The Rutherford Memorial Lecture, 1957

The birth of the nuclear atom

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It is just fifty years since Rutherford took up his appointment as Langworthy Professor of Physics at the University of Manchester and there established the school of research which was to be so fruitful in great discoveries. It is less than fifty years since he first put forward the nuclear scheme of the structure of the atom. The spirit, the scale, the structure and, above all, the organization and administration of research in physics has changed so much in these fifty years that the discoveries in question may truly be said to belong to another age, an age so remote as to seem closer to the times of Goethe and of Beethoven than to those of T. S. Eliot and of William Walton. It appeared to me possible that the aspiring young, among whom are numbered the Rutherfords of the future, might be interested to consider certain notable doings of that distant age, while they can still hear of them from one who was himself young at the time in question. I have therefore taken as my theme, for the Rutherford Memorial Lecture, the birth of the nuclear atom, which dominates so much of the physics of today. And, as, in dealing with any notable birth, it is usual and proper to say something about ancestry, I shall begin with those early speculations on the structure of the atom which led up to the conception of Rutherford's model.

It was in the first years of the nineteenth century that John Dalton laid the foundations of his atomic theory, building up a system of chemistry from the assumption that every element was made up of homogeneous atoms of fixed mass, and that chemical compounds were built up by the combination of atoms of different elements in simple numerical proportions. This led to the allotment of an atomic weight to each element. Dalton's theory clearly required a large number of different kinds of atoms, corresponding to the large number of different elements.

Speculation soon began as to whether these atoms might not themselves be structures built up of particles of some one simple prime substance. As early as 1815 and 1816 William Prout, physician and chemist, put forward the hypothesis that all the elementary atoms might be constructed of hydrogen atoms. On the determinations then accepted all the known atomic weights were close to whole number multiples of the atomic weight of hydrogen, which made the theory plausible. Later, when fractional atomic weights became evident, Prout suggested that bodies 'still lower in scale than hydrogen' might exist, of which heavier atoms were built up. However, more accurate determinations of atomic weight by Berzelius, and later by others, showed that the atomic weights were not simple multiples of that of hydrogen, or even of a reasonable fraction of that of hydrogen. Finally, a position was reached when Stas, who had determined atomic weights...
with particular accuracy, declared 'I have reached the complete conviction, th absolute certainty, if it is possible for a human being to reach certainty in such a subject, that the law of Prout is nothing but an illusion, a pure hypothesis absolutely contradicted by experiment' (Stas 1860). It was left for Aston, by his work on isotopes, to show that Prout's law had a reality unsuspected, very naturally, by Stas, who reached his convictions from the assumption that atoms of a given element had the same mass.

However, in spite of fractional atomic weights, a feeling persisted that there was something in Prout’s hypothesis. For example, in a paper published in 1901 R. J. Strutt, later fourth Baron Rayleigh, having admitted that cases such as that of chlorine, atomic weight 35.455, contradicted the law as enounced, proceeded to a statistical calculation from which he deduced that atomic weights in general tended to approximate to whole numbers far more closely than could be explained by chance. He concluded, correctly, that we had strong reason to believe in the truth of some modification of Prout’s law.

To return to early speculations on atomic structure, in 1844 Faraday, just before the great period of research in which he demonstrated a connexion between magnetism and light by the rotation of the plane of polarization, published a little known paper entitled ‘A speculation touching electric conduction and the nature of matter’. In it, basing himself upon the properties of conductors and insulators, he argued against the then accepted views of atoms as hard particles with well-defined boundaries—minute billiard balls or bullets, as they were sometimes termed. Stating that as little as possible should be assumed, he preferred, following Boscevich, to whom he made due acknowledgement, to think of atoms as mere centres of forces and powers, not particles of matter...’. A consequence of Faraday’s concept was that atoms were penetrable and might be considered as highly elastic, instead of being supposed excessively hard and unalterable in form —‘a difference in degree, or even in the nature of the power consistent with the law of continuity, I can admit, but the difference between a supposed little hard particle and the powers around it I cannot imagine’.

Faraday’s paper has a particularly modern look by virtue of his use of waves to illustrate the combination of atoms and of the words ‘nucleus’ and ‘atomic number’. The words, however, are not used in the modern sense and I have no desire to exaggerate Faraday’s prophetic forethought, which is in little need of aggrandisement. The paper is important as attacking so strongly the conception of the atom as an impenetrable particle and as, among other things, contemplating the conduction of electricity in solids as evidence for atomic properties. It is, perhaps, best to let Faraday speak his own concluding words—‘but it is no part of my intention to enter into such considerations as these [gravitation and cohesion in terms of electricity], or what the bearings of the hypothesis would be on the theory of light and the supposed aether. My desire has been rather to bring certain facts from electrical conduction and chemical combination to bear strongly upon our views regarding the nature of atoms and matter, and so to assist in distinguishing in natural philosophy our real knowledge, i.e. the knowledge of facts and laws, from that, which, though it has the form of knowledge, may, from its including so
such that is mere assumption, be the very reverse’, words which are not without application in the present time.

Clerk Maxwell in 1875, after preliminary discussion in which he did not commit himself as to the nature of the ‘small parts’, effectively assumed finite hard particles when he came to expound the kinetic theory of gases. He considered the problem of light emission: to account for sharp spectral lines he assumed that the molecules, while describing their free paths, were executing vibrations which he compared to those of a bell. This view of atoms as hard particles was general many years later: Rutherford, born in 1871, said ‘I was brought up to look at the atom as a nice hard fellow, red or gray in colour, according to taste’. Maxwell assumed that all atoms of the same element had the same mass. On the whole Maxwell’s considerations do not represent an advance: he is particularly insistent on the impossibility that atoms have been formed or broken ‘either on earth or in the sun or the stars, either now or since these bodies began to be formed’.

Somewhat later, in 1884, Stokes, pursuing the notion that the elements might be very stable compounds of a single kind of matter, suggested that an unidentified line in the spectrum of nebulae might be due to some form of matter more elementary than any known on earth (Stokes 1892). It was, however, two years later that William Crookes (1886), in a famous address full of bold speculation, gave, as a conjecture, the solution of the riddle set by Prout in his hypothesis, which solution is that atoms of different mass may have the same chemical properties. He pointed out, as a general observation, that in more cases than could be due to chance the atomic weights were very near to whole numbers, a point later driven home by Strutt in the paper to which reference has already been made. Crookes explained this prevalence of whole numbers by suggesting that the different kinds of atoms were all built from one prime substance, to which he gave the name of protyle—not so far from proton! This cosmic protyle originally existed at an immensely high temperature and as it cooled the elements were formed in order, hydrogen being the first born.

Crookes then proceeded to foretell the existence of isotopes, more than thirty years before Aston established their existence, by pointing out that the existence of atoms of the same chemical properties but different masses would explain the departures from Prout’s law. To show how clear he was on this point I give his actual words: ‘I have said that the original protyle contained within itself the potentiality of all possible atomic weights. It may well be questioned whether there is an absolute uniformity in the mass of every ultimate atom of the same chemical element. Probably our atomic weights merely represent a mean value around which the actual atomic weights of the atoms vary within certain narrow limits. . . . I conceive, therefore, that when we say the atomic weight of, for instance, calcium is 40, we really express the fact that, while the majority of calcium atoms have an actual atomic weight of 40, there are not a few which are represented by 39 or 41, a less number by 38 or 42, and so on. We are here reminded of Newton’s “old worn particles”. The example chosen turned out not to be a very happy one, since 97 per cent of the calcium atoms are Ca 40 and the other isotopes are all heavier, but this does not affect the clarity and correctness of the pronouncement.
Crookes quoted Johnstone Stoney as having given evidence for the existence of a unit of electricity in nature, the unit to which Stoney later (1891) gave the name ‘electron’ and said ‘definite quantities of electricity are bestowed upon each element at birth, on these quantities its atomicity depends’, it being clear from the context that by ‘atomicity’ is meant atomic properties. This introduction of an atomic unit of electric charge into the question of atomic structure is an added example of astonishing foresight.

Stoney himself, in the paper just cited, which does not seem to have received the attention it deserves,* set out to explain certain features of optical spectra in particular double lines, for which purpose he assumed that spectral emission and absorption were due to the periodic orbital motion of electrons in the atom (which, like Maxwell, he usually terms molecule). This motion was produced by interatomic impact. He further assumed that the electrons moved in a free space in the atom, saying specifically ‘We may therefore adhere with confidence to the hypothesis made in this Memoir, that there are relative motions going on within the molecules which are unimpeded except by the aether’, and elsewhere he refers to the energy given out as electromagnetic waves being the only loss of energy. He takes the number of electrons concerned as equal to the degree of valency, and therefore considers the spectra of the alkali metals, sodium in particular, as being of comparatively simple origin. This bold attempt to explain optical properties by unimpeded electronic orbits within the atom, before the existence of the free electron had been experimentally demonstrated, is astonishing.

Up to 1895, then, there had been much inspired speculation as to the structure of the atom, based mainly upon chemical considerations, although optical properties had already been regarded as relevant. It had been concluded that the atoms of the various elements were built from a common substance: that atoms were penetrable: that they contained electrons. I have quoted the actual words of many of the early atomists to show how clear they were on certain fundamental points. There had, however, been little that could be regarded as convincing experimental evidence on these points. In that year, 1895, Roentgen discovered the rays which bear his name and with this discovery modern physics may be said to start. In 1897 the existence was clearly established of the electron, a negative unit charge, of mass exceedingly small compared to that of the hydrogen atom. It could be derived as a separate entity from elements of all kinds. A definite and authorized brick was then at hand for those who wished to plan the structure of the atom.

The first quantitative success came when H. A. Lorentz (1897) furnished a theoretical explanation of the effect discovered by Zeeman in 1896. On the assumption, already made by Johnstone Stoney, that atoms contained, as part of their structure, electrons circulating in orbits, he calculated the wavelength and the polarization of the line components produced by the magnetic field in the simplest cases. The value of $e/m$ which his method yielded agreed with that found by direct methods. The investigation did much to strengthen the belief that electrons were a constituent of every atom and were responsible for the optical properties of matter.

* I have found no reference to it in E. T. Whittaker’s *History of the theories of aether and electricity*, 1951; or in J. Larmor’s *Aether and matter*, 1900.
About this time Lenard (1895) showed by the help of his 'window', which gave a beam of cathode rays outside the tube, that, with any particular voltage, the absorption of these rays was proportional to the density of the medium traversed, whatever the chemical nature of the substance and no matter whether it was in the solid or the gaseous state. These results pointed to a component, responsible for the absorption, which was common to all kinds of atoms and was present in numbers proportional to the atomic mass. Following up these first absorption results Lenard (1903) showed that for cathode rays accelerated by a large potential all, i.e. for moving electrons of high energy, the absorption was so small that it could only be explained by supposing that the major part of the atom allowed these electrons to traverse it without hindrance. This was a result of prime importance, establishing clearly that, far from being a hard sphere, the atom was mostly empty space. To explain the results Lenard assumed that atoms were built up of particles which he called dynamids, a dynamid consisting of an electron very loosely associated with a positive charge. The dynamid itself was impenetrable: every swift particle could pass freely between the dynamids but the field of force which surrounded the dynamid was responsible for the absorption of slower electrons, by producing a sudden diminution of velocity. The radius of the dynamid estimated as of the order $10^{-12}$ cm—less than $3 \times 10^{-12}$ cm was the figure which he gave. The number of dynamids in an atom was taken as proportional to the atomic weight, to account for the absorption results. Lenard was not definite as to whether the hydrogen atom contained one or more dynamids, but seemed to incline to one. He expressed his conclusions in very striking manner by saying that the space occupied by a cubic metre of solid platinum was empty, in the same sense that celestial space traversed by light was empty, except for the volume of the dynamids, which could not in all exceed a cubic millimetre. This was the first attempt to deduce something quantitative about atomic structure from the behaviour of matter to swift particles.

This conception of Lenard's was a very striking advance. The volume which he found for the impenetrable part was of the same order as the volume of the nucleus of the modern atom: the positive charge was likewise associated with the mass. In a heavy atom, however, instead of a single nucleus Lenard supposed a number of impenetrable particles, surrounded by intense fields of force.

At about the same time J. J. Thomson (1905) approached the problem of constructing an atom from electrons and positive charge from another point of view, being mainly concerned with explaining chemical properties, in particular those expressed in the periodic table, although he referred briefly to spectral properties. On the classical theory of electromagnetism, universally accepted at the time as covering all aspects of charged particle interaction, any system of point charges, obeying the inverse square law of force, was unstable. J. J. Thomson adopted a suggestion of Kelvin's, that an atom was a sphere of positive electricity in which electrons were imbedded. Inside such a uniform sphere the force exerted by the electrification on a negative point charge is an attraction proportional to the distance from the centre. He considered the movement of electrons in a plane and showed that, for stability, they would have to be arranged in a series of concentric rings.
There could be up to, but not more than, 5 electrons in the innermost ring; the next surrounding ring might contain up to 10 electrons; the next up to 17, the essential point being that, for stability, the electronic structure possessed a periodicity of arrangement. Thus 60 electrons were arranged in five rings, containing respectively 3, 8, 13, 16 and 20 electrons, as we proceed out from the centre. 45 electrons in four rings, of numbers 4, 10, 14, 17: 30 electrons in three rings, of numbers 5, 10, 15. There is an obvious suggestion of periodic chemical properties but, equally clearly, no correspondence in numbers. In particular, the 5 electrons of the innermost ring do not correspond to any particular state of chemical properties and the ring was not completed once for all at a certain stage of atom building, but, for instance, varied from 2 to 5 electrons as the total number increased from 59 to 67. To explain the lack of quantitative representation of the periodic table it was pointed out that, whereas the electrons were probably distributed in shells in the actual atoms, the distribution in a plane, considered in the paper, was the only one that was mathematically tractable. The effects of the angular velocity of the rings of electrons was also considered, an instability when this fell below a certain value being used to explain radioactivity. With J. J. Thomson’s immense reputation behind it, this atom model received widespread attention.

There was always the difficulty that, on the classical theory, circulation of electrons in orbits necessarily implied radiation and so continual loss of energy, but the radiation with several electrons in a circular orbit is very much less than that of a single electron in the same orbit, and this was invoked to explain the stability of ordinary atoms.

However, in his article on Matter in the eleventh edition of the Encyclopaedia Britannica, 1911, written in 1910, J. J. Thomson says that positively electrified particles with the mass of the atom of hydrogen—which we now call protons—were the natural units of positive electricity and that ‘we regard’ an atom as built of a number of these units and an equal number of electrons. This would seem to indicate that he was then inclining to the concept of discrete massive units of positive charge as essential components of atomic structure, but the passage in question is followed by a brief account of the uniform positive sphere model which I have just described. The periodic properties which it possessed gave it a great attraction for him and for many contemporary physicists and chemists, especially as it could be prettily illustrated with floating magnets.

Other physicists, of course, had taken a hand at building atoms of positive and negative electricity before 1911. Nagaoka in 1904 actually proposed a kind of nuclear atom, a large number of electrons arranged at equal intervals round a circle, at the centre of which was a massive positively charged particle. This he used to discuss line and band spectra and radioactivity, but he reached no quantitative results of any value. Stark (1910-11), of the Stark effect, spent much not very successful effort on an atom model containing certain positively charged massive particles which he called archions. It is, perhaps, not unfair to say that for the average physicist of the time, speculations about atomic structure were something like speculations about life on Mars—very interesting for those who liked this kind of thing, but without much hope of support from convincing
scientific evidence and without much bearing on scientific thought and development.

It was in May 1911 that Rutherford published in the Philosophical Magazine, under the title ‘The scattering of α and β particles by matter and the structure of the atom’, the paper that gave the nuclear atom to the world. He had read a paper under a like title before the Manchester Literary and Philosophical Society on 7 March of the same year, but only an abstract was published, and that later. The α-rays, which were Rutherford’s favourite particles, had certain great advantages as probes for sounding the atom: single particles could be detected by the scintillation method, which in the hands of Rutherford and his school proved so powerful, and a beam of fixed initial velocity was to hand without need of special devices. Radium emanation suitably enclosed furnished a convenient source.

Rutherford’s concern was to explain the observations on scattering of charged particles by thin metal foils and he started on the first page with the essential fact on which he fastened, although to others it might not have seemed very significant. The experiments of Geiger and Marsden on the scattering of α-rays had shown that, with a certain foil thickness, about 1 particle in 20000 was turned aside through an average angle of 90°, while the most probable angle of deflexion for particles passing through the same foil was less than 1°. On any theory attributing the scattering to a number of small deflexions these figures were quite irreconcilable: with a most probable angle of 1° the chance of a deflexion of 90° was so remotely improbable that there would be no question of observing it. Hence, the large-angle deflexions must be due to single scattering. Just that.

To derive his well-known formula for single scattering Rutherford tells us to consider an atom which contains a charge $\pm Ne$ at its centre surrounded by a sphere of electrification containing a charge $\mp Ne$ supposed uniformly distributed throughout a sphere of radius $R$'. The formula is, of course, the same whether the central charge is positive or negative: in the one case the approaching positive particle is repelled and in the other case, that of attraction, passes the central charge and is then pulled back, like a comet looping the sun. He says that for convenience the charge will be assumed positive, but at the end of the paper repeats that the theory is the same whichever the sign and that so far there is no definite evidence on the point. Of electrons in the atom there is no discussion. He was concerned solely with scattering.

From the hyperbolic orbit described by the α-particle Rutherford worked out the probability of deflexion for a particle shot at random towards a charged centre (he did not use the word ‘nucleus’) and found good agreement with the experiments of Geiger and Marsden. He assumed that the central charge was proportional to the atomic weight and from this deduced results in good accord with experiment. As regards the absolute value of the central charge in terms of the electronic charge, from one set of Geiger’s experiments he deduced for gold the value 97 and from another set 114, from which he concluded that the value was about 100. That the central charge was equal to the atomic number—79 for gold—came later. The large-angle scattering was also found experimentally to be proportional to the thickness of scattering foil for small thicknesses, as it should be on Rutherford’s theory.
In this first paper Rutherford also considered the scattering of $\beta$-particles which had been measured by J. A. Crowther and interpreted by him in terms of the J. J. Thomson atom and multiple scattering. Rutherford found better agreement with single scattering. He wrote to W. H. Bragg on 9 February, three months before his great paper appeared, ‘I have looked into Crowther’s scattering paper carefully and the more I examine it the more I marvel at the way he made it fit (or thought he made it fit) J. J.’s theory. As a matter of fact, I find I can explain the first part of his curve of scattering with thickness, in large scattering alone I believe it is only the use of imagination, and failure to grasp where the theory was inapplicable, that led him to give numbers showing such an apparent agreement.’ I give this as typical of Rutherford’s somewhat ruthless enthusiasm when he went on the critical warpath.

The actual contents of the paper, which, as distinct from the possibilities, was all that was seen at the time, was admirably summarized by Marsden (1911), then working in the Manchester laboratory: ‘The author finds that an atom with a strong positive or negative central charge concentrated within a sphere of less than $3 \times 10^{-12}$ cm radius, and surrounded by electricity of opposite sign distributed throughout the remainder of the volume of the atom of about $10^{-8}$ cm radius, satisfies all the known laws of scattering due to either $\alpha$- or $\beta$-rays.’

The experimental fact that led to the whole conception of a nuclear atom was that a very few $\alpha$-particles were scattered through very large angles, 90° and more, by thin foils. Another man, and a good man, might have seen nothing very important in this—plenty of unexplained trifles turn up in the course of experimental research. The observed effects might have been due in some way to contamination. It may be recalled that an Oxford physicist, Frederick Smith, having found that photographic plates kept in a box near a Crookes tube were liable to be fogged, simply told his assistant to keep them in another place and so missed entering on a path that would have led him to the discovery of Roentgen rays. The characteristic of genius is to distinguish what is of first significance from what is of smaller importance and Rutherford was a towering genius. For him, that in a thin foil, which $\alpha$-particles could penetrate, there should exist atomic forces that could, in favourable circumstances, turn back upon themselves the paths of some of the enormously energetic particles was a prodigious effect. ‘It was’ he said later ‘quite the most incredible event that ever happened to me in my life. It was almost as incredible as if you had fired a 15 in. shell at a piece of tissue paper and it had come back and hit you’. In the atomic world in which his mind moved freely, a world which had a pictorial and personal reality for him, an $\alpha$-particle was a projectile of terrifying energy.

It may seem hard today to realize that the paper caused no commotion in the world of physics. At the time of its appearance I was working in Lenard’s laboratory in Heidelberg, a very active centre of research on electronic physics. I remember that the publication of C. T. R. Wilson’s account of his cloud chamber in June 1911 caused the liveliest interest, but I have no recollection of any attention aroused by Rutherford’s atom. This neglect was not confined to Germany. In Nature for 1911 the only mention is a brief summary of the Manchester paper to
which reference has been made, a summary of the same length and style as that of all the other papers cited. All that I have been able to find in the way of contemporary notice appears under the heading ‘Research Notes’ in the Athenaeum a weekly dealing with literature, science, the fine arts, music and the drama, which ceased publication as a separate journal in 1920.* Here, among other abstracts, there is a 300-word abstract of Rutherford’s paper, concluding ‘Altogether it is a very thoughtful and well-reasoned paper, which goes far to clarify our ideas on the subject’.

Rutherford himself does not appear to have considered his discovery as the epoch-making event that it turned out to be. In April 1911 he wrote to Hahn ‘I have been working recently on scattering of alpha and beta particles and have devised a new atom to explain the results, and also a special theory of scattering’, which is not very enthusiastic. His book Radioactive substances and their radiations, was published in 1913, the preface being dated October 1912, some eighteen months after the nuclear atom has been first worked out. In this book there are only two references to the subject. The earlier one is a two-page summary of the 1911 paper, giving the sign of the central charge as positive or negative. The word ‘nucleus’ occurs once, but in general ‘central charge’, or some similar phrase, is used. This is indexed under ‘scattering of α-rays, theory of’. The later reference, right at the end of the book and so presumably written not long before the date of the preface, is indexed under ‘Atom, structure of, to explain scattering’. Rutherford now declares definitely for a concentrated positive charge at the centre, surrounded by a distribution of electrons ‘which may be supposed to be distributed throughout a spherical volume or in concentric rings in one plane’. The neutral helium atom consists, he states, of two concentrated positive charges and two electrons; in general, the central charge is about half the atomic weight. An advance on the pioneering paper is the contemplation of an outer electronic structure of some kind. There are a few conjectures on the structure of the nucleus, which is assumed to consist, in part, at any rate, of charged helium atoms (helium nuclei) and electrons. These few pages contain remarkable examples of Rutherford’s foresight and immediate grasp of essentials, of which the following sentence affords a striking instance ‘It would appear as if the positively charged atoms of matter attract one another at very small distances, for otherwise it is difficult to see how the component parts at the centre are held together.’ Precisely! But while α-particle scattering and radioactivity are brilliantly, if briefly, discussed in terms of the new atom there is no reference at all to optical or X-ray emission and absorption, or to chemical properties, that is, to the functions of the electrons here definitely considered to form the extra-nuclear structure of the atom.

This neglect of chemical and optical behaviour, of which earlier atoms had attempted to give some account, may have been a cause of the small attention paid to the new model, outside Rutherford’s laboratory—that and the fact, already noted, that physicists did not, perhaps, take the matter of atomic structure very seriously in those days. To support this opinion I may cite the review of Radio-*

* ‘The “Athenaeum” [sic], that was the name! Golly, what a paper!’ R. L. Stevenson and Lloyd Osbourne, The wrong box.
active substances in Nature of 13 March 1913, written by R. J. Strutt, later Lord Rayleigh, to whose interest in the atom I have referred. He discusses the then position and future of radioactivity but to atomic structure makes no reference whatever. Norman Campbell's Modern electrical theory, preface dated March 1913, has a chapter 'The structure of the atom' in which no mention is made of Rutherford's atom, then two years old. Zeeman, in an article on magneto-optics published in 1915, two years after Bohr's papers, refers in a sentence to atom models put forward by Jeans, Larmor, Lenard, Nagaoka and Bohr, adding that in the optical field the atoms of J. J. Thomson and W. Ritz seemed the most successful. O. W. Richardson's The electron theory of matter was long a standard work; the preface of the first edition is dated May 1914. In this book is a considerable chapter on 'The structure of the atom', giving a careful and critical account of the position at the time, although it makes no mention of Stoney or Lenard (who were not, of course, Cambridge men). Of the attempts of Jeans, Nagaoka and Schott to explain spectral series by electronic orbits in the atom he says that they can hardly be considered to have been particularly successful. Most of the chapter is devoted to the J. J. Thomson atom and its chemical consequences. Rutherford, whose atom was then three years old, and Bohr, whose fundamental work we have still to consider, are introduced in the last few pages. Although he devotes little space to it Richardson speaks sympathetically of Bohr's work, saying that although the assumptions are against the accepted views, so are those required to account for the distribution of energy in temperature radiation. But in this book, written by a master of the physics of the time, then in his prime, there is no suggestion that the Rutherford atom represented a new departure of fundamental importance. In the second edition, however (preface dated January 1916), the account of Bohr's theory is greatly extended and it is termed one of the most striking features of recent progress in physics.

Bohr's first paper (1913a) appeared in January 1913, but in 1911, between this and Rutherford's great paper, there occurred a queer interlude. J. W. Nicholson, who died in 1955, published a theory (1911, 1911-12) which, at a hasty glance, might seem to be an application of Rutherford's atom model to chemical and spectral problems. He speaks of elements consisting of single rings of electrons rotating round very small massive nuclei, positively charged. This is, as far as I can find, the first use of the word 'nucleus' in the modern atomic sense. The elements in question are, however, primitive systems or 'protyles', out of which the familiar atoms are built, such protyles being coronium, hydrogen, nebulium and proto-fluorine, with nuclear charges 2, 3, 4 and 5, respectively. Three of the four are mythical substances: the spectral lines attributed to nebulium, for instance, which Nicholson made great play, were, as shown later, due to ionized oxygen and nitrogen. Ordinary atoms, except hydrogen, would then have two, or, in general, several nuclei: his theory was, in a sense, a cross between that of Lenard and Rutherford. His spectral theory was much along the lines developed by J. J. Thomson. The strange thing is, however, that he definitely states that spectra are to be explained in terms of Planck's constant (Nicholson 1912, p. 677) and that the angular momentum of the atom can vary only by discrete amounts. It is as
if he had overheard atomic conversation of a few years later, without really understanding it.

To see how from Rutherford's prototype came an atom that could explain optical, including X-ray, and chemical properties, we must return to early in 1912, when Bohr came to Rutherford's laboratory in Manchester. Here, of course, there was always excitement and discovery, and the nuclear atom was the subject of lively discussion at tea and other times. Bohr spent some months in this atmosphere and became convinced of the general correctness and the great possibilities of the nuclear model. In a paper (1913a), dated Manchester, August 1912, published after this sojourn he dealt with the theory of the passage of charged particles through matter, and here we find the atom consistently treated as a central positive charge, always called the nucleus, surrounded by a distribution of electrons. From his calculations Bohr concluded that the nuclear charge is 1 for hydrogen and 2 for helium. He also found values for the nuclear charges of certain heavier atoms—14, 38, 61 and 65 for aluminium, tin, gold and lead, respectively, which should be 13, 50, 79 and 78. Thus for aluminium the value is astonishingly close to the actual one, considering the method, but the other values are much too low.

This brings us to the extremely important step that was made when it was accepted that the nuclear charge was equal to the atomic number. The suggestion was first put forward by van den Broek (1913a) in a paper dated November 1912. In this paper, however, he assigns a separate place in the periodic table to the radioactive elements. At about the same time Fajans (1913) in Germany and Soddy (1913) and Russell (1913) in England independently formulated the radioactive displacement law, which stated that on the discharge of an \( \alpha \)-particle, with two positive charges, an element went down two places in the periodic table and on the discharge of a \( \beta \)-particle, with one negative charge, went up one place. This was the occasion of the coining of the word 'isotope' by Soddy, to denote elements of different atomic mass but of the same chemical properties and hence the same place in the periodic table. There were still some doubts about the nuclear atom, caused by the troublesome question as to the composition of the nucleus. It could not consist of \( \alpha \)-particles only, for in that case the atomic number would be just half the atomic weight. Towards the end of 1913 the possibility that the nucleus might contain electrons was being discussed (see, for example, van den Broek 1913b). Rutherford's position was that the nucleus had a resultant positive charge, with which he was concerned, but that he was not committed as to how it was made up, although radioactive nuclei probably contained electrons (Rutherford 1913).

The beautiful and systematic work of Moseley, killed in 1915 at Suvla Bay in the Dardanelles, on the characteristic X-ray spectra of the elements definitely decided the prime importance of the atomic number, expressed by the nuclear charge. In his own words (Moseley 1913), 'We have here a proof that there is in the atom a fundamental quantity, which increases by regular stages as we go from one element to the next. This quantity can only be the charge on the central positive nucleus...'. In Soddy's words, Moseley called the roll of the elements, for his connexion of X-ray frequency with atomic number clearly showed the gaps where elements were missing in the periodic table.
Returning to Bohr, it was in July 1913 that he published the first of his fundamental papers (1913b) ‘On the constitution of atoms and molecules’. In this he starts by quoting Rutherford’s first paper as stating that the atom consists of a positively charged nucleus surrounded by a system of electrons kept together by attractive forces from the nucleus, which, as we have seen, is rather more definite than Rutherford’s pronouncement then. He points to the difficulties in matters of stability which haunted the old models making use of classical electromagnetic theory, and says straight off that he proposes to bring Planck’s quantum into the question. Every student of physics knows the principles which he introduced: the stationary orbits, assumed of circular form, in which the electron obeyed the classical laws, but was limited by quantum conditions to certain of the infinity of orbits which these laws permit, and the emission of homogeneous radiation when the system passed, in some unspecified way, from one stationary system to another. In particular he developed the theory of the hydrogen spectrum, and showed how the Balmer series and the Paschen series were to be accounted for. His formula also gave the lines of the Lyman series, which was experimentally discovered later (Lyman 1914, 1915), and for the Brackett series, discovered later still (Brackett 1922). More remarkable still, he calculated from the electronic charge and mass, with Planck’s \( h \), the exact value of Rydberg’s constant. He showed that the helium nucleus, with one electron, would on his theory give series of spectral lines discovered by A. Fowler, and attributed by him to hydrogen, and another series, likewise attributed to hydrogen, observed by Pickering in the spectrum of \( \xi \) Puppis. A minor, but striking, fact to which Bohr drew attention, was that while only 12 lines of the Balmer series were observable in the laboratory, as many as 33 had been measured in the spectra of celestial bodies, where, of course, the pressure is very low and the atoms very sparse. The very large radius required for the electronic orbits of the higher frequency lines on his theory at once explained this. The absorption of luminous gases for light was likewise explained. These striking results, but more particularly the exact calculation from theory of spectroscopic constants, could not fail to rivet the attention of the open-minded physicist.

In this first paper Bohr also mentioned Roentgen spectra, which he said must be attributed to the settling down of an atom after the expulsion of a firmly bound electron, and suggested that the general atom must contain rings of electrons rotating round the nucleus, points which he developed a little later. There has seldom been written a paper so rich in original thought. As Whittaker, probably with Nicholson’s work particularly in mind, has written ‘With unerring instinct Bohr seized upon whatever was right in the ideas of his predecessors, and rejected what was wrong, adjoining to them precisely what was needed in order to make them fruitful, and eventually producing a theory which has been the starting point of all subsequent work on spectroscopy’.

In the next paper (1913c) published two months later, he elaborated the conception of the general atom as built of rings of electrons, with the nucleus as centre, all in one plane, at any rate for all but the heaviest atoms. Neutral hydrogen, of course, consisted of a singly charged nucleus and one electron, as worked out in the first paper: helium, a doubly charged nucleus with two electrons in the same
orbit. Lithium was endowed with two inner electrons and one outer one, and beryllium with two inner and two outer ones, as today. However, Bohr allotted 8 electrons to the inner ring for neon and for all subsequent atoms, the two outer electrons for the rare gases following the pattern of helium. The building up of complete rings of 8, to correspond to the second and third period of the table, was a feature of his scheme. He also pointed out how the chemical properties of the heavier atoms, after argon, demanded a recurrent configuration of 18 electrons in the innermost rings. The valency properties were ascribed to the outermost electrons.

With the lightest atoms he computed approximately correct values for the ionization potentials. Thus, up to beryllium, things were in good order: after that only the general principle of the building up of the complete rings, in stages, was indicated. This, however, was an epoch-making beginning, indicating the lines along which the nuclear atom was to be made to account for periodic chemical properties. It was on this foundation that Kossel built a more complete scheme.

Bohr calculated the energy required to remove an electron from the innermost ring of the atom, where practically the whole nuclear charge was effective in the attraction, and proved that the value obtained accorded well with the potential which experiment showed was necessary to excite the K series of X-rays. It was, however, left for Kossel to explain the excitation of the different series of X-ray lines and the X-ray absorption phenomena.

In a third paper (1913 d) which appeared in November 1913 Bohr was concerned with the combination of atoms to form molecules. The molecule of hydrogen was taken to consist of two nuclei held together, in some way, by two electrons rotating in one circular orbit in a plane normal to the line joining the nuclei and midway between them. This, however, was not very successful and the paper is chiefly notable as a first attempt at molecule building with nuclear atoms.

Thus Bohr in 1913 had explained in detail, on the basis of radically new but simple quantum assumption, optical spectra when the emitter was a nucleus and a single electron, had indicated clearly that in general the X-ray spectra were the concern of inner electrons, while the optical spectra were the concern of outer electrons and had made clear that the facts embodied in the periodic table were to be expressed by building up successive groups of electrons, the rare gases representing completed groups of some kind. There was the unsatisfactory feature that classical electro-dynamics governed the movements of the electrons in the selected orbits but not the radiation, while the selection of possible orbits and the transitions from one to another were the subject of special non-classical laws. The scheme was, however, destined to be the foundation of all modern work on atomic structure, as abundantly acknowledged in the volume published in honour of Bohr's seventieth birthday (Pauli 1955).

The man who immediately recognized the value of Bohr's work was J. H. Jeans, who in a Report on radiation and the quantum theory issued in 1914, and hence written just after Bohr's publication, gave a very sympathetic account of the work in question, concluding 'This brief summary of Dr Bohr's work has only attempted to touch on a few of the very interesting points covered by his original papers.'

Rutherford's attitude at the time was that the theory seemed to work but that
it did not have the pictorial clarity that appealed to him so strongly, an attitude exemplified in the answer—jesting, no doubt—that he gave when Willy Wien said ‘But no Anglo-Saxon can understand relativity.’ ‘No!’ laughed Rutherford ‘they have too much sense’ (Eve 1939, p. 193). Rutherford also picked on what was an undoubted difficulty in the letter which he wrote to Bohr on 20 March 1913 (Eve 1939, p. 221):

‘Your ideas as to the mode of origin of spectra in hydrogen are very ingenious and seem to work out well; but the mixture of Planck’s ideas with the old mechanics makes it very difficult to form a physical idea of what is the basis of it all. There appears to me one grave difficulty in your hypothesis, which I have no doubt you fully realize, namely, how does an electron decide what frequency it is going to vibrate at when it passes from one stationary state to the other? It seems to me that you would have to assume that the electron knows beforehand where it is going to stop.’

It was in 1913 that, as John Harling Fellow, I went to Manchester to work with Rutherford. Among those engaged in research in his laboratory were Walter Makower, who had been with Rutherford since 1907 and carried out extensive work on various aspects of radioactivity; E. J. Evans, a fine spectroscopist, who first definitely showed that lines attributed by A. Fowler to hydrogen were due to ionized helium, as required by Bohr’s theory; D. C. H. Florance, until recently Professor of Physics in the University of New Zealand; H. R. Robinson, who carried out with Rutherford work on the spectra of β-rays which initiated a series of significant researches on allied themes which he later carried out independently; C. G. (later Sir Charles) Darwin, famous for his theoretical investigations; Ernest Marsden, who had carried out with Geiger the fundamental experiments on α-particle scattering through large angles; Stanislaw Loria from Cracow and Bohdan de Szyszkowski from Warsaw. Moseley was working in the laboratory when I arrived, but at Christmas went to continue his work at Oxford, where J. S. E. Townsend, a leader in electronic physics, was professor.

A letter which he, Moseley, wrote to Rutherford from Oxford on 5 January 1914, confirms what I have suggested, that speculations on the structure of the atom were not taken very seriously in many, if not in most, laboratories and that Bohr’s theory was far from gaining immediate recognition. In this letter he records: ‘Here there is no one interested in atom building. I should be glad to do something towards knocking on the head the very prevalent view that Bohr’s work is all juggling with numbers until they can be got to fit. I myself feel convinced that what I have called the ‘h’ hypothesis is true, that is to say one will be able to build atoms out of e, m, and h and nothing else besides. Of the three variations of this hypothesis now going, Bohr’s has far and away the most to recommend it, but very likely his special mechanism of angular momentum and so forth will be superseded’. The discrete orbit mechanism was in due course superseded.

I worked then with Rutherford on the spectra of the γ-rays; one result obtained was that the rays from radius B had the same wavelength as the characteristic X-rays of lead, of different atomic weight, which established that the two were isotopes and so confirmed the significance of the atomic number. I well remember
that, after I had been in the laboratory for a few weeks, Kay, the privileged and indispensable laboratory steward, said to me ‘Papa says you’ll do’, Papa being the usual laboratory nickname for our revered professor.* I was naturally flattered and, fondly imagining that I had said something—at the tea at which we all gathered, perhaps—that had struck Rutherford as showing an intelligent perception in physics, I asked ‘What made him say that?’ ‘He saw you making that plate-holder out of cardboard and thought that you made a good job of it,’ was the reply. Rutherford saw in such improvisations, if effective, a saving in time and money, both of which he valued highly.

The laboratory was a scene of hard work, deep thinking, and light-hearted enthusiasm to which the completely unexpected outbreak of the first world war in August 1914 put an end. I will not attempt to describe the scene, first, because space is lacking and, secondly, because it has been graphically pictured by H. R. Robinson in the paper to which reference has just been made.

It so happens that the British Association met at Melbourne in August 1914, just after the outbreak of the war. On 18 August there was a discussion on the structure of atoms and molecules, opened by Rutherford. The only record which apparently exists is the abstract published in the annual Report (B.A. 1914), but it is interesting as an index to the thought of the time, especially that of the great originator of the nuclear atom. After referring to J. J. Thomson’s atom with respect he described how the measurements made with the scintillation screen, and C. T. R. Wilson’s cloud chamber photographs, showed the existence of large single scattering and how the application of his formula to the measurements proved the existence of a massive nucleus, with radius of the order of $10^{-12}$ cm. He said that there was ‘fairly good evidence’ that the atomic number gave the nuclear charge, which was fundamental, and stressed the importance of Moseley’s work. So far all accords with the modern view. He added, however, that the rate of vibration of the inner parts of the nucleus could be measured by means of the characteristic X-rays emitted, which I cite to show how little, at that time, was known of the electronic structure of the atom. Referring briefly to spectral emission, he said that Bohr had faced the difficulties by bringing in the quantum in a novel way, and that, at all events, there was something going on in the atom which could not be explained by the older mechanics. The impression given is that Rutherford thought that Bohr was probably on the right road, but that he had not attributed its full importance to Bohr’s astonishing work on the spectra of hydrogen and ionized helium.

Nicholson, who took part in the discussion, expressed great admiration for Bohr’s work, although he had some criticisms to make, and pointed out, correctly, that he himself had used Planck’s constant as a measure of angular momentum in his work on spectra. Moseley modestly presented his own work. These two were young men, of 32 and 26 years of age, respectively. Two elderly men also spoke.

* See Rutherford: Life and work to the year 1919. By H. R. Robinson. (The first Rutherford Memorial Lecture of the Physical Society, delivered 6 November 1942.) Published with four other Rutherford Memorial Lectures under the title Rutherford by those who knew him. This lecture gives a brilliant picture of the Manchester laboratory at the height of its achievement.
H. E. Armstrong, the controversial chemist, aged 66, said at some length that the new theories did nothing for chemistry, which was true at the time, and a good deal besides. W. M. Hicks, aged 66, who had spent much labour on the analysis of optical spectra, using a new unit of his own, the 'oun', in which nobody believed, said that neither the atom of J. J. Thomson nor that of Rutherford, had 'shown the slightest aptitude in explaining the series lines of spectra'! So the position was that Rutherford was convinced of the general correctness of the nuclear picture and thought that there might be something in Bohr's work: the younger men thought that Bohr had made a great advance, while the older men there present were sceptical, not to say unjust. Not that old men are, I hope, always sceptical and unjust.

Two important pieces of work by Walther Kossel contributed greatly to the establishment of the nuclear atom by giving fresh demonstrations of its power and scope. Kossel was working under Lenard in Heidelberg at the time, 1910–11, that I was there and we were good friends.* Thence he went to Munich, where Arnold Sommerfeld and Max von Laue were professors, and it was here that he published the papers in question. Sommerfeld was, of course, keenly interested in the theory of atomic structure, to which he made outstanding contributions, and his book *Atombau und Spektrallinien*, first published in 1919, was a standard work which went through several editions.

Already in 1914, before the importance of Bohr's work had been widely realized, Kossel showed how the fundamental features of the X-ray spectra could be explained on the general Bohr model of rings of electrons bound with different degrees of firmness. He fastened on the essential fact that, whereas with optical spectra single lines of a series could be separately excited—for example, the sodium D lines, in resonance radiation—with fluorescent X-rays a whole series of fines appeared together when the frequency of the incident exciting radiation exceeded a certain limit, but until that limit was reached no fines were emitted. He called the successive rings, starting from the nearest to the nucleus, the K, L, M, ... levels, taking the letters from Barkla's K and L series. The M series was discovered by Siegbahn later (1916). Kossel assumed that the excitation of the K series corresponded to the removal of an electron from the K ring to the periphery of the atom, and that the emission of the different X-ray lines was due to the filling of the gap so created by an electron from one of the outer rings, the Kα and Kβ lines, for instance, corresponding to the repairing electron originating in the level, respectively. The L series was similarly excited by the removal of an electron from the L level and its replacement from one of the higher levels, each line of the series corresponding to a different level. This simple scheme accounted quantitatively for a number of fundamental experimental facts—that all fines of a given series had the same excitation energy; that a greater energy was needed to excite the K than the L series (and later that a greater energy was required to excite the L than the M series); that combination relations existed between the different series, of which $K\beta - K\alpha = L\alpha$ is typical, the symbols indicating the quantum energies corre-

sponding to the lines in question; that the greater the atomic number the greater
was the energy needed to excite a given series. The increase of the characteristic
quantum number by 1 on passing outwardly from one shell to the next, gave the
observed relations between the energies of the lines. Moseley's law connecting
the square root of the frequency of a characteristic line with the atomic number
also followed from Kossel's simple conception. It was a great advance.

Not long after Kossel (1916a) pointed out that the expelled inner electron could
settle in any one of the peripheral, optical orbits, which had different energies,
although the differences of energy in question were small compared to the energy
needed to eject an electron from the interior of the atom. Consequently, the X-ray
absorption edge should not be sharp, but diffuse, and in the case when good resolution
was possible it should have a line structure, corresponding to the optical spectrum.
This, too, was experimentally verified, by M. de Broglie. The connexion between
X-ray and optical spectra, which can be traced as the atomic number is syste-

matically diminished so that what are inner levels for more complex atoms
become outer levels for simpler atoms, was a further step that came some
months later (Kossel 1920). Many important details of the structure of X-ray
spectra were left to be elucidated by later work, but Kossel showed satisfactorily
that the nuclear atom could account for the main features of X-ray emission
and absorption.

Turning to chemistry, the problem which Kossel sought to explain (1916b) was
the mechanism of valency and in particular its periodic dependence on atomic
numbers. He used the same basic assumptions as in his work on X-rays—that the
atomic number of any element gave the number of electrons in the neutral atom
of that element, and that the atomic structure consisted of rings or shells of
electrons bound with different degrees of firmness. He was mainly concerned with
explaining heteropolar compounds, i.e. those formed of atoms of an electropositive
and of an electronegative element. Fundamental for his theory was that the rare
gases were represented by structures with completed, particularly stable shells,
and thus that 2, 10, 18, 36, 54 and 86 electrons represented assemblies of closed
groups. Valency electrons were those of an incomplete outer group, and such an
incomplete group tended to disperse itself so as to leave an inert gas structure, or
to build itself up so as to form an inert gas structure, forming in this way an
electropositive or an electronegative ion. Thus, the alkali metal atoms were each
characterized by one electron above the nearest inert gas structure, which they
readily gave up to a halogen atom, this atom thus completing its outer group.
The ions then combined. Alternatively to taking up one electron and assuming
an argon-like structure chlorine could give up seven electrons, as in Cl2O7, and
assume a neon-like structure. An atom of an alkaline earth had two loose outer
electrons, an atom like oxygen was two electrons short of the stable, inert structure.
On these simple lines Kossel explained a variety of chemical compounds between
atoms of different kinds. He was also able to account for such phenomena as the
trivalent N in NH₃ taking another H from H₂O: the triple charge on the complete
spherical nitrogen ion, the 'kernel', gave it a greater binding power for the odd H
than the double charge on the oxygen ion, although one had already three H's
and others only two. The forces of attraction extended all round the kernel. He further introduced the part played by ionic size in combination and was successful in interpreting the structure of complex compounds.

By simple considerations based directly on the nuclear atom, Kossel succeeded, then, in explaining the formation of heteropolar and complex chemical compounds. He did not elucidate the long periods that succeed the two periods of 8, although his scheme covered the behaviour of atoms in the immediate neighbourhood of inert gases all through the table. He did not deal effectively with the behaviour of carbon in organic compounds, nor with homoeopolar molecules, such as those of elementary bimolecular gases. About the same time as Kossel's paper appeared, Lewis (1916) published his theory of homoeopolar compounds based on shared electrons, a fundamental piece of work. But my purpose here is to put before you how the nuclear atom began and how its powers were first shown. Kossel's work arose essentially out of the nuclear atom, that of G. N. Lewis, with its stationary electrons, does not so much depend on it. Any kind of elaboration would, again, take me too far from my intention. It might, however, be permissible to refer to the Kossel-Sommerfeld (1919) displacement rule, which gave a pretty confirmation of Kossel's conception of the status of the outer electrons. This rule states that the character of the optical spectrum, particularly as regards multiplicity of lines, is the same for the arc (neutral atom) spectra of the alkali metals and the spark (ionized atom) spectra of the alkaline earths and, in general, that the spark spectrum of any element corresponds to the arc spectrum of the element preceding it in the periodic table. The rule was later verified for doubly and trebly ionized atoms.

If I have laid some stress upon Kossel's work it is because it seems to me to have been of fundamental importance. Bohr himself thought very highly of it, saying that the stability relationships of atomic structure, as evidenced by the Roentgen spectra and by the chemical properties, were clearly brought out in two very important papers by Kossel (1922). Kossel's ideas were simple and fundamental and may be considered to have completed the first step of the development of the nuclear atom by showing its applications to fields not considered at its inception. It was Rutherford who from considerations of one simple anomaly showed that the nucleus must exist; Bohr who, by the introduction of the quantum theory, laid the foundations on which all subsequent work on the electronic structure of the atom has been built. Kossel, a great, even if a lesser, man, did much to point the way to the establishment of the electronic groups and the role of the outer electrons.

The main body of the work which I have surveyed had, in spite of the first world war, been completed before the end of 1916. Elaborations of the first importance, such as the independent enunciation in 1915 by W. Wilson and A. Sommerfeld of the quantum condition known by their joint names, had taken place within the period covered. If I have said nothing of such advances it is not because I underrate their significance, but because I have deliberately restricted myself to what I regard as first beginnings of the prodigious scheme of atomic structure that is inseparably connected with the research that dominates modern physics,
whether we consider the number of first-class—and of lesser—brains engaged on it, the money expended, the intellectual significance of the results or their significance for the material welfare or the bodily extinction of the human race.

References

Bohr, N. 1913a Phil. Mag. 25, 10.
Bohr, N. 1913b Phil. Mag. 26, 1.
Bohr, N. 1913c Phil. Mag. 26, 476.
Bohr, N. 1913d Phil. Mag. 26, 857.
Broek, A. van den 1913a Phys. Z. 14, 32.
Broek, A. van den 1913b Nature, Lond. 92, 477.
Faraday, M. 1844 Phil. Mag. 24, 136; see also his Thoughts on ray-vibrations, Phil. Mag. 28, 345.
Kossel, W. 1916b Ann. Phys., Lpz., 49, 229. (The paper is 134 pages long; the abstract in Chem. Abstr. is fourteen lines long.)
Kossel, W. 1922 Z. Phys. 9, 1; see particularly p. 12.
Lorentz, H. A. 1897, in Zeeman Phil. Mag. 43, 232.
Lyman, T. 1915 Phil. Mag. 29, 284.
Moseley, H. G. J. 1913 Phil. Mag. 26, 1024; see also 27, 703.
Nagaoka, H. 1904 Phil. Mag. 7, 445.
Rutherford, E. 1911 Phil. Mag. 21, 669.
Stokes, G. G. 1892 Burnett Lectures. In three courses delivered in Aberdeen in November 1883, December 1884 and November 1885.
Strutt, R. J. 1901 Phil. Mag. 1, 311.
Thomson, J. J. 1905 Electricity and matter. (Gives an account of the Silliman Lectures delivered at Yale in 1903; the preface is dated August 1903.) See also Phil. Mag. 7, 237 (1904).