The Rutherford Memorial Lecture, 1956

The discovery of atomic number

By Sir Charles Darwin, F.R.S.

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In a Rutherford Memorial Lecture there are two alternative courses that might be taken. One is to describe one or other of the great developments that have later followed out from the many things which Rutherford started; the other is to describe some aspect of his own work from a historical point of view. If, as we hope and intend, the institution of these lectures should survive for many years, the first policy will probably be more useful in later times, but there still remain a number of people who lived through the wonderful experiences of those days, and while we survive it may be more interesting perhaps for us to leave some small records of what we saw. But there seems little purpose in merely giving again and again a biography recounting all the things that Rutherford did, and so I have chosen one item from among his discoveries, and I propose to give an account of this. It is the discovery of Atomic Number. I am going to try and give a picture of this whole subject; in it Rutherford of course played the leading part, but others made very important contributions, and it will be the whole history of it that I shall try to describe, and not merely his part in it.

In the history of science there has been every now and then what I may call an 'easy' discovery, by which I do not in the least mean that it was easy to discover, but that when discovered it is so easy to understand, that it is difficult afterwards to see how people had got on without it. One example of such an 'easy' discovery was the discovery by Copernicus that the earth goes round the sun. After his time it was possible for anyone almost to forget what astronomy had been like before his day, and yet we have to recognize that the subject had been studied for three or four thousand years by many exceedingly intelligent men. Atomic number is another such 'easy' discovery. Any recent book on chemistry or physics describes the chemical elements in terms of it, and now with the development of atomic energy, even the daily press discusses quite readily the differences between uranium 238 and 235, and possibly even recalls that uranium is element number 92. In all the doubts that we may have about how future scientific discoveries will reshape our outlook on the world, we can feel sure that this one thing will never be changed; that the isotopes of the atoms of chemical elements will always have known atomic numbers and atomic weights. It now seems so simple that it is hard to believe how recently it was all discovered, and I want to show you that this 'easy' discovery was not at all easy to make.

The existence of atoms had been believed in by chemists and physicists since the days of Dalton, who first put forward his theory in 1804. But somehow one gets the impression that they were much less believed in during the nineteenth century than they are now. Thus Dalton's theory was supplemented in 1834 by Faraday's
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discovery of the law of electrolysis; this work gave evidence of almost exactly the same kind as Dalton’s that, if there were atoms of the various elements, there must also be an atom of electricity, yet nobody seemed to attach very much importance to it. It is true that in 1861 Clerk Maxwell carried out beautiful experiments of three different types with the aim of determining the mass of the atom of electricity, but he had no success; indeed, the results he was seeking were not found until nearly sixty years later at the time when the ‘gyromagnetic’ phenomena were detected. In the chapter on electrolysis in his textbook of electricity he as good as says that though the straightforward idea may be to think of atoms of electricity this can only be a temporary hypothesis, which we shall certainly abandon when we have mastered the true nature of the electric current. The chemists too were not very confident about atoms, and indeed as late as 1900 the German chemist Ostwald stated that there was no direct evidence for them, and that he believed that the laws of Dalton could be derived without using atoms at all.

The physicists, or at any rate the mathematical physicists, were also for the most part not very deeply interested; indeed, in my own undergraduate course at Cambridge, starting in 1906 I learnt a great deal of the mathematics concerned with mechanics and electricity and so on, but I was not taught a thing about the mathematics of atoms. Yet there had been very great developments in the theory of gases. Maxwell had shown how the ordinary gas laws followed quite naturally out of the theory that a gas consisted of a set of atoms moving almost independently of one another except for occasional collisions. He also followed up this theory further and made it explain the viscosity of gases, getting a result that was rather unexpected. This result he then verified experimentally, so that there was every reason to believe that his atomic theory was running on the right lines.

Perhaps the reason why there was so little excitement about atoms in those days was that no one could answer the very first question that would be asked—how big is an atom? The chemical theory only asserted that, if the hydrogen atom weighed one unit, the oxygen would weigh sixteen units, but it could not say how big the unit was, beyond the fact that it must be quite small. Maxwell’s theories of the gas laws also did not help. It gave most of the answers in detail; it could tell quite accurately how fast on the average the atoms of any particular gas would be moving, giving the result as so many metres a second, but it too could not say how many of them there were. In all this work, both physical and chemical, there is one number missing, which could not be deduced from any of these experiments. It is called Avogadro’s number. Avogadro was an Italian physicist who, as early as 1811, which was only shortly after Dalton’s theory had been put forward, enunciated the principle that the number of molecules in a cubic centimetre of gas at any given temperature and pressure would be the same no matter what the gas. It was an unproved hypothesis and he had no idea what the number was, and for a long it remained completely unknown. Once this number has been found all the answers come out complete. One can tell the mass of a single atom of hydrogen and so of a single atom of every other element. One can say how many atoms there are in any piece of matter. Moreover, though it was of course only much later that the electron was discovered, it could be identified with Faraday’s atom...
of electricity and its charge could be measured in the ordinary units of electricity.
The number is Avogadro's number, but I ought to say that it has been found
convenient to define it not in terms of the number of atoms in a cubic centimetre
of gas, but as the number in a gram-molecule of any substance. For a long time
Avogadro's number remained a mystery, and I think it was this gap more than
anything else that was the reason why the physicists of the nineteenth century
showed so comparatively little interest in atoms, even though they fully accepted
their existence.

The first estimate of Avogadro's number is due to Maxwell himself. His theory
of the gas laws gives no help, but his theory of viscosity does. It is a much deeper
theory, and it requires a number of rather difficult and slightly uncertain hypo­
theses, but with the help of these he did manage to arrive at an answer. He stated
that in a cubic centimetre of gas at normal pressure and temperature there would
be $19 \times 10^{18}$ molecules; actually as we now know, the answer should be $27 \times 10^{18}$.
This result may not seem very accurate, but when consideration is given to some
of the rather doubtful details in his theory, I think the answer might easily have
come out much further from the truth. He published this result in 1872 in a letter
to the journal *Nature*, and it rather shows the comparatively unimportant place
that atomic theory had in people's minds, that he should have published a fact
of such tremendous importance in a manner that cannot have drawn much
attention to it.

Not long after this a general confirmation of Maxwell's value was given by
Rayleigh from quite a different source. It was Rayleigh who first explained
correctly why the sky is blue. It is due to the sunlight scattered by the air molecules.
I shall not go into the detail, but from the brightness of the sky it is possible to
infer how many molecules are responsible for the sky's light, and hence Avogadro's
number. His value entirely confirmed Maxwell's, but did not narrow the limits of
the accuracy to which it was known.

In 1895 Röntgen discovered the X-rays and this was the start of the new physics.
It almost immediately led to Becquerel's discovery of radioactivity. It also
opened up the subject of the conduction of electricity in gases, which was being
studied already, but for the most part with great difficulty because the only
ionizing agents previously known had been flames and bubbles from acids, and
these were hard to control quantitatively. It was only shortly after this, in 1897,
that J. J. Thomson discovered the electron by a study of the cathode rays in
vacuum tubes, in which he measured how much they were deflected by electric
and magnetic fields. We are now so entirely used to electrons that it is hard to
realize what an exciting discovery it must have been to find that the atom of
negative electricity weighed about $\frac{1}{1840}$ of the weight of a hydrogen atom. This
discovery would surely have delighted the heart of Maxwell, for it would have
removed all his difficulties in understanding the ultimate nature of the electric
current.

All this work, in particular radioactivity, brought out the importance of the
atoms to a degree far beyond anything discovered earlier, and it held the promise
of yielding Avogadro's number. The first determination was done by Townsend
in 1897. He produced an ionized gas from the bubbles coming out of water when it was electrolyzed under various conditions, and he used an electrometer to measure the total charge in the gas. His next problem was to find how many of these individual charges there were. This he did by noticing that in a damp atmosphere a cloud was formed, and he assumed that each cloud-drop had one of the atomic charges on it, either positive or negative. His central problem was to find how many drops there were, and for this he measured the rate at which the cloud fell under gravity. By a known formula this tells how large each drop is, and by weighing the total accumulation of water when all the cloud had fallen to the ground, he could thus get the number. It will be seen that the work involved serious difficulties in almost every part of it. His answer was about 40 per cent too low; in view of all the difficulties, this was a quite wonderfully good accuracy to have attained.

Not very long after this the X-rays were used to produce a much more controllable ionization and C. T. R. Wilson developed his famous cloud chamber which still continues to be one of the most powerful instruments of atomic research. I shall not go into the details, but once again the central point was the rate of fall of a cloud of water drops. Again the value determined was a good deal too small.

Finally, it was Millikan who extended this method by finding that he could watch the behaviour of a single drop—this time it was an oil drop—with a microscope, and see how it behaved both under gravity and in a measured electric field. From this work he fixed the electric charge with a very high accuracy. It is Millikan who in 1909 could justly claim to have determined Avogadro’s number; though in fact since those times it has been evaluated more accurately by a quite different method. X-rays can be used to measure the distances between the atoms of a crystal, and by measuring its size the number of its atoms can be found, but I will not go into this. The value is now accepted as $6.02544 \pm 0.00011 \times 10^{23}$ atoms in a gram-molecule of any substance.

Before the start of the century Rutherford began to take a hand. His early work can be conveniently divided into two parts, one the study of the radiation given out by the various radioactive substances, and the other the different families into which those substances fall. In this second type of work he had the cooperation of Soddy; I shall have nothing to say about it, beyond referring to the fact that it led to an important idea, which I shall need to consider later. This was Soddy’s introduction of the idea of isotopes. It was easy for the most part to separate the various radioactive elements from one another by the ordinary methods of chemistry, but there were some pairs that refused to be separated; for example, no one could separate the elements thorium and ionium, though they emitted recognizably different radiations. Several workers in this field reached the same conclusion with this and other pairs of substances, and the matter was summarized by Soddy’s invention of the word isotope. The word was introduced in connexion with these inseparable radio-elements, though Soddy did raise the question whether ordinary elements might not also be mixtures of isotopes. However, I shall return to this subject later.
It is with Rutherford's work on the radiations that I shall primarily be concerned. He separated them into the $\alpha$-, $\beta$- and $\gamma$-radiations. The $\gamma$-rays were a very penetrating type of radiation which he could identify with the X-rays, and the $\beta$-rays he could identify with electrons; the $\alpha$-rays were much more powerful, and he succeeded in showing that the most natural explanation of them was that they were helium atoms with a positive charge equal to that of two electrons. He could see that these radiations would provide probes, by means of which it should become possible to get inside the atom so as to see what it was made of.

There had already been some work done in this field by J. J. Thomson. His experimental work had shown that there must be a good many electrons in an atom, and in his mathematical work he had made a model of the atom with the help of which he could make calculations. It had a distributed charge of positive electricity to neutralize the charges of the electrons, and so they could take up positions of equilibrium; this model, though it served his turn well, has not survived. The question arose then: how many electrons are there in an atom? He developed two methods of finding out. In one of them he exposed the atoms to X-rays, and saw how the X-rays would be scattered in various directions. The total scattering should be attributable to the scattering produced by each electron, and so its amount should give a measure of the number of the electrons doing it. In the other method he studied the scattering of a beam of swift electrons in going through a gas. Each of these swift electrons will go right through some of the atoms of the gas, and in so doing it may chance to go very near one or other of the atom's electrons. In such a collision there will be a repulsive force between the two; the atom's electron will be knocked away, and the swift one will be deflected a little. If then a narrow beam of swift electrons all travelling on parallel lines is sent through a gas, the emergent beam will not be parallel, but some of the electrons will be scattered into other directions, and the emergent beam will be a cone. By measuring the angle of this cone, an estimate could be made of how many electrons there were in the atoms which brought it about. Neither of these two methods is easy, and, in the light of what we now know, the theory of both of them can be criticized in some of its details, though their broad principles are correct. However, they both agreed in giving a value that was a good deal less than the atomic weight of the atoms of the gas that was doing the scattering. Thus for air the value seemed to be about ten electrons for each atom. In view of doubts about the theory and also in view of doubts which then still remained about the absolute value of the charge of the electron, these results could not be claimed as being at all accurate, but they did give a rough estimate of the number of electrons to be expected in an atom.

The $\alpha$-particle was always Rutherford's favourite, and it was with the $\alpha$-particle that he got the final answer. It has much more energy than the other particles; indeed, it has so much that a single $\alpha$-particle can be made visible. This had been discovered by Crookes. In the early days of radioactivity it had been found that when radium was put near a screen of zinc sulphide crystals, the screen became luminous. By looking at the screen with a microscope Crookes saw that the luminosity was not uniform, but was due to a rapid succession of sparks or
scintillations. It was natural to think that each of these represented a single particle, and that every $\alpha$-particle made a scintillation, but this had to be verified. The verification consisted in counting the number of scintillations given by a piece of radium and comparing it with the count of the particles made by an electrical instrument. This instrument was developed by Rutherford and Geiger in 1908, and it could detect a single ionized atom; in effect, it was the prototype of the Geiger counter, but in those early days it was not at all a convenient instrument to use.

The scintillations thus provided a direct method of seeing how the individual $\alpha$-particles behaved, and much of the work of the Manchester laboratory made use of them. It may sound exciting to be able actually to see individual atoms for the very first time, but the work itself proved to be a rather tedious business. No one can see the scintillations in the microscope until his eye is adapted to the dark, and this adaptation takes about half an hour to attain. An experiment then would go something like this. There must be two rooms and two workers. One of the rooms must be kept a good deal darker than a photographic dark-room, and in it there is one of the men who is to act as the observer. He has to begin by wasting half an hour with nothing whatever to do while his eyes are becoming adapted to the dark. In this room there is a microscope and scintillating screen, and also whatever may be the set-up of radium appropriate to the experiment. In the neighbouring room the other man sits and keeps the record of the count of the scintillations. Thus it may be that what is to be counted is the total number of scintillations made on the screen in two minutes, and he will call out the timing of the two minutes, and at the end of it he will write down the number told him by the observer. When the experimental set-up is to be changed, the observer must first blindfold himself, and then the light is put on in the dark room and the other man comes in and resets the instrument, and he must turn out the light and go out and shut the door, before the observer can uncover his eyes. Altogether it is a laborious business, but it was the use of this method that led to the discovery of the nucleus of the atom.

The $\alpha$-particles provided a method of probing into the atom, which, on account of their great energy, was much more powerful than the electrons that had been used by J. J. Thomson. Experiments were undertaken by Geiger and Marsden in 1909, which showed the effect of the scattering of $\alpha$-particles by various substances, and they confirmed Thomson's general result that the scattering could be attributed to the collisions of the particles with the electrons in the atoms of a variety of substances, provided that each atom contained a number of electrons of the order of its atomic weight. But these experiments revealed an effect that was much more remarkable. It was found that quite a thin film of gold foil could scatter a few of the $\alpha$-particles right backwards. It was only a few, but even so this could not possibly be due to the compound action of the collisions of an $\alpha$-particle with a long succession of electrons. To suppose it were so would be as improbable as to suppose that a tossed coin would come down heads a thousand times running. While the rest of us knew that the scattering had this peculiar characteristic, it was only Rutherford who appreciated how extraordinary it was. For some
months he would only say that there must be the most tremendous forces in the atom, and leave it at that. But I recall one Sunday evening when a few of us had been invited to supper with him, when he announced to us the birth of the nucleus. He described how the scattering could only be explained as the result of a single collision, and not through multiple collisions, and he told us how he had worked out the law of scattering which would be observed if the force that produced it was simply the electrostatic repulsion between two point charges. It was very interesting to see how his mind worked. He had probably given no thought to the properties of a hyperbola since his school-days, but he characteristically remembered exactly what he needed for his purpose and the resulting law of scattering was correct and complete. To the best of my recollection on that evening he was already looking far beyond the mere verification of his theory. He had worked out the distance of closest approach between the two nuclei, and on the assumption that the nuclear charge was numerically of the order of the atomic weight, he had found that the \( \alpha \)-particle would be so strongly repelled that it would never get near the nucleus of a heavy atom such as gold, but that it would go quite close to a lighter atom. He was already thinking that experiments with such lighter atoms would one day be needed in order to reveal what the shape of the nucleus really was. Apart from this I recall that, though he was certainly very familiar with the idea of recoil, he had assumed that the nucleus of the atom was so heavy that it would not be set in motion, and I remember he was much interested when it was pointed out to him that in the case of hydrogen the recoiling atom itself might be worth study. As a matter of minor historical interest it may be noted that it was some time before the name ‘nucleus’ was thought of; in the earliest papers of that period it was merely called the ‘central positive charge’.

The theory with its formula for scattering obviously had to be verified in all sorts of ways. It had to work right as regards the comparative numbers of \( \alpha \)-particles thrown off in the various directions when the particles were sent through a piece of gold foil. It had to hold when foils of different thicknesses were used, and when the \( \alpha \)-particles had different speeds. It had to give the value of the nuclear charge of the gold atom, and then the work had to be repeated with foils of other substances.

All this work was undertaken by Geiger and Marsden, and it was done by counting scintillations. The theory was brilliantly verified in every respect. The result came out that gold had a nuclear charge equal to that of about 100 electrons, and aluminium of about 10. Of course we now know that actually the nuclear charge of gold is 79, but the discrepancy was well within the margins of error, for this part of the work demands a knowledge to high accuracy of the thickness of the gold foil at the exact place where the particles are passing, and that cannot have been easy to measure accurately. Years later, after the whole theory of atomic number had become well established, the work was repeated by Chadwick and the correct value was verified within a unit or two.

By the end of 1911 the existence of the nucleus was firmly established. In Manchester we all knew that it was very important indeed, and we had the broad idea that all the elements must have atomic numbers. However, perhaps we did
not quite realize how tremendous the consequences were going to be. The results of the experiments were published in full, but no generalization was made for the other elements. It is a little hard to recall how one was thinking more than forty years ago, but it may be worth attempting to describe roughly the state of mind prevailing in the laboratory under the influence of Rutherford’s great discovery.

It must be remembered that in those days very little was known about the mechanics of the atom. Taken by itself the new discovery did not even imply that the nuclear charge must necessarily be equal to that of an exact whole number of electrons, but it evidently had to be so, because otherwise all the ordinary results of electrolysis would go wrong. So we did take it for granted that the charge must be an integer. I can recall speculating whether the number for gold, given as near 100, might really be 79, and wondering if there might not be considerable gaps in the list of atomic numbers. However that may be, about this time a short paper appeared by van den Broek which put forward the full hypothesis of atomic number. I can recall that we felt a little annoyed at this, because it was based on the Manchester work, and it ran exactly on the lines we had all been thinking. We rather felt that an opportunity had been missed of stating an almost obvious fact.

Then, too, it must be remembered that the idea of isotopes was not yet well established, except for the case of the radio-elements. It is true that Soddy had suggested that the principle might apply to the other elements; this idea had indeed been put forward by Crookes as long ago as 1886, but there was a good deal of opposition to it on the part of the chemists. Thus the atomic weight of chlorine was always found to be 35·46, no matter from what source the chlorine came, and this seemed to provide a very strong presumption that every single chlorine atom had this weight. It must be remembered that at that time very little was known about the atomic forces, and it seemed likely that the mass of the atom should have at least some perceptible influence on its chemical behaviour. So it could plausibly be argued that chlorine coming from sea water would be likely to have a different proportion in its isotopes from chlorine derived from some solid mineral. There was of course nothing absolutely to prevent a chlorine atom from having weight 35·46, and yet an integral atomic number—presumably it would be 17—though even that was uncertain—but such a curious atom did seem to spoil the grand simplicity of the principle. Then, too, there was another troublesome little point to remember. In the periodic table of the elements there are a few places where the order of atomic weights differs from the chemical order. It seemed probable, but not certain, that the atomic number would agree with the chemical order. Here was another difficulty to be resolved.

I have dwelt on these doubts, because I think it is interesting to see the atmosphere that is apt to prevail immediately after an important new discovery has been made. There is always a whole field of difficulties to be faced, and one cannot at first tell clearly which among them are the important ones. I think it would be fair to say that at this time we fully recognized how very important the nucleus was, and that if we had the idea of atomic number, but it was recognized that there were a good many points to be cleared up before it could be accepted quite whole-heartedly.
However, happily just about this time Bohr came to Manchester, and almost at once provided confirmation for the theory of the nucleus from quite a new direction. Bohr has a deeper understanding of the basis of physics than anyone I have ever met, and he at once fastened on the discovery of the nucleus as the most important new thing in physics. Though he fully accepted it, he could see better than anyone the fundamental difficulty it would involve, which is that if the atom is composed of a nucleus and electrons round it, there can be no mechanical principle to tell the atom how big it is to be, whereas all atoms do have a rather definite size. There were of course already known many things in physics which refused to obey mechanical principles, but which fitted into the quantum theory. This theory was till very mysterious in those days, and I have not the time to go into its history, but Bohr succeeded in applying it in quite a new way to the case of the hydrogen atom. He supposed this atom to have a nucleus of charge one, with one electron circling round it, and, by two very remarkable extensions of the quantum principle, he got out not only a rule that would tell the atom how large it was to be, but he could also confirm his theory very completely by explaining the whole optical spectrum of atomic hydrogen. This result was quite as important, and quite as exciting as the discovery of the nucleus by Rutherford. It absolutely confirmed the nuclear principle, but I cannot go into it here. I will only say that one of the further consequences of Bohr's theory was that he could explain some of the results obtained a few years before by Barkla on the X-rays. This leads me to the next important event in my history.

It may be that one of the reasons why the Manchester discovery did not draw much greater attention to itself was that it coincided rather closely in time with another discovery of the very greatest importance. This was the work by von Laue and his collaborators, which cleared up all doubts about the nature of the X-rays, by showing that they could be diffracted in passing through a crystal. This experiment was of course the start of the whole great new science of crystallography, but it also threw light on the earlier X-ray work. Barkla had shown that each element could be made to emit characteristic X-rays, though he could at that time only demonstrate it by measuring the absorption of the rays in going through different types of matter, and this gives only a rather rough measure. After von Laue's work the X-rays could be confidently recognized as having the same nature as ordinary light but of much shorter wavelength. Bohr's theory predicted that when an atom has a heavy nucleus its inner electrons ought to emit light of very short wavelength, and it seemed likely that this must be the source of the X-rays which Barkla had discovered.

Practically all the work in Manchester during these years was directly on subjects connected with radioactivity, but when Laue's discovery was reported to us, Moseley, who up to then had been following the common course, thought he would like to branch out into the new subject. Rutherford was at first rather discouraging, saying that we in Manchester had no familiarity with X-rays and that we would be at a disadvantage in a new field which was much better understood in other places. However, Moseley persisted in his wish, and for a time I joined him in this work, though he contributed practically all the experimental
skill. Moseley was a tremendous worker. He had two rules. The first was that in setting up an experiment one must never stop until it is set up and working, and the other that once the instrument is known to be working one must of course carry through the experiment to its very end. Consequently, life was very irregular, and all-night sessions were by no means rare. In these experiments we studied the diffracted X-rays by means of their ionization, and we got out some interesting results concerning the continuous spectrum of the X-rays, but the chief thing perhaps was that we mastered the techniques of X-rays.

At the conclusion of this work I reverted to the theoretical side of the subject, and Moseley started the experiments which were finally to establish the principle of atomic number. He set out to find the wavelengths of the characteristic spectrum of the X-rays, with the aim of applying Bohr’s theory to them. He embarked on this work with his typical energy, repeatedly modifying his instrument, so that it looked different nearly every time I saw it. In the end he had a sort of railway in a vacuum, on which the trucks had plates of a sequence of elements, and each truck in turn could be brought under the discharge so as to send X-rays towards his crystal. In these experiments he used photography. He soon found the spectrum of the sequence of elements between calcium and zinc. The results clearly showed that the charges on the nuclei of the elements increased successively by one going along the periodic table. The actual atomic numbers could be set down after making allowance for a correction that had been predicted by Bohr. In later work he extended these results, and found the atomic numbers of practically all the elements, though not unnaturally, in working over such an enormous range, he had to some extent to vary his methods. He could also enumerate a few gaps where the elements were missing; the existence of some of them had been already suggested by the chemists. With the completion of Moseley’s work the conception of atomic number became universally accepted. Every element could be assigned its number, which measured the charge on its nucleus, and so the number of electrons circulating round it.

There was nothing more to be said about atomic number, but there remained the fact of fractional atomic weights, and to complete the subject the resolution of this difficulty must be briefly recounted. As I have mentioned, Soddy had already propounded the idea of isotopes, but had only applied it in the first instance to the radio-elements. Now, more than a century earlier the chemist Prout had noted that a good many of the atomic weights were nearly whole numbers, and he had suggested that they could all be built up out of hydrogen. His idea broke down over elements like chlorine with atomic weight 35.46, but the curious fact remained that a good many of the weights were very close to integers, far too many of them for this to be a matter of chance.

It was J. J. Thomson who gave the first experimental indication that ordinary elements might be composed of isotopes. He was working with the positive rays in a vacuum tube, and by these means he measured the masses of a good many atoms and molecules. When he came to measure neon, which has atomic weight 20.2, he got two lines in his instrument, the main one at 20 and a weak one at 22. In this work there is little to show whether a line is due to an atom or a molecule.
and it was not quite impossible that the 22 line might be a molecular compound of helium with hydrogen, but the result did point rather strongly to the existence of two atomic isotopes. Aston, who was working also in the Cambridge laboratory, tried to separate the two by the method of diffusion. This method depends on the fact that the average speed of a heavy atom is less than the speed of a light one. If then the atoms of a mixture can be made, so to speak, to run a race, the lighter ones will win. The race is run by making the gas slowly diffuse through a porous surface—actually it was the stem of a churchwarden pipe. He got a distinct but not a very marked separation by these means, but the work was stopped on the outbreak of the 1914 war. Simultaneously with this work another set of experiments also indicated that at least one of the ordinary elements was composed of isotopes. The element lead was known to be the end-product of the two series of radio-elements deriving from uranium and from thorium, and it was to be expected that these should have different weights. Several workers in the difficult field of atomic weight determination therefore set to work to determine the weight for samples derived from uranium, from thorium and from ordinary lead, and they got small but perceptible differences between them. This work did therefore confirm the idea that ordinary elements may be composed of isotopes.

In 1919 Aston returned to the subject. This time he worked by a more direct method than he had used previously. With great and artistic skill he designed and made the first mass spectrograph, and with it he was able to measure the mass of practically all the elements and of a good many molecules too. For example, he soon found that chlorine had two isotopes at 35 and 37, but I ought to add that it was by no means as easy as it sounds, because he also got lines at 36 and 38, and it took him a good deal of trouble to prove that these were compounds of chlorine with hydrogen. However, with the help of his mass spectrograph Aston did succeed in clearing up the whole field, and he could finally give a list of the common isotopes of practically all the elements. I ought perhaps to mention that by this time Rutherford had moved from Manchester to Cambridge, so that, though he personally had nothing to do with Aston's experiments, he was able to witness and encourage the completion of this great work.

We now knew the principles of atomic number and atomic weight. Prout's hypothesis was justified and indeed extended, for not only were the atoms built out of hydrogen as to their weights, but also as to their electric charges. But there remained one rather puzzling thing. The proton or hydrogen nucleus has unit charge and unit mass, and was obviously the main brick, but since in the other elements the atomic number is not the same as the atomic weight, there must also be another kind of brick. It was natural to suppose that this was the electron, the only other primitive particle known in those days. Thus helium has atomic number 2 and weight 4, so its nucleus would be expected to contain four protons and two electrons, or six particles in all. But if it was actually to be built out of protons and electrons that would mean that at some instant of time six particles must simultaneously have collided so as to stick together. This looked to be a very improbable event, and it was hard to believe that all existing atoms of helium could have been through this experience. The matter was cleared up by the
discovery of the neutron, which has the mass of a proton, but no charge. In some earlier speculations Rutherford had suggested that such a particle might exist, though he did nothing about it at the time. Later, it was looked for and discovered by Chadwick working independently, also in Cambridge. By means of protons and neutrons all the elements can be built up without demanding improbable kinds of collision between the various particles. The neutron then completes the picture of atomic structure, but I cannot end without saying that the subject is still by no means a closed one. At first we thought that everything had to be made of electrons and protons, then came the neutron, and now a dozen other types of elementary particle are known, mostly only capable of existence for less than a millionth of a second. We still await a theory of these particles, and all that I can say here is that they seem to demand a wholly new kind of dynamics the principles of which have not yet been found.

This completes my history. It may now seem easy to understand that every atom is composed of a nucleus built out of protons and neutrons with an equivalent number of electrons surrounding it, but I hope I have shown that there were a great many difficulties to be resolved before it was established. In a sense one can now say that the subject is finished, because it is hard to believe that any new physical theory can upset it; every isotope of every element will always be described in terms of two numbers, its charge and its weight, or alternatively the number of protons and neutrons in its nucleus, and we know these two numbers exactly for all of them. But the solution of no problem in science is final; whenever it closes one field it always opens up another. In this case the new field was opened first by the neutron, and then by the various new elementary particles. It is certainly an exciting field and it promises to be of quite fundamental importance. I will conclude by quoting words which I have often heard from Rutherford in connexion with any subject he was working at. He would say ‘I think this is a grand subject; there are so many things in it we don’t know’.