J.P.V.MADSEN: CORRESPONDENCE FROM LORD RUTHERFORD 1911-1937.

THE STRUCTURE OF THE NUCLEAR ATOM & RADIO WORK IN AUSTRALIA TO 1937.

Prepared by Roger Madsen, Sydney 2019.

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(Prepared by R.W.Madsen, September 27, 2019).

Introduction.

In 1909 Prof. W. H Bragg had returned to England from Adelaide to take up the position of Professor of Physics at Leeds University in close proximity to Manchester where his good friend Ernest Rutherford was engaged on his continuing work on radioactivity. J P V Madsen had returned to Sydney University in 1909 as Lecturer in Electrical Engineering having worked closely with W H Bragg in Adelaide in research work from 1905-1908 & Bragg took it upon himself by lengthy letters to keep Madsen updated on research work in England & to also draw to Rutherford's attention the work that Madsen had published in 1909 on the Scattering of Beta Rays. This correspondence has been documented in the Historical Records of the Australian Academy of Science in 1982 by Prof. R.W. Home & the purpose of this paper is to publish the original letter sent by Ernest Rutherford on March 8, 1911 to Madsen in Sydney on the day after Rutherford's proposal of the nuclear atom made in Manchester.

Madsen's work was referred by Rutherford not only in his famous paper of May 1911 "On the scattering of alpha & beta rays & the structure of the atom" but also in 1913 on work on Gamma radiation. In 1927 & over the following 10 years Madsen was in contact with Rutherford, by then at the Cavendish Laboratory in Cambridge, concerning the initial selection of personnel for the Australian Radio Research Board of which Madsen was Chairman (1927-1958) & also by correspondence to arrange publication by the Royal Society of research papers produced by the Aust. RRB.

"The Scattering of the Beta rays of Radium: (Phil Mag.18, 909, 1909)" J.P.V.Madsen.

In a striking experiment Madsen devised an apparatus consisting of a hemispherical ionization chamber made of wood & lined with aluminium foil & a block of wood which had a conical hole & 3 positions where slides of 4 materials (gold, silver, aluminium & paper) each of 10 different thicknesses could be positioned with radium placed at the bottom of the block & an electrometer (presumed to be of the Quadrant Dolezalek Type) placed in the chamber to record readings of the scattered & more scattered rays. The most outstanding result of this experiment was that Madsen found for the thinnest foils the ratio of the more scattered rays to the scattered rays was constant & he concluded that when this happened the beta ray had only been scattered once, which at the time was evidence that J J Thomson's theory of a "plum pudding" atom did not hold true as multiple scattering was expected.

With Rutherford & Bragg's assistance in England, Madsen had found a benefactor in Sydney (a tobacco merchant, H.Dixson) & also funds from the University to buy 30 mg. of radium from Braunschweig to carry out further experiments with ultra thin films. The cost of this radium was extremely expensive involving a cost of 500 pounds & after Madsen had finished with its use it was given to The Royal Prince Alfred Hospital for medical use.

Rutherford's Nuclear Atom of 1911.

It took some years after Rutherford's proposed theory of a nuclear atom in 1911 for it to become more widely recognized outside his immediate colleagues, principally following the work of his student Neils Bohr, who followed on with his proposals for the electron arrangements within each atom outside of the nucleus. Work by H.Mosely using X-ray spectra to determine element atomic numbers also helped establish the new concept.

After his initial announcement in March 1911, Rutherford was quick to carry out a series of experiments on alpha particle scattering to prove all the important points of his theory. Madsen unfortunately was not able to complete the further work on beta particle scattering that Rutherford was planning.

Rutherford had received the Nobel Prize for Chemistry in 1908 for his work on radioactivity but never received the Nobel Prize in Physics, which he clearly should have been entitled to on his theory of the nuclear atom.

Cavendish Laboratory, 3rd December, 1935.

Rutherford promptly communicated an interesting paper by Martyn & Pulley of the RRB to a referee at the Royal Society for publication. It was an important part of Madsen's role to see that papers originating from the RRB were published in the leading overseas journals as quickly as possible to obtain the recognition for pioneering results.

Rutherford added a hand written note to his letter: "The Russian business is now through & the first batch of apparatus has gone off". As explained in his letter about the Kapitza apparatus, Rutherford was involved with at least 6 high level bodies to make the arrangements for the transfer to benefit his student, Peter Kapitza, trapped in Russia by Stalin's withdrawal of the Soviet permission to leave the country. It seems that subsequently Kapitza was not directly involved with the Russian atomic or hydrogen bomb developments but his presence in Russia at these times would have been helpful given his training at the Cavendish with Rutherford.

Cavendish Laboratory, 14th June, 1937.

This letter contains reference to Professor Frederick White, the new Professor of Physics in Christchurch who Rutherford was commending for a role in the work of the newly formed Radio Research in New Zealand. In 1941 Madsen as Chairman of the Radiophysics Advisory Board arranged for Fred White to come across to Sydney to head Radiophysics at a difficult time while Madsen was away from May to early December in 1941 in the U.S. & England in his role as Australian Director of Scientific Liaison. White was originally only to stay 3 months but remained with CSIRO for a further 29 years in senior executive positions including Chairman (1959-1970). White's role in initiating a Radar Air Warning project at the Radiophysics Laboratory in July 1941 was crucial to the Laboratory's success in developing a Lightweight Air Warning set used extensively by the RAAF & US Army Signals units in New Guinea as well as around the Australian coast.

Cavendish Laboratory, 31st July, 1937.

This letter from Rutherford, as it turns out, was written only a short time before he died unexpectedly in October from complications with a hernia that could not be dealt with in Cambridge. He was only 66.

Rutherford in his letter refers to his friend Harry Wimperis, an aeronautical engineer who had played a key role in the development of British radar & who was coming to Australia to advise the Australian Government on its plans to organize an Air Defence programme involving aircraft manufacture. Madsen met Wimperis in Melbourne & in a secluded hotel room Wimperis asked Madsen if he had any idea what might be happening with Air Defence in England & in reply Madsen said he thought radio means would be used for detecting raiders. Wimperis was deeply shocked at this accurate appraisal & hastily ended his enquiry. Work by the RRB at Liverpool near Sydney had indicated interference on cathode radio tubes when an aircraft flew through the signal path & this is what Watson-Watt had tested in England in February 1935 & had proven the concept of radar there.

University Library

Cambridge

15th June 1966

Sir John Madsen,
c/o Radio Physics Laboratory,
C.S.I.R.O. University Grounds,
Sydney.

Sir,

I have the honour to acknowledge the receipt of the work mentioned below, which you have been good enough to send as a present to the Library, and to convey to you on behalf of the Library Syndicate the best thanks of the University for this addition to our Collection.

Your most obedient Servant

F. J. Norton, Under-Librarian,

for the Librarian

A letter of Lord Rutherford.

U.L.C. 122

17, Wilmslow Road,

Withington.

March 8th, 1911.

Dear Mr. Madsen

I saw Bragg yesterday and he was telling me about your work on the large scattering of β particles for different materials. As I have been working at this problem theoretically for the past few months, it may be of interest to you to give an account of the relations that should hold experimentally on the theory.

In the first place, the theory of small scattering as developed by J.J.Thomson is fairly correct as far as it goes; but it takes no account of large scatterings which we know from your work, and that of Geiger and Marsden on the α particles, must always be present. The model atom of J.J.T. only admits of comparatively small scattering, so I have made calculations on an atom which consists of a central point charge, either positive or negative, surrounded by a uniform spherical dsitribution of electricity opposite in ~~amount~~ sign. One may suppose provisionally that this sphere has a diameter of the same order as that of the atom as ordinarily understood. I will give in the accompanying ~~paper~~ abstract the main deductions from the theory which I find, as far as experiment~~s~~ has gone, fits in well with the observed facts. I find that the large scatterings due to the central charge really control the scattering phenomena, although a small scattering becomes important when the probability of a deflexion through any given angle is greater than one half.

I gave an account of my paper yesterday to the Manchester Literary and Philosophical Society, and will publish it shortly in the Philosophical Magazine. Dr. Geiger is testing for me the correctness of the main assumptions, using the α rays and the scintillation method. As far as he has gone, he has found an extremely good agreement between the experimental and theoretical distribution of α particles for thin metal foils and it seems to me probable that the theory is a fairly correct expression of the facts, at any rate for small thicknesses of matter, where the probability of a given large deflexion is comparatively small. On the theory, the laws of the scattering are independent of the sign of the central charge, and I have not so far been able to settle this question with certainty. I have calculated approximately the magnitude of the central charge, and it corresponds for the atom of gold to about 100 unit charges; the magnitude of the charge is proportional to the atomic weight, at any rate for substances heavier than aluminium. At the same time, it is quite possible that the charge may ultimately be found to be twice as great as that mentioned.

It is interesting to note that the main conclusions deduced by Crowther or small scattering can be explained equally well on my theory of large scattering, and in fact, I am confident that his results are mainly due to this effect. I also feel sure that his curve for aluminium of variation of scattering with thickness is wrong in the initial parts. The curve should be much more nearly a straight line.

I may mention that the theory of large scattering will hold equally well if instead of one large central charge one supposed the atom to consist of a very large number of smaller charges distributed throughout the atom. It can be shown, however, that on this view the small scattering should be much greater than that experimentally observed. It is consequently simplest to consider the effect of a single point charge.

I understood from Bragg that you have found some interesting relations between the scattering for different materials. You will see from the theory on the assumption that the central charge is proportional to the atomic weight, that the fraction of α particles deflected through an angle phi is proportional to nA^2 where n is the number of atoms per unit volume, and A the atomic weight. This ~~weight~~ ought to hold for very small thicknesses; but I can easily see that this relation will be somewhat departed from for thicknesses where the probability of a large deflexion exceeds 1. It is evident in such a case that the theory must be modified, probably by a mixture of the theory of large and small scattering.

I am writing thus fully as I had intended to test my theory by experiments with β rays along very similar lines to that which I understand you are doing. I shall be glad, however, to leave the matter to you if you will be able to get through the work in reasonable time. I shall be very glad to hear from you how your ~~results~~ results are going.

Yours sincerely,

E Rutherford

Give my remembrances to Professor Pollock. I am hoping to visit Australia at the time for the Brit. meeting

Abstract of theory.

Nomenclature.

Ne = central charge on atom.

E = charge on scattered particle

m = its mass

u = its velocity.

t = thickness of matter

n = number of atoms per unit volume.

ϕ ~~phi~~= angle of deflexion

p = perpendicular distance from centre of atom on direction of motion of entering particle.

If we suppose the central charge positive, an α particle directed straight to the centre of the atom will be tu ned back at a distance $b = \frac{2NeE}{mu^2}$; b is an important constant.

It can easily be shown that in order to suffer a large deflexion an ordinary α or β particle should approach within 10^{-11} or 10^{-12} cms of the central charge. In this region, the forces may be supposed to be entirely due to the central charge, and to vary inversely as the square of the distance. The path of the particle is consequently a hyperbola, and the value of the deflexion ϕ ~~phi~~ can be shown to be $\cot \phi/2 = \frac{2p}{b}$.

Since the chance of a large deflexion is proportional to the number of atoms traversed, the chance of passing within a distance p of the centre is $\pi p^2 n.t$

From this it follows that the fraction of the particles scattered through the angles~~is~~ between ~~phi + d phi~~ ϕ and $\phi + d\phi$ is equal

$$\frac{\pi}{4} b^2 n.t \cot \phi/2 \operatorname{cosec}^2 \phi/2 \, d\phi.$$

The fraction scattered through an angle greater than ~~phi~~ ϕ is equal to $\frac{\pi}{4} b^2 n t \cot^2 \phi/2$ (1)

The general data available shows that the value of Ne is proportional to the atomic weight A. It is consequently seen from the formula (1) that the fraction of particles scattered is proportional to (1) thickness, ~~(2) A nA2~~ supposed small (2) nA^2 (3) $1/(mu^2)^2$

Leaving out the small part of the cross section of the atom where large deflexions are produced, the average angle of scattering due to my atom is $\frac{3\pi b}{8R}$. or three times that due to J.J.T's atoms with corresponding constants.

For heavy atoms like gold, the corpusclar scattering is small compared with that due to the ~~small~~ electric field of the atom. It can easily be shown that the fraction of α particles falling on a unit area of a screen at a constant distance from the centre of the scattering material varies as $\operatorname{cosec}^4 \phi/2$

where ~~phi~~ ϕ is the angle of deflexion of the particle. Geiger finds this relation to hold quite closely for thin foils over the range examined, viz. from 30° to 150°, where the number of particles varies [in a range?] nearly 300 times.

I think there is no doubt that the large scattering is proportional to thickness. The proof of this will show conclusively that large scattering c[illegible] due to accumulative small scattering.

E R

Cavendish Laboratory,

Cambridge.

3rd December, 1935.

Dear Madsen,

I have just received your letter and the paper from Martyn and Pulley. I have read it through and it seems to me a very interesting discussion of the state of the upper atmosphere. I am communicating it at once to the Royal, but it will have to go to a referee whom I hope will act promptly.

Of course I am not an expert in these fields, but it seems to me that the paper has great merit, and in any case may lead to a valuable discussion with regard to the interpretation of the electrical state of the upper atmosphere.

I am glad to say we are all very well, but I have been kept extraordinarily busy. As you may have seen, we have had to deal with the transfer of the Kapitza apparatus to Russia which has involved negotiations with our own and the Soviet Government, the Royal Society, the D.S.I.R., and the University, not to mention the Managing Committee of the Mond Laboratory ! It looks as if the proposal will go through, and we are preparing to send off some of the apparatus within a week or so when the first payment is made.

With kind regards,

Yours sincerely,

Rutherford

The Russian business is now through & the first batch of apparatus has gone off.

Cavendish Laboratory,
Cambridge.

14th June 1937.

My dear Madsen,

I have received safely the paper of Godfrey and Price, which you sent me, and I have had time to glance through it. It seems to me an excellent piece of work. I am communicating ~~with~~ to the Royal Society, who, I trust, will arrange for its early publication. I will see that the proofs are sent to Piddington.

I am very interested to hear of the good progress of your Council in promoting scientific work along so many lines: in particular, I was glad to learn that they have formed a Radio Research Board in New Zealand, and I hope the new Professor in Christchurch, White, will take an active part in its work.

I am naturally very interested also to hear that you have got an annual grant of £30,000 for five years to encourage research in Australian Universities. This cannot but prove a wise move in developing the scientific resources of your country. We are ourselves here considering the possibility of giving more help to the Universities to tackle some of the bigger problems which are outside their financial possibilities. I hope something will come of it.

I shall of course want to know whether you have any luck in starting a National Physical Laboratory at Camberra. Incidentally, I am pleased to hear that Briggs will be able to obtain some financial support for his researches. He is a genuine researcher who keeps in the background, but I consider him one of the best men you have in Australia: so help him all you can.

You may have heard that I am going to India in November with a British Association party, to take part in the Jubilee of the Indian Science Congress. We leave in November for Bombay, and hold most of our meetings in Calcutta. I hope that we shall get a fair number of

(Temp. changes & Radiation in the upper atmosphere) scientific men to go from this country, for I think it important to show our interest in the development of scientific work in India.

I am glad to say that we are all well.

Yours sincerely,

Rutherford

Cavendish Laboratory,
Cambridge.

31st July 1937.

Dear Madsen,

I have just received your letter and the copy of the letter for Nature which is sent in by Martyn and others of your group of workers.

I have heard the subject of their letter mentioned from time to time as a possibility, but it is very interesting to see the excellent relation between the radio observations and the disturbances in the sun. Unfortunately, Appleton is away on holiday for a week or two, so I have not had a chance to show him the letter and discuss the matter with him. He is an expert on the evidence in this type of problem.

The only trouble I have is that the letter is rather long for Nature, owing to the fact that so many points are introduced and briefly discussed. If I might make a suggestion, I think it would be better in a future letter to concentrate on the main question and to leave out some of the details for subsequent publication in the ordinary way. Gregory tells me that he is deluged with letters, and, while he is anxious to publish as representative a number as possible, there is a limit to his space. However this is a small matter, and I should like to congratulate you all on the success that is attending your radio work. I hope that you will keep closely in touch with the corresponding work in this country. I was wondering whether you are in contact with the latest developments in connection with air defence, but I suspect that you will be, through the Australian authorities. My friend Wimperis is, I believe, visiting New Zealand and Australia shortly in connection with the Air Ministry. I hope you will have an opportunity of meeting him. He is a thoroughly sound fellow and a good friend of mine. We have played many a game of golf together.

Yours sincerely,

Rutherford