

## RUTHERFORD MEMORIAL LECTURE, 1977

### Some episodes of the $\alpha$ -particle story, 1903–1977

BY N. FEATHER, F.R.S.

*Department of Physics, University of Edinburgh, James Clerk Maxwell  
Building, Mayfield Road, Edinburgh EH9 3JZ, U.K.*

*(Lecture delivered at McGill University, Montreal, Canada  
on 28 September 1977 – Typescript received 19 May 1977)*

For the fifth time, since the series was inaugurated in the University of New Zealand at Christchurch, a quarter of a century ago, in the place where Rutherford embarked on that amazing career in experimental research, relying only on his own instinct – and on the genius that, in the short space of five years, was to bring him a professorship in this great country on the other side of the world – the Royal Society Memorial Lecture comes to Canada. I deem it a great privilege to be chosen as your lecturer today. Although, almost to the day, forty years have now passed since Ernest Rutherford died, I can claim to have worked under his general direction, and in the end as his junior colleague, during the last eleven years of his life, with only two breaks of a year each in other appointments.

Indeed, I remember as if it were yesterday, my last meeting with him. He knew that he was not well, and he summoned me to his house and asked me to take his next lecture for him. We sat talking for a while over a cup of tea in his study: there was a cake on the tea-tray, but neither of us, for different reasons, had the appetite for it. Then he handed me his lecture notes, and walked with me down the paved path to the door in the garden wall. He paused, then shook me awkwardly by the hand. This was an unusual gesture; hand-shaking was not his way, with intimates. The garden door closed behind me, I put the lecture notes in my bicycle basket and returned to my office in the Department. An hour later he telephoned me to ask after an experiment in progress in the laboratory. I had nothing of importance to report. So my association with the great man ended – on a note of inconsequence. It was his last contact with the Cavendish: five days later he was dead. The odd lecture that I had been asked to give stretched out for a further seven years – and if during those years I had any success in keeping alive the tradition of the master, possibly the reason is to be found, in some measure, in the circumstance in which his mantle as lecturer fell on my shoulders.

You must forgive me this personal preamble. Its serious purpose is to establish the background from which I approach today's assignment. If you accept that background, you will not be surprised when I confess that any memorial lecture that I give cannot be other than Rutherford-centred. The time will come, in fact

it has come already, when the Rutherford lecturer will have had no direct contact with the man in whose memory he is speaking: then he will be at liberty to choose his topic without constraint. That is not the case with me. In private duty bound – as a well-worn Cambridge formula runs – I must choose a Rutherfordian theme. Twelve years ago, Philip Dee, my classmate in the Cavendish of 1926, lecturing here in Canada, was similarly motivated. His theme was built around Rutherford's speculations concerning the rôle of the  $\alpha$ -particle as a constituent particle of atomic nuclei in general.<sup>1</sup> That is one aspect of the  $\alpha$ -particle story: today I wish to recall another.

Possibly it was through some misunderstanding of the nature of his achievement that Rutherford was awarded the Nobel Prize for chemistry rather than for physics in 1908, but the title which he chose for his prize lecture 'The chemical nature of the  $\alpha$ -particles from radioactive substances', while deftly accepting the compliment that was intended, turned the interest firmly back into the more familiar world of physics. Already in 1908 ten years had passed since Rutherford had himself brought the  $\alpha$ -particle into that world (little realizing, at the time, what in fact he had done); already during those years he had come to appropriate it as singularly his own creature, and for the rest of his life it remained for him his most reliable, most intimate, contact with the new world that he was creating. It was, of course, as any philosopher of science will insist, a conceptual world, but Rutherford's concepts followed the route from his fingers to his brain: they lodged in his bones. He knew in his bones that his  $\alpha$ -particles would not betray him.

'In private duty bound,' I have said. On a much lower plane than his, my own interests have found continuity linked by a similar thread. My first substantial research (with Robert Nimmo, in the Cavendish) concerned the long-range  $\alpha$ -particles of the active deposits of radium and thorium: now, fifty years later, I and my younger colleagues are still intrigued – and tantalized – by the problem of the long-range  $\alpha$ -particles of fission. However, those topics belong to later episodes in my story: let me waste no more time on preliminaries – let me start at the beginning.

The beginning was at McGill, in the MacDonald Physics Laboratories, in the winter of 1902–3. It was there that the  $\alpha$ -radiation of 1898 finally assumed the character of a stream of 'charged bodies projected with great velocity'.<sup>2</sup> It was there that the  $\alpha$ -particle was born. In my view the relevant experiment, carried out with a tin-can electroscope and a miniscule array of metal plates at one-millimetre spacing, the intervening gaps half-closed by a superimposed grid, is one of the most amazing experiments in the whole history of science.<sup>3</sup> It must surely be an inspiration, if a somewhat chastening experience, for the young physicist of today to examine Rutherford's original apparatus in the McGill museum.

Historians of science, presenting the success story, have passed over without notice an interesting fact recently brought to our attention by Dr Trenn.<sup>4</sup> During the autumn of 1902 Rutherford had steadily been developing the view that the  $\alpha$ -radiation must be particulate in nature. The phenomenon of the deposition of

'excited radioactivity' on solid surfaces exposed to emanation appeared to involve the recoil of positively charged atoms arising from the emission of  $\alpha$ -radiation from atoms of the emanation. This could be understood if the radioactive emission process itself were to involve the expulsion of a particle of atomic mass – an  $\alpha$ -particle – provided that the particle was negatively charged. It was with real surprise, therefore, that Rutherford found a positive charge when his deflexion experiments established beyond peradventure that the  $\alpha$ -radiation was indeed particulate, as he was coming to believe.

To what extent this partial failure of his expectations troubled Rutherford we shall never know: with hindsight, however, we can appreciate the sureness of his instinct in disregarding it completely at the time – no mention is made of it in 'Radioactive Change' of May 1903. In that remarkable paper<sup>5</sup>  $\alpha$ -radioactivity consists in the emission of positively charged  $\alpha$ -particles from individual atoms, just simply that and nothing more. If there was a skeleton in the cupboard, Rutherford kept the cupboard door securely closed for ten years or more, until the complexity of the recoil process could be understood in terms of the nuclear atom model as developed by Bohr.

My next episode, like the first, is centred in McGill. In 1903 it had been necessary to employ an array of twenty channels, with an electroscope as detector, to derive significant information concerning the magnetic deflexion of  $\alpha$ -particles. Three years later source strengths had been increased sufficiently for a single slit to be substituted, and for photographic detection to be possible. Even so, the apparatus was on a small scale: Rutherford's 'photographic plates' were in general little more than one centimetre square. It was with such equipment that, in 1906, he investigated the loss of velocity of  $\alpha$ -particles traversing thin sheets of mica.<sup>6</sup> Deflexions were minute, and in one series of exposures Rutherford sought to record the two groups of particles – those slowed down by the mica, and those coming directly from the source, simultaneously – by covering only half of the slit by the mica sheet. He noticed that the traces on the photographic plate (they were, of course, shadow images of the slit) were different in appearance, those produced by the particles that had passed through the mica were slightly more diffuse than the others. He therefore made exposures with the magnetic field switched off, so that the two traces appeared one above the other in the undeflected position. The difference was now undeniable. Rutherford was not entirely surprised by this result; he had previously obtained rather fuzzy traces when exposures had been made with a certain amount of air in the apparatus<sup>7</sup> and he had interpreted the effect as due to 'scattering' of the  $\alpha$ -particles in the air. But the circumstances of his new observation immediately brought home to him the full implication of his observation. Using the most intense magnetic field at his disposal he had at last been able to deflect an  $\alpha$ -particle through two or three degrees over a path of a few centimetres in vacuum. Here a deflexion of the same magnitude was occurring naturally along a path of three thousandths of a centimetre in mica. For him it was a matter of simple arithmetic: he wrote '[this] would require over that distance an

average transverse electric field of about 100 million volts per cm', and he concluded 'Such a result brings out clearly the fact that the atoms of matter must be the seat of very intense electrical forces.'<sup>6</sup> This was not a skeleton to be locked in a cupboard; it was a live issue to be kept in the forefront of the mind for future exploration. I must not pursue the matter farther: in 1906 Rutherford had taken the first step on the road to the discovery of the nucleus – but you know the rest of that story.

I had the privilege of going through Rutherford's collection of papers after his death. Among them I found some slips of photographic plate belonging to the series I have just been describing. I have had these in my possession ever since. I think they should now return home to McGill. With the agreement of Rutherford's grandchildren, I propose to leave them where they rightly belong.

At this stage I cannot omit reference to an early Manchester episode (Rutherford moved there in 1907) though I have recounted it in detail in other places. When he left McGill, Rutherford had a reasonably accurate value for  $e/m$  for the  $\alpha$ -particle: effectively the same value whatever the source of the particles investigated.<sup>8</sup> He had only to measure the charge in order to know the mass. He could envisage three possibilities: that the  $\alpha$ -particle (I quote) 'is (1) a *molecule* of hydrogen carrying the ionic charge of hydrogen; (2) a helium atom carrying *twice* the ionic charge of hydrogen; or (3) *one-half* of the helium atom carrying a single ionic charge'.<sup>8</sup>

In the next year, in collaboration with Geiger, he determined the  $\alpha$ -particle charge.<sup>9</sup> It was  $9.3 \times 10^{-10}$  e.s.u. At that time direct determinations of the ionic charge were quoted as giving values in the range  $3.0 \times 10^{-10}$  to  $3.4 \times 10^{-10}$  e.s.u. Could it be, as the crude results appeared to suggest, that the  $\alpha$ -particle was triply charged? Rutherford would have none of it. He knew in his bones (he had known it, as he might say, with increasing conviction for the past five years) that  $\alpha$ -particles were charged helium atoms. They must be doubly charged. Thus the fundamental unit of charge must be  $4.65 \times 10^{-10}$  e.s.u., or thereabouts, whatever the earlier experimenters had said.

In 1909 the British Association came to Canada for its annual meeting. J. J. Thomson was the Association's President and Rutherford was President of Section A. His presidential address, delivered in Winnipeg, was devoted largely to a discussion of 'the various methods that have been devised to determine the values of certain fundamental atomic magnitudes', as he said in his introduction.<sup>10</sup> Naturally, the value of the fundamental unit of charge was high on his list for attention. It was a masterly survey. The printed account reveals only the modesty of the speaker: 'I trust that my judgment is not prejudiced by the fact that I have taken some share in these investigations. . .', and again, 'It is difficult to fix on one determination as deserving of more confidence than another; but I may be pardoned if I place some reliance on the radioactive method previously discussed. . .'. If only the lecture had been recorded on video-tape, we might today be able to hear the tell-tale inflexion in that sonorous voice, and catch the glint in the President's eye.

So Rutherford returned to Manchester, and to the discovery of the nucleus.

Within four years the  $\alpha$ -particle was not merely a doubly charged atom of helium, it was the bare nucleus of that atom, a single minute entity, positively charged, unencumbered by attendant electrons so long as it was moving rapidly enough through matter. How small, then, was the  $\alpha$ -particle – how small, indeed, were the nuclei of atoms other than helium – and had it, possibly, a shape different from the spherical? Rutherford's scattering law, which the experiments of Geiger and Marsden had so extensively verified in relation to  $\alpha$ -particle scattering in thin foils of gold, had been based on the coulombian force-law and the point-charge approximation. Rutherford was no follower of Bosovich, as Faraday, in his time, was inclined to be: he expected to find evidence for the failure of his point-charge model in collisions of closer approach, if only these could be studied. In Manchester, as the First World War was drawing to its close, he found that evidence in experiments on the scattering of  $\alpha$ -particles in hydrogen.<sup>11</sup> Obviously, his observations would have to be extended before any significant conclusions could be drawn, but he saw no reason why such observations should not eventually find explanation in purely electrostatic terms, if the  $\alpha$ -particle and the hydrogen nucleus were assigned finite size, and the distribution of charge on one, or both, of them was other than spherically symmetrical.

Soon after the war was over, Rutherford succeeded to the Cavendish Chair. Almost continuously over the next eight years there were experiments in progress in the laboratory on  $\alpha$ -particle scattering. He was not personally involved in all of them, but the overall strategy was obviously his alone. To begin with he set Chadwick & Bieler to extend his Manchester experiments with hydrogen.<sup>12</sup> Then Bieler was left on his own to investigate scattering in aluminium and magnesium.<sup>13</sup> Rutherford & Chadwick took over this investigation in due course and extended it to cover the scattering in gold and uranium.<sup>14</sup> Finally, in 1927, continuing their collaboration, they investigated the scattering of  $\alpha$ -particles in helium.<sup>15</sup> I wish, at this stage, to refer only to the first and last of these studies.

Having accumulated much more detailed information concerning the scattering of  $\alpha$ -particles in hydrogen than Rutherford had obtained two years previously, Chadwick & Bieler concluded that their observations could not be explained on the basis of purely electrostatic forces as Rutherford had hoped, even though finite size were attributed to the charge distributions. Forces of shorter range and of a different origin had to be invoked. The model they suggested, assuming the hydrogen nucleus to be small and spherical, involved an  $\alpha$ -particle which had the form (I quote) of 'an oblate spheroid of semi-axes about  $8 \times 10^{-13}$  cm and  $4 \times 10^{-13}$  cm respectively, moving in the direction of its minor axis'.<sup>12</sup> Electrostatic forces would determine the distribution of charge over the spheroid; its elasticity represented the short-range forces effective in the closest collisions.

It is a little surprising, in retrospect, that, with this result achieved, Rutherford did not immediately switch the investigation to the scattering in helium. Six years later, when that study was at last undertaken, he stated what must have been self-evident from the beginning: 'In this case both particles concerned in the collision

have the same structure. There is therefore no need to assume a structure for one nucleus in order to deduce that of the other.<sup>15</sup> Broadly, the experimental results were consonant with those obtained in hydrogen; a marked departure from the predictions of the classical law based on an inverse-square-law repulsive field, increasing rapidly as the energy of the  $\alpha$ -particle was increased beyond 5 MeV. Rutherford & Chadwick concluded: 'it does not seem possible to explain the collisions with hydrogen or helium nuclei without some additional assumption about the non-spherical "shape" of the  $\alpha$ -particle'.<sup>15</sup> This conclusion, as I have said, was reached in 1927.

During the next two years the whole language of the subject was changing. The ideas of de Broglie and Schrödinger had been found to be fundamentally relevant to the physicist's world, theorists began treating the phenomena of particle scattering on the basis of the new mechanics, and Gamow, and Condon and Gurney, independently gave the first inherently reasonable account of the process of  $\alpha$ -disintegration on the same basis. About the same time Heisenberg formulated the uncertainty principle, and this fundamental result, all other considerations apart, immediately raised the question how far the classical impact-parameter approach to the scattering problem could be justified. It had been the basis of Rutherford's calculations of  $\alpha$ -particle scattering by the heaviest elements in 1911, and the experiments of Geiger & Marsden in the following year had verified the predicted angular distribution over a range which involved a factor of  $10^5$  in the intensity observed.<sup>16</sup> In 1920 Chadwick had identified the nuclear charge number of the medium-light element copper with the atomic number of that element, with near 1% accuracy, on the basis of an absolute determination of the probability of  $\alpha$ -particle scattering – again using the classically derived scattering law.<sup>17</sup> There was no conflict whatsoever between theory and experiment: thus it appeared quite paradoxical to suggest that in some fundamental way the classical theory was misconceived. Yet this is precisely what the adherents of the new mechanics were compelled to assert. The concept of the classically determinate path of the particle during the scattering process was altogether foreign to the new approach.

In point of fact, by 1929, the paradox had been resolved, at least at the level of experiment. Wentzel<sup>18</sup> and Mott<sup>19</sup> had already shown that, uniquely for scattering in an inverse-square-law field, the final result (the expression for the differential cross-section for scattering) is precisely the same whether classical mechanics or wave mechanics is used in the calculation.

My object in introducing this episode of the  $\alpha$ -particle story is to pose a question. How did Rutherford himself react, at the end of quarter of a century knowing the  $\alpha$ -particle, his own peculiar creature, as an honest-to-goodness bit of primordial matter, structured no doubt, but secure in its individuality – how did he react to this fundamental revolution in the theorist's way of describing its behaviour?

I am not sure that I can give a comprehensive answer. There could be no doubt that with the aid of the  $\alpha$ -particle he had discovered the 'real' nucleus. The theoretical ground from which that discovery sprang was now believed to have been

insecure, his success the result of an accident of circumstance. Was it a pure accident, though; was it conceivable that the structure of the universe is such that fundamentally the only long-range interaction capable of persisting is of an inverse-square-law character as seen within our framework of space and time, and that this constraint is mirrored in another, so that any reasonable being who is daily in contact with that universe at the macroscopic level, inevitably fashions his theories of the micro-world to give prominence to this fact? There is no evidence to show that Rutherford ever asked himself such questions.

On 7 February 1929 he took the chair at a discussion meeting at the Royal Society in London, the day's topic being 'The Structure of Atomic Nuclei'.<sup>20</sup> In his opening address he welcomed Gamow as one of the invited participants, and referred very briefly to his application of the wave mechanics to the problem of  $\alpha$ -disintegration. His only comment, in anticipation of Gamow's contribution, was 'It will be seen that this theory makes the radius of the uranium nucleus very small, about  $7 \times 10^{-13}$  cm. . . It sounds incredible but may not be impossible.' The reflex of disbelief, followed by the gesture of tolerance, is strongly reminiscent of the last sentence in the paper of February 1914 in which Rutherford reviewed the situation as it then was, regarding the structure of the atom as a whole.<sup>21</sup> You may remember that he wrote: 'While there may be much difference of opinion as to the validity and of the underlying physical meaning of the assumptions made by Bohr, there can be no doubt that the theories of Bohr are of great interest and importance to all physicists. . .'. Fifteen years had not changed Rutherford's attitude to the theorist. 'I have, however, such a strong belief in the ingenuity of our theoretical friends', he said in another context in his 1929 opening address 'that I am confident that they will surmount this difficulty in some way. . .'.<sup>20</sup>

I have, perhaps, dwelt too long on this particular episode. In conclusion, I think it may be said that Rutherford soon came to accept the wave-mechanical description of the process of  $\alpha$ -disintegration as providing an acceptable vehicle for his own thoughts on the matter; in respect of the phenomena of  $\alpha$ -particle scattering, probably, the situation was otherwise. For some years after 1929 he continued to be involved in experiments on rare modes of  $\alpha$ -disintegration, but 1927 saw his last paper on  $\alpha$ -particle scattering. Indeed, between 1927 and his death ten years later only one experimental paper on  $\alpha$ -particle scattering was published from the Cavendish. Brief reference must be made to that paper now.

The paper in question was published by Chadwick in 1930<sup>22</sup>. It reported a re-investigation of the scattering in helium, with particular attention to the more distant collisions, those of  $\alpha$ -particles of the lowest attainable energy. Earlier that year Mott had discussed the special case in which projectile and target nuclei in a scattering experiment are identical particles, and had examined theoretically the interference effects which a wave treatment must predict, and for which there is no counterpart in a classical mechanical treatment of the problem.<sup>23</sup> In this connexion we may say loosely that a collision between two nuclei of the same mass and charge is necessarily a collision between 'completely identical' particles only if the nuclei

are devoid of any axis of rotational symmetry; otherwise, for example if the nuclei possess intrinsic spin, there are various possible aspects at collision in some of which the particles appear 'more identical' than in others. Chadwick's experiments fully confirmed the predictions which Mott's calculations made in respect of 'completely identical' particles according to our loose classification. There could be no other conclusion than that the  $\alpha$ -particle was spinless and that its force field was spherically symmetrical in the undisturbed state. The model of the spheroidal  $\alpha$ -particle had been consigned, once for all, to the limbo of ancient conceits.

I have said that Rutherford continued his life-long involvement in problems of  $\alpha$ -disintegration, for some years beyond 1929, in experiments on certain disintegration modes of relatively small yield. If I make this the next episode of my story, I must go back in time to discover its origins. In fact, it is a long-drawn-out episode, covering a span of seventeen years, and it has a tortuous history. It shows Rutherford for once bemused (and for a while misled through his own enthusiasm) by the 'ingenuity' of one of his 'theoretical friends'. However, he soon recovered his characteristic scepticism, and he won through in the end. It is the episode of the long-range  $\alpha$ -particles; it began in 1916, and it was not brought to a final conclusion until 1933.

In February 1916 Rutherford sent forward for publication in the *Philosophical Magazine* a short paper in joint authorship with A. B. Wood.<sup>24</sup> It described an unfinished investigation which 'neither of the authors is likely to have time to continue. . . in the near future'. The opening sentences of that paper were positively worded: 'In the course of an examination of a strong source of the active deposit of thorium by the scintillation method, one of us observed the presence of a small number of bright scintillations. . . [which] were undoubtedly due to  $\alpha$ -particles. . . of greater velocity than any previously observed.' A later sentence in the same paragraph made it abundantly clear that the 'one of us' mentioned was indeed Rutherford himself, though it gives no clue to the reasons why he had been examining a thorium preparation at the time. He was not using such sources for any other purpose just then; in fact it was nearly three years, according to the evidence of his published papers, since he had made use of one. Be that as it may, in 1916 Rutherford and Wood were quite convinced that they had discovered a new mode of  $\alpha$ -disintegration arising in the active deposit of thorium, of unusually high energy and very small yield – about 1 in 10 000 in relation to the previously known modes. For Rutherford, who ever had the eye for the unconsidered trifle in any experiment, it was an intriguing result.

World War I dragged on, and, as I have already said, towards its end Rutherford began to spend some time in his old laboratory pursuits, assisted only by William Kay, the steward. I have dealt with the experiments on the scattering in hydrogen; in other experiments Rutherford examined the effects which could be observed in helium, carbon dioxide, nitrogen and oxygen. In all these experiments,  $\alpha$ -particle sources of radium active deposit were employed. The results were presented in four papers dated from Manchester in April 1919 and published under a single title in



June of that year. The first two dealt with the hydrogen results;<sup>11</sup> the third and fourth were subtitled, respectively, 'Nitrogen and oxygen atoms'<sup>25</sup> and 'An anomalous effect in nitrogen'<sup>26</sup> The fourth paper has become a classic; the third, on the other hand, has been forgotten. I wish to direct your attention to it now.

Again, I quote Rutherford's opening sentence: 'Bohr has worked out a general theory of the absorption of electrified atoms in passing through matter, and has verified his conclusions by consideration of the absorption of  $\alpha$ -particles.' For Rutherford that was an unusual opening: never before in any paper of the Manchester period had he made it clear at the outset that basic to what he was going to say was the acceptance of a particular theoretical result. And in this case, when he did so, he showed himself in the outcome (and in retrospect) to have been bemused. Bohr's theory<sup>27</sup> had worked very well in relation to the observations in hydrogen: the hydrogen nuclei projected in the forwards direction in collision with  $\alpha$ -particles had four times the residual range of the  $\alpha$ -particles projecting them, as the theory predicted. Rutherford looked for similar success in applying the theory to collisions in other light elements, such as nitrogen and oxygen, but the situation was very different in the two cases. The title of Bohr's papers referred to the absorption of 'swiftly moving electrified particles' – meaning electrons and bare nuclei, such as protons and  $\alpha$ -particles – not of 'electrified atoms', as Rutherford misconstrued it.<sup>25</sup> The theory had nothing to say regarding the range of an energetic recoil atom, a nucleus carrying with it at least some of its extranuclear electrons. Rutherford, however, used the theory to calculate the forwards ranges of singly charged recoil atoms of the light elements, and on this basis predicted ranges for such atoms of nitrogen and oxygen as some 33% and 12% greater, respectively, than the residual range of the responsible  $\alpha$ -particle, and he set himself to look for the effect which he had predicted. Sure enough, with his radium active deposit source, the  $\alpha$ -particles of which have an air-range of 7 cm, he found a few bright scintillations which were not suppressed until 9 cm air-equivalent absorption had been inserted. With complete objectivity Rutherford recorded other observations which might have given pause to an uncommitted observer, but his experiments in oxygen and nitrogen had been the 'cleanest' of the series, and he did not allow himself to be dissuaded from his adopted stance. However, his earlier confidence regarding the interpretation of the observations with thorium active deposit was now undermined. He wrote: 'In [those] experiments, the  $\alpha$ -rays . . . were absorbed in mica . . . This . . . raises the question whether these long range  $\alpha$ -particles are not in reality due to collisions of  $\alpha$ -particles with the oxygen atoms in the mica.'<sup>25</sup>

It was not long, however, before he had to revise his views again. He had devised a crude method of examining the magnetic deflexion of particles of small yield, and by June of the following year (1920) he had proved to his satisfaction that the particles of 9 cm range could not be singly charged atoms of oxygen or nitrogen, after all; rather they were doubly charged particles of more nearly the  $\alpha$ -particle's mass. It is clear, in retrospect, that Rutherford did not fully appreciate the difficulties of interpretation inherent in his experimental approach, since he was not

content with that qualitative conclusion (which would have been unassailable) but proceeded to commit himself to the view that the long-range particles were of mass number 3, rather than 4, and that they were produced in the gas in disintegration collisions of greater intrinsic probability than those which gave rise to the disintegration protons which he had identified the previous year.<sup>28</sup>

So it was that, when in the autumn he returned to the problem of the long-range particles from thorium active deposit, the question he was asking was not whether these had been recoil atoms of oxygen arising in the mica absorbers, but whether, either from the source itself, or in a secondary process in the absorbers, there were produced particles of  ${}^3\text{He}$  of long range and small yield.

At last he was able to do a really clean experiment. He had acquired a fresh supply of radiothorium which enabled him to use sources 400 times as strong as before. He could dispense with absorbers, and carry out magnetic experiments in vacuum. Again he had A. B. Wood as collaborator. Between them they showed conclusively that the long-range particles came from the source, and that, if they were doubly charged, then they were of mass number 4, not 3.<sup>29</sup> Indeed, they were real  $\alpha$ -particles as Rutherford has asserted when first he observed their scintillations four years previously. Here, I might add in parenthesis that on the basis of the experimental evidence – the measured range and the magnitude of the magnetic deflexion – it could have been maintained, with almost equal conviction, that the particles were  ${}^3\text{H}$ . Happily, this possibility, if it was ever recognized, was not allowed to confuse the issue yet again.

So, what was now the position regarding the less-abundant long-range particles observed with sources of radium active deposit? Were they, in spite of all the observations previously made, real  $\alpha$ -particles originating in the source material, also? Before long, Rutherford was beginning to think that they must be. In the course of a lecture to the Chemical Society in the spring of 1922 he said: ‘While a large amount of experiment will be required to fix definitely the nature of the radiation, the general evidence indicates that it consists of particles of mass 4, which are projected from the source. . . .’<sup>30</sup>

Some two years later, in collaboration with Chadwick, Rutherford found time to engage in the ‘large amount of experiment’ to which he had referred – and when it was all published in the *Philosophical Magazine* of September 1924<sup>31</sup> no one could seriously doubt the final conclusion: rare modes of  $\alpha$ -disintegration had been set in evidence in the active deposits of radium and thorium, presumably involving very short-lived bodies, having regard to the long ranges of the  $\alpha$ -particles. As we have seen, it took Rutherford eight years to win through to this conclusion: when he had done so, from one point of view, his problem had only just been identified – that is the problem of understanding the facts as he had finally established them. You will remember that I said that the episode of the long-range  $\alpha$ -particles lasted for seventeen years: I have covered only the first eight of them!

Obviously, I must be brief concerning the remaining nine – the years from 1924 to 1933. During that period there were three separate investigations mounted in

Cambridge: two yielding a spectrum of ranges,<sup>32</sup> and the last a magnetic spectrum, by semi-circular focusing, with the purpose-built 'mushroom' magnet designed by Cockcroft.<sup>33</sup> When, in collaboration with Lewis and Bowden, Rutherford published the results of that last investigation in 1933, they had identified twelve separate long-range  $\alpha$ -particle groups, of combined intensity no more than 30 per million normal disintegrations, with radium active deposit – and, more importantly, they knew how to interpret this spectrum: it was essentially the spectrum of the energy levels of the radium C' nucleus as populated in the  $\beta$ -disintegration of radium C. The  $\alpha$ -particles of long range were emitted from these excited nuclei in competition with the generally more probable process of  $\gamma$ -ray emission. It had been a long haul, but the account was closed at last.

So I come to my last episode: it concerns the long-range  $\alpha$ -particles of fission. It is, in fact, the longest enduring of the episodes I have identified; it began under cover of secrecy during the last war, and it is still in progress.

Let me digress for a moment on the fission phenomenon itself. As you all know, Rutherford did not live to read of the experiments of Hahn and Strassmann and Meitner and Frisch which early in 1939 set all the world of physics agog. But he was familiar with the liquid-drop model nucleus of v. Weizsäcker and Bohr (strictly, he was present at the birth, at the Royal Society discussion meeting of 1929 to which I have already referred, and the original progenitor was, in fact, Gamow, as reference to the published account<sup>20</sup> will confirm). He found it a comfortable classical model, and he would have been well pleased had he known that it provided such a satisfactory basis for the initial understanding of the fission process. Here was a new phenomenon of great interest, and, for its consideration, a ready-made classical model congenial to his habit of mind; he would have been in his element. He would not have needed to be reminded, as Bohr and Wheeler reminded their readers,<sup>34</sup> that when an unstable jet of liquid breaks up into individual drops it is frequently the case that a much smaller drop is left behind in the space between neighbouring drops of normal size. The 'splash' photographs of Worthington<sup>35</sup> would have been clear in his mind's eye.

In 1939 Bohr and Wheeler put forward this fact of classical physics as providing one possible model-explanation of the emission of secondary neutrons in fission. In that connexion it did not survive for long. The other mechanism which they suggested for consideration proved to be more consonant with experiment: that the secondary neutrons are evaporated from the accelerated fission fragments as the nuclear temperature rises through the collapse of the initial deformation. However, the hydrodynamical analogy was not thereby rendered wholly irrelevant. It came into its own once the long-range  $\alpha$ -particles were discovered.

These particles were first observed by Alvarez, in 1943, when he was working for the Manhattan Project.<sup>36</sup> Over the next ten years their main characteristics were established: one  $\alpha$ -particle in some 400 fission events, or thereabouts; a broad spectrum of energies peaked at about 16 MeV; an angular distribution heavily peaked just to the light fragment side of the plane at right-angles to the fission axis.

The conclusion was inescapable: these particles must originate in the space between the separating fragments, possibly at the very moment of scission, their initial velocities being quite small, and their final energies and directions being determined by the joint action of the coulomb fields of the fragment nuclei in the early stages of their separation. If the liquid-drop model was admissible in respect of the primary process, these indeed were the droplets which, within the full scope of the analogy, the model demanded. In fact we now know, as a result of detailed experiments, that the fission droplets range in complexity from single protons to oxygen nuclei of mass number 20. However, I am concerned today with the  $\alpha$ -particle story, and, in any case,  $\alpha$ -particles comprise about 90 % of the long-range particles of fission.

There were great expectations, once the general picture of long-range  $\alpha$ -particle emission had been accepted, that close study of the effect would provide a key to the understanding of the scission process itself. That expectation has yet to be realized: a thoroughgoing dynamical theory of fission has not yet been written. At the macroscopic level, although Rayleigh faithfully recorded the presence of the secondary droplets in his illustrations of the breakdown and collision of water jets, his successful theory of jet instability took no account of them.<sup>37</sup> In the fission situation it is the case that the  $\alpha$ -particles are released predominantly in those events in which the filament of nuclear matter joining the nascent fragments becomes more than normally elongated before rupture occurs. That is about all we can say. Halpern has expressed our present ignorance more colloquially: 'ternary fission is just binary fission that happened to get over-enthusiastic at the moment of scission'.<sup>38</sup> Some day, perhaps, we shall be able to describe in less anthropomorphic terms how that sudden burst of enthusiasm develops.

So I conclude my episodic account of more than seventy years of  $\alpha$ -particle physics. I hope I have not wearied you. Much, of course, has been omitted: other episodes might have been chosen of comparable interest, and a better balance have been achieved by including more from the post-war era. In the upshot my discourse has become even more Rutherford-centred than I expected it to be. I offer no excuses; I make no claim that the development of the subject over the last forty years would have been different had Rutherford lived. I only know that as long as he lived he would have continued to take a fatherly interest in his  $\alpha$ -particles, in the long-range  $\alpha$ -particles of fission, and in all those  $\alpha$ -emitters among the transuranic elements whose discovery and systematization resulted in the addition of the names of McMillan and Seaborg to the list of Nobel Laureates in Chemistry along with his own. In the spring of 1903, at the very beginning of the period covered by my survey, Rutherford and Soddy wrote: 'If elements heavier than uranium exist it is probable that they will be radioactive. The extreme delicacy of radioactivity as a means of chemical analysis would enable such elements to be recognized even if present in infinitesimal quantity.'<sup>5</sup>

Truly, there were certain things that Rutherford and Soddy had known in their bones from the beginning.

## REFERENCES

- <sup>1</sup> Dee, P. I. 1967 *Proc. R. Soc. Lond. A* **298**, 103–122.
- <sup>2</sup> Rutherford, E. 1903 *Phil. Mag.* **5**, 95–117.
- <sup>3</sup> Rutherford, E. 1903 *Phil. Mag.* **5**, 177–187.
- <sup>4</sup> Trenn, T. J. 1971 Ph.D. thesis (Wisconsin).  
1975 *Historical studies in the Physical sciences*, vol. 6, pp. 513–547.
- <sup>5</sup> Rutherford, E. & Soddy, F. 1903 *Phil. Mag.* **5**, 576–591.
- <sup>6</sup> Rutherford, E. 1906 *Phil. Mag.* **12**, 134–146.
- <sup>7</sup> Rutherford, E. 1906 *Phil. Mag.* **11**, 166–176.
- <sup>8</sup> Rutherford, E. 1906 *Phil. Mag.* **12**, 348–371.
- <sup>9</sup> Rutherford, E. & Geiger, H. 1908 *Proc. R. Soc. Lond. A* **81**, 162–173.
- <sup>10</sup> Rutherford, E. 1909 Report Brit. Assn. Winnipeg, pp. 373–385.
- <sup>11</sup> Rutherford, Sir Ernest 1919 *Phil. Mag.* **37**, 537–561, 562–571.
- <sup>12</sup> Chadwick, J. & Bieler, E. S. 1921 *Phil. Mag.* **42**, 923–940.
- <sup>13</sup> Bieler, E. S. 1924 *Proc. R. Soc. Lond. A* **105**, 434–450.
- <sup>14</sup> Rutherford, Sir Ernest & Chadwick, J. 1925 *Phil. Mag.* **50**, 889–913.
- <sup>15</sup> Rutherford, Sir Ernest & Chadwick, J. 1927 *Phil. Mag.* **4**, 605–620.
- <sup>16</sup> Geiger, H. & Marsden, E. 1912 *Sitz. Akad. Wiss. Wien IIa*, **121**, 2361–2390.
- <sup>17</sup> Chadwick, J. 1920 *Phil. Mag.* **40**, 734–746.
- <sup>18</sup> Wentzel, G. 1926 *Z. Phys.* **40**, 590–593.
- <sup>19</sup> Mott, N. F. 1928 *Proc. R. Soc. Lond. A* **118**, 542–549.
- <sup>20</sup> Rutherford, Sir Ernest (Discussion opened by) 1929 *Proc. R. Soc. Lond. A* **123**, 373–390.
- <sup>21</sup> Rutherford, Sir Ernest 1914 *Phil. Mag.* **27**, 488–498.
- <sup>22</sup> Chadwick, J. 1930 *Proc. R. Soc. Lond. A* **128**, 114–122.
- <sup>23</sup> Mott, N. F. 1930 *Proc. R. Soc. Lond. A* **126**, 259–267.
- <sup>24</sup> Rutherford, Sir Ernest & Wood, A. B. 1916 *Phil. Mag.* **31**, 379–386.
- <sup>25</sup> Rutherford, Sir Ernest 1919 *Phil. Mag.* **37**, 571–580.
- <sup>26</sup> Rutherford, Sir Ernest 1919 *Phil. Mag.* **37**, 581–587.
- <sup>27</sup> Bohr, N. 1913 *Phil. Mag.* **25**, 10–31.  
1915 *Phil. Mag.* **30**, 581–612.
- <sup>28</sup> Rutherford, Sir Ernest 1920 *Proc. R. Soc. Lond. A* **97**, 374–400.
- <sup>29</sup> Rutherford, Sir Ernest 1921 *Phil. Mag.* **41**, 570–574.  
Wood, A. B. 1921 *Phil. Mag.* **41**, 575–584.
- <sup>30</sup> Rutherford, Sir Ernest 1922 *Nature, Lond.* 584–586, 614–617.
- <sup>31</sup> Rutherford, Sir Ernest & Chadwick, J. 1924 *Phil. Mag.* **48**, 509–526.
- <sup>32</sup> Nimmo, R. R. & Feather, N. 1929 *Proc. R. Soc. Lond. A* **122**, 668–687.  
Rutherford (Lord), Ward, F. A. B. & Lewis, W. B. 1931 *Proc. R. Soc. Lond. A* **131**, 684–703.
- <sup>33</sup> Rutherford (Lord), Lewis, W. B. & Bowden, B. V. 1933 *Proc. R. Soc. Lond. A* **142**, 347–361.
- <sup>34</sup> Bohr, N. & Wheeler, J. A. 1939 *Phys. Rev.* **56**, 426–450.
- <sup>35</sup> Worthington, A. M. & Cole, R. S. 1897 *Phil. Trans. R. Soc. Lond. A* **189**, 137–148.  
1900 *Phil. Trans. R. Soc. Lond. A* **194**, 175–199.
- <sup>36</sup> Farwell, G., Segre, E. & Wiegand, C. 1947 *Phys. Rev.* **71**, 327–330.
- <sup>37</sup> Rayleigh (Lord) 1879 *Proc. R. Soc. Lond.* **29**, 71–97.  
1882 *Proc. R. Soc. Lond.* **34**, 130–145.
- <sup>38</sup> Halpern, I. 1971 *Ann. Rev. Nucl. Sci.* **21**, 245–294.