The Rutherford Memorial Lecture.

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Much has already been written about Rutherford and much more will be written in times to come. It is not my purpose in this lecture to try to describe, much less to assess, his work as a whole, which would be beyond my powers, but rather to sketch his influence on the progress of chemistry and to leave on record a true impression of his character as well as of his genius. I shall deal at some length with those aspects of his scientific work which were of special importance to chemistry. I shall confine myself mainly to Rutherford's early life and work. The later part of his life is fresh in our memories, but we are apt to forget the way by which he reached his commanding position. Further, it was the early part of his work which relaid the foundations of chemistry.

Many members of my audience will understand and sympathise when I say that I approach the task with a sincere and profound feeling of inadequacy. I never worked under Rutherford, never shared in the smallest degree in his triumphant progress. I am merely one of many who were given his friendship, who talked and laughed with him, and who were influenced as much by his sane, unselfseeking, and happy outlook on life as by his joy and success in his work.

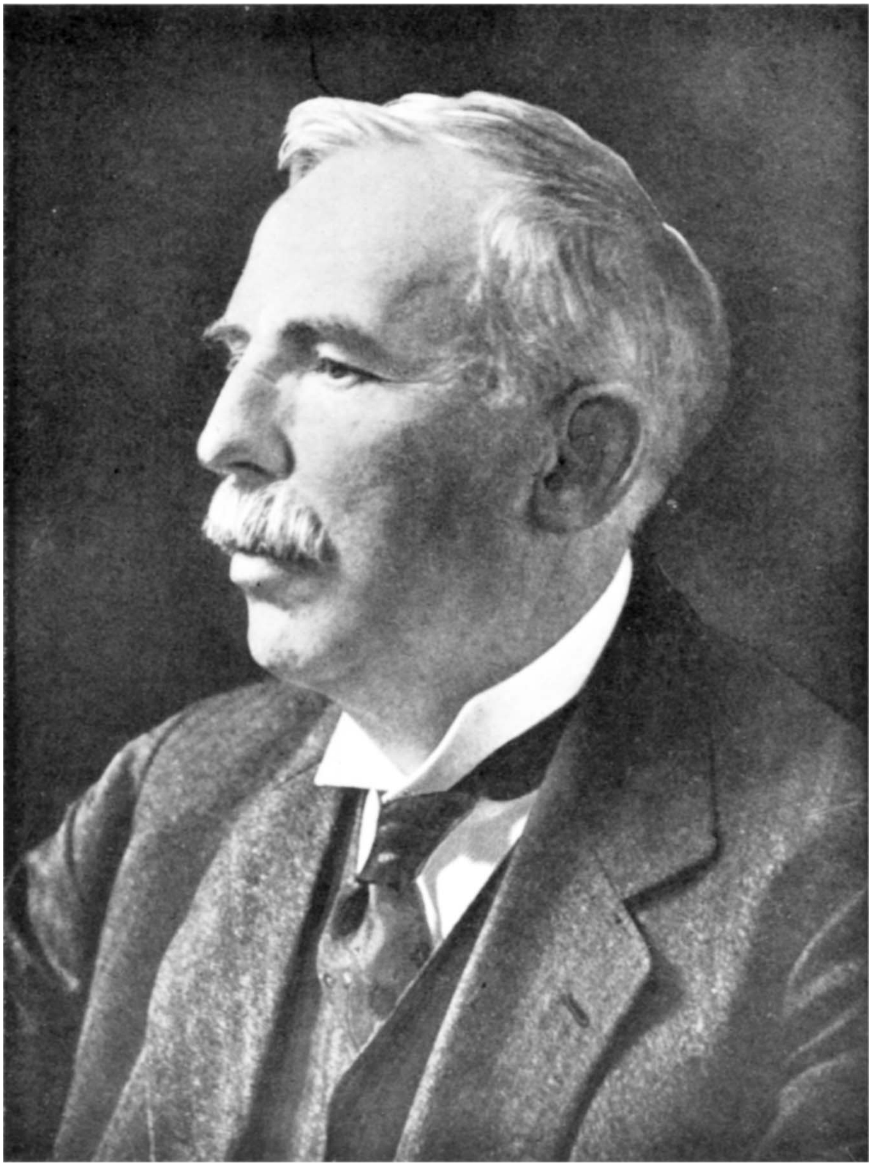
In preparing this lecture I had the curiosity to look up the first Faraday Lecture delivered in 1869 by Dumas. I found these words: " His hand, in the execution of his conceptions, kept pace with his mind in designing them; he never wanted boldness when he undertook an experiment, never lacked resources to ensure success, and was full of discreetness in interpreting results. His courage, which never flinched when once he had undertaken a task, and his caution, which felt its way carefully in adopting a received conclusion, will ever serve as models for the experimentalist."

That might equally be said of Rutherford. Which was the greater, Rutherford or Faraday, is a matter of no importance. It is enough that we put them together.

In one of his lectures Rutherford said: " In assessing the merit of any scientific discovery, it is always of much importance to view it against the background of the knowledge, and of the instrumental and technical facilities available at the time of the discovery." Let us spend a few minutes in getting a background to Rutherford's work in this way. Physics, at the beginning of the last decade of the nineteenth century, seemed to be almost finished. All the foundations had been laid; the impressive structure rested on Newton's laws, and on the fundamental laws of thermodynamics. In his Presidential address to the British Association in 1909 J. J. Thomson referred to " the pessimistic feeling, not uncommon at that time (20 years ago) that all the interesting things had been discovered, and all that was left was to alter a decimal or two in some physical constant." There were, it is true, some men who were interested in very low temperatures, and high vacua, but no one thought that these experiments would lead to a fundamental revision of first principles. Johnstone Stoney, and, more forcibly, Helmholtz, had pointed out that if the atomic hypothesis were accepted it necessarily followed from Faraday's work that electricity also was atomic in nature; but no great attention was paid to this, and there was not the slightest evidence of the nature of an atom of electricity, in spite of Crookes's speculation about a fourth state of matter. In a text book on electricity which was used for the sixth form in my School as late as 1902 there is no reference at all to the atomic nature of electricity. We spent much of our time at school studying the vagaries of the electrophorus and learning, so to speak, how to pour the positive or negative fluid of electricity from one tin can to another.

Chemistry was not so stagnant, for the natural reason that a science which was predominantly experimental in nature, and which had at its disposal some seventy elements to experiment with, could hardly ever be supposed to be coming to an end. Chemistry had also received a great stimulus from the development, by Arrhenius, of the electrolytic dissociation theory, and from van't Hoff's application of thermodynamics to chemical reactions. A new branch of the science, physical chemistry, had sprung up, and there were many active investigators, particularly in Germany, busy in uniting two sciences which had for a long time developed independently. But the history of chemistry in the nineteenth century is, in the main, the history of the development of the atomic theory, of the theory of valency, and of the periodic classification of the elements, which was begun in 1869 and later developed so brilliantly by Mendeléeff to systematise the science and to point the way to new discoveries. By 1890 the periodic system was well established, and atomic weights were known to a high degree of accuracy. But there were two alarming blots upon the system. Neither nickel nor tellurium fell into their right places. Nickel could perhaps be ignored, but tellurium was a bad blot. Its atomic weight was higher than that of iodine, not lower as it should have been. An immense amount of time and labour was spent in trying to prove otherwise, but in vain. Even as late as 1912 the question of the homogeneity of tellurium was still regarded as open. We may, I think, count it greatly to the credit of chemists that, in spite of the strongest temptation, experimental accuracy triumphed over theoretical conclusions.

The position of the atomic theory in 1894 was well summed up by Lord Salisbury in his Presidential Address to the British Association at Oxford. He said: " Of the scientific enigmas which still, at the end of the nineteenth century, defy solution, the nature and origin of what are called the elements is the most notable. It is not perhaps easy to give a precise logical reason for the feeling that the existence of our sixty-five elements is



Rutherford

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a strange anomaly and conceals some much simpler state of facts. But the conviction is irresistible. . . . Many have been the attempts to solve this enigma; but up to now they have left it more impenetrable than before. . . . The theory was advanced that all these (atomic) weights were multiples of the weight of hydrogen—in other words, that each elementary atom was only a greater or a smaller number of hydrogen atoms compacted by some strange machinery into one. . . . But the reply of the laboratories has always been clear and certain—that there is not in the facts the faintest foundation for such a theory. . . . What the atom of each element is, whether it is a movement or a thing, or a vortex, or a point having inertia, whether there is any limit to its divisibility, and if so, how that limit is imposed, whether the long list of elements is final, or whether any of them have any common origin, all these questions remain surrounded by a darkness as profound as ever. The dream which lured the alchemists to their tedious labours, and which may be said to have called chemistry into being, has assuredly not been realised, but it has not yet been refuted."

The laboratory equipment available for the experimental physicist at the end of the nineteenth century was, judged by modern standards, crude in the extreme. There were no efficient accumulators available, no high-tension batteries, no convenient instruments for measuring voltage and current, no fast diffusion pumps, no valves, no Wilson cloud chamber, which Rutherford described as " that most original and wonderful instrument in scientific history." The early *X*-ray tubes were pumped out slowly and with hard labour by Toepler pumps. In his first experiments in New Zealand in 1893, on the magnetic properties of iron, Rutherford had to start every day by preparing a battery of Grove cells. Electrometers were then unreliable, capricious, and exasperating to work with. In his first book on radioactivity in 1904 Rutherford devotes the best part of a chapter to a description of the construction of electrometers and of methods of using them to ensure accuracy. All these things have to be remembered in reviewing his researches and the advance of knowledge in our time.

This then was the state of affairs when Rutherford came to England in 1895 with an 1851 Exhibition. He was then twenty-four years of age, robust, full of energy and confidence, and endowed with a fighting spirit that never left him. He arrived with one great advantage over Faraday; he had had a good education. One is tempted to add that he had an equally great advantage over many other people then and now; he had not had too good an education. He had been taught science at Canterbury College by a man who was completely unorthodox, who, it would not be unfair to say, did not know very much, but who was convinced that there was a great deal yet to be known and discovered. Bickerton must surely have credit for stimulating, if not for moulding, the genius of Rutherford, whom he left alone to pursue his own experiments on the detection of Hertzian waves in a miserable, cold, draughty, concrete-floored cellar, which was usually known to students as the " den," and in which they were accustomed to hang up their caps and gowns. One of the most famous of contemporary men of science once said to me : " If you ever think of investigating anything, don't start by looking up the literature on the subject. If you do, you will probably come to the conclusion that everything is known about it." Rutherford had little chance in New Zealand of " looking up the literature." Later in life it might almost be said that there was no necessity for him to look up the literature; he made it for himself as he went along.

He arrived in Cambridge just in time to be the first research student at the Cavendish Laboratory under a new statute allowing graduates of other Universities to become eligible for a research degree after two years' work at the University. Other young graduates there at the time or a little later were J. S. Townsend, C. T. R. Wilson, McClelland, Langevin, and H. A. Wilson. There is a group photograph of them in the Cavendish Laboratory, grim, camera conscious, and moustachioed. Contemporary memories of Rutherford are to the effect that he had the usual difficulties of a newcomer, and of a new-fangled newcomer at that, and got over them by force of character and good nature; that he worked very hard, but liked to stroll round the Laboratory and see what the other people were doing, and help them if he could; and that his own apparatus generally looked like nothing on earth, but worked. A delightful and revealing picture of his doings and ambitions at Cambridge is given in a series of letters written to his future wife, Mary Newton.

Rutherford had not been in Cambridge for long before it became quite clear, in the mature judgment of his professor and of others, that he was a man of quite exceptional originality and powers of mind. He wanted no directions; only advice and encouragement, which he got in full measure. He continued his researches on the detection of electrical waves, and developed a magnetic detector with which, within a year, he was able to receive signals at a distance of half a mile. He described and demonstrated this at the meeting of the British Association at Liverpool in 1896. Marconi, who was present, and who had been experimenting with a vertical aerial on Salisbury Plain, afterwards improved Rutherford's detector for practical use. I mention the episode, not because it has any particular importance to chemists, but because of its historical interest in indicating the probable trend of Rutherford's career if a greater fate had not been reserved for him. As it was, the discovery of *X*-rays and of the radioactivity of uranium within a few months of his arrival in England determined his life's work, and ushered in what he afterwards called the heroic age of physics.

In the early part of 1896 Rutherford started working with Thomson on the ionisation of gases by *X*-rays. In October 1896 he wrote : " I am working very hard in the Lab., and have got on to what seems to me a very promising line—very original needless to say. I have some very big ideas which I hope to try, and these if successful would be the making of me. Don't be surprised if you see a cable some morning that yours truly has discovered half a dozen new elements, for such is the direction my work is taking."

Soon after the publication of Thomson's researches on the cathode rays there appeared a paper by

Rutherford on the velocity and rate of recombination of the ions of gases exposed to *X*-rays. The great interest of this paper now is that it contains the essence of the methods by which he subsequently unravelled the mystery of radioactivity. He then went on to examine by similar methods the electrical conduction caused by uranium radiation. It had already been observed that *X*-rays are in general complex, and include rays of widely different penetrating properties. In the simplest possible way Rutherford showed that the radiation from uranium consisted of at least two types: one, which he called the alpha ray, which was very readily absorbed; and another, which he called the beta ray, of a more penetrating character.

In 1898, at the early age of 27, he was appointed Professor of Physics at McGill University, Montreal, and sailed for Canada in September, passing rich on £500 a year. By the time he left, the discovery of the electron by J. J. Thomson had provided the first definite indication that all the different kinds of matter might have a common origin.

The tale of the next few years of researches on radioactivity reads like a good detective story. Indeed it was a detective story, coming out in parts, mainly in the Philosophical Magazine, the successive numbers of which were almost snatched from the hands of the postman and read with breathless interest. False clues there were in plenty, and led many men astray; wild theories were put forward; but the great man disclosed no theory until he had ascertained the facts, and followed with unerring instinct the clue of the thorium emanation, and the clue of the alpha particle. And in the end all was made clear, and the lookers-on said: "How simple, after all. Why didn't we think of that ourselves?" At least that is what the younger lookers-on said; the older ones said: "How far fetched."

When Rutherford went to Canada the origin of the radiation emitted by uranium compounds was indeed a complete mystery, and remained so until he and Soddy put forward the famous disintegration theory in 1902. Opinions ranged from the vague view that it was a kind of phosphorescence to Crookes's fantastic suggestion that uranium had the property of "throwing off the slow moving molecules of the atmosphere while the quick moving molecules have their energy reduced." This quite natural confusion of thought, shared by men of deservedly high scientific reputations, should be remembered when we try to recall the years of frenzied, arduous, and forceful experimental work which elapsed before Rutherford arrived at the only conclusion which would satisfactorily account for all the diverse phenomena observed. It should be remembered, too, that although radium and polonium were discovered in 1898, some time elapsed before Rutherford was able to secure feeble preparations of radium only about one thousand times as active as uranium. It was not until 1903 that he managed to get 100 milligrammes of radium bromide which was about 75% pure.

I suppose that if one were to ask students of Rutherford's work which of the one hundred and fifty or so scientific papers written by him, alone or with collaborators, was the best, one would get as many varied answers as if one asked lovers of Kipling which was their favourite story. I myself, speaking perhaps with the bias of a chemist, think that no papers are more illustrative of his genius, when we take into account his age, the baffling nature of the subject, and the equipment at his disposal, than those he published on thorium radiation within a year after his arrival at McGill. In a series of simple experiments he showed that thorium gave off a kind of gas or emanation, which could be blown along a tube by a slow current of air, and detected in a testing vessel by the conductivity it produced. He showed, too, that any solid object in contact with the emanation became temporarily radioactive, and that this temporary activity decayed to half its value in about eleven hours irrespective of the nature of the surface. If metal surfaces exposed to the emanation were charged negatively, this "induced" radioactivity tended to concentrate on them. He made a fine platinum wire very active in this way, and then showed that it lost most of its activity when dipped into dilute sulphuric acid. Finally he evaporated the sulphuric acid to dryness and found that the dry glass surface was strongly active. Hence the radioactivity was not destroyed, and the only satisfactory explanation of the observations was that thorium continuously produced a minute quantity of radioactive gas, which then deposited particles of matter of a different degree of radioactivity and with different chemical properties on surfaces with which it came in contact. It was this investigation that really gave him the clue to his subsequent work which led to the disintegration theory; and this was the first time in which chemical experiments were made with a quantity of matter less than one-billionth of that which could be detected by the most delicate balance. How simple, and how beautiful. To read the papers after all these years still gives one a thrill.

Dr. Johnson once advised Boswell that a biographer should amongst other things endeavour to give an author's opinion of his own work. I can give you two opinions of Rutherford's on his work at the time. The first was written in December 1899: "I sent off on Thursday another long paper for the press which is a very good one, even though I say so, and comprises a thousand new facts which have been undreamt of . . . suffice it to say that it is a matter of considerable scientific moment." Towards the end of life he said on one occasion: "I've just been reading some of my early papers, and, you know, when I'd finished, I said to myself, Rutherford, my boy, you used to be a damned clever fellow."

Soon after the publication of this paper Soddy arrived in Montreal as a lecturer in the Department of Chemistry, and began that fruitful association with Rutherford which was to end in results so surprising and at first so unacceptable to chemists. In a series of what now seem simple investigations they showed that radioactivity must be an atomic phenomenon accompanied by the continuous production of new types of matter with distinctive chemical properties. The only reasonable explanation was that radioactive elements must be undergoing spontaneous transformation. In the light of their results they put forward the suggestion that the presence of helium in minerals containing uranium and thorium must be connected with their radioactivity. There was no evidence at this stage to identify the alpha particle with helium. Soddy went back to England in 1903, and he and Ramsay conclusively proved, by spectroscopic evidence, that helium was a product of the disintegration of radium emanation.

Rutherford and Soddy's work was predominantly chemical in character. Indeed this is true of a large part of Rutherford's researches in Canada. The electrometer was merely the physical instrument used to detect and identify infinitesimal quantities of different kinds of radioactive matter. It was not therefore at all inappropriate that when he received the Nobel Prize in 1908 it was given for his researches in chemistry. Rutherford always professed himself highly entertained by this; but he handsomely said that he had no objection to being regarded as a chemist provided his chemical friends did not mind.

Needless to say the revolutionary theory of the spontaneous disintegration of atoms did not escape severe criticism. At Montreal it was murmured that Rutherford was bringing discredit on the University. In England Kelvin headed a band of lesser men in expressing his disapproval. Even Becquerel and Curie, who were far more entitled to express an opinion, were critical. Some years later, in 1907, Smithells expressed the opinion of many chemists when he said: " There is an uneasy feeling that developments of great importance to chemists are being made by experiments on quantities of matter of almost inconceivable minuteness." Rutherford answered the sceptics politely but forcefully. This was one of the few occasions when he troubled to answer criticisms in print. His avoidance of public controversy was one of his most remarkable characteristics; we can all imitate him in that, even if we cannot aspire to his genius.

After his first lecture at the Royal Institution in 1904 Rutherford was invited to Terling by Lord Rayleigh to meet Kelvin. Kelvin talked radium most of the week-end but would not pay attention to Rutherford's views. Rutherford had, however, a strong supporter in Strutt, the present Lord Rayleigh, who laid a modest bet of five shillings with Kelvin that he would live to change his mind. Kelvin paid the five shillings after the meeting of the British Association in 1904; rather prematurely, it appears, because two years later he wrote in a letter that " the disintegration of the radium atom is wantonly nonsensical." However, in the end he was completely converted.

After this lapse of time we can at least be as gentle with Rutherford's critics as he was, merely noting this one more striking example in the history of science, as of other subjects, how men who have had established doctrines dinned into them in youth, and have spent their lives in studying and trying to improve them in detail, violently resist a complete change in their habits of mind forced on them by a younger generation. Let us resolve to be on our guard as we ourselves grow old. What is noteworthy is the way Rutherford dealt with the situation. He suppressed his natural impatience with those who were so blind because they would not see, marshalled all his evidence, direct and circumstantial, with the skill of a great lawyer, and drove his points home one by one until he got a unanimous verdict. Later on, when he was at the height of his fame, I think we got to regard Rutherford as someone to whom Nature had imparted her secrets in a mysterious way, and that as he knew the answer to any problem beforehand it was comparatively easy to devise the experiment to prove it. This is far from the truth. He had to grope his way like anyone else, and his consistent success was due in the main to hard work and brilliant experiment; to his exceptional insight and imagination he added an infinite capacity for taking pains. He himself said on one occasion that " if you look at my work you will see that any success I have had has been due to my continually trying to press forward experimental methods little by little in every possible direction." These may not be his exact words but they express what he meant. No one who reads again his papers, or the successive editions of his books on radioactivity, or his classic Bakerian Lectures, or indeed any of his masterly reviews of progress from time to time, can fail to realise the truth of this remark.

Take for example the history of the alpha particle. In the ten years between 1899 and 1909 Rutherford published over twenty papers which dealt mainly if not entirely with the nature and properties of alpha rays. He came early to the conclusion that the alpha rays consisted of material particles projected with great velocity. He thought that they were charged particles, but would not commit himself. He made experiments to determine whether they were deviated in a magnetic field, but got at first negative results, and they were referred to for some time as the non-deviable rays. In 1902 he secured a stronger preparation of radium, and with an improved apparatus and technique showed that they were deflected by a strong magnetic field, and that they were positively charged. On the reasonable assumption that each particle carried one unit of positive electricity it followed that the mass was about twice that of the hydrogen atom, and therefore that if the alpha particle consisted of any known kind of matter it must be either hydrogen or helium. But still he was not satisfied. In 1905 he wrote to Boltwood: " I feel sure that helium is the alpha particle of radium and uranium products, but it is going to be a terrible thing to prove definitely. . . . It may conceivably be a hydrogen molecule, half atom of helium, or helium atom with two charges, and nothing but a pure scientific nose can say with certainty that one is more probable than the others. My nose (which may be prejudiced) leads me to avoid the H molecule like the devil." In 1907, after two more years' work, he was still uncertain and wrote to Hahn: " It may yet turn out that the alpha particle is hydrogen, and that helium comes from a rayless product. . . . The whole problem is very mixed."

In 1908, when he was at Manchester, he finally succeeded in counting alpha particles one by one with the Geiger counter, and in determining the number projected from radium in a given time, and the total charge, from which the charge on each particle was deduced. It was clear then that the charge was one of two units of positive electricity, and that the particle must have a mass of four. Then he got finally a direct experimental proof by firing alpha particles through a thin glass-walled tube into a vacuum, and showing that helium was produced outside.

By the time Rutherford left Canada for Manchester in 1907 the general nature of radioactivity had been made clear, the disintegration theory was well established, and the long series of changes in uranium, thorium, and actinium was well understood. It had been shown almost certainly that lead was the final product of disintegration (an assumption which, said Rutherford in 1905, will make even the metaphysicians dizzy), and over twenty new radioactive elements of short life had been discovered. Some chemists were busy trying to fit them in to the periodic table; with considerable ingenuity places were found for some sixteen of them, but the rest had to be regarded as pseudo elements—another example of the usefulness of a knowledge of the Classics when we wish to cloak our ignorance. Within two more years it was established that uranium-X and thorium were chemically indistinguishable, that radium-D could not be separated from lead, and that there were many other cases which indicated that elements of different atomic weights could have identical chemical properties. The observations led finally to Soddy's theory of isotopes and to the displacement law, which showed definitely that the properties of the elements were not primarily determined by atomic weights, but that some broader generalisation was necessary.

The happiest part of Rutherford's life was spent at Manchester, if one can say that of a life that was always happy. He was welcomed with open arms by the University. He had many friends outside the University : the prominent business men of Manchester, then, as now, were brought up in a liberal atmosphere, had strong cultural interests, and did not judge other men solely by their salaries. The South African War was a thing well of the past, income tax had been lowered to 1/- from the unprecedented level of 1/3 in 1902, and there was every prospect of long years of peace and prosperity. Rutherford found a well equipped laboratory, a prince of research assistants in Geiger, and of laboratory stewards in Kay, and he soon gathered round him a team of able and enthusiastic younger workers. It was at Manchester that the most dramatic event of his scientific career happened—the discovery of the real nature of the atom. Rutherford was fond of telling the story. He told it again in his last lecture at Cambridge before his death, in that delightfully intimate way in which he used to talk when he felt quite at home with his audience. Marsden had been told to see if he could detect if alpha rays could be scattered through large angles when they were projected at thin sheets of metal. " I may tell you, in confidence," said Rutherford, " that I did not believe they would be." The unexpected happened, and two or three days later Geiger came along to say that some of the alpha particles had bounced backwards. " It was quite the most incredible event that has ever happened to me in my life. It was almost as incredible as if you fired a 15-inch shell at a piece of tissue paper and it came back and hit you." Rutherford went away, and by mathematical processes which I suspect a good mathematician would think crude but which were effective and sound, showed that the observation could only be accounted for if the greater part of the mass of an atom were concentrated in a volume very small compared with the apparent volume of the atom. On the assumption that this nucleus was positively charged he calculated the general laws of scattering of alpha particles, which were afterwards completely verified by experiments.

Rutherford was fully conscious that his atom should not be stable according to the current theories of electromagnetic radiation, but he was so confident of his results that that meant there was something wrong with the theory, not with the atom.* How he was justified in the event we all know. Bohr, by a theoretical investigation which Rutherford afterwards described as one of the greatest triumphs of the human mind, showed that if Planck's quantum theory, with certain assumed conditions, were applied to the Rutherford atom, the complicated relations of line spectra could be explained. His theory has been considerably modified since it was first put forward, but it quickly served to show how the Rutherford atom could account for the general properties of the different elements, and could provide an explanation of the periodic law. In more recent years it has led to a new electronic theory of valency, which has revolutionised chemistry by bringing a large number of disconnected facts into a harmonious whole and providing a new and fruitful basis for chemical research.

By 1914 Moseley's researches on the *X*-ray spectra of the elements had added evidence of the utmost importance in favour of Rutherford's and Bohr's theories, and had quickly led to the discovery of missing elements. The position, so far as chemistry was concerned, was that yet another revolution had been effected through Rutherford's work. Atomic weights were no longer the decisive factor in determining the chemical properties of elements; what mattered was the atomic number, or charge upon the central nucleus. Nickel, tellurium, and argon were no longer blots upon the periodic system, which was being replaced by a better. The uncertainty about the number of missing elements was removed; it was known there could be only ninety-two elements, distinct in chemical properties, up to and including uranium. But it was also established by then that there could be varieties of elements which differed in their atomic weights, but not in their chemical properties. Lead from radioactive minerals had been found to have a different atomic weight from ordinary lead ; a severe shock for analytical chemists, hard to believe, but amply confirmed by other experiments within the next year or so. Fortunately they have long since recovered from the shock. Nature has provided that while the masses of individual atoms of elements having the same chemical properties may differ widely, the average atomic weight, with rare exceptions, is practically the same from whatever source on the earth the sample is taken.

* " The difficulty of stability is common to all theories of the atom; but what it points to is that there is something wrong with the theory of electro-magnetic radiation, not of the atom " (Rutherford, British Association, Australia 1914).

In 1914 Rutherford's work was rudely interrupted by the War, and for the next four years he was engaged on work of immediate national importance. He got back to his own work in 1918 with renewed vigour and started at once to attack the structure of the nucleus, a problem which he had said a few years back must be left to the next generation. His efforts were quickly rewarded. Within a year he proved unmistakably that nitrogen was destroyed by alpha particles and that hydrogen was a product. From that day to this there has been a continuous and almost bewildering addition to our knowledge of the structure of the nucleus and of the transmutation of the elements. The pace was quickening even at the end of Rutherford's life. Many of the results are of no direct interest to chemistry except in so far that every advance in fundamental physics is bound to affect chemistry. What is of special interest is the discovery of heavy hydrogen and of neutrons, and the production of artificial radioactivity by neutrons and by other methods. The discovery of heavy hydrogen, or deuterium, and of the neutron, followed one of the most remarkable predictions in the history of science. " It seems very likely," said Rutherford in his Bakerian Lecture in 1920, " that one electron can also bind two H nuclei and possibly also one H nucleus. In the one case this entails the possible existence of an atom of mass nearly 2 carrying one charge, which is to be regarded as an isotope of hydrogen. In the other case, it involves the idea of the possible existence of an atom of mass 1 which has zero nuclear charge. Such an atomic structure seems by no means impossible. . . . Such an atom would have very novel properties. Its external field would be practically zero, except very close to the nucleus, and in consequence it should be able to move freely through matter. Its presence would probably be difficult to detect by the spectroscope, and it may be impossible to contain it in a sealed vessel. On the other hand it should enter readily into the structure of atoms, and may either unite with the nucleus or be disintegrated by its intense field, resulting possibly in the escape of a charged H atom, or an electron, or both." It was after some years and many abortive trials that this prediction was fulfilled. Neutrons were discovered in 1932 and heavy hydrogen in 1933. In the same year the first artificial radioactive element was discovered, " which showed," said Rutherford, " how little we know about radioactivity." The properties of neutrons were found to be just as Rutherford predicted, and the last few years have seen the production of radioactive varieties of the large majority of known elements. Every year adds to their number, and what one says today is liable to become out of date tomorrow. To the chemist, and particularly perhaps to the bio-chemist, these radioactive elements provide a means of experimental attack on problems which have hitherto been out of their reach.

So we can summarise Rutherford's influence on chemistry in the following way. By his early work on the disintegration of elements he destroyed the chemist's conception of the nature of atoms, and in doing so gave the atomic theory of matter a reality which it never had before. By his work on the scattering of alpha rays he removed the blots on the periodic system by removing its foundation and replacing it with something better. In so doing he caused the development of a new and fruitful conception of valency. By his work on the transmutation of elements he opened up an immense field of experimental work for the chemist, a field which few can doubt will yield results of the utmost importance to biology as well as to chemistry.

The secret of Rutherford's success in inspiring others lies not only in his genius but in his unselfishness. Surely there never was a great man who gave so much credit to others. This was not a quality of his later years when his reputation was established and could not possibly be affected by anyone else's reputation. It was a quality that he had from the very beginning. In his report on the thesis which Rutherford submitted when he was an applicant for the 1851 Exhibition, Professor Gray drew attention to some of Rutherford's results which were not new, but added : " As the author is most careful to acknowledge his indebtedness to others I feel sure he arrived at his conclusions independently." It was that quality that endeared Rutherford to his associates, that brought him loyalty and affection as well as admiration. He would always do anything he could to encourage others. In his last published lecture at Cambridge he said : " Scientists are not dependent on the ideas of a single man, but on the combined wisdom of thousands of men, all thinking of the same problem and each doing his little bit to add to the great structure of knowledge which is gradually being erected." There spoke the genuine Rutherford. It was for this co-operation that he worked all his life, and it was this that was not the least of his achievements.

I have heard it said that it was a pity that Rutherford had no Boswell, that no one of the many men who worked in daily contact with him made notes of his ways, of his sayings, of his few mistakes as well as his many successes, of his manners, good and bad, and of his faults as well as of his virtues. I share the feeling. His name will live for ever in the history of science, and yet one would like to feel that Rutherford himself lived in the history of our times as no man of science has ever lived before. In many ways Rutherford was indeed very like the great Dr. Johnson. He came to dominate the scientific world of his time in exactly the same way as Johnson had dominated the literary world. Like Johnson he had a deliberate and strong utterance and loved to fold his legs and have his talk out. Like Johnson he had occasional sallies of heat of temper and at times of passionate unreasonableness. He was the centre of attraction at any scientific gathering, and especially at the dinners of the Royal Society Club where some of us used to hang about in the hopes of hearing him say, " come and sit by me." Then just as people were always a little frightened of Johnson so I think we were a bit frightened of Rutherford, not because he could or would do us any harm but because we might fall short of his own standard of work and conduct. Of course, the shortcomings were there, but we preferred him not to observe them. Like Johnson, too, he never considered whether he should be a grave man or a merry man, but just let inclination for the time take its course. He had a boisterous sense of fun and a loud laugh. By precept and by example he helped to keep our minds free from cant. He hated pomposity and artificiality. He loved simple people, and simple ways, and lived a simple life. He brushed little annoyances aside.

But in other respects he was very unlike Johnson. Poor Dr. Johnson suffered from ill health all his life, and was subject to severe fits of depression. Rutherford had abounding health and vigour, and there can hardly ever have been a man of such intellectual power who was so little subject to mental depression. So if we can find similarities we can also find great contrasts. Johnson lived on the whole a life of laziness interrupted by periods of feverish activity to which he was driven by lack of money. Rutherford was at the top of his form at breakfast, a thing that weaker men affect to despise, and lived a life of feverish intellectual activity relieved by short periods of magnificent idleness. When he took a holiday it really was a holiday.

There is another curious contrast with Johnson. Whereas Dr. Johnson used to relieve his literary labours by doing chemical experiments and by talking science, so Rutherford used to relieve his scientific labours by a study of literature. In his early days he was a great novel reader. When he was at Montreal, working long hours in the laboratory, the librarian found it difficult to supply him with enough light literature for his leisure moments. But later on in life he gave up the reading of novels or any form of imaginative literature, and confined himself to history and biography, to books that dealt with facts. I think he must have surprised many people from time to time by his knowledge of ancient history. He had a retentive memory, and an absurdly entertaining way of burlesquing hackneyed quotations or common proverbs, as for instance on one occasion when we were discussing short-lived reputations of men who rashly announced discoveries which were soon detected as false, he said: " Well, 'tis better to have boomed and bust than never to have boomed at all."

He took great pains in making his written papers clear and concise; they are models of what scientific papers should be. But although his writings were uniformly good and clear, Rutherford was never a good speaker except on his own subject. On formal occasions, or when he had to speak extempore, he would hesitate, fumble for words, and repeat himself. When he had to make an official speech with notes perhaps supplied by others, those who knew him well would fidget in their seats, waiting for the time when he would put the notes aside and say to himself, almost audibly, " now let's tell them about something interesting," and become himself again. But when he lectured about his own work he was superb, and unique. I expect that all who have heard him have their own private memories of him. I can almost see him now, standing in this famous lecture room, massive and commanding, demonstrating to an enthralled audience the transmutation of matter by high speed protons and deuterons. A train of valves amplified the effect of the transformation of a single atom to such an extent that it could operate a counter within sight and sound of the audience. He showed first the transmutation by protons. The counter ticked slowly as the process went on. " Now," said Rutherford, " if you will allow me, we will bombard the same target with deuterons, and I think you will observe a greatly accelerated rate of transformation." The assistants made the necessary adjustments and the current was turned on again. The counter ticked if anything rather more slowly than before, and the audience began to titter. The assistants came forward to see what was wrong. " No," roared Rutherford defiantly, " leave it alone." The words were hardly out of his mouth when the ticker obediently went off with a rush, and the audience dissolved into laughter and cheers. I was told afterwards that some of the valves used were sensitive to sound.

I hope that you will not think my stories of Rutherford unsuited to the occasion, and disrespectful to his memory. For my part I feel cheerful when I think of him, and glad to have lived in times that he made so intensely interesting; and I do not think he would wish us to be too solemn when we meet to commemorate his life and work. Besides, what I wished so much and have tried to do, was to bring back to you for a few moments the real Rutherford, as he was, his features unobscured by a smooth mask of panegyric. Of course he knew he was a great man. All his life he was like a young man rejoicing in his strength. He enjoyed, unaffectedly, the many honours that came to him, and cabled the news of each one, as it came, to his mother in New Zealand, the evening of whose long life was gladdened by the fame of her son. There was no false modesty about Rutherford; but neither was there any vanity. He never seemed to be driven by the very human motive to excel other men. His compelling passion was the search for truth.

" Well, it's a great life," Rutherford often used to say, in high spirits. It was, indeed, a great life. Happy stories will be told of him till death takes away the last of those who knew him well.