SIR JOSEPH J. THOMSON.

A MEMORIAL LECTURE GIVEN AT THE ROYAL INSTITUTION THEATRE ON APRIL 16TH, 1942.

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IT is fitting that a lecture in memory of J. J. Thomson should be given in this room, where his living voice has so often been heard. For the older part of the company, including myself, it is hard to realise that we shall not see him enter the room and take his position behind this table again. To the younger part, on the other hand, it probably seems that he belongs altogether to the great tradition of a bygone day.

To begin with a few biographical details. Thomson was born in Manchester in 1856, the son of a bookseller and publisher of that city. His father died early, and there was little of special note in his early home surroundings. He did not go to a public or secondary school; nor was it easy for those who knew him in later years to see that he had suffered in any way for the lack of this experience. He was only 14 when he went to Owen's College, Manchester, and it was probably at this time that he remarked in the presence of Miss Gertrude Mellor, then a little girl, that he intended when he grew up to "go in for original research." This remark was not taken very respectfully. Mr. G. V. Vernon, a cousin of the older generation, tapped him on the head and said, "Do not be such a little prig, Joe."

At Owen's College, Thomson came under the influence of Balfour Stewart, and it was probably from him that the idea of original research was derived. It soon bore fruit, and a small piece of work was published by the Royal Society which was done in Stewart's laboratory. Thomson's marked mathematical ability had attracted the attention of Barker, the professor of that subject, and he went to Trinity College, Cambridge, with a Minor Scholarship in 1876.

At Cambridge his great abilities were soon recognised, and he became in due course a fellow and lecturer at Trinity. When the late Lord Rayleigh resigned the Cavendish Professorship of experimental physics at the end of 1884, J. J. Thomson was elected to succeed him. It was at the Cavendish Laboratory that the great work of his life was done. From the first he gave his attention to the discharge of electricity through gases. His stimulating personality soon attracted a band of research students. Some of them were Cambridge graduates, others came from various parts of the Empire, and later from the United States, and to a less extent from other countries. Conspicuous among the early arrivals were E. Rutherford and J. S. E. Townsend. The discovery of the X-ray by Röntgen happened fortunately at this time, and opened up a new vein of ore which was vigorously exploited by Thomson and his brilliant research corps, as Lord Kelvin called it.

There do not appear to exist any snapshots of J. J. going on his daily rounds in the laboratory. It is a great pity that no such were taken. I had daily opportunities of doing it myself for years, but it never occurred to me, nor so far as I know to any of my fellow workers. Let me appeal to the younger generation not to neglect their opportunities in the same way. In the absence of any such, I will show you a picture of J. J. with Dr. Irving Langmuir, which was taken in America in 1923, at the laboratory of the General Electric Company. It recalls very well the spirit of his daily rounds at the Cavendish laboratory. You see how he had pushed up his spectacles on to his forehead to examine whatever it was that was being shown him. This was a very characteristic gesture. He used his spectacles in order to focus the distance, and discarded them for anything near.

I do not think we realised in those days the enormous energy that must have been expended on those daily rounds. We each expected J. J. to propose the problems for investigation, to enter into our individual difficulties, to suggest what we should do next when our own efforts were baffled and exhausted. Further, he had to see that we had the necessary equipment. If money had been plentiful, this would have been less difficult, but in fact there were hardly any resources available except certain fees from lectures and there was a constant struggle for the possession of the more desirable pieces of apparatus. Possibly the picture I have drawn above may seem a little over-charged. Of course men like Rutherford and Townsend did not require to be helped over every or perhaps any stile: but J. J. spent as much or more time with them as with the weaker brethren. No doubt he enjoyed discussion with them, when he could hope to learn something himself. Nobody needed to be afraid to put any view before him, on the ground that it was not well considered. He seemed to enjoy argument with anyone who could or would stand up to him, though he was at times curiously obstinate in maintaining an untenable point of view, and I have seen him abruptly change the subject rather than admit that it *was* untenable.

Considering that J. J. had at times 30 or even 40 research students working under him, and that he went the round daily, when they laid their difficulties before him, it may well appear that the energy he expended in this way must have been prodigious. People differ very much in the extent to which they can do this sort of thing, and in how much it costs them to do it. I do not think Stokes or Rayleigh could have done it at all—they would generally require notice of the question put to them. On the other hand Kelvin or J. J. Thomson or Rutherford, especially the first two, were ready with their comments and seemed able to give them with zest, and without much cost to themselves in effort or fatigue.

J. J. was never, in my experience at least, cross or short-tempered, or dangerous to approach. He was also in general very patient with the assistants, conscious no doubt that even if things did not go well, they were, nevertheless, doing their best. If they made mistakes through want of scientific knowledge, he patiently explained wherein they were wrong. For instance, he found his assistant one day putting pieces of indiarubber into liquid air, to make it boil more freely, which he supposed would lower the temperature just as if the liquid air was caused to boil under an air-pump. J. J. betrayed no symptom of irritation, but calmly explained the mistake.

It is impossible in the course of an hour's lecture to give an adequate account of his life's work, but at the same time something must be attempted, and I shall try to explain how, starting with the phenomenon of the cathode rays, Thomson arrived at the conception of the electron. This was perhaps the highest peak of his achievement. I was personally privileged to hear the development of his ideas from time to time while it was going on, and to get some glimpses of his experiments.

By way of preface to what is to follow, we must now make the meaning of the expression cathode rays quite clear. It originated in Germany (Kathoden-strahlen). Let us suppose the electric discharge of high tension to pass through a highly exhausted space, using as the negative electrode a flat metal disc. Then we shall see that an influence is propagated normally from this flat cathode in some respects reminiscent on a small scale of the beam from a searchlight : for its track is marked out by a blue luminous haze, and it produces a patch of light when it comes up against the wall of the glass tube, just as the searchlight does when it comes up against a cloud. If the cathode is concave, the beam converges to a focus. If convex, it diverges. Its direction is independent of the anode, which may be placed to one side, or may be a plate with a hole suitably placed through which the beam will pass. This beam of pencil constitutes the cathode rays. It differs from a searchlight beam in a striking particular, in that it can easily be bent about by bringing a magnet near it. If the magnet is so placed that the lines of force stretching between its poles are at right angles to the beam, then the beam will be displaced in a direction at right angles to itself and to the magnetic lines.

A further important fact about the cathode rays is that when they strike a solid obstacle, such as the wall of the glass tube, they heat it strongly where they impinge, and give rise at the same time to an emission of *X*-rays. These rays are not connected with the visible (green) fluorescent light which is given out by the glass wall, and which has already been mentioned. This is proved by the fact that the cathode rays are received on a metal target, this gives out *X*-rays, but no visible light. Although, as we have mentioned, the path of the cathode rays, like the path of a searchlight beam, is marked out by a luminous haze, this is not the essence of the phenomenon in either case. The track of the searchlight, as seen from the side, is conspicuous if the air is misty : otherwise very much less so. Similarly, the track of the cathode rays is easily traced if the gas pressure is say a ten-thousandth of the atmospheric pressure. If the pressure is much less, say a millionth of the atmospheric pressure, the track is no longer traceable but the characteristic effects are produced when the rays strike a solid obstacle.

There were two schools of thought about the nature of cathode rays. It may be said broadly that the English school considered them to be corpuscular, carrying a charge of electricity, and that the German school considered them to be of the nature of a wave propagation. In the present state of science this issue is by no means so definite as it was at the time between 1870 and say 1905, of which we are now writing. I do not think that it would be useful or would conduce to clearness if the difficulties of the present day, which were then undreamt of, were imported into the history of thought in those times. In the present account the issue will be presented in the way which appealed to contemporary thought.

The corpuscular view was probably first insisted on by C. F. Varley in 1872, and Lord Kelvin was always strongly insistent on the merits of Varley's contribution. The beautiful experiments of Crookes about 1879, which from the point of view of showmanship have perhaps been scarcely rivalled in any field of scientific experiment, were also interpreted in this way, and although in some cases the interpretation was too naïve there can be no doubt that he strengthened the corpuscular view considerably.

Crookes was perhaps the first to show in a really clear and satisfactory form the magnetic deflection of the rays, though this had been in a sense foreshadowed by Plücker as early as 1858. The rays are found to be notably deflected by even weak magnetic forces such as can be produced by a small horse-shoe permanent magnet. This experiment is quite easy to repeat, and has generally and rightly been considered crucial. We shall see why in the sequel. In the meantime, it is desirable to remark that a wire carrying an electric current experiences the same kind of force, in a magnetic field, and is pushed at right angles to its own direction and to the magnetic force.

The most important exposition of the anti-corpuscular point of view was in a paper by Hertz in 1883. He made experiments with a view to detecting an effect of the cathode stream on a magnetic needle. These experiments seem open to obvious objections which he never mentions. As regards the magnetic deflection his point of view will best be explained in his own words (translated) : " It seems to me probable that the analogy between the deflection of the cathode rays and the electro-magnetic action is quite superficial. Without attempting any explanation for the present, we may say that the magnet acts upon the medium, and that in the magnetised medium the cathode rays are not propagated in the same way as in the unmagnetised medium. This statement is in accordance with the above-mentioned facts, and avoids the difficulties. It makes no comparison with the deflection of a wire carrying a current, but rather suggests an analogy with the rotation of the plane of polarisation in a magnetised medium."

Besides Hertz, E. Wiedemann and E. Goldstein, two other well-known workers on this subject, advocated a similar point of view. Von Helmholtz, who was Hertz's master, also seems to have supported it for a time, though a few years later he came to think otherwise.

Another important point tested by Hertz was to try whether the cathode rays, fired into a metallic vessel (known in this connection as Faraday's cylinder or Faraday's ice-pail), would carry with them an electric charge, detectable by an electrometer connected with the vessel. He failed to observe this effect, but the design of his experiment was open to certain objections which were removed in a later investigation by Perrin in 1895, directed to the same question. Perrin got definite evidence that the rays carried a negative charge. J. J. Thomson, in a modification of Perrin's experiment, showed that if the Faraday cylinder was put out of the line of fire of the cathode rays, it acquired a charge when, and only when, the cathode rays were so deflected by a magnet as to enter the cylinder.

J. J. Thomson previous to 1895 had been much exercised by these difficult and dubious questions, and he recapitulated the controversy in a course of lectures on electric discharge which he gave in 1894 and which I attended as a freshman. This was certainly not what a freshman ought to have been doing, but I do not regret it. Much of what has been said above recapitulates what I then learnt from him. He left the impression that he considered the magnetic deflection almost conclusive evidence for the corpuscular theory, and allowed us to see that he was not impressed by Hertz's suggestion of an analogy with the rotation of the plane of polarisation. The observation by Hertz that the cathode rays could get through gold leaf and the development of it which had just been made by Lenard, who got the rays out into the open air through a thin aluminium window, was felt to be a hard nut to crack; it was difficult to envisage electrified particles as getting through an air-tight metal partition, and Thomson was inclined, if I remember rightly, to think that perhaps a new corpuscular stream might be generated on the far side.

Thomson had always been impressed by the magnetic deflection of the cathode rays, which distinguishes them so sharply from light and from X-rays, as giving the key to the whole problem. In this he differed from the German physicists, who, prepossessed with the other view, were inclined to emphasise the phenomena which seemed to them to confirm it. He began by measuring the amount of magnetic deflection. This he did by arranging that the beam of cathode rays should be immersed in a region of uniform magnetic force of known amount, produced by a large coil of wire. This field did not need to be very strong. It was found to be enough to use a field of 35 units, *i.e.*, about 200 times the horizontal magnetic force of the earth, and this would bend the cathode rays used into a circle of 9 cm. radius. J. J., writing about this subject long afterwards, said : " I had for a long time been convinced that these rays were charged particles, but it was some time before I had any suspicion that they were anything but charged atoms. My first doubts as to this being the case arose when I measured the deflection of the rays by a magnet, for this was far greater than I could account for by any hypothesis which seemed at all reasonable if the particles had a mass at all approaching that of the hydrogen atom, the smallest then known."

On the corpuscular view, the stream of charged particles constitutes an electric current, and experiences a lateral force in a magnetic field in the same way as a wire carrying an electric current. It is true that there is a difference, since in the one case it is a stationary piece of metal that experiences the force, and in the other (we suppose) a stream of separate electrified particles in rapid motion. This difference is partly bridged over by an experiment made by Rowland on the magnetic effect of electrical convection. But in discussing the deflection of the cathode rays it is assumed that their current-carrying aspect is the essential one. We calculate the sideways force as being the same as on an element (short length) of a current-carrying wire. In a magnetic field of 1 gauss the wire carrying 1 ampère experiences a force of $\frac{1}{10}$ dyne for every centimetre of its length. Now transfer this point of view to the cathode beam in a magnetic field of one gauss. If it conveyed a whole ampère, a whole coulomb of electricity would pass any point in a second, and 1 cm. length of the beam would have a charge of $1/v$ coulombs, if v were the velocity. Upon this charge there would be a force of $\frac{1}{10}$ dyne. This illustrates by a special case how the sideways push on a moving charge can be calculated in terms of the velocity and the amount of the charge, and the magnetic field in which it moves.

Some writers have used with advantage the phrase " magnetic stiffness " of cathode rays to express the strength of the transverse magnetic force necessary to bend them, just as one measures the stiffness of a spring by the mechanical force necessary to bend it. (It must not be forgotten, however, that the spring is deflected in the direction of the transverse mechanical force applied, whereas the cathode rays are deflected at right angles to the direction of the transverse magnetic force applied to them.) Now the magnetic stiffness does not depend only on the mass of the particles as the above quotation from Thomson might suggest, if the qualifying phrases were ignored. It depends really on two things, one of them being the velocity of the particles, and this, as may be imagined, is variable according to circumstances. But there is another quantity involved, which requires a little more explanation. Evidently the electric charge carried by the particles enters, since it is on this that the electro-magnetic action depends, but the acceleration which the electro-magnetic force produces in moving the particles sideways will be less if the particle is massive than if it is less massive. The mass of the particle and its charge enter into the question not independently, but as a ratio. It is fairly easy to see from another point of view that this must be the case. All the particles of the stream move along the same curved path. If we supposed two of them temporarily stuck together, nothing would be changed. The curved path would still be followed by the particles, now imagined to be Siamese twins instead of mere neighbours. The aggregate mass is double, but the charge is double also, and the change is without effect. This indicates that the mass and the charge enter as a ratio. This ratio, combined with the velocity, determines the amount of the magnetic deflection; or, conversely, knowing the magnetic deflection, we can obtain some information about the ratio of charge to mass and the velocity. If we make a guess at the velocity, we can determine what ratio of mass to charge would follow. Thomson thought that the velocity was almost certainly large compared with ordinary molecular velocities.

It may be suggested what we want to know is the mass, and not merely the ratio of mass to charge. As a matter of fact, the latter was the more instructive, because Thomson knew the ratio of mass to charge of ordinary atoms, by the phenomena of electrolysis, and it was a comparison of this kind that led him to doubt whether the cathode ray particles could be atoms. He has not given us his provisional calculations in detail, but it is possible to reconstruct them. Take the case mentioned when the rays are bent into an arc of 9 cm. radius by a magnetic force of 35 units (gausses) applied transversely. If the rays consisted of charged hydrogen atoms, then, as in the electrolysis of water, about 10^{-5} gram of hydrogen is associated with a coulomb of electricity.* We can find what velocity a stream of such particles would need to have in order to bring its magnetic stiffness to the observed value. The required velocity is 31 kilometres per second. It may assist the imagination to recall that this is about the velocity of the earth in its orbit round the sun.

If the atom of hydrogen were left to find its natural velocity when in temperature equilibrium with the molecules of air in a room, the velocity would be about 2·6 kilometres per second, about $\frac{1}{12}$th the hypothetical velocity we have calculated for it in the cathode rays. Thomson did not think that 12 times the normal velocity they would have had anyhow was enough to confer upon hydrogen atoms the extraordinary properties possessed by the cathode stream : even allowing for the fact that they were electrically charged. The argument in this form was not conclusive. We are merely trying to imagine in a little more detail what Thomson has hinted to us of his intellectual gropings at this stage.

Evidently it was necessary to know something more about the cathode rays than their magnetic stiffness if the argument as to their nature was to be made in any sense complete. One sufficient reason for this is that the magnetic stiffness is variable, becoming greater as the tube is more highly exhausted and the discharge potential increases. It is not in itself a standard datum, because it involves not only the nature of the particles, but also their velocity. We must have some other information if we are to get any further. In mathematical language we require two equations to determine two unknown quantities.

Now there were other properties of the cathode rays which lent themselves fairly easily to measurement, and Thomson saw clearly that he was certain to get some result which would help to clarify his ideas if he measured (1) the heating effect of the rays, and (2) the electric charge carried. Both these effects, as we have seen, had been well and definitely observed in a qualitative way, and it could not be doubted that the measurement was feasible, and indeed comparatively straightforward to carry out. Neither of these quantities would be of any definite use *alone*, because they would depend on the arbitrary intensity of the rays. It is no use, *e.g.*, cutting off a piece of rope at random, and expecting to find out anything of value by carefully measuring its length *only*. If, however, we measure the *length and weight* of the same piece, we shall learn something about the character of the rope. In the present case it was necessary to measure the charge and the energy carried by a certain arbitrary quantity of cathode ray stuff—no matter what quantity as long as we carry out both measurements on the same (arbitrary) portion.

Thomson now made a quantitative experiment on these lines. He measured the electric charge in the way already indicated, and by placing a thermo-couple inside the metal case or " Faraday cylinder " into which the rays were received, he measured the rate of heating of the thermo-couple as well as the rate of charging of the Faraday cylinder and the electrical condenser connected to it. Knowing the thermal capacity of the one in calories per degree, and the electrical capacity of the other in microfarads, he could compare the energy received with the electric charge received. But the energy depends on the mass and velocity. In this way a relation is obtained between the electric charge on the one hand and the mass and velocity on the other; or if we prefer so to express it, between the velocity on the one hand, and the ratio of charge to mass on the other. One such relation had already been obtained by measuring the magnetic stiffness. Combining the two relations it was possible to determine without ambiguity the velocity and the ratio of mass to charge.

Thomson had found that a field of 35 gausses would bend the rays into an arc of 9 cm. radius. His further experiments showed that the rays carried energy at the rate of $2{\cdot}6 \times 10^{10}$ ergs per coulomb of electricity. Combining these data, it is possible to deduce that :

The velocity is 15,000 kilometres per second.

The ratio of mass to charge is 2×10^{-8} gram per coulomb.

Thus the more complete information obtained by the measurement of charge and energy showed that, as Thomson had guessed, the velocity was enormously larger than molecular velocities, and that the particles were something entirely different from hydrogen atoms, having a much smaller ratio of mass to charge.

This conclusion, with the arguments which we have presented so far, was announced at a Friday evening lecture at the Royal Institution on April 30th, 1897. Thomson does not labour the momentous conclusion to which the experiments had led him, but says merely : "These numbers seem to favour the hypothesis that the carriers of the charges are smaller than hydrogen atoms."

It does not appear that this lecture made a great sensation in the scientific world, still less in the world outside. I do not think that I myself heard anything about it at the time, and only heard the conclusion he had reached some weeks later at Cambridge. The probabilities are that few of the audience really took in Thomson's argument, which, after all, requires the assembling of a good many lines of reasoning which were not then familiar. However, they no doubt realised that he was saying that he had found bodies smaller

* The coulomb is one ampère-second.

than hydrogen atoms, a statement which, in the then condition of science, was thought to be paradoxical, or even self-contradictory, an atom being (it was said) the smallest portion of matter that did or could exist. He did not himself think that what he said made many converts, and he believed that some of his audience did not think he was speaking seriously.

Thomson had gone forward so far on a fairly secure path—the properties of the cathode rays which he had measured were qualitatively quite well established, and even conspicuous, and when once the conception of measuring them had been grasped, he was able to proceed so far without serious difficulty. But there remained a formidable obstacle in the path, and until it was resolved the whole position was uncertain, and might be found to rest on unsound basic hypotheses.

If the cathode stream really consisted of electrified particles, it ought to be capable of deflection by a transverse electrostatic force. Thus, if it passed between the plates of a condenser, the negatively charged particles should be attracted by the positive plate, and repelled by the negative. That this should be the case was clearly appreciated by Hertz in 1883, but he had not succeeded in making the experiment work, and he regarded its failure as telling against the corpuscular nature of the rays. Since Hertz's work, the matter had been carried somewhat further by Goldstein, who described an experiment, not difficult to carry out, which certainly seemed to show that under some conditions the rays could be electrostatically deviated. Goldstein's experiment consisted in arranging two wire cathodes along the length of a cylindrical glass tube. They were parallel to one another, and lay on either side of the axis. If only one of these was connected, then the other one acted merely as an ordinary shadow-throwing obstacle, and cast a shadow on the opposite wall, because it screened this wall from the cathode rays. But if the second wire was connected with the first, the shadow became very much broader, though at the same time less dark. That it became less dark was natural, because the second cathode was now a source of rays. But why was it wider? This might be explained by assuming that there was electrostatic repulsion by the second cathode regarded merely as an electrified body. The rays from the first cathode would curl away from it on either side and it would form a much wider shadow than before, when the rays were propagated in straight lines.

Though Goldstein's experiment was definite as far as it went, and a helpful contribution towards solving a puzzling problem, it was not felt to have clarified the situation. The conditions around an active cathode contained many elements of uncertainty. The electric force near a wire could not be uniform under any circumstances, and when a discharge was going, its value at any particular place was incalculable owing to lack of knowledge of the distribution of free electricity in the surrounding space. Moreover, a space which was traversed by a luminous discharge was subject to unknown conditions. It was a land of magic and mystery, where anything might happen. What was wanted was a deflection in a space where there was simply a uniform measured electrostatic force and no unknown complications or uncertainties.

Thomson had thought about these things a good deal, and suspected that the conductivity of the residual gas might be the disturbing cause. He repeated Hertz's experiment, passing the beam of cathode rays between a pair of parallel plates, connected to a battery of storage cells. The beam was arranged to be narrow, and the position when it fell on the glass end of the tube could be pretty accurately located by the phosphorescence.

Thomson at first got the same result as Hertz—no deflection where the battery was connected. But his attention being concentrated on the question of conductivity of the residual gas, he measured this in the usual way, and found it to diminish rapidly as the pressure in the discharge tube was diminished. This gave him encouragement to try whether he could observe the electrostatic deflection at the lowest pressures. It was found at a certain stage that the expected deflection occurred for a moment when the deflecting battery was connected, but that the phosphorescent spot soon crept back to its undeflected position. Lowering the pressure still further, it was found that a permanent deflection could be obtained, proportional in amount to the voltage applied to the deflecting plates. He could detect it when this was only 2 volts. It was considered that the failure to get deflection at higher pressures was due to the accumulation of electric charges on or near the deflecting plates, which prevented the electric field between them being uniform and in effect protected the beam of cathode rays from really experiencing the lateral electric force which it was attempted to apply to it. This success in getting the electrostatic deflection greatly helped to clear the situation, and left little room for doubt that the corpuscular theory of the cathode rays was the right one. More than that, it formed the basis of an independent method of investigating quantitatively the properties of the rays, and checking the results already described.

Let us see what information can be got from observing both the magnetic and the electric deviation, or arranging for a balance between them. It is easy to arrange that they shall give deflections in opposite directions, and we shall suppose this done. We shall suppose also that the electric and the magnetic field are uniform, sharply limited and coterminous, conditions which unfortunately cannot be accurately realised in practice: but for our purpose the simplification can be allowed. Let us suppose further that the electric and the magnetic field are kept fixed at a constant value. Then, if a particle travels slowly along the length of the fields it will be pushed sideways by the electric force in full strength, just as if it were not moving at all. But when we come to consider the magnetic force, the case is far otherwise. A slow procession of particles means a small electric current conveyed, and therefore the mechanical force on the procession or stream is small, and it only exerts a small sideways push on each of them. In this case, then, the electric deflection predominates.

Consider now the other extreme, when the stream of particles is moving very fast. In this case the push of the electric field is the same as before, but the push of the magnetic field is enormously increased, and if the motion is fast enough, it predominates and there is outstanding magnetic deflection.

It is clear that an intermediate speed must exist at which these two opposite deflections will neutralise one another. What this critical speed is will clearly be found from the ratio of the two fields. If we doubled both, the balance would be preserved. If we increased, *e.g.*, the electric field only, the necessary speed would have to be increased in order that the magnetic push should still be able to balance the increased electric push.

We see, then, that the critical speed is tied up to the relative value of the fields, and we can say, from the theory of electromagnetism, what speed would be critical. If the speed is unknown, we can fix it by determining the relative value of the fields which will make it critical. If, for example, we applied a transverse electric field of 100 volts per cm. and found that we were able to compensate its action by a magnetic force of 10 gausses, the velocity must be 10^9 cm. per second. This was about what Thomson found in some of his experiments.

If the coterminous fields are 10 cm. long, the particle will traverse them in 10^{-8} second. The theory which we have already sketched shows that the transverse force due to the magnetic field in the case mentioned is 10^9 dynes on every coulomb of charge conveyed. It would of course require an enormous number of particles to make up as much as 1 coulomb. However, that is not the essence of the matter. The calculations we are now considering apply to cathode-ray stuff in the aggregate, and are not limited to one particle, or to a thousand particles.

We wish to consider what sideways drift this ought to produce on the stream when it traverses the full length of the magnetic field, the electric field being now removed, and the magnetic field acting alone. That depends on the mass associated with a coulomb of electricity. Let us make a tentative supposition about this, and suppose it were the same as in the case of hydrogen atoms in the electrolysis of water. We saw that this hypothesis broke down hopelessly before, but let us give it another chance, and see if it can do any better this time. A coulomb of electricity passing through acidulated water sets free 10^{-5} g. of hydrogen, very nearly. There are therefore 10^{-5} g. associated with a coulomb of electricity. The question is then how far would a mass of 10^{-5} g. be moved by a force of 10^9 dynes acting for 10^{-8} second. Those who have studied, *e.g.*, the free fall of a stone under gravity will be able to answer this question. The answer is that the distance would be only $\frac{1}{20}$th mm., a distance in any case difficult to measure by the unaided eye, and imperceptible in the conditions of experiments like these.

Actually, however, when the magnetic field of 10 gausses alone acts, the sideways displacement in traversing this field is very conspicuous, and amounts to about 5 cm.; so that evidently it would again be quite wrong to suppose, as we have done provisionally, that every coulomb of electricity was loaded with as much as 10^{-5} g. of mass, like hydrogen in electrolysis. The inertia of the stream is far less, and the distance it makes sideways is far more than could possibly be reconciled with this supposition, and in fact the large sideways displacement actually made shows that in the cathode ray stuff there can only be about 10^{-8} g. associated with a coulomb of electricity or 10^8 coulombs associated with 1 g. This confirmed the former result obtained by quite a different method, so the position was now very much strengthened. In no case known before this time was electricity associated with so small a mass as in the case which we have cited; so the provisional supposition which was proposed above was the best attempt that could be made to meet the facts. If we wanted to get the maximum of electricity on to the minimum of matter, charged hydrogen atoms as revealed in electrolysis represented the best that contemporary conceptions could do. But, as Thomson pointed out, this best was an entirely inadequate best. It was necessary to invent some kind of stuff such that a gram of it would carry not merely 10^5 coulombs, but 10^8 coulombs. In this account we have only used round numbers.

So far in discussing Thomson's work of 1897, I have avoided saying much about atoms or particles, because after all the phenomena described are not of such a nature as to reveal directly a particulate character in the cathode stream. They might equally occur if the matter concerned were a continuous fluid. But there could be little or no doubt that if the stream were to be regarded as material at all, it must consist of discrete particles. The low density of gases and their general behaviour had forced the conclusion that the molecules in them were moving freely, and were separated by wide interspaces, and the same must apply *a fortiori* to the cathode stream, which was much more tenuous still. Crookes had indeed spoken of the cathode stream as constituting a fourth or ultra-gaseous state of matter. What kind of particles did the stream consist of? Helmholtz had emphasised that the hydrogen atoms in electrolysis must be regarded as each carrying a specific charge. The following quotation is from his Faraday Lecture of 1881:

"The most startling result of Faraday's law is perhaps this. If we accept the hypothesis that the elementary substances are composed of atoms, we cannot avoid concluding that electricity also, positive as well as negative, is divided into definite elementary portions, which behave like atoms of electricity. As long as it moves about in the electrolytic liquid, each ion remains united with its electric equivalent or equivalents. At the surface of the electrodes decomposition can take place if there is sufficient electromotive force, and then the ions give off their electric charges and become electrically neutral."

If this conception of "atoms of electricity" was to be retained, it became very probable that the cathode ray particles carried each its atom of electricity, that is to say, that it carried the same charge as the hydrogen atom or other univalent atom in electrolysis. But, if so, it was necessary to assume that this charge was carried on a much smaller mass than the hydrogen atom. This followed from the fact, now proved, that cathode-ray stuff could carry a coulomb of electricity on a much smaller mass than electrolytic hydrogen could do.

This then was the argument which led Thomson to the most important result of his scientific life—the existence of masses of a smaller order of magnitude than atoms. The cathode ray stream was made up of these small masses, each charged with negative electricity. Thomson at this time called them " corpuscles." The word electron came later into general use.

It is a difficult matter to ensure the sufficient purity of gases used for experiments at these low pressures, but Thomson satisfied himself that the above properties of cathode-ray stuff were independent of the nature of the residual gas in the discharge tube, and also of the material of the electrodes. He summed up his further conclusion in the following way: " The explanation which seems to me to account in the most simple and straightforward way for the facts is founded on a view of the chemical elements which has been favourably entertained by many chemists. This view is that the atoms of the different chemical elements are different aggregations of atoms of the same kind. In the form in which this hypothesis was enunciated by Prout, the atoms of the different elements were hydrogen atoms; in this precise form the hypothesis is not tenable, but if we substitute for hydrogen some unknown primordial substance X, there is nothing known which is inconsistent with this hypothesis. . . .

" Thus on this view we have in the cathode rays matter in a new state, a state in which the subdivision of matter is carried very much further than in the ordinary gaseous state; a state in which all matter—that is matter derived from different sources such as hydrogen, oxygen, etc.—is one and the same kind; this matter being the substance from which all the chemical elements are built up."

Having reached the conception of the electron, it was almost inevitable that an active mind like Thomson's should attempt to interpret the structure of the atom in terms of it. There was not much to go on beyond the bare fact that all kinds of atoms apparently contained the same kind of electrons, and the structure of theory which Thomson built up from this fact was no doubt very speculative. Though it cannot be considered to stand in the condition of science to-day, yet it contained much which contributed to and foreshadowed our present notions.

To begin with, the general conception that the atom has a certain resemblance to a planetary system is used. I do not know who first suggested this comparison, but the facts of spectroscopy were enough to prove that the structure of the atom could not be simple, and the comparison is probably at least some decades earlier than Thomson's time, but it hardly amounted to more than a phrase. Prout had supposed that the constituent members of the more complex atoms were atoms of hydrogen. Thomson considered them to be electrons. We now think that there was truth in both notions. However, we are considering Thomson's contribution.

If the atom was a complex system containing electrons, the latter might be supposed to be describing orbits, or to be held stationary. The latter hypothesis was preferred, not so much on the ground that it was more likely as that it seemed more manageable. If the electrons were stationary in the atom, the structure must be such that it would give them stable equilibrium: that is to say, if an electron were displaced from its position, there must be a force tending to restore it to that position. The electrons being negatively electrified, they could only be held by positive electrification and the question arises how this positive electrification can be supposed to be distributed so as to keep the electrons in stable equilibrium. This is a form of what is sometimes called the problem of " Mahomet's coffin," which according to legend is supposed to float between heaven and earth without touching anything. This result cannot be got out of forces which vary as the inverse square of the distance. We cannot hold an electron in equilibrium by any forces arising from electrified bodies at a distance from it—as may readily be proved from the theory of attractions. On the other hand, it can be done if we put the electron inside a uniform distribution of electrification.

Thomson made use of this conception, which, as he mentioned, had been used by Lord Kelvin a little earlier. He supposed a sphere with positive electrification uniformly distributed inside it, and he placed his electrons inside this sphere, leaving them to find their positions of equilibrium under their mutual repulsions and the attraction of the positive electricity.

If there is only one corpuscle, it will place itself at the centre of the sphere. If there are two, they will be in equilibrium at equal distances from the centre, along a diameter. If there are three, an equilateral triangle meets the case, if four a regular tetrahedron.

In these cases the corpuscles rest in equilibrium, and they are all at the same distance from the centre, lying as we may say on a single shell. Thomson was able to show that when the number was greater, say seven or eight, this could no longer be the case. The corpuscles distribute themselves over two concentric shells; and with a certain further increase, three shells become necessary.

In these more complicated cases the theoretical problem becomes unmanageable, and Thomson appealed to certain experiments made with magnets by Prof. A. M. Mayer of the Stevens Institute of Technology, U.S.A. In these experiments (originally made about 1878) a long bar magnet was held vertically over a bowl of water, and on the water a number of thin permanent magnets made from needles floated in corks. The magnets were long enough for only those poles which were near to the water surface to count. The upper pole of the fixed bar magnet, and the under poles of the floating magnets were thought of as far enough off not to be of much account. The acting pole of the fixed magnet was positive, and of the floating magnets negative, and these poles then became the analogues of the positive charge and its surrounding electrons. The constraint introduced by the flotation secures stable equilibrium without the device of a sphere of positive attracting matter in which the electrons are placed.

The important feature of these experiments is that they show the formation of successive rings of magnets.

For example, any number of magnets up to five will arrange themselves at the angles of a regular polygon, but beyond this number they will not do so, and a new inner group begins to form itself by one magnet occupying a central position. This group develops as magnets are added until with fourteen magnets there are nine outside and five inside, when a third group begins to develop; this is complete when there are twenty-six magnets. I am pleased to be able to repeat these experiments for you to see. They are not as easy as they look on paper, and as some descriptions suggest. But a great improvement has been achieved by using magnetised needles of modern magnet steel, which were very kindly made for me by Mr. D. A. Oliver, of the research department of Messrs. William Jessop and Son. These magnets are more powerful than those made from sewing needles, and take up their positions more definitely, without being much affected by disturbing forces arising from capillarity.

J. J. emphasised these experiments as probably giving the key to the periodic law in chemistry, which, broadly speaking, states that the properties of the chemical elements are a periodic function of the atomic weight, just as the structure of the magnet pattern is a periodic function of the number of magnets thrown in. He used to show these experiments in his elementary lectures some years earlier and explain his ideas about them in relation to the periodic law. I think he did this before he had the electron idea at all. It was rather too strong meat for some of his students, and I remember a fellow student remarking to me that he thought it altogether fanciful.

The modern view derived by the detailed study of spectra on the principles developed by Bohr and his school is very like this, and far more definite. It affirms very definitely, for example, that the rare gases correspond to completed rings of electrons. Take neon as an example. The preceding electro-negative element fluorine contains one electron short of the complete ring, and the electro-positive sodium which follows neon, has one more, which cannot find a place in the ring. It would be tempting to read all this into J. J.'s account, and he seems to be rather near it; but not quite there. He was no doubt at a disadvantage in that the periodic law really refers to atomic numbers. It was in those days formulated for atomic weights, which do not follow quite the same order, and cannot be forced into the scheme without some " cooking ". His ideas can only be considered suggestive. The magnets were only a rough model of his model of the atom; and the atom itself was doubtless far from either. Nevertheless he did open up a new train of thought: and it has in a broad sense been singularly justified by events.

Passing over J. J. Thomson's activities in wartime, and as President of the Royal Society, I must say a few words in conclusion about his tenure of the Mastership of Trinity College, to which he was appointed by Mr. Lloyd George on the death of Dr. Montagu Butler in 1918. He resigned the Cavendish Professorship and the directorate of the laboratory, though he continued to work to some extent and to direct the work of others. But the college now became his headquarters, and he gradually established a great position there, winning the respect and affection of the fellows and of the undergraduates, and keeping a wise guiding hand on the college administration, and on its financial policy, for which he had a natural aptitude. His was a happy and successful life, and he made no small contribution to the happiness and success of his pupils, and to the fame of his country.