THE COLLECTED PAPERS OF LORD RUTHERFORD OF NELSON

Volume Two: Manchester

Ernest Rutherford

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Volume 16

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ERNEST RUTHERFORD Edited by JAMES CHADWICK



First published in 1963

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THE COLLECTED PAPERS OF LORD RUTHERFORD OF NELSON

O. M., F. R. S.

PUBLISHED UNDER THE SCIENTIFIC DIRECTION OF SIR JAMES CHADWICK, F.R.S.

> VOLUME TWO MANCHESTER

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FOREWORD

A **PROPOSAL** to publish the scientific work of the late Lord Rutherford was discussed shortly after the war, but the acute shortage of paper at that time, and, as a corollary, the heavy commitments of those publishers who were interested in the matter, caused it to be abandoned. This earlier proposal was for the publication of a selection of Rutherford's most important papers.

The present venture was, from the outset, conceived on different lines. It was proposed to include every scientific paper which Rutherford had published either alone or with collaborators; and also a number of general articles, formal public lectures, letters to editors, and other communications which seemed worthy of preservation.

This comprehensive and, indeed, ambitious scheme was brought to my attention in the autumn of 1956 by Dr Paul Rosbaud. I readily agreed to give it my full support and to act as scientific editor.

This publication of Rutherford's Collected Works will consist of four volumes, of which the first three will contain the papers published in the usual way in scientific journals.

The first volume includes his work in New Zealand, at the Cavendish Laboratory and in Montreal, covering the years from 1894 to April 1907; the second volume will contain the papers of the Manchester period, 1907 to 1919; and the third volume will cover his period as Cavendish Professor from 1919 to 1937. The fourth volume will include miscellaneous articles, public lectures, letters to editors and, in addition, some obituary notices of Rutherford. It will also include a bibliography which, it is hoped, will be complete.

Each of the four volumes will contain accounts of personal recollections and appreciations by some of his friends and colleagues and also portraits and photographs of historical interest.

Some of Rutherford's papers were published at about the same time in German or in French as well as in English journals. When the two versions are identical, the English version has generally been chosen for publication here. There are, however, occasions when the German or French version contains additional material, and in these cases that version has been adopted.

The primary purpose in this publication of Rutherford's Collected Works is, of course, to set up a visible memorial to one of the greatest figures in the history of science; and, at the same time, to make it readily possible for the succeeding generations of young scientists to see what he did, to follow the development of his ideas, and to get at first hand some idea of the magnitude of his contribution to our knowledge of the physical world. No reader of these volumes can fail to be impressed by the vigour and directness of Rutherford's mind, or fail to become aware that the pursuit of scientific truth was to him an activity of the highest intensity, and also a very personal activity.

It has been the wish and endeavour of all concerned with this project that

these volumes should be produced at the lowest price consistent with good printing and reproduction. In furtherance of this aim, the publishers, Messrs George Allen & Unwin, have been most generously helped by large grants towards the very substantial cost of a four-volume publication and the work of preparation and revision which it entailed. It is with deep appreciation and gratitude that such aid is acknowledged from the Government of New Zealand, the National Research Council of Canada and the Leverhulme Trust.

The copyright of Lord Rutherford's publications is held by his grandchildren, Dr Peter Fowler, Mrs Elizabeth Rutherford Taylor, Mr Patrick Fowler and Dr Ruth Edwards. We are indebted to them for permission to publish.

Acknowledgments for Volume Two

As stated in the Foreword, this second volume of the Collected Papers contains the scientific papers published by Rutherford from Manchester, where he was Langworthy Professor of Physics from October 1907 to October 1919.

These papers are introduced by a general survey of his work during that period under the title 'Rutherford at Manchester: an epoch in Physics', contributed by Professor N. Feather; and I record here my gratitude to him for this article, in which he sets Rutherford's work against the background of that time.

Also included are some reminiscences of Rutherford and his Manchester laboratory by the late Professor H. Geiger, by Professor E. N. da C. Andrade, and by Dr A. B. Wood. I am grateful for permission to publish these contributions.

It is a pleasure to give thanks to Professor Otto Hahn for the loan of the photograph of Rutherford and Geiger, to Professor Sir Nevill Mott and the Cavendish Laboratory for the reproductions from Rutherford's notes, and to Dr A. B. Wood for the photographs of the laboratory group at Manchester in 1913, and of the Admiralty Physics Board, the latter being reproduced by courtesy of the Editor of the *Journal of the Royal Naval Scientific Service*.

In my acknowledgments at the end of the Foreword to Volume One I referred to my debt to Dr Paul Rosbaud—'but for whose initiative and sustained interest this publication would not have been undertaken'. I record with sorrow that Dr Rosbaud died in January 1963. He had carried out most of the work required to get this second volume ready for publication, but he did not live to see it in a complete state. I repeat with emphasis what I have said above; and I add, in justice to his memory, that any merit which uhis publication of the Collected Papers may have is almost entirely due to him.

J. CHADWICK

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PROFESSOR ERNEST RUTHERFORD 1909

Rutherford at Manchester: an epoch in physics

by N. FEATHER, F.R.S.

In the summer of 1907, Rutherford moved from Montreal to Manchester. Twelve years previously he had arrived in Cambridge, a raw student from an outpost of empire; twelve years later he was to return there, as Cavendish professor. The first three years which he spent in Cambridge (1895–98) were the years of his maturing; the last three or four years in Manchester were lean years—they were years of war. Thereafter, as head of the most famous physical laboratory in the world of those days, he was to lose, gradually but inevitably, the opportunity for personal participation in experimental research. The summer of 1907, therefore, provides a natural climacteric: Rutherford's personal achievements during the preceding nine years of his Montreal professorship may be compared on equal terms with his achievements in Manchester—over the first nine years of his tenure of the Langworthy chair, until the privations of war brought fundamental research to a stop in the universities of Europe. On this basis we set the contents of this volume of the *Collected Papers* against the background of Volume I.

But there is one observation, before we embark on our survey. To the papers included in these volumes must be added the books. In his sixth year at Montreal, Rutherford sent the manuscript of his book *Radio-activity* to the Cambridge University Press; at the beginning of his sixth year at Manchester he wrote the preface to *Radioactive Substances and their Radiations* and despatched the completed work to the same publisher. These two substantial monographs remain major classics to this day. Before he left Montreal, Rutherford had already revised the first of them for a new edition—and he had seen his Silliman Memorial Lectures at Yale (1905) published in an impressive volume (*Radioactive Transformations*) to fulfil the conditions of the lectureship—but here we concentrate on the originals, the Cambridge books of 1904 and 1913, whose titles have already been quoted.

In 1903 the theory of spontaneous disintegration had first been formulated explicitly. In 1911 the large-angle scattering of α -particles was 'explained', for the first time satisfactorily, on the basis of the nuclear model of the atom: in that context the atom nucleus was discovered. Universally, after fifty years, these two achievements are regarded as Rutherford's outstanding contributions to physics in Montreal and Manchester, respectively. From this point of time, then, it is interesting to look back to the books, written when these particular contributions were new—and their ideas, to many, strange—in the hope of assessing Rutherford's own estimate of their significance and value.

The reader who makes this enquiry for the first time is likely to be

surprised by the outcome. In the preface to Radio-activity, written in February 1904, he will find the statement, simple and direct, 'The interpretation of the results has, to a large extent, been based on the disintegration theory . . .'---and in the 382 pages of text which follow he will find this claim amply substantiated: the entire account is informed by the new ideas which Rutherford and Soddy had given to the world, whole and irrefutable, in the paper 'Radioactive Change' in May of the previous year. In the preface to the other book, by contrast, he will look in vain for any reference either to experiments on α -particle scattering, or to the nuclear atom. He will find a paragraph which begins, 'It is of interest to signalise some of the main directions of advance since the publication of the second edition of my Radio-activity'. But he will not find any mention of these topics there. Yet this preface was written in October 1912. In the 670 pages of following text, the paper of May 1911, 'The Scattering of α and β Particles by Matter and the Structure of the Atom', is quoted only three times, and the space devoted to the problem of large-angle scattering is no more than three pages in all. The atom nucleus had been discovered in a particular context, but the new concept had not been assimilated into physics generally; radioactivity had not been recognized explicitly as a property of the nucleus ('the transformation theory advanced in explanation of radio-active phenomena has undergone no essential modification', p. vi), even the α -particle is still a doubly-charged atom of helium rather than a bare helium nucleus ('it does not seem possible that the α particle can retain any of its constituent electrons in escaping from the radio-active atom', p. 620). To some extent, no doubt, it was merely the obtuseness of words and the difficulty of a new vocabulary, to some extent it was Rutherford's natural caution which imposed restraint, but, whatever may have been the reason, the fact is clear: the discovery of May 1911 was accorded no central place in the book of 1913 as the hypothesis of May 1903 had been, in the book of 1904. Perhaps throughout 1911 and the following years the process of re-orientation was unaccountably slow, but let an unbiased reader look through the papers included in this volume, and follow the references, and he will discover that, slow or not, the advance from the first experimental observation of Geiger and Marsden in 1909 to the atom model of Bohr, exhibited in its full potentiality six years later, was a product of Rutherford's inspiration almost exclusively. Independent contributions of others were trivial in comparison; the whole episode was focused in Manchester.

Let us, then, look at the collected papers in more detail, surveying the whole period, and bearing in mind the comparison to which we are pledged. From Montreal Rutherford published seventy papers, twenty-five in collaboration with colleagues; here, in this Manchester volume, there are seventy-two reprinted, thirty-two of joint authorship. The prodigious output of the earlier period was thus maintained in the later; it is only surprising that the proportion of joint papers was so little increased. For at Manchester Rutherford was the administrative head of a big department, with responsibilities for teaching as well as for research; at Montreal, as Macdonald professor, he had been spared such routine duties to a large extent. But ceaseless enquiry of nature was his way of life, and in the full power and authority of his pre-eminent position he was not to be denied the exercise of his genius. At the end of a long day he could say, simply, to a young colleague, 'Robinson, I'm sorry for those fellows who haven't got laboratories to work in'—and the remark came from the heart, and rang true. In the light of it we need enquire no further how it was that the output of publication was maintained.

It was a diverse output, all the time, with interweaving threads of enquiry continually in evidence, but we may see in it, also, the steady development of a planned attack: at first a preponderance of work on the α -emitters, on the nature of the α -particle itself and on the chemical and radioactive properties of radium and radium emanation; then a detailed study of the β - and γ -emitters, involving the first precision spectrometry of these radiations; overlapping both in time, the investigation of α -particle scattering, revealing the nucleus in 1911, and, eight years later, providing the first faint hint of artificial disintegration—'the anomalous effect in nitrogen', as Rutherford described it. For the historian, the nucleus and artificial nuclear transmutation provide the highlights of the period (we have already given pride of place to the former), but the greatness of Rutherford's genius touches all these investigations and we shall do well to consider each in turn.

Broadly, the work on the nature of the α -particle comes first in time—and its origins lay in Montreal rather than Manchester. In November 1902 Rutherford and Soddy had written ('The Cause and Nature of Radioactivity', Part II): 'In light of these results . . . the speculation naturally arises whether the presence of helium in minerals and its invariable association with uranium and thorium may not be connected with their radioactivity'. Then, in August of the following year, Ramsay and Soddy announced the discovery of the presence of helium in the gases obtained from radium bromide, and of its production by radium emanation. Rutherford was in Britain, on holiday at Bettws-y-Coed, at the time. As soon as his copy of Nature reached him, he sat down and made an order-of-magnitude calculation. A few months previously the rate of evolution of heat by radium had been determined by Curie and Laborde, and he himself had 'shown that the α or easily absorbed rays from radium consist of a stream of positively charged bodies, of mass about twice that of the hydrogen atom'. Now he assumed that the heating effect was a measure of the dissipation of the kinetic energy of the α -particles in the radioactive material, and that 'the α bodies after expulsion can exist in the gaseous form': on this basis he calculated the rate of evolution of gas. 'The determination of the mass of the α body, taken in conjunction with the experiments on the production of helium by the emanation, supports the view that the α particle is in reality helium', he wrote (Volume I, p. 610). This was on August 15th, two days after the communication of Ramsay and Soddy had been published in London. Rutherford's reaction had indeed been swift: within a matter of hours there had crystallized in his mind a precise hypothesis which it took him six years to bring to conclusive test. But, as events were to prove, his intuition was unerring.

The conclusive test belongs to the Manchester period; at Montreal Rutherford could only pave the way for the future. There he was content to establish that the α -particles from all radioactive substances are identical. differing only in velocity of emission. Otto Hahn was his collaborator in some of this work. In July 1906, on a visit to Berkeley, in the intervals of lecturing to an advanced class in the summer session of the University of California, he found time to write up the details in two papers for the Philosophical Magazine. Herein was described the work that he had done over the past year, alone and with Hahn, on the magnetic and electric deflection of the α -particles from radium and its products, and from the active deposits of thorium and actinium. Technically, it was a remarkable achievement-and the result left little room for doubt. Within fairly narrow limits of experimental uncertainty, the α -particle was indeed characterized by the same value of e/m whatever its origin. In the longer of the two papers there was a section headed: 'Connexion of the α particle with the helium atom'. In this section Rutherford wrote (Volume I, p. 896): 'The value of e/m for the α particle may be explained on the assumptions that the α particle 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'. He dismissed the first explanation as being against the evidence; of the other two he preferred (2) for its 'simplicity and probability', but he retained an open mind, admitting the plausibility of (3).

Assumptions (2) and (3) differed primarily in respect of the charge assigned to the α -particle. At Manchester, with Geiger's help, Rutherford determined this charge directly. He had already made a careful examination of the conditions necessary for the successful determination of the total rate of transfer of charge by the α -particles emitted from a strong source of radium, in 1905 in Montreal (Volume I, p. 816); now he had merely to refine the experimental procedure and to develop a successful method of determining the rate of emission of α -particles. The papers describing these classic experiments are included in this volume (pp. 89, 121), and need not be referred to in detail here. Suffice to say that the chosen method of α -particle counting was the electrical method (the first success of which owed everything to Geiger's persistence and experience)-and to point out that before the investigation was completed Regener's simpler scintillation method was also tested and, to Rutherford's initial surprise, was found to be equally trustworthy. The details of the experiments can be read and appreciated at leisure; our concern is only with the final result-and with Rutherford's interpretation of it. The result, expressed in his own words, was as follows: 'the positive charge E carried by an α -particle from radium C is 9.3×10^{-10} E.S. units'; the final conclusion: 'that an α -particle is a helium atom, or, to

be more precise, the α -particle, after it has lost its positive charge, is a helium atom'.

Obviously, this is the favoured assumption (2) of the paper of October 1906, justified only if the charge on the α -particle is twice 'the ionic charge of hydrogen'. But, as Rutherford himself admitted, the ionic charge of hydrogen, as originally determined by Townsend, by Thomson and by H. A. Wilson lay between 3.0×10^{-10} and 3.4×10^{-10} e.s.u. How, then, could the charge on the α -particle possibly be two, rather than three, units of charge? True, a preliminary report of a new determination by Millikan and Begeman had pointed to a value of 4.06×10^{-10} e.s.u. for the ionic unit. But Rutherford was never unduly swayed by preliminary reports of unfinished experiments: he relied rather on his own intuition. For five years his intuition had told him that the α -particle was indeed a charged atom of helium, and, as independent evidence, he put forward an ingenious argument (though it was clumsily presented in the paper) to show that the number of ionic charges on the α -particle could be deduced from measurements of the heating effect, the electrostatic deflection of the α -particles, the magnitude of the faraday, the atomic weight of radium and the rate of growth of radium from ionium. This roundabout calculation, gave nothing more decisive than $2 \cdot 2$ ionic charges, with the uncertain data available, but honour was thereby satisfied. The question of probable error was by-passed: the α -particle was a doubly charged atom of helium, as it had to be!

Once this conclusion was accepted, 'the value of e, the charge on a hydrogen atom, becomes 4.65×10^{-10} ', so Rutherford wrote. Then, the intriguing aside: 'It is of interest to note that Planck deduced a value of $e = 4.69 \times 10^{-10}$ E.S. unit from a general optical theory of the natural temperature radiation'. Now, Planck had utilized the values of Stefan's and Wien's constants, of the universal gas constant, the velocity of light and the faraday, to deduce this result—and in 1908 only a minority of physicists regarded his 'general optical theory' with approval. It is the more intriguing to find Rutherford among these early adherents—and for no other reason than that he knew all along that the α -particle is a doubly charged atom of helium!

On December 11, 1908, Rutherford, having received a Nobel Prize, as the citation said, for 'researches on the disintegration of the elements and the chemistry of radioactive matters', delivered his prize lecture before the Royal Academy of Science at Stockholm. Clearly content, for a day, to be dubbed a chemist, he entitled the lecture, 'The chemical nature of the α -particles from radioactive substances' (*this volume*, p. 137). The chance was too good to be missed, the situation being what it was. Introducing his subject, he said 'during the last six years there has been a persistent attack on this great problem, which has finally yielded to the assault when the resources of the attack seemed almost exhausted'. That was his sober estimate of the state of affairs after the publication of his two papers with Geiger: 'the resources of attack almost exhausted'—in spite of the con-

viction with which his conclusions had been expressed at the time. α -particles, of mass about four units, and helium, from radium, certainly, but 'It might be argued, for example, that the helium atom* appeared as a result of the disintegration of the radium atom in the same way as the atom of the emanation and had no direct connection with the α -particle'. But, happily, the problem had 'finally yielded to the assault', and Rutherford was able to tell his audience of the success of the experiment which, with Royds, he had completed only four weeks previously. The formal paper describing this work appeared in the Philosophical Magazine in February of the following year (this volume, p. 163). This was the conclusive test of which we wrote earlier: Mr Baumbach, the departmental glassblower, had succeeded 'after some trials' in blowing very thin-walled glass tubes which proved completely impervious to helium gas under the conditions of the experiment but which were thin enough to allow the α -particles to pass through. In this way the α -particles were separated physically from the emanation in which they originated: so separated, they were shown to build up a sample of helium capable of identification spectroscopically in a capillary discharge. 'The long and arduous path trodden by the experimenter', as Rutherford described it, rather uncharacteristically, in the opening sentences of his Stockholm lecture, had at last come to its appointed end.

The long path had come to its end, or nearly so-for in the years that followed there were the researches of Rutherford and Boltwood on the production of helium by radium (this volume, p. 221) and of Rutherford and Robinson on the heating effect (this volume, p. 312) and on the value of e/mfor the α -particles of radium emanation and of its short-lived products (this volume, p. 383). Even Geiger's electrical method of α -particle counting, though it was to be abandoned for twenty years in favour of the scintillation method, received further attention in its own right (this volume, p. 288). The electrical method had served its purpose, admirably, in the researches of 1908, and the rough values of e/m, and of the rates of production of helium and heat, then available, had likewise been adequate for the use to which they were put, but the future might require more accurate values (indeed, the remaining sceptics had to be converted, come what may), and it was not Rutherford's intention to leave a job half-done. The conclusive proof of the identity of the α -particle and the doubly-charged atom of helium was completed in 1908, but the consolidation of the secondary evidence in the case continued for another five years, providing challenging problems for the younger members of the Manchester group-and ultimate satisfaction for Rutherford himself, as numerical precision was improved, and the doubts of the most inveterate critic were removed.

The small-angle scattering of α -particles in passing through thin sheets of mica was first observed by Rutherford, in Montreal, in 1906. This was in an experiment in which the magnetic deflection was being studied, and at one

* The printed text has ' α -particle', but this is an obvious error undetected in proof-reading.

stage Rutherford covered half of the defining slit with a sheet of mica less than 20 microns thick. The edges of the photographic trace formed by the particles were thereby blurred, indicating a change of direction of some two degrees for a large fraction of the particles in traversing the mica sheet. When the magnetic field was applied, the overall deflection of the particles which passed through the unobstructed portion of the slit was not much greater than this, although the particles travelled several centimetres in the field. Rutherford was not slow to appreciate the significance of the conjunction of these two simple, almost qualitative, observations. To produce the observed deflection in a distance equal to the thickness of the mica it 'would require . . . an average transverse electric field of about 100 million volts per cm.' he wrote (Volume I, p. 867), 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'. Although he regarded this result as self-evident, once 'the electronic theory of matter' had been accepted, Rutherford also realized, from this single observation, that he possessed in the α -particle a natural probe well suited to the exploration of fields of force of atomic dimensions. It is not, then, surprising that one of the entries in his first list of 'Researches possible', which he drew up in the summer of 1907, on his arrival in Manchester, was 'Scattering of α -rays'. Opportunity offered when he and Geiger had completed their experiments on the rate of emission of α -particles from radium, and had verified the claim that Regener had made for the scintillation method of observation: Geiger at once applied this new method to an investigation of the distribution of intensity in angle in the α -particles emerging from very thin foils of metal on which they had impinged as a well-defined pencil. Preliminary results were published in August 1908, and a full account of the investigation followed in April 1910.

By and large, there was nothing unexpected in the results described in these papers: 'the electronic theory of matter', in the form of the Thomson atom model, appeared adequate to encompass them, if the overall (smallangle) deflection of the α -particle were regarded as the resultant of the individual (even smaller) changes of direction suffered by the particle as it traversed successive atoms in its path. Only in one respect was this description manifestly artificial: it required the volume distribution of positive charge in the Thomson atom to be 'transparent' in relation to the particle traversing it.

But there was one very real skeleton in the cupboard. In the paper of April 1910, Geiger wrote: 'It is also of interest to refer to experiments made by E. Marsden and myself on the diffuse reflection of the α -particles. . . It does not appear profitable at present to discuss the assumption which might be made to account for [the results of these experiments]'. These results had already been published in July of the previous year. They were entirely anomalous. The statistical theory of 'multiple' scattering seemed to be verified so far as more than 99.9 per cent of the incident particles were concerned, but for the small residual fraction—one particle in ten thousand,

perhaps—the theory was wildly inadequate. The particles which Geiger and Marsden had found 'diffusely reflected' should not, according to the theory, have been there at all.

Rutherford had deliberately assigned this search for possible diffuse reflection of α -particles to Marsden, in the spring of 1909, ostensibly as a training exercise, under Geiger. What prompted him to do so will remain unknown. In later years he confessed, openly, to his complete amazement at the positive result of the search: 'It was quite the most incredible event that has ever happened to me in my life'—but one must believe that irrational as well as rational elements enter into the make-up of the experimenter of genius, and in this instance, at least, Rutherford was asking a simple question of nature, outside the realm of the rational, not knowing how pertinent a question it was.

It was more than a year before Rutherford was able to formulate a satisfactory explanation of Marsden's results. Intuitively, it seems, he came close to the beginnings of such an explanation at a very early stage, but for some reason which we do not know the clue was lost for a while, and in April 1910 Geiger found it unprofitable to discuss the problem further. At Winnipeg, in the late summer of the previous year, in his presidential address to Section A of the British Association, Rutherford had almost seized on the idea of 'single' as distinct from 'multiple' scattering in order to explain the anomalous results: 'otherwise it would be impossible to change the direction of the particle in passing over such a minute distance as the diameter of a molecule', he said. But he made no further reference to the problem in public during the closing months of that year, and throughout 1910 he was equally reticent. Then, on March 7, 1911, he made a preliminary announcement at a meeting of the Manchester Literary and Philosophical Society (this volume, p. 212), and a month or so later he sent off a full-length paper for publication in the *Philosophical Magazine*. This paper, 'The scattering of α and β Particles by Matter and the Structure of the Atom' (this volume, p. 238) marks the discovery of the nucleus.

There is some reason to suppose that the order of treatment in this classical paper—first large-angle, single, scattering, then multiple (or 'compound') scattering—is the reverse of the order in which Rutherford explored the properties of his nuclear model by detailed calculation: he would naturally realize that any new model must at least do what the old Thomson model could do. It may even be that it was the apparent difficulty of assimilating the two aspects of the phenomenon in a single explanatory scheme which impeded progress and delayed for so long any public discussion of the issue at stake. It was not in Rutherford's character to play into the hands of others—even his colleagues—by the premature announcement of a halfbaked scheme. However this may be, it is certain that in the published paper the sections on multiple scattering are the least convincing. There is the implicit claim, nowhere quite explicit, that the evidence from small-angle scattering in thick foils itself favours the nuclear model as against the Thomson model. Twenty years later (*Radiations from Radioactive Substances*, 1930, p. 209), any such claim was specifically disowned: 'The experiments on multiple scattering have indeed led to no definite conclusions about atomic structure . . . the best that can be done is to show that the results are in accord with the nuclear theory'.

The paper of May 1911 ends with a section of 'General Considerations'. Here is an open-minded survey in which Rutherford admits that an atom model involving several massive scattering centres might be as satisfactory as a single-centre model, except that the total charge on the scattering centres would have to increase as their number increased—or a model with a negatively charged central mass be as satisfactory as one with a positively charged centre. It was obvious that the model which had been explored mathematically was merely the simplest possible model: the next step was to submit its predictions to the test of experiment.

We have said that the paper of May 1911 marks the discovery of the nucleus. In fact, the word 'nucleus' is not used in the paper. In Rutherford's published writings on the subject it occurs first in the short account of the scattering problem which is given in *Radioactive Substances and their Radiations*, written probably in the following year. Here (p. 184) we find the statement: 'This indicates that the atom must contain a highly concentrated charged nucleus. . . .', but in the next sentence the writer slips back into his previous usage, 'central charge'; thereafter the word 'nucleus' does not recur throughout the rest of the volume.

At Manchester, Geiger and Marsden were already well advanced with experiments designed to provide a thorough-going test of Rutherford's scattering formula when the paper of May 1911 was published. Indeed, Geiger had been able to present some preliminary results which were decidedly favourable towards the nuclear model, when Rutherford first presented it in public at the meeting of the Literary and Philosophical Society on March 7th. For nearly two years these experiments continued, then in April 1913 a full account of them appeared in the Philosophical Magazine. Within the experimental limitations of the time, the test was exhaustive; within those limits the predictions of the theory were verified in every particular. Nowhere else in the world had any other physicist taken up the challenge which the scattering formula presented: only in Manchester, therefore, in these two years, had it been possible for anyone to sense the growing conviction that the bold hypothesis of 1911 was destined to become the central tenet of belief of the physicist of the future. Those around Rutherford had this experience; in particular, Niels Bohr shared it during the four months which he spent in the laboratory in the spring and early summer of 1912.

Bohr returned to Copenhagen towards the end of July. On March 6, 1913, he sent Rutherford the first draft of 'the first chapter of my paper on the constitution of atoms', adding, 'I hope that the next chapters shall follow in a few weeks.' After an exchange of letters, Rutherford agreed to send the first chapter, duly amended, for immediate publication. It appeared in the *Philosophical Magazine* of July 1913. The first sentence of the published paper clearly identifies the source of its inspiration, the fifth reflects its author's enthusiasm for the task he had undertaken: 'In order to explain the results of experiments on scattering of α rays by matter Prof. Rutherford has given a theory of the structure of atoms. . . . Great interest is to be attributed to this atom-model . . .'.

Rutherford received the other chapters in due course. They were published serially, in September and November 1913, in March 1914 and September 1915, respectively. We have already briefly assessed this series of papers by Bohr; here it is unnecessary further to apostrophize the theoretical genius of the young Dane who first attempted 'to show that the application of [Planck's] ideas to Rutherford's atom-model affords a basis for a theory of the constitution of atoms'. From that time onwards, the nuclear model of the atom has commonly, and with justice, been referred to as the model of Rutherford and Bohr. Nothing more need be said.

In the summer of 1913 the scientific world, generally, became suddenly aware of the nucleus. At the British Association meeting in Birmingham. at the Solvay Conference in Brussels and at an international congress in Vienna, the new ideas provoked the greatest interest. A little later, in London, on March 19, 1914, the Royal Society held a special discussion on the subject. Except in Vienna, Rutherford was the central figure on each occasion. In the end, there could be no permanent opposition, for there was no plausible alternative to the new theory, and its quantitative successes appeared utterly convincing. But at Brussels, Thomson did not acquiesce, unreservedly. In his original paper, Rutherford had not thought it necessary to question the validity of the coulombian law even when distances of the order of 10^{-12} cm. were involved. Thomson took the opposite point of view: 'Now we have no direct evidence', he said, 'as to what is the law of force between electrified bodies when the distance separating them is as small as this . . .'-and, later, he continued, 'This seems to me to indicate that the large deflections ... of the α particles are not produced by forces ... due to electrical charges . . . which would act upon a charged corpuscule as well as upon a charged α particle. They are in my opinion more likely to be due to special forces which come into play when two α particles are within less than a certain distance of each other. That in fact when two α particles come into collision inside an atom the forces between them are not merely [electrostatic forces]'. To Rutherford, understandably, this appeared as an obtuse opinion: had not the experimental results of Geiger and Marsden confirmed, at the same time, both the essential correctness of the nuclear model and also the validity of the coulombian law at nuclear distances? Obviously they had-for all practical purposes: Thomson's was indeed an obtuse opinion, in so far as the collisions of 8 MeV α -particles with heavy atoms were concerned, but, from the vantage point of today, we might ask whether it would have been equally obtuse in relation to the collisions of 8 MeV α -particles with free α -particles (or with helium atoms). Luck—or the very nature of things—was on Rutherford's side!

It is no coincidence, perhaps, that, when the special meeting of the Royal Society took place on March 19, 1914, the then current issue of the Philosophical Magazine carried a paper by Rutherford surveying the position as he saw it at the time (this volume, p. 423). He had not written specifically on the nuclear model, at least for general publication, since his first formal paper on the subject nearly three years previously. Now he was able to 'deal with certain points in connexion with the "nucleus" theory of the atom which were purposely omitted in my first communication' and to give 'a brief account . . . of the later investigations which have been made to test the theory'. Among the matters 'purposely omitted', we may identify, first of all, the old problem of the validity of the coulombian law. Rutherford was now able to report that Darwin had proved conclusively (and the proof was in an accompanying paper in the same journal) that no other power-law would serve to explain the results of experiment. And we may also identify the problem of the scattering law as it applies to collisions with the lightest atoms. for example, hydrogen and helium (for this problem was referred to, and definitely held over for later discussion, in the original paper). Again, Darwin had worked out the necessary modification of the theory-and Rutherford and Nuttall had already made some tests of its validity (this volume, p. 362). But the 'later investigations' provide the real content of the paper: the unexpected confirmation of the assumption of single scattering in the first photographs of α -particle cloud tracks taken by C. T. R. Wilson in Cambridge; the fitting together of van den Broek's simple suggestion regarding the magnitude of the nuclear charge, both with Bohr's dictum that α - and β -particles alike originate in the nucleus, and with the empirical 'displacement law' of Fajans, Russell and Soddy; the brilliant experiments of Moseley ordering the lighter elements, at least, uniquely in terms of the frequencies of the characteristic X-radiations. Finally, there was Bohr's own massive contribution, as it existed at that time in the three papers which had then been published.

We may note with amusement that Rutherford mentioned Bohr's papers last of all in his survey—almost as an afterthought. 'While there may be much difference of opinion as to the validity and of the underlying physical meaning of the assumptions made by Bohr', he was prepared to applaud his intentions! But he had already said of Moseley, with no such reservations, 'he has shown that the variation of wave-length can be simply explained by supposing that the charge on the nucleus increases from element to element by exactly one unit'. The mere onlooker may be forgiven for thinking that, at that stage, the two issues were very much entangled.

The paper of March 1914 marks the beginning of the assimilation of the nucleus into the general world-picture of the physicist; it is also remarkable in that it indicates the lines of Rutherford's thinking on the problem of nuclear constitution. The signature of the chemical atom was the number of unit charges on the nucleus, but the nucleus itself was a system of some considerable complexity, its mass almost certainly less than the sum of the masses of its constituent particles—hydrogen nuclei and electrons, with helium nuclei (α -particles) as sub-units of 'very stable configuration'. Obviously, Rutherford had done a lot of thinking in the three years which had elapsed since the original announcement. And he had done some adventurous experimenting, too: 'In conjunction with Mr Robinson, I have examined whether any charged atoms are expelled from radioactive matter except helium atoms . . . if such particles are expelled, their number is certainly less than 1 in 10,000 of the number of helium atoms'. So, let us leave the nucleus for a while, noting only that Marsden was already counting the scintillations of hydrogen nuclei projected forwards 'elastically' with roughly four times the residual range of the α -particles (according to Darwin's prediction), and consider another line of work. It is a natural breaking-point: we have followed the history of the nucleus to the outbreak of war.

During much of the period that we have just been describing, Rutherford was heavily committed with work on the β - and γ -radiations. As first discoverer—indeed, almost as 'onlie begetter'—of the α -particle, he had at times been somewhat neglectful of the claim of the more penetrating radiations to serious study. 'I have often pointed out what an important part the α particle plays in radioactive transformation. In comparison, the β and γ rays play quite a secondary rôle', he wrote in July 1906 (Volume I, p. 895). Five years later the position had changed. von Baeyer, Hahn and Meitner had discovered the magnetic line spectrum of the electrons from mesothorium 2 and other β -emitters, and Danysz, confirming their results, had already revealed the great complexity of this spectrum in certain cases. Obviously, there was much detailed information to be gathered, precise energies to be determined—and used to develop and test an interpretative scheme.

Rutherford did not at once enter into competition with the workers in Berlin and Paris in the field of magnetic spectroscopy. His first reaction was to assign to Moseley the problem of determining the mean number of electrons emitted in the successive β -disintegrations of radium B and radium C. This was merely one of the many assignments which, during his three years at Manchester, Moseley tackled and fulfilled with unflagging energy and consummate skill. Then, with Robinson, Rutherford re-determined the heating effect of radium and its products, paying particular attention to the heating due to the more penetrating radiations, the β -particles and the γ -rays (this volume, p. 312). These measurements completed, he wrote up his own provisional views on the subject in a paper for the Philosophical Magazine (this volume, p. 280). Some of his views were soon to be discarded, in the light of the displacement law and the other evidence which we have already discussed: 'The instability of the atom which leads to its disintegration may be conveniently considered to be due to two causes . . . the instability of the central nucleus [which 'leads to the expulsion of an α particle'] and the instability of the electronic distribution [which leads 'to the appearance of β and γ rays']'. None survived unchanged, save the basic view that there must exist a close connection between the appearance of the electrons of the line spectrum and the emission of the γ -rays: 'It may prove significant that only those products which emit well-defined groups of β rays emit also a strong γ radiation'.

Rutherford naturally followed the clue which this last quotation suggested; the fact that 'as far as observation has gone, the β rays from uranium X and radium E give a continuous spectrum in a magnetic field', was awkward, but he shrugged it off, for the time being, as an effect which a young man in a hurry was unlikely to make sense of.

At this stage Rutherford, himself, was really in a hurry to obtain further information regarding the spectra of the β - and γ -radiations of as many products as possible. As providing a background to more detailed studies, a general survey of the γ -radiations was instituted, using the standard method of absorption analysis. Much of this work was done by H. Richardson, under Rutherford's personal direction (this volume, pp. 342, 353, 410), but Moseley, Fajans and Makower also contributed-and, somewhat outside the main line of interest. Russell and Chadwick detected weak radiations of the γ -ray type with the α -emitters radium, ionium and polonium, and examined them by the same method. As regards the more detailed studies, Robinson was chosen to be chiefly responsible for the magnetic spectroscopy: to develop an instrument with which the work of Danvsz could be checked-and overtaken. Andrade was given an even more exacting assignment: no one had yet applied the new method of crystal diffraction, which Moseley was using to such good effect in the domain of the soft X-rays, to the more penetrating γ -rays. And rade was to do just this. Let us look at the results of these detailed studies, very briefly, in turn.

Before the experiments of Rutherford and Robinson had produced much of significance, Danysz published some further results (Le Radium, January 1913), in obtaining which a new experimental technique had been employed. In Manchester, the main technical problem had already been broadly identified: 'to devise a method of bringing out the presence of groups of β rays, the total energy [intensity] of which might be only a small fraction of that distributed in the more intense groups'-and it appeared that Danysz's new arrangement met this requirement. Rutherford and Robinson were quick to take advantage of the new technique, and to improve on it. Later, they wrote laconically (this volume, p. 371), 'we have used a special method which appears to be very similar in principle to that employed by Danysz in his latest investigation'. So was the method of semi-circular focusing brought to bear on the problem of the moment. Surprisingly, the principle of the method was not presented in the published paper (Philosophical Magazine, October 1913) with the clarity and directness of which Rutherford was undoubtedly capable, but it was applied with real insight and the results to which it led were remarkable in their scope and complexity.

The sources which Rutherford and Robinson had used were 'line' sources-thin-walled emanation tubes and wires coated with the active deposit of radium, or with radium C. The β -radiation from these sources was spread out into a momentum spectrum in the magnetic field: the y-radiation went free. Rutherford, with good reason, suspected a close connection between the two. The whole situation cried out for someone to ask Nature the next question-with his hands. The question when it cameand there was no unseemly delay-came in the authentic Rutherford manner. A thin sheath of lead was slipped over the line source in the spectrograph; then the y-radiation did not all go free, some of it produced 'secondary' electrons in the lead, and the momentum spectrum of 'the β rays excited by γ rays' was observed for the first time. As Rutherford expected, the general structure of the 'excited' spectrum—in so far as it could be observed, for the experiment was technically a difficult one-was very similar indeed to that of the 'natural' line spectrum observed with the unsheathed source. Very definitely there was a close connection between the electrons of the natural line spectrum and the γ -rays.

The experiments that we have just been describing were reported by Rutherford, Robinson and Rawlinson in the *Philosophical Magazine* for August 1914 (*this volume*, *p. 466*). The last paragraph of this paper begins: 'Experiments on this subject will be continued by Robinson and Rawlinson'. That was to reckon without the war-lords of Europe.

Rutherford and Andrade published the results of their experiments by the crystal diffraction method in two papers in May and August 1914 (this volume, pp. 432, 456). The first paper described a straightforward experiment. using the technique of Bragg reflection from the cleavage face of a crystal. It required intense sources and long exposures. Qualitatively, it demonstrated without doubt that the least penetrating component of the radiation was L X-radiation, according to the nomenclature of Barkla, Technically, the achievement was considerable (the spectrum 'lines' were faithful images of the emanation tube used as source, showing bright edges and dark centres, as would be expected from the distribution of active deposit over the inner walls of the tube), but it is doubtful whether the identification of the characteristic X-radiation helped, at that stage, to clarify the situation generally. It directed attention away from the nucleus as the emitting body. And, for once, by some strange circumstance, Rutherford must have allowed his own convictions to colour his estimate of the merit of an experimental determination. According to the displacement law, the atomic number of radium B had to be that of lead; according to Moseley, the atomic number of lead was 82. The authors of the paper of May 1914 expressed the conviction that they had a large factor of safety in hand in claiming that the L X-rays in the spectrum of radium B were demonstrably those of an atom of atomic number 82, not 81 or 83. It all seemed to be so obviously straightforward. Yet, something had been at fault. More than ten years later Rutherford and Wooster repeated the experiment in Cambridge. The L X-rays in the radiation spectrum of radium B were shown to be those of an atom of atomic number 83. Which is not to say that radium B is not an isotope of lead!

The second paper of Rutherford and Andrade is noteworthy for the remarkably simple and effective arrangement, therein described, by which sharp spectrum lines were obtained, in a pattern of four-fold symmetry, by 'focal isolation' of the diffracted ('reflected') radiation transmitted through a rock-salt crystal. The source was a short 'line' source of radium emanation. directed along the normal to a crystal face, the isolating 'stop' a circular hole in a lead screen placed at an equal distance on the other side of the crystal. With this arrangement, exposure-times were reduced and background fogging was almost eliminated. But the elegance of the method could not alone ensure that the results which were achieved added anything of significance to the general theoretical picture. Rutherford and Andrade convinced themselves-and rightly so-that they had observed for the first time the K series lines due to radium B, but preoccupation with ideas of characteristic X-radiations led them to regard some γ -rays of shorter wavelength which they also observed as belonging to 'the "H" series, for no doubt evidence of a similar radiation will be found in other elements when bombarded by high speed cathode rays' (so they wrote). They were pioneering an entirely new field of experimental research; it should not be held to their discredit that, in discussing the assignment of the homogeneous component radiations as between the two constituents of the active deposit, they were entirely unaware that more than 95 per cent of the γ -radiations of radium C in fact lay outside the range of analysis of the method they were using.

For nearly two years, as we have seen, much of the effort of the Manchester laboratory had been devoted to a concerted attack on the problem of β - and y-ray changes. On June 30, 1914, having directed this attack, Rutherford completed his second paper reviewing the general situation. Essentially, it had the same title as the first, and it appeared in the September number of the Philosophical Magazine (this volume, p. 473). Seen from this distance in time, it scarcely appears to advance our understanding of the problem, in any notable particular. Experimentally, much information had been gathered, but a coherent scheme of interpretation still eluded the Manchester group. Bohr was still in Copenhagen. Lip-service was paid to the displacement law: 'Suppose . . . that the disintegration of the atom leads to the expulsion of a high speed β particle from or near the nucleus'. But the basic simplicity of the atomic model which provided the only available explanation of Moseley's results-the only explanation which made them intelligible. and utterly fundamental, in the scheme of things-was not seized upon firmly; instead, localized 'vibrating systems' were postulated within the atom, responsible each for the emission of a characteristic radiation when suitably excited. The exciting and entirely novel result which Chadwick had obtained in a few months' work with Geiger in Berlin was recorded, and accepted at its face value, but the attempt to specify the conditions under which the electrons of the line spectrum should appear was artificial in the extreme. The line spectrum was missing, so Rutherford said, when the disintegration electron never succeeded in exciting the 'vibrating systems' on its way out of the atom-and he made much play with hypothetical directional preferences as a basis for such possibility. He accepted the electrons of the continuous spectrum as the disintegration electrons—he had no other alternative in the light of Chadwick's results-and in one sentence he reached an entirely luminous conclusion, 'The present theory supposes that the homogeneous groups of β rays arise from the conversion of the energy of the γ rays into the $\hat{\beta}$ ray form', but the confusion between γ -rays and characteristic X-rays remained and clouded the whole picture. How soon a way would have been found through the complexities of the problem, had not war intervened, is a matter of speculation. In the upshot, the issue remained unresolved for eight years, until Ellis, in Rutherford's laboratory in Cambridge, produced compelling evidence for the modern view that the γ -rays, like the α -particles and the β -particles, originate in the nucleus. Even then, the difficulty of the continuous spectrum was in no way abated, and another ten years or more were required to bring it into focus in a symbol. Pauli's neutrino. On reflection, surely, our strictures have been too severe: in 1914 the time was not ripe for an understanding of the problem of the β - and γ -ray changes.

There is only one postscript to be added to this opinion here. In the first year of the war Rutherford was able to organize some work in Manchester. with Barnes and Richardson, on the X-radiation obtainable with the then newly developed Coolidge-type tube. Two papers were published describing the experiments in the Philosophical Magazine in September 1915 (this volume, pp. 505, 524). Thereafter, he managed to continue these experiments, sporadically, with such help as he could enlist, and a further paper appeared in the same journal exactly two years later (this volume, p. 538). Throughout all this work, amid all the distractions and duties of the time, quite obviously Rutherford had kept the problem of the penetrating γ -rays not far out of mind. In the end he had come round by a devious route to a most important conclusion. Let it be expressed in his own words: 'In our present ignorance ... it is only possible to estimate the actual wave-length of the most penetrating gamma rays. It is clear, however, that ... they correspond to waves generated by voltages between 600,000 and 2,000,000 . . . that the gamma rays from radium C . . . are of considerably shorter wave-length than any so far observed in an X-ray tube, with the highest voltages at our disposal . . . the B rays from radium C consist mainly of groups lying between 500,000 and 2,000,000 volts . . . It would thus appear probable that the observed groups of β rays are due to the conversion of the energy, $E = h\nu$, of a wave of frequency v into electronic form, and that consequently the energy of the β ray groups may be utilized by the quantum relation to determine the wave-lengths of the penetrating gamma rays'. It should now be clear how it was that Ellis was able to start off on the right foot, when he began research under Rutherford in Cambridge, after the war!

Rutherford finished writing the paper, from which we have just quoted, on May 12th. In less than a week he was in Paris, where a joint Anglo-French mission was assembling, for the journey to Washington. They were going to acquaint the naval authorities of the United States with the situation in respect of anti-submarine research and development as it then was. This had been Rutherford's main concern for nearly two years: it had taken him on frequent journeys to Rosyth, and Harwich, and London, and it had involved much hard work and improvisation in a field of experiment which was new to him. The visit to America provided a welcome break, and marked the end of his active involvement in the British effort, though it was arduous in itself. It provided opportunity for renewal, also: Rutherford saw Boltwood at Yale, and received an honorary degree from the university, and he made the journey to Montreal, as well. He was back in Manchester by the end of July.

Perhaps his visit to Washington reminded Rutherford of the last occasion on which he had been there—in April 1914, when he had delivered the first course of William Ellery Hale lectures before the National Academy of Sciences. His title had been: 'The constitution of matter and the evolution of the elements', and he had been bold enough to discourse in public on the possibilities of bringing to pass in the laboratory some of the transmutations --some of the stages of the 'inorganic evolution of the elements'--concerning which Lockyer had speculated so many years previously. 'It is possible', he had said, 'that the nucleus of an atom may be altered either by direct collision of the nucleus with very swift electrons or atoms of helium such as are ejected from radioactive matter'. We have mentioned, already, that early in 1914 Marsden was at work investigating the long-range hydrogen nuclei projected forwards by α -particles passing through hydrogen gas. When Rutherford returned from Washington in May of that year, he surely had in mind to continue these investigations using gases other than hydrogen. Indeed, some few observations were made, though not deliberately, of the effect in air. But, by that time, the country was at war, and the matter could not be pursued to its conclusion. Had it been so pursued, with Rutherford's active participation, it is unlikely that the effect would have been missed; as it was, Marsden and Lantsberry merely reported their observations. They had observed what appeared to be long-range hydrogen nuclei with an a-particle source situated in air-but they concluded (Philosophical Magazine, August 1915) that the hydrogen nuclei were probably emitted by the source. In this they were doubtless mistaken, but they had at least convinced themselves that the magnitude of the effect was too great to be explained in terms of water vapour, or other hydrogenous material, condensed on the source-and in that conviction they were probably correct.

When Rutherford returned from Washington in July 1917, he took up, on his own, the work which Marsden had begun. He was without assistance, save for William Kay, the laboratory steward. But Kay was a prince among assistants, and the work went well. It started in earnest on Saturday, September 8th, and within three days the 'unexpected' scintillations of long-range particles had definitely been observed, with air in the tube. By September 28th, similar experiments were being made with nitrogen, and oxygen, and carbon dioxide, in turn. Some days earlier, helium had been used. Already there was fairly definite evidence of an effect specific to nitrogen. But Marsden had thought that there were hydrogen nuclei of high energy emitted from the source. Rutherford, therefore, spent laborious days throughout October investigating the scintillations observed when the α -particles were allowed to pass into an absorbing foil in an otherwise evacuated tube. He found nothing to substantiate Marsden's suggestion. Early in November he returned to the experiments with air. Careful drying, rigorous elimination of dust: nothing that he could do by way of purification produced any diminution in the number of scintillations due to the longrange particles. Under otherwise identical conditions, he compared the effect in chemically prepared nitrogen with the effect in air. The ratio was 5:4, as nearly as his statistical accuracy could be trusted. He was now convinced that the effect arose in α -particle collisions with nitrogen nuclei. The next step, as he recorded in his laboratory notebook on November 9. 1917. was: 'To settle whether these scintillations are N. He. H or Li'.

Admittedly, the four possibilities symbolized in this brief exhortation were not equally plausible. That unchanged nitrogen nuclei, or α -particles, should have acquired an abnormally large increment of energy in mutual interaction, when carbon nuclei and α -particles, or oxygen nuclei and α -particles, did not interact in this (unclassical) way, was most unlikely. But these possibilities were at least entertained, although the other two were the less outrageous. If the long-range particles were hydrogen nuclei, the suggestion was, in effect, that a 'chip' had been knocked off a nitrogen nucleus in close collision with an α -particle; if the long-range particles were lithium nuclei, then the collision had split the nitrogen nucleus in two. Rutherford set to work to examine these possibilities—and on January 10, 1918, he added another to his list: the long-range particles might be deuterons. He did not, of course, use that word (the word 'proton' was not yet in the vocabulary of physics): he wrote 'atom charge + e and mass M = 2 called x'.

It was a brave exhortation—this exhortation to take each possible identification in turn, devise experimental tests, and sift the evidence. Naturally, it did not work out in that way. The scintillations that Marsden had observed with α -particles in hydrogen were certainly those of hydrogen nuclei. Rutherford knew what they looked like, and he was prepared to bet that the 'unknown' scintillations were due to hydrogen nuclei, also. So he planned a long series of experiments in which he compared objectively the two radiations: the hydrogen nuclei projected by elastic collision from hydrogenous materials, and the particles of long range produced in nitrogen. It required all his ingenuity—and the greater part of his faith in the rightness of his judgment. But the demonstrable results of the comparisons were definite enough to give colour to his view: 'It is difficult to avoid the conclusion that the long-range atoms arising from collision of α -particles with nitrogen are not nitrogen atoms but probably atoms of hydrogen, or atoms of mass 2'.

The quotation is from the last paper that Rutherford wrote during the Manchester period. Under the title: 'An anomalous effect in nitrogen', it constituted Part IV of a sequence of papers published in the *Philosophical Magazine* for June 1919. The general title of the sequence (*this volume*, p. 547) is: 'Collisions of α -particles with light atoms', and its length fifty pages of original text. Part IV occupies a mere six pages of this total. In this compass one of the most momentous experiments in the history of science is presented unostentatiously, without emphasis, or any sensational claim.

Single-handed, in the last dark days of war, Rutherford had been adventuring into the unknown. In happier times, much of this adventure would have fallen to the lot of the young men who thronged his laboratory and knew the inspiration of his genius. He had not failed them: forty-four of the fifty pages of the published account of the work that he had done in their absence was of work such as they might have achieved under his guidance honest work, and worth while. Only the kernel of it, the residue which did not belong to any previous category of thought, was outside the range of their powers. That residue, those six pages of print, were to startle the world. From Nature, 76, 1907, p. 126

IN a previous letter to NATURE (January 17)* I gave an account of some experiments which I had made upon the growth of radium in preparations of actinium. The results obtained were in substantial agreement with the earlier observations of Boltwood in this Journal (November 15, 1906), but it was pointed out that there was no definite evidence that actinium itself was the true parent of radium. The experimental results could be equally well explained by supposing that the parent substance of radium was ordinarily separated from radio-active ores with the actinium, but had no direct radio-active connection with the latter.

Observations have been continued upon the growth of radium in the actinium solution prepared in the manner indicated in my first letter. The rate of growth was found to be uniform over a period of 120 days, and to agree closely with the rate of growth observed in the solid preparation of actinium which had been set aside for a period of two and a half years. Another sample of actinium was then taken and successively precipitated with ammonium sulphide in order to remove the radium from the solution. In this way a solution of actinium was obtained initially almost entirely free from radium. By examination of the α -ray activity, it was found that the actinium after this chemical treatment contained an excess of radio-actinium. This was shown by the rise of the activity to twice its initial value in about twenty days, and then a gradual decay to a steady value. Special care was taken to measure accurately the rate of growth of radium in the solution at short intervals in order to see whether it depended in any way upon the variation of the activity. No such connection was observed, for the radium was produced at a constant rate over the whole period of examination, viz. 111 days.

For equal quantities of actinium, the rate of growth of radium observed in this solution was 1.5 times greater than the normal. This indicated that only a portion of the actinium had been precipitated, while the radiumproducing substance had been precipitated with the actinium in excess of the normal amount. This conclusion was confirmed by an examination of the filtrates, which were found to contain more than half the actinium. After suitable chemical treatment, a small precipitate of actinium was again obtained which was about one hundred times as active, weight for weight, as the original preparation. This actinium precipitate was dissolved in hydrochloric acid, and observations of the amount of radium in it were made at regular intervals. No appreciable growth of radium was observed over a period of eighty days. If there were any growth at all, it was certainly less than one two-hundredth part of that normally to be expected. In order to make certain that the absence of apparent growth of radium in this solution could not be ascribed to the precipitation of the radium in some non-emanating form, the solution was again chemically treated. The actinium was precipitated with ammonia and re-dissolved in hydrochloric acid. Again no growth was observed over the period of examination, viz. twenty days. The solution in its present state contains a just measurable quantity of radium, viz. about 2×10^{-12} gram.

From these observations I think we may safely conclude that, in the ordinary commercial preparations of actinium, there exists a new substance which is slowly transformed into radium. This immediate parent of radium is chemically quite distinct from actinium and radium and their known products, and is capable of complete separation from them.

It is not possible at present to decide definitely whether this parent substance is a final product of the transformation of actinium or not. It is not improbable that it may prove to be the long-looked-for intermediate product of slow transformation between uranium X and radium, but with no direct radio-active connection with actinium. If this be the case, the position of actinium in the radio-active series still remains unsettled.

It is intended to continue observations on the growth of radium in the solutions described above. Experiments are also in progress to isolate this new substance in order to examine its chemical and radio-active properties.

E. RUTHERFORD.

Manchester May 30

The Effect of High Temperature on the Activity of the Products of Radium

by professor E. RUTHERFORD, F.R.S. and J. E. PETAVEL, F.R.S.

Abstract of the British Association Report, 1907, pp. 456-7

BRONSON has shown that the activity of the products of radium is not appreciably altered by exposure to a temperature of 1600° C. On the other hand, Makower, working with the active deposit of radium, found that there was a small decrease of its activity, measured by the β and γ rays, when exposed for some time to a temperature of about 1100° C. The experiments of Schuster and of Eve have shown that the highest obtainable pressures have no influence on the activity of radium.

In the present experiments the emanation from about four milligrams of radium bromide was momentarily exposed to the influence of the very high temperature produced by the explosion of cordite in a closed steel bomb. The bomb used in these experiments was constructed by Mr Petavel, and had been used by him in previous experiments on the pressures developed during explosions. The bomb was a complete sphere of mild steel, about 4 inches internal diameter and about 2 inches thick. About forty-six grains of cordite were placed in the bomb, and after exhaustion the emanation was introduced. About four hours later the emanation is in equilibrium with its products, and the activity due to the γ rays, which passed through the bomb, was observed by means of an electroscope placed outside the bomb. The cordite was fired electrically, and observations were made of any change of activity. By running the electroscope during the explosion, it was found that no sudden burst of activity occurred, showing conclusively that the normal rate of disintegration of the product, radium C, was not much altered by this process. Three experiments were made with equal weights of cordite, but of different diameter, in order to vary the suddenness of the explosion. In every case the activity measured by the γ rays was found to have decreased about 9 per cent. after the explosion. The activity gradually rose again, reaching nearly the equilibrium value after three hours. A special experiment showed that the rate of change of the emanation itself was not altered by the explosion.

The maximum pressure of the gases during the explosion was about 1200 atmospheres, and the maximum temperature certainly not lower than 2500° C.

The change of activity produced by the explosion may be due either to a sudden alteration of the distribution of the active deposit or to a change in the amount or period of the products, radium B and radium C. Since the active deposit of radium is volatilised at about 1200° C, it would be rendered gaseous by the high temperature of the explosion, and redeposited when it cooled. Since the bomb was exactly spherical, a change of distribution of the active deposit does not appear very probable. In one experiment two electroscopes were used, one by the side of the bomb and the other underneath it. Both showed about an equal decrease of activity.

The experiments recorded here are preliminary, and it is intended to examine still further whether there is a real change of activity of radium products by the action of the high temperature. From Nature, 76, 1907, p. 661

IN a letter to NATURE (June 6)* I gave the experimental evidence which led me to conclude that in ordinary actinium preparations a new substance was present which was slowly transformed into radium. By a chemical method this substance was separated from actinium, and a solution of the latter was obtained which showed no appreciable growth of radium over a period of eighty days. Observations on this solution have been continued over a total period of 240 days, and there is still no detectable increase in the quantity of radium. The growth of radium, if it occurs at all, is certainly less than 1/500 of that observed in other experiments.

In two recent letters to NATURE (September 26 and October 10) Dr Boltwood has given the results of his later experiments in this direction. He has confirmed my conclusions, and has, in addition, been successful in devising a satisfactory method of separating this new substance from actinium, and has examined its radio-active and chemical properties. He suggests that the name 'ionium' be given to this new body, which is probably the immediate parent of radium. Dr Boltwood is to be congratulated for his admirable work on this very difficult problem, for, apart from the chemical operations, the radio-active analysis required for correct deduction is unusually complicated and difficult.

Dr Boltwood has not been able to separate the parent of radium from actinium by the reagent employed by me, viz. ammonium sulphide, but has found the use of sodium thiosulphate effective. In explanation of this discrepancy, he suggests that I employed old ammonium sulphide. As a matter of fact, I did not use the ordinary laboratory solution of ammonium sulphide, but added ammonia to the actinium solution, and then saturated it with sulphuretted hydrogen. The complete separation effected in my experiment was, I think, probably due to an accidental production of finely divided sulphur in the solution.

In a letter to NATURE of last week, Mr N. R. Campbell raised objections to the name 'ionium' given by Dr Boltwood to the new body, from the point of view that every radio-active substance should be given a name to indicate its position in the scheme of radio-active changes. This system is very excellent in theory, but I have found it extremely difficult to carry out in practice. The continual discovery of new products in very awkward positions in the radioactive series has made any simple permanent system of nomenclature impossible. Besides uranium and thorium, twenty-four distinct radio-active substances are now known to exist in radio-active minerals. The number of products still to be discovered is, I think, nearly exhausted. When there is a general consensus of opinion that this is the case, I feel it will be very desirable for physicists and chemists to meet together in order to revise the whole system of nomenclature. There is not much to be gained in doing so immediately, as the discovery of a new product in the midst of a series would entail the alteration of the names of a possible half-dozen others which follow it. At the same time, I think it will be desirable to retain a distinctive name for those radio-active substances which, like radium, have a long enough life to be separated in sufficient quantity for an examination of properties by the ordinary chemical and physical methods. It is probable that the parent of radium fulfils these conditions, and should thus have a distinctive name like radium.

Personally, I do not much like the name 'ionium,' but for similar reasons neither do I care for the name 'actinium.' It is not easy to suggest a name that is at once simple and explanatory. I have for some time thought that possibly 'paradium' or 'picradium' might be suitable for the new substance. The former name suggests that it is the parent of radium, but I recognise that a possible play on words may make it unsuitable. The name uranium A, suggested by Mr Campbell, in itself innocuous, is open to the objection that in the case of radium, thorium, and actinium the suffix A is applied to the first product of the disintegration of the respective emanations, while no such emanation has been observed in the initial series of changes of uranium.

E. RUTHERFORD

University of Manchester October 27

The Production and Origin of Radium

by professor e. RUTHERFORD, D.SC., F.R.S.

From the *Proceedings* of the Manchester Literary and Philosophical Society Abstract of a paper read before the Society on October 29, 1907

AN account was given of the historical development of our ideas in regard to radium. On the disintegration theory, radium is regarded as a substance undergoing slow spontaneous transformation with a period of about 2,000 years. In order to account for the existence of radium in minerals of great age, it is necessary to suppose that radium is produced from another substance of long period of transformation. There is an undoubted genetic connection between uranium and radium, for investigation has shown that the amount of radium in minerals is in all cases proportional to their content of uranium. If this be the case, radium should gradually appear in a preparation of uranium, initially freed from radium. No such growth of radium has been observed over a period of several years although a very minute growth of radium can be easily detected. This is not necessarily inconsistent with the disintegration theory for if one or more products of slow transformation exist between uranium and radium, no appreciable growth of the latter is to be expected in a short interval. A search for this intermediate product has recently proved successful. Boltwood found that a preparation of actinium, initially freed from radium, grew radium at a constant and rapid rate. Boltwood at first considered that actinium was this intermediate product and that actinium changed directly into radium. The growth of radium in actinium solutions was confirmed by the writer, who had commenced experiments in that direction three years before. The experiments showed, however, that actinium did not, as Boltwood supposed, change directly into radium. By a special method, a preparation of actinium was obtained by the writer which showed no appreciable growth of radium over a period of 240 days. The growth of radium, if it occurred at all, was certainly less than 1/500th of that ordinarily observed.

In another case, a solution of actinium was obtained which produced radium faster than the normal.

These results are completely explained by supposing that a new substance of slow transformation is present with actinium, and this substance is transformed directly into radium. This parent of radium has distinct chemical properties, which allow it to be separated from both actinium and radium. The absence of growth of radium observed in the actinium solution mentioned above is due to the fact that, by the special method, the parent of radium had been completely separated from the actinium. In recent letters to *Nature*, Boltwood has confirmed the results of the writer, and has devised a satisfactory method of separating the radium parent from actinium. He has shown that this new body, which he proposes to call 'ionium,' gives out α and β rays, and has the chemical properties of thorium.

The Royal Society recently loaned the writer the actinium residues from about a ton of pitchblende. These residues contain the parent of radium, and experiments are in progress to isolate and concentrate both the actinium and ionium in these residues.

The Production and Origin of Radium

by E. RUTHERFORD, F.R.S.

Professor of Physics, University of Manchester

From the *Philosophical Magazine* for December 1907, ser. 6, XIV, pp. 733-49 Read before the British Association, Leicester, August 1907. Previous accounts of the results were given in letters to *Nature*, January 17 and June 6, 1907 (*Vol. I, p.* 907; *this Vol. p. 34*)

§ 1

THE present point of view of regarding radium as a substance which is undergoing slow transformation was first put forward definitely by Rutherford and Soddy in the paper entitled 'Radioactive Change' (*Phil. Mag.*, May 1903, p. 590), in the following terms: 'In the case of radium, however, the same amount, (viz. about 1 milligram), must be changing per gram per year. The "life" of the radium cannot in consequence be more than a few thousand years on this minimum estimate, based on the assumption that each particle produces one ray at each change. . . . So that it appears certain that the radium present in a mineral has not been in existence as long as the mineral itself, but is being continuously produced by radioactive change.' (*Vol. I*, p. 607.)

On this theory, the parent substance which produces radium must always be present in minerals containing radium. Uranium from the first appeared to be the most probable parent, since it possessed a life long compared with radium and was always found associated with it. There were two obvious methods of attack to throw light upon this question, one direct and the other indirect. The first consisted in an examination to see whether in course of time radium appeared in a solution of uranium initially freed from radium. The second depended upon an examination of the relative amount of radium and uranium in radioactive minerals. According to theory, if uranium is the parent of radium, the ratio of the amount of radium in any mineral to that of uranium should be constant. The constancy of this ratio has been completely substantiated by the independent work of Boltwood*, Strutt[†], and McCoy[‡]; and there can be no doubt that uranium and radium are genetically connected. Rutherford and Boltwood[§] have

- ‡ McCoy, Ber. d. D. Chem. Ges., No. 11, p. 2641, 1905.
- § Rutherford and Boltwood, Amer. Journ. Sci., July 1906. (Vol. I, p. 856).

^{*} Boltwood, Phil. Mag., April 1905.

[†] Strutt, Proc. Roy. Soc., March 2, 1905.

found that for every gram of uranium in a mineral, there is present 3.8×10^{-7} gram of radium.

The question of the growth of radium in a uranium solution was first attacked by Soddy^{*} and, later, by Boltwood.[†] Without entering into the details of these important investigations, it suffices to say that, in carefully purified uranium solutions, no growth of radium has been observed, over the space of the few years that observations have been in progress. If radium is produced at all, it is certainly produced at less than one-thousandth of the rate to be expected theoretically. This result is not necessarily inconsistent with the view that radium is a transformation product of uranium, for the absence of observable growth of radium in a limited time is to be expected, if one or more products of slow transformation exist between uranium and radium.

In the meantime, Boltwood[‡] had approached the problem from a different direction. By a special method, the actinium was separated from a kilogram of carnotite. A solution of this actinium, initially containing very little radium, was placed aside and examined 120 days afterwards. A notable increase in the amount of radium was observed. In addition, the rate of growth in this interval was about that to be expected if radium were half transformed in about 2,000 years—a result in conformity with calculations of the probable life of radium. The work of Boltwood marks a definite and important stage in the attack on this problem, for it clearly shows that radium, as theory predicted, is produced from another substance and that this parent substance is normally present with actinium.

Boltwood concluded that actinium was the direct parent of radium and was itself an intermediate product between uranium and radium. This conclusion was strongly supported by his observation that the amount of actinium in minerals, like the amount of radium, was proportional to the amount of uranium. Since actinium has probably a life comparable with that of radium, such a conclusion is consistent with the observed absence of growth of radium in uranium solutions, for the uranium must first form a considerable quantity of actinium before the transformation product of the latter, viz. radium, could be detected in the solution. This question will be discussed later in the paper after the consideration of further experimental results. It will be seen that the problem is more complicated than at first appeared.

§ 2. Old Experiments

It may be of interest to give a brief account of some experiments commenced by myself in 1904 to determine whether radium was continuously produced from actinium. A preliminary account of this work was given in the Bakerian

- * Soddy, Phil. Mag., June 1905; August 1907.
- † Boltwood, Amer. Journ. Sci., September 1905.
- ‡ Boltwood, Nature, November 15, 1906.

Lecture (*Phil. Trans.*, A, p. 218, 1904).* Two grams of an active preparation, of activity about 250 times that of uranium, obtained from Giesel, were taken and dissolved in acid. The initial content of radium was determined by the emanation method, and the greater part of it then removed by successive precipitations in the solution of small quantities of barium as sulphate. Measurements were then made of the amount of radium in this solution at intervals over a space of three months, but with no certain evidence of the growth of radium. The amount of radium was estimated by the emanation method. The radium emanation, which was allowed to collect in the solution for a known interval, was removed into a large electroscope by aspirating a considerable amount of air through the solution. Later work of Boltwood has shown that this aspiration method is unreliable for an accurate determination of the amount of radium present, but it no doubt serves for comparative measurements under identical conditions.

In the light of later knowledge, the method employed for the separation of the radium present initially in the solution was very unsuitable for several reasons. A trace of sulphuric acid remaining in the solution after the removal of the barium might possibly precipitate the radium as sulphate—a form in which it would be very unlikely to release all its emanation by aspiration of the solution.

After three months' observations, this solution was put aside with the intention of testing its radium content at intervals; but the pressure of other work and the recognition of the danger of contaminating the solution in a laboratory in which a large quantity of radium was in use, led to a postponement of further tests for a period of over two and a half years. On the appearance of Boltwood's paper I immediately examined this solution to see whether there had been a growth of radium in this long interval. A preliminary test showed that there had been a considerable increase in the content of radium, but in making a more accurate determination, the solution was unfortunately contaminated with radium, probably by the use of some tap grease for a stopcock. This accident brought home to me the danger of making experiments of the experimental work recorded in this paper was carried out in the Chemical Laboratory, in which no radioactive matter had been introduced.

At the same time that the actinium solution had been prepared, a quantity of a solid actinium preparation weighing 0.32 gr. of activity about 250 times uranium, had been set aside in a closed glass tube. The radium content of a gram of the same sample had been determined in 1904 by comparison with a standard radium solution prepared at that time. Corrected in terms of the recently prepared radium standards of Rutherford and Boltwood, the amount of radium per gram of actinium in 1904 was 1.16×10^{-8} gr. Assuming, as was probable, that the content of radium was equally distributed throughout the whole mass of the actinium, the amount of radium * Vol. I, p. 721. in the 0.32 gr. was 3.7×10^{-9} gr. After an interval of 2.7 years this actinium was removed and dissolved in hydrochloric acid, and the amount of radium present was found by the emanation method, described later, to be 1.05×10^{-8} gr. The growth of radium in the interval of 2.7 years between the two tests was thus 6.9×10^{-9} gr., or assuming the rate of growth constant 2.9×10^{-9} gr. per year. While not much weight can be attached to this result by itself, on account of the imperfect aspiration method employed in the initial determination of the radium constant, yet the rate of growth observed will be seen to be in good agreement with that determined later for a similar preparation.

§ 3. Experimental Methods

Before considering further experiments, a brief description will be given of the experimental methods employed to determine accurately the quantity of radium in the various preparations. The preparation was obtained in the form of a solution and placed in a glass flask. The solution was then boiled to expel completely all the emanation, and the exit-tube sealed before the flask had cooled. After a definite time-interval, the flask was opened and the air, mixed with emanation, was expelled by vigorous boiling and collected over water. Boltwood has shown that boiling is the only satisfactory method of expelling all the radium emanation. The air plus emanation was then introduced through a drying-tube of phosphorous pentoxide into an exhausted electroscope of capacity greater than the volume of air to be introduced. Air was then let in to fill the electroscope to atmospheric pressure and the electroscope closed. The electroscope employed in most of these experiments consisted of an Erlenmeyer flask of about one litre capacity silvered on the inside, in which the gold-leaf system was insulated after Wilson's method by a sulphur bead. The motion of the gold-leaf was read through openings in the silvering by a microscope with a scale in the evepiece in the usual manner. The natural leak of the electroscope was small and corresponded to 0.14 division per minute of the scale in the evepiece. Since the emanation from 10^{-9} gr. of radium in equilibrium produced a movement of 11.5divisions per minute, the natural leak was equivalent to that produced by 1.2×10^{-11} gr. of radium. As is generally observed, the natural leak increased gradually for several days on standing, probably due to a small trace of radium present, but always came back to the same value if the electroscope was exhausted and refilled two hours before observations were begun. Since the vessel was always exhausted to introduce fresh emanation, this peculiarity of the electroscope was no disadvantage. The natural leak tested in this way was remarkably steady, and never varied more than 10 per cent over the course of several months.

Readings of the movement of the gold-leaf were commenced three hours after the introduction of the emanation. At this time, the active deposit is nearly in equilibrium with the emanation, and there is only a very slight change of the rate of movement for several hours. The electroscope was calibrated and standardized by means of the emanation from a standard solution of radium bromide, prepared by Rutherford and Boltwood. For the above electroscope the emanation from 10^{-9} gr. of radium gave a movement of the gold-leaf of 11.5 divisions per minute.

An amount of emanation which increased the natural leak by 10 per cent could be detected with certainty, so that the electroscope was capable of showing the presence of 10^{-12} of a gram of radium in a solution. Ten times this quantity could be measured with a probable error not more than a few per cent.

In the experiments to be described later, it will be shown that there was a constant rate of growth of radium in most of the solutions under examination. Since the amount of emanation in the various solutions was determined at irregular intervals, it is necessary to consider how the electroscope readings are connected with the amount of radium existing in the solution at the moment of expulsion of the emanation.

Let q_0 = amount of radium present initially.

Let q = rate of growth of radium.

Then after the solution has stood for a time t, the amount of radium present is $q_0 + q.t$.

Suppose that the emanation is completely removed after a time t_1 since the preparation of the solution, and is tested for the amount of emanation after a further interval t_2 . If a constant quantity of radium is allowed to produce emanation for a time t, it is well known that the fraction of the equilibrium quantity of emanation produced is $1 - e^{-\lambda t}$, where λ is the constant of decay of the radium emanation.

Consequently the amount of emanation present after a time of collection t_1 is proportional to:

$$(q_0 + qt_1)(1 - e^{-\lambda t_2}) + q \int_0^{t_2} (1 - e^{-\lambda \overline{t_2 - t}}) dt.$$

The left-hand side of the expression is proportional to the amount of emanation due to the radium present in the solution at the time t_1 , while the integral is proportional to the emanation produced by the quantity of radium formed in the interval t_2 .

After reduction, the amount of emanation is seen to be proportional to

$$\left[q_0+q\left(t_1-\frac{1}{\lambda}\right)\right](1-e^{-\lambda t_1})+qt_2.$$
 (1)

This expression is proportional to the observed rate of movement of the gold-leaf, so that knowing q_0 , t_1 , t_2 and λ , the value of q may be expressed in terms of divisions per minute of the electroscope.

In all the experiments to be discussed, the value of q was found to be constant over the whole time of observation. The value q_0 —the initial amount of radium present—is best determined from the first observation assuming that the rate of growth of radium during the first few days is the same as that found later. As an example of the method of calculation, let us consider the solution labelled actinium II (see § 7). The average rate of growth of radium per week corresponded to 0.26 divisions per minute of the electroscope; i.e., the equilibrium quantity of the emanation from the amount of radium produced per week would give that rate of movement of the electroscope.

The first measurement was made after an interval of four days, and the emanation from the solution gave 0.050 division per minute of the electroscope. Since $t_1 = 0$, the equation given above reduces to:

$$\left(q_0-\frac{q}{\lambda}\right)\left(1-e^{-\lambda t_2}\right)+qt_2=0.050.$$

For convenience, we shall take a week as the unit of time. Since the emanation is half transformed in 3.8 days,

$$\lambda = 1.28 \text{ (week)}^{-1}$$
; $q = 0.26 \text{ and } t_2 = \frac{4}{7} \text{ week}$.

Substituting these values, $q_0 = 0.02$; i.e., the emanation from the amount of radium initially present in the solution would give a rate of movement of the electroscope of only 0.02 division per minute—a just detectable quantity.

§ 4. New Experiments

A part of the contaminated actinium solution, previously mentioned, was chemically treated to free it from radium. For this purpose, ammonium sulphide was added; this precipitated the actinium and left the radium in solution. By two successive precipitations the greater part of the radium was removed. The precipitate was dissolved in hydrochloric acid, and the radium content of the solution tested at intervals. The quantity of radium initially present in the solution (called actinium I) was found from the first observation by the method already described. The results are given in the following table. Column I gives the interval since the preparation of the solution; column II the time of collection of the emanation; column III the observed movement of the gold-leaf in scale divisions per minute due to the emanation in the solution. In column IV is given the value of q, the average quantity of radium produced per week, calculated from equation (1) and expressed in terms of divisions per minute of the electroscope. The value of q is calculated from each observation on the assumption that the rate of growth has been constant since the preparation of the solution. In column V is given the value of qt, the amount of radium present in the solution at the time of testing.

The amount of radium initially present corresponded to 3.95 divisions per minute.

The results are shown graphically in Fig. 1 in the curve marked

Actinium I. The ordinates represent the amount of radium present in scale divisions per minute (taken from column V), and the abscissae time in days. It will be seen from the curve and also from column IV that the rate of growth of radium is constant within the limit of experimental error over

Growth of Radium in Actinium I T П ш IV V Time in Time of collec- Observed movement days tion in days of electroscope q qt 4 4 $2 \cdot 13$ 0.560.3211 7 3.33 0.580.91 18 7 3.740.581.4925 7 $4 \cdot 12$ 0.562.0032 7 4.530.56 $2 \cdot 56$ 38 6 4.650.60 $3 \cdot 26$ 53 15 $7 \cdot 25$ 0.55 $4 \cdot 16$ 82 29 10.1 0.566.56 121 7 9.360.549.33

Mean value $\dots 0.566$





the time of observation, viz. 121 days. In that interval, the amount of radium in the solution has increased $2 \cdot 36$ times the initial value. The equilibrium

amount of emanation from a standard solution of 10^{-9} gr. of radium gave $11 \cdot 5$ divisions per minute in the electroscope. The rate of growth of radium in the solution thus was $4 \cdot 9 \times 10^{-11}$ gr. per week, and assuming the rate of growth constant, $2 \cdot 55 \times 10^{-9}$ gr. per year.

§ 5. Activity Measurements

In order to follow the results of the chemical operations, the activity due to a definite fraction of the solution was examined over a long interval. 1/2,000 of the solution was taken and evaporated to dryness on a watch-glass. This gave an extremely thin film of active matter from which the α rays escaped with little absorption. The α ray activity of this film was tested in an α ray electroscope. The variation of activity is shown in Fig. 2, where



Fig. 2

the ordinates represent the activity in arbitrary units, and the abscissae time in days. It will be seen that the activity at first rises, passes through a maximum in a little over 20 days, and then gradually decays to a constant value about equal to that initially observed. This decrease of the activity after rising to a maximum shows clearly that some of the actinium had not been precipitated by the ammonium sulphide. In addition, the rise of activity to a maximum in about 20 days shows that an excess of the normal amount of radioactinium was removed with the part of the actinium precipitated. Most of the actinium was left behind in the filtrate. The radioactinium in

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the precipitate produced fresh actinium X and the density rose. The gradual fall of the activity to a constant value is due to the decay of the excess of the radioactinium together with its transformation products. The activity curve will be seen to be very similar to the curves given by Hahn (*Phil. Mag.*, January 1907) for cases where radioactinium is initially in excess. It will be seen that the activity curve is of great value for determining the effect of the chemical operation in removing the various products associated with actinium.

§ 6. Further Experiments with Actinium I

After an interval of 121 days, the solution was removed, precipitated with ammonia, and the growth of radium in the precipitated actinium again observed. The object of this experiment was to test whether this reagent was as effective as ammonium sulphide in removing radium from actinium, and also whether the substance that produced radium was precipitated completely with the actinium. Most of the radium was removed by a single precipitation, while the rate of growth of radium in the actinium was unaltered by the process. Such a result is of importance in showing that while radium is soluble in ammonia, the substance that produces it is not, but is precipitated with the actinium. The observation of the growth of radium in the solution has been tested over a further period of 184 days or 305 days in all. There has been no certain change in the rate of growth of radium in this interval.

§ 7. Experiments with Actinium II

The experiments with actinium I show that radium is produced at a constant rate in a normal actinium solution, but do not show whether radium is produced from actinium itself or from another substance ordinarily separated with the actinium. The products of actinium in order of sequence are radio-actinium, actinium X, emanation, actinium A and actinium B, with periods of half transformation of 19.5 days, 10 days, 3.9 sec., 34 min. and 1.5 min., respectively. Is radium the final product of actinium, i.e. is radium formed from actinium B? There are two methods of attacking this question:

- (1) To examine whether the active deposit of radium (composed of actinium A and B) produces radium; or
- (2) To test whether the rate of growth in actinium is initially altered by the removal from it of actinium X or radioactinium.

Experiments using the method (1) are described later in § 9 of this paper, but we shall here only consider the second method. If radium is produced directly from actinium B, the rate of production of radium should be nearly proportional to the amount of actinium X, since after a few hours the rapidly transformed products actinium A and B are in equilibrium with it. Consequently, if actinium X is removed from an actinium solution in equilibrium, the rate of growth of radium in the latter should at first be very small, but should gradually increase as fresh actinium X is formed. Since the half period of actinium X is 10 days, the rate of growth of radium should reach half value in 10 days and be within less than 2 per cent of the final constant value in about 60 days.

In order to test this point accurately, it is necessary that the solution under examination should initially contain an amount of radium small compared with the growth per week. In the case of actinium I, the initial content of radium was too large to be quite certain that the rate of growth in the first few days was identical with that determined later.

A part of the actinium preparation of weight 0.32 gr., discussed in § 2, was used for this purpose. It was dissolved in hydrochloric acid and twice precipitated with ammonium sulphide and finally with ammonia, in order to be sure that the actinium X as well as the radium was almost completely removed. These operations were successful, for a solution of actinium was obtained whose initial content of radium corresponded to only 0.02 division per minute in the electroscope, while the rate of growth per week was 0.26 division. A careful examination of the radium content was made at successive intervals of four days until it was clear that the rate of growth was constant. The results are shown in the following table, and are arranged in the same way as the results in the table for actinium I.

I Time of preparation 4 days		II Time of collection 4 days		III Observed movement of electroscope 0.05 div. per min.		IV q in divs. per min. 0.26	V qt in divs. per min. 0.15
12	"	4		0.194	22	0.24	0.41
16	,,	4	,,	0.265	,,	0.24	0.55
22	,,	6	,,	0.443	"	0.23	0.72
33	,,	11	,,	0.965	"	0.27	1.27
40	,,	7	,,	1.06	,,	0·275	1.57
50	,,	10	,,	1 · 44	"	0.255	$1 \cdot 82$
60	,,	80	,,	1.85	"	0.27	2.32
111	,,	51	••	3.93	**	0.259	4.11

The amount of radium at different times is shown graphically in Fig. 1 (curve actinium II), drawn on the same scale as the curve for actinium I. It will be seen from column IV that the rate of production of radium per week, within the limits of experimental error, is constant over the whole interval of 111 days. If radium had been produced directly from actinium B, the rate of growth observed at an interval of 8 days should have been about 0.11, or less than half that actually observed. We may thus conclude that (1) radium is not produced directly from actinium B, and (2) that if radium

is produced directly from actinium, a product of slow transformation must exist between actinium B and radium.

On account of my departure from Montreal, the experiments were stopped after 111 days. The electroscope was removed and set up in Manchester and the solution tested at intervals. The results indicate that the rate of growth is still the same after a period of 246 days.

§ 8. Experiments with Actinium III

The variation of the α ray activity of a thin film of the solution actinium II is shown in Fig. 2 on the same scale as that of actinium I. It will be seen that the variation of the activity is very similar to that observed for actinium I, and is to be explained in a similar manner. As in the first case, only a part of the actinium was precipitated by the addition of ammonium sulphide. The residue of the actinium remained in the filtrates. After suitable treatment of the latter, ammonia was added in order to precipitate the remaining actinium. A very small precipitate was obtained which was not more than one hundredth of the weight of that initially obtained using ammonium sulphide. This small precipitate contained more than three quarters of the actinium in the original preparation, showing that, under the experimental conditions, a considerable concentration of the actinium had been effected. This small precipitate (called actinium III) was dissolved in hydrochloric acid and its activity examined. The variation of its activity is shown in Fig. 2 (curve actinium III). The ammonia removed most of the actinium X, while most of the radioactinium had been separated with actinium II. The activity consequently rapidly increased, due to the fresh production of radioactinium, and was still rising after an interval of 120 days. This curve is very similar in shape to that given by Hahn (Phil. Mag., June 1907) for the rise of α ray activity of actinium freed from all its products.

The solution actinium III was then tested to see if there were any growth of radium in it. The observations are shown in the following table:

Actinium III

I Time in days	II Divisions per minute of electroscope	III Divisions per minute in equilibrium
4	0 .071	0.140
8	0.074	0.148
12	0.077	0.154
19	1.07	0.151
46	1.39	0.139
64	1.30	0.135

Mean 0.144

Column II gives the observed rate of leak of the electroscope due to the emanation; column III the calculated rate of leak of the equilibrium amount of emanation, supposing there was no growth of radium in the solution. Considering the very small rate of leak to be measured, the differences between the numbers in column III are not greater than the experimental errors. The results thus clearly show that there is no certain growth of radium in the solution. If there is any growth of radium at all, it is certainly less than 0.02 of a division per minute over a period of 64 days. The growth of the radium per week, in the solution III, corresponds to 0.26 division or to 2.38 divisions in 64 days. For equal quantities of actinium, the growth of radium in solution III, is certainly less than 1/200 of that observed in solution II.

In order to make certain that the observed absence of growth of the radium cannot be ascribed to some chemical action, the solution, after 64 days, was removed and again treated with ammonia. The precipitate was dissolved in hydrochloric acid and again tested for growth of radium. By this treatment the initial content of radium was reduced from 0.14 to 0.04. No growth of radium was observed over a period of 20 days. The solution was then removed to Manchester and tested at intervals over a further period of 136 days. The growth in the total interval of 220 days is certainly less than 0.03 or not greater than 1/500 of the normal growth to be expected.

§ 9. Experiments with the Active Deposit

We have seen in § 7 that the observed constant growth of radium in a solution freed from actinium X shows that actinium B is not directly transformed into radium. This result has been confirmed by testing directly whether there was any growth of radium in the active deposit of actinium. The active deposit (actinium A and B) was concentrated on a platinum plate by keeping it negatively charged in the presence of the emanation from an active solid preparation of actinium. Four different experiments were tried in which a platinum plate was exposed for 4, 7, 7, and 14 days respectively. After exposure, the platinum plate was placed in a solution of hydrochloric acid to dissolve off the deposited matter, and the solution with the platinum plate in situ tested for radium. The first two experiments showed a just measurable quantity of radium, but with still greater precautions against radium contamination, the last two experiments showed no measurable amount. It may be of interest to consider briefly a method of calculating the amount of radium theoretically to be expected, if actinium B changes directly into radium. Suppose as the basis of calculation that the growth of radium in the solution actinium I (§ 4) is normal, and compare the amounts of actinium B in this solution and on the platinum plate. The α ray activity of the actinium and its products in the solution spread in a thin film corresponded to 9,900 divisions per minute in the α ray electroscope, while the activity

of both sides of the platinum plate, tested immediately after removal from the actinium emanation, was 3,800 divisions. Now the actinium in equilibrium contains 4 α ray products whose ranges are 4.8, 6.55, 5.8, and 5.5 cm. respectively, while the active deposit contains only one range of 5.5cm. Taking as a first approximation that the ionization due to an α particle from each product is proportional to its range in air, the solution contains an amount of active deposit corresponding to an activity of 2,400 divisions. Consequently, the amount of radium to be expected on the platinum plate is 1.6 of the growth of radium in the solution in the time of exposure of the platinum plate to the constant supply of actinium emanation. Now this solution grew per week an amount of radium corresponding to 0.56division per minute of the electroscope. Consequently, on this hypothesis, the platinum plate exposed for 2 weeks should contain an amount of radium corresponding to 1.8 divisions per minute. The actual amount observed was not more than 0.01 division, or less than 1/180 of the theoretical amount. Such a result conclusively shows that actinium B does not change directly into radium. It is not inconsistent, however, with the possibility that a slowly changing product exists between actinium B and radium. In such a case radium, in the course of time, should appear in the solution containing the platinum plate.

§ 10. Discussion of Results

In the following table are given the collected results of the experiments on the growth of radium in the various solutions. In column II is given the total activity of the preparations when in radioactive equilibrium, expressed in divisions per minute of the α ray electroscope. This activity was determined in each case by taking a definite small fraction of the preparation in solution and evaporating it to dryness on a watch-glass. Column III gives the observed rate of growth per week expressed in grams of radium. Column IV gives the rate of growth per year, on the assumption that it is constant

I	II	III	IV	V
Preparation	Total constant activity of prepara- tion	Rate of growth of radium per week	Rate of growth of radium per year	Relative growth of radium
Actinium I	9,900	$4\cdot9 imes10^{-11}$ gr.	$2\cdot 55 imes 10^{-9}$ gr.	1
Actinium II	3,000	$2 \cdot 3 \times 10^{-11}$ gr.	$1\cdot 2 imes 10^{-9}$ gr.	1.55
Actinium III	6,000	not measurable		not greater
0.32 gr. of ac- tinium tested over a period of 2.7 years	12,900	$5 \cdot 6 \times 10^{-11}$ gr.	$2.9 imes 10^{-9}$ gr.	0.97

over that interval. Column V shows the relative growth of radium for equal quantities of actinium, taking the rate of growth in the solution actinium I as unity.

It is seen from the above table that for equal amounts of actinium, the growth in actinium I is very nearly the same as for the 0.32 gr., examined over a period of 2.7 years. The closeness of the agreement is no doubt accidental on account of the uncertainty (see § 2) in regard to the initial content of radium in the 0.32 gr. of actinium preparation. The table brings out clearly the differences in the growth of radium in the three solutions for equal quantities of actinium.

The solution actinium II grows radium at 1.5 times the rate of actinium I, while solution III, if it grows radium at all, certainly does so extremely slowly.

These experiments can all be readily explained on the simple hypothesis that in ordinary actinium preparations there exists a new substance of slow change which is directly transformed into radium. This new substance is separated with the actinium from the mineral by the methods ordinarily employed for the removal of the actinium. It differs, however, in chemical properties both from radium and actinium, and by special chemical methods can be separated from them both. For example, the preparation actinium I may be considered as possessing the normal quantity of the radium producing substance; while the preparation II contains an excess over the normal. In the case of the solution II, the treatment with ammonium sulphide precipitated all this new substance, but only a fraction of the actinium. The filtrate consequently contained actinium, but no trace of the parent of radium. Under such conditions, there was relatively a large growth of radium in the solution II, but none in the solution III.

As far as the investigations have gone, there is no definite evidence whether this new substance is itself produced by actinium, or whether it is merely associated with the actinium in the same sense that barium always appears with the radium. In the former case, the solution III is gradually producing this new substance, and in the course of time there will be an appreciable growth of radium in it. In the latter case, the solution III will never show any growth of radium comparable with that ordinarily observed. It is not unlikely that this new substance is in reality an intermediate product in the direct line between uranium and radium, and has no direct genetic connexion with the actinium.

The observed constant growth of radium in the solution shows that the parent of radium has a slow rate of change. At a minimum estimate, its period cannot be less than several years and may be much longer.

Summary of Results

(1) Over the time of observation (305 days) radium is produced in actinium preparations at a constant rate.

(2) By suitable chemical treatment actinium preparations can be obtained which grow radium extremely slowly.

(3) The active deposit of actinium does not change directly into radium.

(4) The results indicate that in the ordinary actinium preparations there exists a new substance which is slowly transformed into radium. This direct parent of radium can be chemically separated both from actinium and radium.

(5) Observations have not extended over sufficient time to settle whether this direct parent of radium has any direct genetic connexion with actinium or not.

Experiments are in progress to devise more definite methods for separation and isolation of this new substance in order to examine its physical and chemical properties, and to determine its position in the long series of transformations of uranium.

Manchester September 20, 1907

A Method of Counting the Number of a Particles from Radio-active Matter

by professor E. RUTHERFORD, F.R.S., and H. GEIGER, PH.D.

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THE total number of α particles expelled per second from one gram. of radium has been estimated (Rutherford *Phil. Mag.*, Aug. 1905)* by measuring experimentally the total positive charge carried by the α rays from a thin film of radium, on the assumption that each α particle has the same charge as an ion produced in gases. If the α particle is an atom of helium it is necessary to assume that each α particle carries twice the ordinary ionic charge. The need of a method of directly counting the number of α particles shot out from radio-active matter has long been felt in order to determine with the minimum of assumption the charge carried by the α particle and also the magnitude of other radio-active quantities.

It can be calculated that an α particle expelled from radium produces about 80,000 ions in a gas before its ionizing power is lost. With very sensitive apparatus, it should be just possible to detect the ionization produced by a single α particle by electrical methods. The effect, however, would be small and difficult to measure with accuracy. In order to overcome this difficulty, we have employed a method which automatically increases the ionization produced by an α particle several thousand times and so makes the electrical effect easily observable with an ordinary electrometer. This is done by making use of the property discovered by Townsend, that an ion moving in a strong electric field in a gas at low pressure, produces a number of fresh ions by collision with the gas molecules. If the electric field is adjusted nearly to the value required for the passage of the spark, a single ion generated in the gas by external agencies, produces in this way several thousand fresh ions by collision. In the experimental arrangement, the testing vessel consists of a brass tube 60 cms. in length, along the axis of which passes a thin insulated wire attached to the electrometer. With a gas pressure of about 2 cms. a potential difference of about 1,000 volts between the brass tube and the wire is required. The α particles are fired down the tube through a small hole at the end of the tube about 2 mms, in diameter covered with a thin plate of mica. In order to use a narrow pencil of α rays, the active matter in the form of a

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thin film on a surface about one square cm. in area is placed in an exhausted tube which is a prolongation of the testing vessel. The distance of the active matter from the hole is usually between 50 and 75 cms. and the amount of active matter adjusted so that from six to ten α particles are fired through the hole per minute. The effect of the α particle entering the testing vessel is shown by a sudden throw of the electrometer needle. Under good conditions this throw is about 50 divisions using an electrometer which has a sensibility of 300 divisions per volt. By observing the number of throws of the electrometer needle, we can count the average number of α particles shot through the opening per minute. The total number fired out by the active matter can be calculated from the known area of the opening and the distance of the latter from the active matter. Preliminary observations show that the number of α particles counted by this method is of the same order as the calculated number, but special experiments are in progress to determine with accuracy the value of this important constant. By counting at intervals the number of α particles expelled per minute, we have been able to obtain the curves of decay of activity of a plate coated with radium C or actinium B.

The α particles from a constant source are shot out at irregular intervals. The time interval between the entrance of successive α particles has been observed over a long interval, and the results show that the distribution curve with time is similar in general shape to the probability curve of distribution of the velocity of molecules in a gas. Further observations, however, are in progress to determine the distribution curve with the accuracy required for comparison with the mathematical theory. From Nature, 77, 1908, pp. 422-6 A discourse delivered at the Royal Institution on Friday, January 31, 1908

IN 1904 I had the honour of giving an address at the Royal Institution on the subject of radio-activity. In the interval steady and rapid progress has been made in unravelling the tangled skein of radio-active phenomena. In the present lecture I shall endeavour to review very shortly some of the more important advances made in the last few years, but as I cannot hope to mention, even briefly, the whole additions to our knowledge in the various branches of the subject, I shall confine my attention to a few of the more salient facts in the development of which I have taken some small share.

In my previous lecture I based the explanation of radio-active phenomena on the disintegration theory put forward in 1903 by Rutherford and Soddy, which supposes that the atoms of the radio-active bodies are unstable systems which break up with explosive violence. This theory has stood the test of time, and has been invaluable in guiding the experimenter through the maze of radio-active complications. In its simplest form, the theory supposes that every second a certain fraction (usually very small) of the atoms present become unstable and explode with great violence, expelling in many cases a small portion of the disrupted atom at a high speed. The residue of the atom forms a new atomic system of less atomic weight, and possessing physical and chemical properties which markedly distinguish it from the parent atom. The atoms composing the new substance formed by the disintegration of the parent matter are also unstable, and break up in turn. The process of degradation of the atom, once started, proceeds through a number of distinct stages. These new products formed by the successive disintegrations of the parent matter are in most cases present in such extremely minute quantity that they cannot be investigated by ordinary chemical methods. The radiations from these substances, however, afford a very delicate method of qualitative and quantitative analysis, so that we can obtain some idea of the physical and chemical properties of substances existing in an amount which is far below the limit of detection of the balance or spectroscope.

The law that governs the breaking up of atoms is very simple and universal in its application. For any simple substance, the average number of atoms breaking up per second is proportional at any time to the number present. In consequence, the amount of radio-active matter decreases in a geometrical progression with the time. The 'period' of any radio-active product, *i.e.* the time for half the matter to be transformed, is a definite and characteristic property of the product which is uninfluenced by any of the laboratory agents at our command. In fact, the period of any radio-active product, for example, the radium emanation, if determined with sufficient accuracy, might well be taken as a definite standard of time, independent of all terrestrial influences.

The law of radio-active transformation can be very simply and aptly illustrated by an hydraulic analogy. Suppose we take a vertical cylinder filled with water, with an opening near the base through which the water escapes through a high resistance.* When the discharge is started the amount of water escaping per second is proportional to the height of water above the zero level of the cylinder. The height of water decreases in a geometrical progression with the time in exactly the same way as the amount of radio-active matter decreases. We can consequently take the height of the column of water as representing the amount of radio-active matter A present at any time. The quantity of water escaping per second is a measure of the rate of disintegration of A and also of the amount of the new substance B formed per second by the disintegration of A. The 'period' of the substance is controlled by the amount of resistance in the discharge circuit. A high resistance gives a small flow of water and a long period of transformation, and vice versa. By a suitable arrangement we can readily trace out the decay curve for such a case. A cork carrying a light vertical glass rod is floated on the water in the cylinder. A light camel's hair brush is attached at right angles, and moves over the surface of a smoked-glass plate. A vertical line drawn on the glass through the point of contact of the brush gives the axis of ordinates, while a horizontal line drawn through the brush when the water has reached its lowest level gives the axis of abscissæ. If the glass plate is moved with uniform velocity from the moment of starting the discharge a curve is traced on the glass which is identical in shape with the curve of decay of a radio-active product, where the ordinates at any time represent the relative amount of active matter present, and the abscissæ time. With such an apparatus we can illustrate in a simple way the increase with time of radio-active matter B, which is supplied by the transformation of a substance A. This will correspond, for example, to the growth of the radium emanation with time in a quantity of radium initially freed from emanation. Let us for convenience suppose that A has a much longer period than B. In the hydraulic analogy A is represented by a high head of water discharging at its base through a circuit of high resistance into the top of another cylinder representing the matter B. The water from the cylinder B escapes at its base through a lower resistance. Suppose that initially only A is present. In this case the water in the cylinder B stands at zero level. On opening the stop-cock connecting with A, water flows into B. The rise of water with time in the cylinder B is traced out in the same way as before by moving the glass plate at a constant rate across the tracing brush. If the period of A is very long compared with that of B the water is supplied to B at a constant rate, and the water in B reaches a constant maximum height when the rate of supply to B equals the rate of escape from the latter. The curve traced out in that case is identical in shape with the 'recovery curve' of

* A short glass tube in which is placed a plug of glass wool is very suitable.

a radio-active product supplied at a nearly constant rate. The quantity of matter reaches a maximum when the rate of supply equals its own rate of transformation. The relative height of the columns of water in A and B represents at any time the relative amounts of these substances present.

If the period is comparable with that of B, the height of water in B after reaching a maximum falls again, since as the height of A diminishes the supply to B decreases. Ultimately, the height of B will decrease in a geometrical progression with the time at a rate corresponding to the longer period of the two. This is an exact illustration of the way the amount of a radio-active substance B varies when initially only the parent substance A is present. By using a number of cylinders in series, each with a suitable resistance, we can in a similar way illustrate in a quantitative manner the variation in amount with time of a number of products arising from successive disintegrations of a primary substance. By suitably adjusting the amount of resistance in the discharge circuits of the various cylinders, the curves could be drawn to scale to imitate approximately the variation in amount of the various products with time when the initial conditions are given.

During the last few years a very large amount of work has been done in tracing the remarkable succession of transformations that occur in the various radio-active substances. The known products of radium, thorium, actinium, and uranium are shown graphically below, together with the periods of the products and the character of the radiations they emit. It will be seen that a large list of these unstable bodies are now known. It is probable, however, that not many more remain to be discovered. The main uncertainty lies in the possibility of overlooking a product of rapid transformation following or succeeding one with a very slow period. In tracing out the succession of changes, the emanations or radio-active gases continuously evolved by radium, thorium, and actinium have marked a very definite and important stage, for these emanations can be easily removed from the radio-active body and their further transformations studied quite apart from the parent element. The analysis of the transformation of the radium emanation has vielded results of great importance and interest. After passing through three stages, radium A, B, and C, of short period, a substance, radium D, of long period, makes its appearance. This is transformed through two stages E and F of short period into radium G, of period 140 days. Meyer and Schweidler have conclusively shown that radium D is the primary constituent of the radioactive substance separated by Hofmann and called by him radio-lead. Radium G is identical with the first radio-active substance separated from pitchblende by Madame Curie, viz. polonium. We are thus sure that these bodies are transformation products of radium. It will be seen that I have added another product of period 4.5 days between radium D and polonium. The presence of such a product has been shown by Meyer and Schweidler.

In the case of thorium, a very long list of products is now known. For several years thorium X was thought to be the first product of thorium, but Hahn has recently shown that at least two other products of slow trans-