The Rutherford Memorial Lecture, 1965

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Lord Rutherford died at Cambridge in 1937 and the first Rutherford Memorial Lecture was delivered by Sir John Cockcroft fifteen years later at Canterbury College, part of the University of New Zealand, where Rutherford first matriculated in 1889. Since the date of that lecture nine further Memorial Lectures have been delivered in the main countries of the British Commonwealth. Two of these lectures were given in Canada, at the University of McGill, where as Professor of Physics from 1898 to 1907 Rutherford performed that remarkable series of experiments which laid the foundations of the science of radioactivity. It seemed to me appropriate, on this occasion of the third Memorial Lecture in Canada, to choose a laboratory further to the west, and the growth of the Physics department here at Saskatoon, with its present major enterprise for research upon atomic nuclei, made this choice both natural and personally attractive.

In previous lectures of this series the achievements and character of that remarkable man whom we meet to honour today have been very fully described. Sir James Chadwick, in his lecture at McGill in 1953, gave a documentary account of Rutherford's scientific career. Chadwick had worked with Rutherford at Manchester from 1910 until 1913, the critical period during which the nuclear atom was formulated, and then returned to Manchester in 1918 shortly after Rutherford's first successful experiments upon the disintegration of nitrogen. With Rutherford he then extended these experiments to other elements and moving to Cambridge in 1919, when Rutherford accepted the Cavendish Chair, continued to work in collaboration with him during nearly the whole of the rest of Rutherford's life. His description of that association must clearly be regarded as the definitive document upon Rutherford's scientific work and attitude.

Subsequent Memorial Lectures by distinguished men who had worked closely with Rutherford at one stage or another, at Manchester or Cambridge, have added a wealth of detail to these first accounts. Marsden, a native of New Zealand, who also worked with Rutherford from 1907 to 1914 at Manchester, has given a rich account of Rutherford's boyhood days and of his participation in the \( \alpha \)-particle scattering experiments at Manchester which led to the enunciation of the idea of a nuclear atom. Professor Andrade's memorial lecture in 1957 at Melbourne, entitled 'The birth of the nuclear atom', is a characteristically erudite account not only of Rutherford's part in that conception but also of contemporary thought in this and related fields which were affected by those developments.
I mention these lectures in the hope that some of you may be tempted to read these fascinating surveys of these scientific advances. With this wealth of personal and historical material it is not surprising that later lecturers have tended to seek in wider or more modern fields for the subjects of their memorial lectures and it is with some trepidation that I have decided to revert again to some description of Rutherford's personality and to one or two, albeit restricted, areas of his wide researches. I am tempted to do this because I had the good fortune, after the exodus in post-war years of Rutherford's chief collaborators at Cambridge, to be in charge of the Cambridge high voltage accelerator and for the last year or two of Rutherford's life to be in regular association with him. To me Rutherford was not only a supreme genius among experimental scientists but a wholly remarkable, outstanding and lovable person. I think that not even the slightest opportunities to record all that is personally and certainly known about such a man should be neglected.

Before delineating the particular topic around which I shall attempt to frame this lecture it seems to me to be necessary, despite what I have said, to refer very briefly to the overall nature of Rutherford's scientific work, not only for the purpose of explaining the relevance of my subject but also for this present memorial to his honour and for the benefit of those here today who may not yet have read the literature to which I have referred or be immediately familiar from other sources with its general content.

Rutherford's researches fall into three main phases: the first at Montreal, the second at Manchester and finally in Cambridge. Rutherford took up his post as Professor of Physics at Montreal at the age of 27, shortly after Becquerel's discovery of the blackening of photographic plates by salts of uranium. These salts had been shown also to possess the property of making gases conductors of electricity. Rutherford set out to investigate the properties of the radiations supposed to be responsible for these effects. Viewed from the standpoint of our present knowledge of these processes the problem then facing him seems to be of almost unsurmountable difficulty and complexity. A large number of different and at that time unknown phenomena were involved in these experiments. Two types of radiation, the $\alpha$- and $\beta$-rays, were emitted from these substances with quantitatively different ionizing effects. The emission of radiation from each radioactive body decayed exponentially and with different time constants. As a consequence of each emission new bodies were formed which themselves were radioactive. In some cases these products were gases which, under the influence of draughts, gave rise to wildly inconsistent measurements. The radioactive decay of these gaseous emanations formed radioactive deposits throughout the apparatus. Simple ionization chambers and electrometers were the only basic tools available for these investigations. One saving feature was the fact that, often, bodies with different radioactive properties possessed different chemical behaviour. By a series of investigations, brilliant both in conception and execution, Rutherford & Soddy (1902) within five years laid down the basic principles underlying all these phenomena and in 1903 put forward with incontrovertible evidence their theory of 'radioactive change'. Almost immediately these principles were applied to the
successive decay products of the radium family and the nature of the genealogical
tree for a connected series of radioactive decay products was put forward in a
form which has not since required substantial modification.

Rutherford was destined later to obtain results of even greater importance and
generality, but to me it seems that this first phase of his work represents the most
brilliant connected sequence of imaginative experiments ever performed in the
history of physical science, and despite the fact that the story has already been told
much more comprehensively and precisely in these lectures I feel compelled to
remind you of it here today.

After nine years at Montreal Rutherford accepted the Langworthy Chair of
Physics at Manchester. Here, in collaboration with Geiger and Marsden, tech­
niques were developed for the detection of individual atomic particles and were
applied to the study of the scattering of $\alpha$-particles by very thin metallic foils. In
these experiments it was observed that on very rare occasions $\alpha$-particles were
deflected through large angles. Rutherford’s grasp of the fundamentals of his
subject, combined with his great imaginative insight, stimulated him to pursue this
trivial clue which many would have neglected. He found it possible to reconcile
the observations only with a concept of the atom as possessing a massive minute
nucleus. The law of scattering which followed mathematically from this assump­
tion was worked out and within a few months accurate experimental observations
were made which confirmed the hypothesis in detail. Full accounts of this work
have been given in previous memorial lectures and I do not propose to do more
today than to remined you of this great phase of his work which, with the sub­
sequent application of quantum mechanical principles by Bohr, gave rise to the
planetary model of the atom.

The third great phase of Rutherford’s work began at Manchester in 1917 with
the discovery of the artificial transmutation of nitrogen under bombardment with
$\alpha$-particles. Shortly after this date Rutherford succeeded J. J. Thomson as Cavend­
ish Professor of Experimental Physics at Cambridge. Here for the rest of his life
his work was concentrated upon the performance and direction of experiments in
this field of artificial transmutation of the elements. The climax of this work came
in the year 1932 when Cockcroft and Walton disintegrated the atom of lithium by
artificially accelerated protons. In that same year Chadwick discovered the
neutron and during the five years that followed a host of related investigations was
performed in the Cavendish Laboratory under Rutherford’s stimulus and direction.

Following this brief record of the main features of Rutherford’s career I now
wish to bring to your attention in rather more detail one or two particular aspects
of these discoveries. These are, first, the development of Rutherford’s conceptions
of the nature and structure of the atomic nucleus, as distinct from the discovery of
its existence, and, secondly, his views about the nature of the structural elements of
that nucleus and the relation of these views to those held at the present time.

In Rutherford’s (1911) paper, entitled ‘The scattering of $\alpha$- and $\beta$-particles by
matter and the structure of the atom’, where the nuclear atom is first proposed, the
basic assumption which he shows to be necessary to explain the experimental
results is that there shall be a concentration of charge within a distance of less
than $10^{-13}$ cm. The critical point upon which he fastened was that the $\alpha$-particle scattering data of Geiger and Marsden could not be reconciled with scattering by electrons or by a distribution of positive charge 'unless it be assumed that the diameter of the sphere of positive electricity is minute compared with the diameter of the sphere of influence of the atom'. The dynamical theory of the collision process was developed mathematically and it was shown that Geiger's data were consistent with a central charge for different atoms approximately proportional to their atomic weights. For the case of gold he deduced that the charge was about 100 e, 'that to be expected if the atom of gold consisted of 49 atoms of helium, each carrying a charge 2 e. This may only be a coincidence, but it is certainly suggestive in view of the expulsion of helium atoms carrying two units of charge from radioactive matter'.

It must not be thought that the atomic model, as proposed in this paper, embraced all of the basic elements of the planetary atomic model. Although Nagaoka's consideration of a 'Saturnian' atom with a central attracting mass surrounded by rings of rotating electrons was mentioned no attempt was made to develop those considerations in this paper. Indeed, Rutherford was at pains to point out that the data and considerations would be equally consistent with a negative central charge and that it had not yet been found possible to determine whether that charge be positive or negative. He pointed out, however, that if the charge were positive a positively charged mass released from the centre of a heavy atom would acquire a great velocity in falling through the electric field and that the high velocity of $\alpha$-particles emitted from radioactive bodies might thus be explained.

Three years later, in 1914, Rutherford wrote a paper on the 'Structure of the atom' to deal with 'points purposely omitted from the 1911 paper'. During these three years some very critical developments had been made which now greatly simplified the discussion of nuclear structure. The proposal, originally made by van den Broek, that the nuclear charge on an atom was identical with its ordinal number, had been brilliantly established by Moseley's X-ray experiments which had exhibited a parameter which increased by regular steps from atom to atom—a quantity which 'can only be the charge on the nucleus'. Bohr over this same period had developed the considerations of planetary electrons and had accounted quantitatively for the hydrogen spectra. Rutherford's nucleus had now certainly a positive charge and the first tentative considerations of nuclear constitution were possible. It was at this stage that the first significant conceptions of nuclear structure began to emerge and it is not surprising that the $\alpha$-particle should have been chosen to play a dominant role in these conceptions. It may be remembered that Rutherford's Nobel Prize Lecture in 1908 had been entitled 'The chemical nature of the $\alpha$-particles from radioactive substances'. I quote from the concluding paragraph of that lecture:

'We have seen that there is every reason to believe that the $\alpha$-particles, so freely expelled from the great majority of radioactive substances, are identical in mass and constitution and must consist of atoms of helium. We are consequently driven to the conclusion that the atoms of the primary radioactive elements like uranium
and thorium must be built up in part at least of atoms of helium... Apart from their radioactivity and high atomic weight, uranium, thorium and radium show no specially distinctive chemical behaviour. Radium, for example, is closely allied in general chemical properties to barium. It is consequently not unreasonable to suppose that other elements may be built up in part of helium, although the absence of radioactivity may prevent us from obtaining any definite proof. On this view it may prove significant that the atomic weights of many elements differ by four—the atomic weight of helium—or a multiple of four'.

This supposition, that \( \alpha \)-particles constituted a structural element in matter, was then, in 1914, clearly transferable from the atom to the nucleus. The 1914 paper is very interesting also for the reason that it contains the first inklings of the conception of nuclear binding energy. After a discussion of the sizes of \( \alpha \)-particles, hydrogen nuclei and heavy nuclei, Rutherford pointed out, following Lorenz, that the mass of an atom depends also upon the interaction of the electric fields of the nuclear constituents and he emphasized that the packing of these units must be very close in order to explain the data and to produce an appreciable alteration in mass due to this cause. He suggested that here might lie the explanation of the fact that the mass of the helium atom was significantly less than that of four nuclei of hydrogen. The \( \alpha \)-particle, in this paper, is described as a combination of four hydrogen nuclei and two electrons and its great stability 'surviving the intense disturbances resulting in its expulsion from the radioactive atom' is attributed to similar causes; these arguments all again being advanced as evidence that the \( \alpha \)-particle is 'one of the units of which the great majority of atoms are composed'.

In all of these earlier considerations of the constitution of nuclei it was generally supposed, in order to explain the fact that atomic numbers were approximately one half of atomic masses, that electrons constituted a necessary secondary unit in nuclear structure. It must of course have been evident to Rutherford that \( \alpha \)-particles and electrons alone would not suffice to explain the constitution of all nuclei, but he pointed out that a careful search of radioactive emissions had failed to exhibit protons or any other particles. In the following year, in an article in *Popular Science Monthly* (1915), Rutherford makes specific reference to the need for hydrogen nuclei in heavier species and concludes 'I personally am inclined to believe that all atoms are built up of positive electrons—hydrogen nuclei—and negative electrons and that atoms are purely electrical structures'.

The progress in the elucidation of nuclear structure was now halted by the war, except that, as we have seen, the first evidence of nuclear disintegration by \( \alpha \)-particles, leading to proton emission, was obtained towards the end of that period. The next convenient signpost for this survey is perhaps Rutherford's Bakerian Lecture in 1920. By this time the results of Aston’s researches with the first velocity focusing mass spectrograph were available and Soddy’s original conception in 1907 of isotopic constitution of the heavy radioactive elements was seen to be applicable with certainty throughout the whole range of natural elements. The accuracy of the observations with this instrument was sufficient also to establish that, on the scale of \( O = 16 \), the masses of the isotopes were throughout, with the
solitary exception of hydrogen, very close to whole numbers. The difficulties associated with fractional chemical atomic weights which, periodically throughout the previous 130 years, had prevented the acceptance of Prout's original proposal that all atoms were composed of hydrogen were now removed and, as Rutherford pointed out, the apparent exceptional case of hydrogen did not exclude this proposition but, rather, within the very small nuclear dimensions, indicated 'either that the grouping of hydrogen nuclei and electrons is such that the average electromagnetic mass is nearly 1, or, what is more probable, that the secondary units, of which the atom is mainly built up, e.g. helium, or its isotope, have a mass given nearly by a whole number when O is 16'. (The reference here to the helium isotope need not concern us in this connexion. It was not, of course, the $^3$He discovered later in 1935, but instead a particle which Rutherford had incorrectly hypothesized in order to explain some recent work, later unconfirmed, on the disintegration of oxygen and nitrogen by $\alpha$-particles).

It is very remarkable that in the same lecture the possible existence of a neutron was forecast by Rutherford twelve years before its later discovery by Chadwick. At the present time the basic constituents of atomic nuclei are commonly accepted to be protons and neutrons, integral atomic masses on the $O = 16$ scale being broadly due to mass defects, approximately 0.008 and 0.009 respectively, of these constituents compared with their masses in the free state. These mass defects are attributable to binding energies in a general relativistic extension of the Lorenz type electromagnetic mass defect as envisaged by Rutherford in 1914 to be applicable to the helium atom. The simplicity of accounting for the atomic numbers and isotopic masses of all elements by the use of these two particles as constituent units must certainly have been often in Rutherford's mind and it is surprising that, having indeed here proposed a neutron, he did not propose such a nuclear model.

However, Rutherford's suggestion, in this paper, of the existence of a neutron arose, not in regard to overall nuclear statistics, but in an attempt to explain the nature of the $^3$He particle which I mentioned earlier. He envisaged this particle as three hydrogen nuclei, bound by one electron and continued:

'If we are correct in this assumption it seems very likely that one electron can also bind two H nuclei and possibly also one H nucleus. In the one case this entails the possible existence of an atom of mass nearly 2 carrying one charge, which is to be regarded as an isotope of hydrogen. In the other case, it involves the idea of the possible existence of an atom of mass 1 which has zero nucleus charge'.

I expect you all know that he then proceeded to predict the likely properties of such a neutral particle, and described many of the now well known properties of the neutron.

Rutherford then proceeded to envisage possible static models of nuclear structure using these entities. He clearly regarded these models as wildly tentative and the details are now of no importance. But for the purpose of our present considerations it is interesting to note that, close as he was to what we now regard as the true situation, he did not propose the neutron and proton as basic nuclear constituents. His structural units were fundamentally perhaps protons and electrons but, effectively, were secondary units, $\alpha$-particles and $^3$He nuclei relatively loosely
bound together by electrons. Rutherford was, above all else, a realist. He could propose a neutron and envisage experiments for its detection but for the sound interpretation of past work he no doubt preferred his well established \( \alpha \)-particle and the simple fact that the emission from natural radioactive elements consisted only of \( \alpha \)-particles and electrons.

During the following few years the first serious attempts were made to construct models for the atomic nucleus. Dominating these attempts were the assumptions which I have mentioned earlier, namely that nuclei were effectively composed of \( \alpha \)-particles and electrons, the \( \alpha \)-particles themselves being structures of four protons and two electrons whose great stability was evidenced by the large mass difference between the \( \alpha \)-particle and four hydrogen atoms. Further investigations of disintegration of light elements by \( \alpha \)-particles, leading to proton emission, had shown that whereas such emission was relatively prolific for elements with masses \((4n + 2)\) or \((4n + 3)\), \((B, N, F, Na, Al)\), there were few or no protons emitted from \( A = 4n \) type nuclei \((O, O, Ne, Mg, Si)\). This result was of course in agreement with Rutherford's \( \alpha \)-particle model and led him further to suggest that the few protons, emitted in these experiments, existed as satellites of the main nuclear system.

In parallel with these developments over this period more systematic \( \alpha \)-particle scattering experiments had been undertaken by Chadwick & Bieler and by Rutherford & Chadwick, to determine the law of force between the \( \alpha \)-particle and the target nucleus, in order to set more precise limits to the nuclear dimensions. Broadly speaking it appeared that for the heaviest nuclei the radius did not exceed \( 5 \times 10^{-12} \text{ cm} \), but that for aluminium nuclei, where owing to the smaller nuclear charge a closer approach of the \( \alpha \)-particle was possible, a breakdown of the inverse square law took place and that at distances of less than \( 5 \times 10^{-13} \text{ cm} \) attractive forces came into play. That such attractive forces should eventually exist at very small distances was of course a necessary condition for nuclear stability.

In a lecture to the Franklin Institute in 1924 the above considerations were summarized and fresh evidence was tentatively advanced in support of an hypothesis of an \( \alpha \)-satellite nuclear model. Rutherford here pointed out that often among the natural radioactive elements there were striking resemblances in the modes of transmutation of nuclei having the same atomic number but different masses; for example, Ra\(^{214}\) and Th\(^{214}\), both with atomic number 83 but with masses 214 and 212 respectively. He suggested that this behaviour might be explained if the satellites, composing the \( \alpha \) and \( \beta \) rays ultimately emitted, were held in equilibrium by attractive forces from the core, these forces being the same for both elements. On this view radioactivity would be a property of the satellite distribution which, although different for the two elements, could possibly possess many points of similarity. There is, of course, obvious here, an attempt to extend the Bohr orbital electron model, which had been so successful in explaining atomic properties, to atomic nuclei. This model must at the time have seemed very attractive in view also of the contemporary work of Ellis upon the quantum energies of the \( \gamma \)-rays emitted by radioactive nuclei. Ellis had shown that, as for optical spectra, combination rules existed between the frequencies of the \( \gamma \)-rays.
and this naturally led to the need for discrete levels of excitation of atomic nuclei, a need which could apparently be highly successfully envisaged by the conception of \(\alpha\)-particles rotating in stable orbits about a nuclear core. Rutherford pointed out that the transition of these \(\alpha\)-particles from one level to another would lead to \(\gamma\)-ray emission, and even suggested that if this were so the differences in the energies of the \(\alpha\)-particles emitted from the same radioactive element might be connected quantitatively with the observed quantum energies of the emitted \(\gamma\)-rays. He remarked that the evidence, at that time available, was not definite enough to give a final decision on this problem. It is very amusing to reflect that such quantitative correlation of \(\alpha\)- and \(\gamma\)-ray energies was to be abundantly forthcoming in the years that followed, although the \(\alpha\)-satellite model was to have only an ephemeral existence and to take no part in explaining this correlation.

But perhaps the dominant reason for a temporary existence of this model was further experimental evidence upon the law of force between \(\alpha\)-particles and nuclei derived from improved \(\alpha\)-particle scattering experiments. In 1925 Rutherford & Chadwick reported experiments, upon \(\alpha\)-particle scattering, aimed at an investigation of the law of force down to the smallest possible distances of approach of the \(\alpha\)-particle to the target nucleus. This condition was realized by arranging to detect \(\alpha\)-particles which had suffered very large deflexions, 90° and 135°, compared with the 30° deflexion experiments by which Chadwick in 1920 had confirmed directly the identity of nuclear charge and atomic number. In these new experiments it was found that the inverse square law of repulsion held accurately, for gold, for the fastest \(\alpha\)-particles available and that the gold nucleus behaved as a point charge of 79 \(e\) for distances of approach between \(3.2 \times 10^{-12}\) and \(10^{-11}\) cm.

Similar experiments with uranium, less accurate because of the difficulty of preparing and measuring the thin scattering film, showed that the charge on that part of the uranium nucleus which lay within about \(4 \times 10^{-12}\) cm of the centre was at least 90% of the whole nuclear charge. These results were in flat contradiction with the known energies of \(\alpha\)-particles emitted by uranium which, if they acquired their energy by falling through the nuclear electrostatic field, must have originated at distances of about \(7 \times 10^{-12}\) cm from the nuclear centre where they would necessarily, in view of the scattering experiments, be under the influence of a strong repulsive field and therefore incapable of prior stable existence. A marked discrepancy therefore existed between the nuclear size exhibited by the energy of emission of \(\alpha\)-particles from radioactive bodies (\(7 \times 10^{-12}\) cm) and the upper limit of \(3 \text{ to } 4 \times 10^{-12}\) cm for the heaviest nuclei as evidenced by the scattering experiments. The proposed solution, in conformity apparently with all of the arguments advanced in the Franklin Lecture, was to assume that neutral \(\alpha\)-satellites existed in circulation about a central core, these neutral satellites having no effect in the scattering experiments. These satellites were imagined to consist of \(\alpha\)-particles each with two neutralizing electrons, less tightly held than the two electrons within the \(\alpha\)-particle, but more tightly held than in the helium atom. These neutral satellites were imagined to be circulating in quantized orbits under attractive forces arising from their polarization in the electric and magnetic fields of the central nucleus. In the process of radioactive \(\alpha\)-decay these satellites were
imagined to lose their neutralizing electrons and to emerge as $\alpha$-particles with the quantized energy appropriate to the electrostatic energy of repulsion from their point of origin. A paper in 1927 developed this model quantitatively along lines similar to the Bohr atomic model. The known energies of the $\alpha$-particles emitted from 22 radioactive elements were compared with the predictions of the theory in terms of only one variable, associated with a quantum number $n$. A fair measure of agreement was obtained and although Rutherford was clearly aware that the large values of $n$ necessary to explain the data (varying from 14 to 25, with in some cases half integral values) might be expected to yield fortuitous agreements, he pointed out that the fourth power dependence upon $n$ provided a possible refutation of a purely fortuitous result.

Although this model of the nucleus was doomed to extinction within a year or so it is perhaps interesting to summarize its description. It was supposed to possess a main core of radius $10^{-13}$ cm surrounded by a shell of electrons and charged nuclei of small mass extending to about $1.5 \times 10^{-12}$ cm. From this distance to about $6 \times 10^{-12}$ cm, neutral satellites, helium nuclei with closely bound electrons, were supposed to exist in the case of natural radioactive elements. The possibility of existence of other neutral satellites 'of mass 2 or mass 3 or even of mass 1 neutrons' is also mentioned and the protons, ejected in artificial disintegration, are envisaged as possibly 'part of an electrically neutral combination'. I think that if Rutherford could then have been told the present viewpoint he would probably have cheerfully remarked 'How near and yet how far!'.

In the year following the publication of the paper which I have just reported, the explanation of the paradoxical situation of the $\alpha$-particle scattering and the energies of the $\alpha$-particle radioactive emission were resolved in terms of wave mechanical theory by Gamow (1928). Henceforward the $\alpha$-particles could be imagined confined within a potential well, of radius perhaps only $10^{-13}$ cm, beyond which distance a precise inverse square law of force would yield accurate Rutherford scattering for incident $\alpha$-particles of the highest available energies. Radioactive decay, by $\alpha$-emission, became a consequence of wave penetration through this barrier, analogously to the penetration of the disturbance into the second medium in the case of the total reflexion of light. Thus the repulsive nuclear potential, through which this $\alpha$-particle consequently fell, as evidenced by its ultimate emission energy, could naturally be far less than the total potential barrier which would oppose an incident particle. In the Discussion on the Structure of atomic nuclei at the Royal Society in 1929 Rutherford clearly accepts this explanation and in his references to the gold-uranium paradox he does not refer at all to the satellite model.

'It will be seen that this theory makes the radius of the uranium nucleus very small, about $7 \times 10^{-13}$ cm, and in this small nuclear volume 238 protons and 146 electrons have to be made room for. It sounds incredible, but may not be impossible'.

At that meeting the results of Aston's latest atomic mass measurements, made with his refined spectrometer, were also reviewed and with the new data then available upon the packing fraction (binding energies) of nuclei throughout the whole range of mass numbers, Rutherford was able again to speculate upon nuclear
structure. Again his views were dominated by the need to accept the α-particle as the basic structural element:

'...We have not only to account for a nucleus made of α-particles, but have also to make room for the additional electrons, and it is not easy to confine an electron in the same cage with an α-particle.'

The difficulty to which Rutherford here referred became very dominant and critical during the years that followed and were finally resolved only when Chadwick discovered the neutron in 1932. During the three years which elapsed until that date Rutherford and his co-workers were much engaged upon the precise determination of the energies of α-particles and γ-ray quanta. The precise correlation of such data, as envisaged earlier by Rutherford, was well established by these experiments but their interpretation made no use of specific nuclear models and they do not therefore concern us here.

I now pass to the year 1932 in which the discovery of the neutron provided the structural unit which is now the basis of all existing nuclear models. The occasion of that discovery was also the reason for my own good fortune in coming to Rutherford's notice and I hope that I may be forgiven if my account of that exciting year is unduly personal. But after all, as a friend reminded me, it was for that reason that I was invited to give this lecture.

The manner in which Chadwick established the existence of the neutron, by the investigation of the radiation emitted from beryllium under α-particle bombardment, will be well known to you and need not be repeated here. At the time when these experiments were prompting this conception I happened to call upon Feather in the Cavendish Laboratory and found him running an automatic cloud track chamber in order to photograph recoil tracks of nitrogen nuclei projected by neutrons. He told me of Chadwick's experiments and the reasons for the neutron hypothesis and also showed me some of the first photographs of the disintegration of nitrogen nuclei under neutron impact. Shortly after this Chadwick gave a preliminary account of his experiments to the Kapitza Club. Before this work the radiation from beryllium had been regarded as γ-radiation, but it was not possible to reconcile the very high quantum energy which such radiation would have to possess, in order to explain the energies of the recoil particles observed by Chadwick, with even the highest energy which could reasonably be regarded as available from the assumed process

\[ ^9\text{Be} + ^4\text{He} \rightarrow ^{13}\text{C} + \gamma. \]

On the other hand, Chadwick's results could very reasonably be reconciled with an assumed process

\[ ^9\text{Be} + ^4\text{He} \rightarrow ^{12}\text{C} + n. \]

Essentially then the choice was between a photon or a neutron interpretation for the radiation from beryllium. That the imagined particle must have a mass close to unity was clearly established by the relative magnitudes of the energies of recoil nuclei of different types (H, N, Ar, etc.) which Chadwick had found in his ionization chamber pulse measurements. The frequency of occurrence also of these recoil pulses favoured the neutron hypothesis and the whole argument seemed so
convincing that it is difficult now to imagine how any reasonable doubt could have persisted. However, old ideas tend to die hard, especially when such a revolutionary assumption needs to be made. At the Kapitza Club meeting it seemed to me that the study of electrons recoiling from the beryllium radiation would very clearly distinguish between photons and neutrons because of the large difference of the recoil energies to be anticipated on the two hypotheses. For neutrons the energy would be only a few keV, yielding electron tracks of 2 to 3 mm in length, whereas photons of appropriate energy would yield tracks of many meV in energy and many metres in length.

At this time I was working with C. T. R. Wilson, at the Solar Physics Laboratory at Cambridge, upon problems connected with the condensation of water upon gaseous ions. For this work we had investigated many of the factors which cause background in cloud-track photographs and had perfected a technique which enabled one to operate a chamber under very clean conditions. At the particular time to which I refer we were applying this technique to a determination of the energy expended per ion pair produced by low energy electrons. For this experiment photoelectron tracks produced by copper $K$ quanta were photographed under conditions in which the positive and negative ions were separated by an electric field before the expansion. The experimental conditions were such that these ions could be accurately counted in an almost complete absence of background. It seemed to me, therefore, that I was in an excellent position to look for electron recoils from the impact of neutrons, since these would be expected to lie close to the energy range that we were studying.

I therefore approached Chadwick in this connexion and was delighted when he immediately arranged for me to have the Po–Be source for overnight experiments while he and Feather were perhaps resting from their daytime labours. It soon, however, became clear that this experiment did not produce the desired result. Although the nitrogen recoil tracks and occasional disintegration events were often evident, no short tracks of the required type could be found. On the other hand, long tracks, such as would be expected on a photon hypothesis for the radiation, were observed although it was soon realized that these were probably due to natural $\gamma$-radiations from the polonium source. C. T. R. Wilson, who had been perhaps a little sorry to interrupt our other experiments, was by now so interested that he arrived one morning with all of his gold medals, including the Nobel Prize one, for use, instead of lead, as absorber for these $\gamma$-rays. He demurred, however, when I suggested that a further improvement would be possible if the embossed heads were hammered out flat. Occasionally short tracks, in about the required energy range, were superficially detectable but in every case these could be seen to be branch tracks ($\delta$-rays) from the fast electron tracks to which I have referred. Under conditions such as these, where shuttering of the radiation was not possible, long tracks had often only very sparse ionization, due to the falling off of supersaturation during the period between expansion and illumination. Careful experiments however established with certainty that every case of a ‘likely’ neutron–electron recoil track had its origin upon a fast electron track and no evidence of neutron–electron interaction could be obtained. If this account of this experiment is causing
you some amusement, may I remind you that the result was quite unexpected. Had the cross-section for neutron–electron collision been of the same order as that for neutron–nuclear collision, the small magnitude of the energy given to the electron, due to the large mass ratio, would still have permitted the high penetrability through matter which the neutron radiation was known to possess.

The reason that I have told you this rather personal story in such detail is that it formed my first personal contacts with Rutherford. Almost daily throughout this period I met Rutherford and had to report my continued failure to find these tracks. Andrade in his memorial lecture has referred to Rutherford’s ‘ruthless enthusiasm when on the critical warpath’. At first his reactions could only be described as devastatingly scathing. He took pains to ensure that I realized that it should be \(7 \times 2\) times easier to find these recoils than to find nuclear recoils in air. I countered this by carrying photographs of nitrogen recoils which, under my experimental conditions, looked rather like trunks of trees and also photographs of \(\beta\)-rays, with accompanying \(\delta\)-rays, along which every ion was detectable—often in nicely resolved multiples of two. At first these attempts to convince him were met with disbelieving grunts though these became less frequent with the passage of time. Chadwick’s confidence was unshaken by these results. He accepted them as evidence that the neutron–electron collision cross-section was very small and urged me to attempt to set an upper limit to its magnitude.

It was abundantly clear to me that Rutherford was completely convinced that the neutron which he had predicted had been discovered. The way of its discovery, by the straightforward application of the laws of conservation of momentum and energy to the collision processes in the explanation of the relative sizes of Chadwick’s pulses convinced him beyond all other considerations. He once also referred to the naturalness of the neutron concept in the overall description of isotopes and concluded one conversation with the remark ‘Well, they have to be there (the electron recoils); we have to have it’ (the neutron). Under these pressures, however, the true situation gradually emerged and the theoretical explanation of the small neutron–electron cross-section was realized. It would have saved me many painful hours if I could have obtained some comfort from theorists over this period but none had ever been forthcoming, despite my attempts to find it. At a colloquium in the Cavendish in April of 1932, when Chadwick, Feather and I reported all these experiments, I had my first experience of Rutherford in another mood. At the end of my contribution to this meeting, while showing a slide of a small electron track, with every droplet clearly accountable to normal tracks, he interjected with a loud ‘And what is that spot in the top right-hand corner?’ I had to admit that it must be contamination. He replied with an equal severity, ‘You will have to be more careful in future’. A very long time afterwards I overheard him tell this story with boisterous amusement. I had not realized until then that he had been teasing.

Shortly after Chadwick’s discovery of the neutron, Cockroft & Walton did their famous experiment upon the disintegration of lithium by artificially accelerated protons and Rutherford arranged for me to go to work in the Cavendish to obtain cloud track photographs of this process. From then until his death in 1937 I was

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engaged in work of this kind. During the later years Rutherford came to my room regularly, usually towards the end of the day, to see any recent photographs. He clearly derived intense pleasure from inspecting the latest disintegration photographs and above all from hypothesizing about new processes which might explain the observed tracks. With a stub of pencil he would rapidly calculate expected ranges—for example, of $^3\text{H}$ nuclei with the same momentum as protons of observed range—or other such quick applications of momentum and energy conservation and of the theory of loss of energy by charged particles. He exercised his grasp and easy facility in small calculations of this kind with exactly the same pleasure and assurance as are shown by a master chess player demonstrating combinations on a chess board. In these quick calculations his feeling for orders of magnitudes, cross-sections and statistical probabilities was a dominant feature. This facility had of course dominated his discovery of the nucleus from $\alpha$-particle scattering. There is a very appropriate story of an occasion when, playing golf, he struck the player in front with his golf ball. As that individual strode back to protest in anger about this breach of etiquette, Rutherford met him with ‘Now for heaven’s sake, be reasonable!’ But nuclear physics was really Rutherford’s all-absorbing pastime. He was utterly and completely absorbed, satisfied and happy in his work—this was, I think, his main characteristic. Coupled with this absorbing interest was the warmest possible personality. I now realize that he was also intensely interested in people and in their reactions. Often when inspecting cloud track pictures and making speculations he would refer to some strange feature, which in fact arose from some complication of ionic condensation, and suggest a rather wild interpretation. I think that he did this for the pure pleasure of hearing my simple explanations or testing my reactions. I began to realize this on one occasion when he told me that when sharing a ball with Aston, in the regular Sunday golfing foursome at Cambridge, he liked to put the ball into a bunker to hear Aston complain about having to extract it. Rutherford always seemed to listen intensely to what one said, often with his eyes cast down, a study in concentration. Then when he spoke it was with certainty and emphasis. His mind had been made up or else he would forcibly conclude that the time for decision was not yet. His pride in the Cavendish Laboratory and his interest in the welfare of his collaborators knew no bounds. Nothing which could help to add to the efficiency of that group was beneath his interest. On one occasion at a Sunday tea-party at Newnham Cottage, shortly after my marriage, he walked my wife to the bottom of the garden and explained to her that physicists’ wives must ensure that their husbands had no household tasks, so that they could be free to devote themselves wholly to their work. This story always goes down very well with members of my departmental staff but seems to be less appreciated by their wives.

The discovery of the neutron laid the foundation for the development of theories of nuclear structure. We have seen that early nuclear models, with protons and electrons as constituent particles, could be made tolerably consistent, in view of binding energy considerations, with integral nuclear masses on an $O = 16$ scale but there was a host of attendant difficulties. The spins and statistics of nuclei
generally disagreed with this model. Thus $^{14}\text{N}$, for example, with 14 protons and 7 electrons should not have an integral spin, and should not obey Bose statistics, but both of these conclusions were in direct contradiction with the experimental data.

The acceptance of neutrons and protons as the basic constituents of nuclei provided, of course, an admirably simple representation of isotopic constitution. Thus $^{14}\text{N} = 7$ protons and 7 neutrons, $^{15}\text{N} = 7$ protons and 8 neutrons, and so on over the whole range of elements. In many cases, such as that of $^{14}\text{N}$ quoted above, the correct nuclear spins and statistics were immediately predictable. The wave mechanical difficulties associated with confinement of electrons within nuclear dimensions were no longer relevant. The way seemed clear, perhaps, for a nuclear model which might be as successful for nuclei as the Bohr–Rutherford model had been for atoms.

The first model of this nature was the liquid droplet model in which nuclear stability is a consequence of balance between strong attractive forces exerted by the nucleons upon one another and electrostatic repulsive forces between the protons. This model was very successful in explaining the broad trends of much nuclear data. Thus the main features of the variation of the binding energies of nuclei with mass number were explained by Weizsacker (1935). Bohr’s introduction, in 1936, of the conception of the compound nucleus, in combination with this model, explained the strong resonances in slow neutron capture cross-sections and also the sharpness and close spacing of these resonance levels. An outstanding success of this model was the theoretical explanation of nuclear fission by Bohr & Wheeler (1939), soon after the discoveries of Hahn, Meitner & Frisch.

Although this model was very successful in describing the general trends of nuclear data it was clearly inadequate in one important respect. It had long been obvious that in a detailed consideration marked periodicities existed in the general trends of nuclear data, for example, binding energies and abundancies, with mass number. The situation was very similar to that which existed in respect of atomic properties where periodicities had long been recognized and represented in the Mendeleef classification. These periodicities were explained, with the development of the Bohr–Rutherford atomic model, by the application of the Pauli principle to the filling of successive levels by electrons. At the centenary of the birth of Mendeleef, in 1934, in a lecture to the Chemical Society Rutherford said, in reference to the periodicities of nuclear properties, ‘It may be that a Mendeleef of the future may address the Fellows of this Society on the “Natural Order of Atomic Nuclei” and history may repeat itself’.

These evidences of strong discontinuities in the variation of nuclear properties, already evident in 1934 (Guggenheimer, Elsasser), were reinforced in many directions by further study. The general trends of the numbers of stable isotopes and isotones, nucleon binding energies (especially the binding energy of the last ‘added’ neutron), energies of first excited states of nuclei, nuclear magnetic moments, nuclear reaction cross-sections and other nuclear data all showed marked discontinuities and led to the recognition of the ‘magic numbers’, 2, 8, 20, 28, 50, 82, 126 which were associated with the numbers of neutron or proton constituent particles in the nuclei at which these discontinuities occurred. (An example is
These data are in marked disagreement with the expected behaviour of a statistical assembly as envisaged in the liquid drop model and suggest, instead, some type of closed shell structure of neutrons and protons within nuclei. This was forthcoming in the shell model which was extensively developed by Mayer (1948) and Haxel, Jensen & Suess (1950). It is generally assumed in this model that each nucleon moves in an orbit under an average force, due to the other nucleons and acting towards their centre of mass. The various states of motion available to the orbital nucleon and the relative energies appropriate to these states were calculated and, with the introduction of spin-orbital forces in addition to the normal nuclear short range exchange forces, these states were classified and arranged as shown in figure 1. These data are in marked disagreement with the expected

shown in figure 2. The magic numbers, corresponding to closed shells, appear in this diagram whenever the differences of energy between adjacent levels is particularly large. When we remember the broad span of nuclear data which exhibit the magic numbers, the success of this theory is very impressive. Even more convincing is the fact that a host of nuclear data receives a systematic explanation in terms of this theory. To take one example, $^{11}$B has 5 protons and 6 neutrons. Thus the 6 neutrons will fill all the $1s$ and $1p_{3/2}$ states and the 5 protons all of the $1s$ states and all but one of the $1p_{3/2}$ states. The spin therefore of this nucleus would be $\frac{3}{2}$ and its parity negative. These predictions agree with the experimental facts. Similar predictions agree with experiment in a multitude of cases and constitute the clearest possible evidence of the validity of the theory.

I must not, however, give the impression that there are no difficulties in these conceptions, or that the shell model is the final answer to all problems of nuclear structure. For example, one would expect that for the nuclear states, envisaged in this theory, to be clearly defined the orbital nucleon would need to have free motion for many circuits about the remainder. All the experimental evidence which is explained by the liquid drop model suggests that the close packing of the nucleons in an atomic nucleus would prohibit such motion, free from disturbing collisions. Indeed the success of the conception of the compound nucleus in

\[ \text{Figure 1. Variation of the parabolic constant } b^1, \text{ derivable from } \beta\text{-decay data, as a function of mass number. The points where magic numbers should show their influence have been indicated. Dots mark values obtained from even mass, crosses those from odd mass nuclides. (From Wapstra, A. H. 1958 Handb. Phys. 38/1, p. 21.)} \]
explaining the close spacing of nuclear excitation levels, as revealed by the capture of slow neutrons, relied basically upon the conception that the entering nucleon shared its energy at once with the others and that the emergent particle resulted from a concentration of this energy from a statistical assembly of particles in which the entering particle had no distinguishable role. We have here a paradoxical situation and a prime difficulty in the construction of a wholly satisfactory

\[ V(r) \]

\[ r \approx 10^{-12} \text{ cm} \]

Figure 2. Energy levels in a nuclear potential well, allowing for spin-orbit coupling. The basic states, shown on the left, correspond to a potential \( V(r) \) of the form shown, which is intermediate in shape between an oscillator and a square well potential. The half integral numbers give the angular momenta \( j = l + \frac{1}{2} \) of the nucleons in the level. The number of sublevels contained in each level is then \( (2j + 1) \) and the whole numbers shown correspond to the total number of levels, or in other words the total number of nucleons required to fill all states of the nucleus, below the positions so indicated. (From Burcham, W. E. 1963 Nuclear physics, p. 371.)
nuclear model. One area of data requires the nucleons to behave as independent particles with mean free paths large compared with nuclear dimensions. Another area of data requires that the nucleons interact strongly with mean free paths short compared with nuclear dimensions. Each point of view enjoys considerable success and the task in recent years has been to reconcile these contradictory hypotheses. The application of the Pauli principle has been of great assistance in easing this situation. Collisions which would lead to states of motion which are already occupied by other nucleons are forbidden by this principle and the mean free path of the nucleon is therefore effectively increased, but the general conflict between these two hypotheses still dominates the evolution of nuclear model theory and has led, in its attempted reconciliation, to a wealth of particular models, each of which can enjoy success in the interpretation of different fields of nuclear data (figure 3). Of particular importance in these attempts at reconciliation is the development of collective models which envisage correlated motion of many particles and intermediate models which envisage small interacting clusters of nucleons, quasi α-particles, etc., which may have but transient existence. The above descriptions of the development of nuclear models has been unavoidably brief and I fear sketchy, but it was necessary to present it in order to be able to assess, in a modern perspective, Rutherford’s insistence that nuclei were basically composed of α-particles. It will be remembered that Rutherford’s reasons for this assertion arose from the known facts of α-particle emission by the natural radioelements, from general considerations of abundances of isotopes, from the relatively prolific emission of α-particles in processes of artificial transmutation and from the manner in which 80% or more of the binding energy of nuclei could be attributable

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**Figure 3.** Models of the nucleus. (From Segré, E. 1964 Nuclei and particles, p. 188.)
to the summation of the intrinsic binding energies of the presumed constituent \( \alpha \)-particles. Much more detailed later data of this general character, for example the binding energies of the last ‘added’ nucleon and other experiments to which I shall refer later, reinforce this view that the \( \alpha \)-particle cannot reasonably be ignored in the construction of a wholly satisfactory nuclear model. The great weight of evidence, to which I have referred, in support of neutrons and protons as the basic nuclear constituents prompts us to seek a reconciliation in terms of an \( \alpha \)-particle structural component, itself a consequence of a more natural and comprehensive \( n-p \) model. Even without the spin-orbital force requirement of the shell model such a consequence is naturally forthcoming by virtue of the Pauli principle. This principle requires that no two of the constituent nucleons within a nucleus can occupy precisely the same defined state. Now a nucleon possesses four internal degrees of freedom, it may have two values of charge (corresponding to neutron and proton) and two possible spin orientations (corresponding to spin up or down relative to some arbitrary direction). The four nucleons in \( ^4 \)He can therefore all be in the same spatial state but a fifth nucleon can be present only in a different state which a more detailed consideration in terms of exchange forces shows to be a state of lower binding energy. The non-existence of the nuclei \( ^4 \)H and \( ^4 \)Li is similarly explained. Such nuclei would contain a third neutron or a third proton respectively and these again could be present only in less tightly bound states. In this manner the four nucleons constituting the \( \alpha \)-particle can be shown to be a very tightly bound structure. The extension of considerations such as these has made it clear that much of the evidence which I have quoted in support of \( \alpha \)-particles as nuclear constituents may fairly be regarded as a natural consequence of the more comprehensive models. For example, it was fashionable, about ten years ago, to develop nuclear models in which \( \alpha \)-particles, moving under forces calculable from \( \alpha-\alpha \) scattering data, could have various types of vibrational or rotational excitation but without internal excitation of the \( \alpha \)-particle itself. Such ‘molecular’ type models could be shown to possess characteristic excitation levels in good agreement, in some cases, with experimental results. However, the shell model, even with no spin-orbit coupling, could similarly give the same differences of energy between levels. Such accidental resemblances between models must be regarded as an indication that both contain the same underlying symmetries. With \( N = Z \) and both even in light nuclei, for example, because of the great degeneracy of the states of one particle in an oscillator potential, any individual wavefunction is identical with a sum over the wavefunctions for clusters of two neutrons and two protons. The fact that in some states this sum can take a particularly simple form, so that a cluster wavefunction is a good approximation, can now be seen as a clue to the nature of the shell model states.

Rutherford’s conclusion that \( \alpha \)-particles were basic nuclear constituents finds, therefore, its present-day expression in the interest in cluster formation, in particular \( (2n2p) \) clustering, within nuclei which are primarily considered as aggregates of neutrons and protons. This interest is still very lively and I must refer briefly to more recent experimental work which bears upon this problem of clustering within nuclei. Wilkinson (1961) has reviewed the evidence from three
main lines of experimentation and concluded that 'α-particle' clustering is an important feature in nuclear structure. The energies and angular distributions of α-particles emitted by nuclei under bombardment by very high energy neutrons and protons are in good agreement with the assumption of a process of direct interaction. The results are very similar to those which would be expected if the ejected α-particles were already present as relatively free nuclear constituents. Such experiments suggest that a nucleon, in the nuclear surface, is for about one half of its time virtually a constituent of an α-particle.

Secondly, measurements of the variation of reaction cross-sections with bombarding energy and of the decay constants for α-emission from natural radioelements provide a parameter (reduced width) which may also be calculated theoretically in terms of the independent particle nuclear model. We may regard this reduced width as the probability of a nucleus becoming dissociated into two units (one of which may be an α-particle). The reduced width expresses this probability as a quantity from which all factors exterior to the nucleus (such as the nuclear potential barrier) have been eliminated. For example, from the known decay constant of a natural α-emitter, by correction for barrier penetration, the probability of pre-existence of the α-particle within the nucleus may be calculated. The observed reduced width obtained in this manner for 210Po and 212Po are some thousand times greater than the reduced widths calculated by means of the independent particle model. Similar discrepancies in the same sense exist between calculated reduced widths and those determined experimentally by other experiments and suggest that there is a strong predilection towards α-clustering of nucleons, at least within the surface regions of heavy nuclei.*

Thirdly, there is evidence of clustering from the absorption of K− mesons by nuclei. As these mesons, captured by nuclei, spiral down towards the nuclear surface they may be absorbed in some particular interaction process. Usually the capture process is accompanied by meson emission but one possible process, involving a final interaction with a close pair of nucleons, leads to capture without meson emission. It is found that this latter form of capture has a relatively high probability (~30%) suggesting strongly that there is much 'pairing' of nucleons in the nuclear surface. This would be the case if α-clusters were frequent at the surface and this conclusion is reinforced by the observation that the probability of non-mesonic K−-absorption by helium itself is quantitatively almost the same as for similar absorption by heavy nuclei.

To summarize, there is much evidence, both old and new, in favour of the conception of strong α-type clustering of nucleons within atomic nuclei. We may pause here to reassess the nature of the question whether α-particles exist, or not, within nuclei. The ‘reality’ or otherwise of a fundamental particle in physics is perhaps only a matter of convenience, applicability and familiarity. We do not hesitate to regard a neutron as a ‘real’ particle because the hypothesis of its existence is so

* Footnote added in proof, 13 February 1967. It was pointed out to me by E. W. Vogt following the lecture at U.B.C. Vancouver that improved penetrability calculations by Rassmussen (1959) and improved shell model calculations by Harada (1961) yield ‘measured’ and theoretical reduced widths which differ by only about one order of magnitude. See also Vogt (1966).
overwhelmingly convenient in the comprehensive association and interpretation of a multitude of experimental facts. Whether \( \alpha \)-particles 'exist' within nuclei or not depends then upon whether the calculations in terms of a universally accepted nuclear model predict clusters to such an extent that an assumption of explicit pre-existing \( \alpha \)-particles becomes unnecessary. We have seen that some lines of evidence suggest that we have not yet reached the point at which this may be affirmed. I think that Rutherford, were he alive, would quite justifiably cheerfully feel that he was right, so far as it mattered, whichever way this issue is ultimately settled.

This memorial lecture has been primarily concerned not with the great discoveries for which Rutherford's name will always be outstanding in scientific history but with his contributions to the conception of the structure of the atomic nucleus, a subject which, from a present standpoint, was in its infancy during his lifetime.

In view of the vast amount of both experimental and theoretical research which has been done upon this subject during the twenty-eight years which have elapsed since Rutherford died, it is surely very remarkable to find that many of his views are still fundamental to modern conceptions and still excite lively interest. But however great his contributions to science, Rutherford will, I think, always be remembered best, by those who knew him personally, for his boisterous and friendly personality and for his happy and total absorption in his own work and that of his collaborators.

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