties. The experimental results on which he seemed to base his prediction were in fact unreliable, and the interpretation of them which he then gave was later withdrawn. But, as I learned from many discussions with him, his belief in the neutron did not grow out of these experiments, but from long and deep consideration of the problem of the building up of nuclei from elementary units and of the question of what type of units would best meet this purpose and provide the broad rules known to hold for the numbers and masses of the stable nuclei. I soon came to share his belief. From time to time over several years we made experiments, some of them far-fetched and even wild, to try to detect this neutron. In later years he was too occupied with outside duties for such adventures, and I pursued them myself. I mention this matter here to acknowledge my debt—in truth a small part of my debt—to Rutherford, and to record that the discovery of the neutron came naturally in the general line of advance marked out by him years before.

These two advances, the discoveries of the neutron and of transmutation by artificially accelerated particles, fully justified the years of preparation to which Rutherford had devoted the main effort of his laboratory. With the discovery of artificial radioactivity by Joliot and Curie in Paris shortly afterwards, they opened up a new era in nuclear physics, which now rapidly became one of the most popular fields of research. Rutherford took great pleasure in these developments and he threw himself into the new work with ardour. He still looked forward to the opening up of further paths of discovery and to the exploitation of that ‘enormous store of latent energy resident in the atoms’ to which he had first drawn attention in 1903, but he did not live to see it. Although a host of nuclear reactions soon became known, their practical value as a source of energy was completely negligible. They were rare events and the reactions were not self-propagating. The clue to the useful realization of atomic energy did not come until 1939, when Hahn and Strassmann discovered the fission of uranium. The stupendous developments which have taken place since then do not belong to this story; but I hope I have made clear that the foundations of nuclear physics and of atomic energy are largely the work of this one man—Rutherford—in whose memory we are gathered here to-night.
What he would think about the position to-day I hesitate to guess; of one thing we may be sure, that he would rejoice to see the important part that Canada has taken, and is taking, in the peaceful developments of atomic energy.

And we may be sure, too, that he would be even more interested in the new story of sub-nuclear matter which is now being unfolded by the study of the collisions of the very energetic particles of cosmic radiation and of particles accelerated in the gigantic machines built since the war. In these collisions, a whole family of new particles—or perhaps I should call them objects—of quite unexpected properties has emerged; we know neither why they exist, why they behave as they do, nor how they fit into the scheme of Nature. They confront us with strange and bewildering problems, a veritable 'Tom Tiddler's ground' as Rutherford would say. The new world into which he led us turns out to be even stranger than either he or anyone else suspected. He would have been delighted to be puzzled by these queer objects, confident in his ability to put them in their place in the scheme of things, and confident too that this would lead to just as strange and interesting puzzles in the future.

It has been said of Galileo that he left science in quite a different state from that in which he found it. How truly this can be said of Rutherford I have tried to show in the brief compass of this lecture. Perhaps it was hardly necessary to do so, for the story of his work is still thrilling and is still fresh to those of us who lived through some of those heroic days, and, though the history of science is neglected in our schools and universities, Rutherford's achievements are so astonishing that every generation of scientists will hear and venerate his name. But I wanted to do more than to excite your wonder at the splendour of his contributions to science. I wanted also to indicate the way in which he came to his discoveries, to show how one discovery led naturally—to him—to the next, and how so much was foreshadowed in his early writings from McGill; and I hoped that in doing this I might be able to suggest to you something of the extraordinary quality of his mind.

It is easier to recognize the divine flame of genius than it is to describe it, and I feel that it would be as futile as it might appear presumptuous to try to make an analysis of Rutherford's gifts. But it is perhaps not out of place if I mention a few aspects of his work and of his qualities which most deeply impressed me.

One of the most striking features of his work is its directness and continuity, almost as if the end had been foreseen in its beginning. From his earliest days in research, Rutherford's 'overall strategic objective', to use an apt if cumbersome expression of some military circles, was to discover new scientific truths; he had little interest in inventing new devices and techniques except to use them for discovery. From his first work in Montreal his dominating interest was the problem of atomic structure and of the fundamental particles of matter. All other questions were subordinate to this; and in considering alternative paths of research he selected those which would lead most directly to the solution of this problem. This singleness of purpose reinforced his natural ability to seize upon the essential features of any phenomenon and helped him to avoid the confusion of thought into which others might be led. It helped him, too, to ask the right questions—generally a more difficult thing to do than to find the answers.
It was sometimes said, and it sometimes seemed to his fellow-workers, that Rutherford knew the answer to his questions before doing the experiment, almost that the experiment was required only to convince others, as if he were blessed with an intuition which revealed the secrets of Nature without effort on his part. I do not deny the role of intuition in scientific discovery, or question that he possessed this gift of intuitive, supra-logical thought; but I believe that inspiration and illumination came to him, generally at least, as the final distillation of hard and prolonged labour. Rutherford had, in a greater degree than any other man I have known, the ability to fix the whole power of his mind on a matter for a long time without getting tired or losing interest; he considered a problem so thoroughly from every angle that he got a comprehensive picture in his mind—a kind of bird’s eye view of the territory he was exploring, a view in which the main features fell into their true relationship and in which details did not obtrude—a picture to which he would return again and again until the solution came to him. Familiarity with new facts and problems did not diminish his wonder at them, as it does with most of us, or damp his interest.

Another significant characteristic was that he never regarded techniques, or hypotheses, as anything but tools to be used to discover new aspects of Nature; they were not ends in themselves. As soon as a new technique was in his judgement adequate for his purpose, he used it; he did not wait to perfect it; that would come during its use or could be left to others. Any man who worked with him, from Montreal to the Cavendish, will remember his constant injunction to ‘Get on with it’, stimulating some, goading others, not to linger on unessential details but to press on to the true goal of their endeavours. He himself set a great example; he never suffered from the itch for perfection in experimental technique which so often leads to a paralysis of the urge to discovery.

I shall not attempt to describe his personality, not because I think this has no relevance to his work but because an adequate description is beyond my powers. Those who knew him will need no reminding of his generous nature, his friendly approach to all honest men whatever their age or status in life, his frank enjoyment of his work and of his fame, his modesty without any false modesty, and above all perhaps his rumbustious vitality; his memory is fresh in our minds. To those who did not know him I can best describe the impression he made by quoting the words of John McNaughton, formerly Professor of Classics in McGill University, written after hearing a lecture by Rutherford in his young days:

‘Emanations of light and energy, swift and penetrating, cathode rays strong enough to pierce a brick wall, or the head of a professor of literature, appeared to sparkle and coruscate from him all over in sheaves. Here was the rarest and most refreshing spectacle—the pure ardour of the chase, a man quite possessed by a noble work and altogether happy in it.’

As he was in his youth in McGill, so he happily remained to the end of his life.