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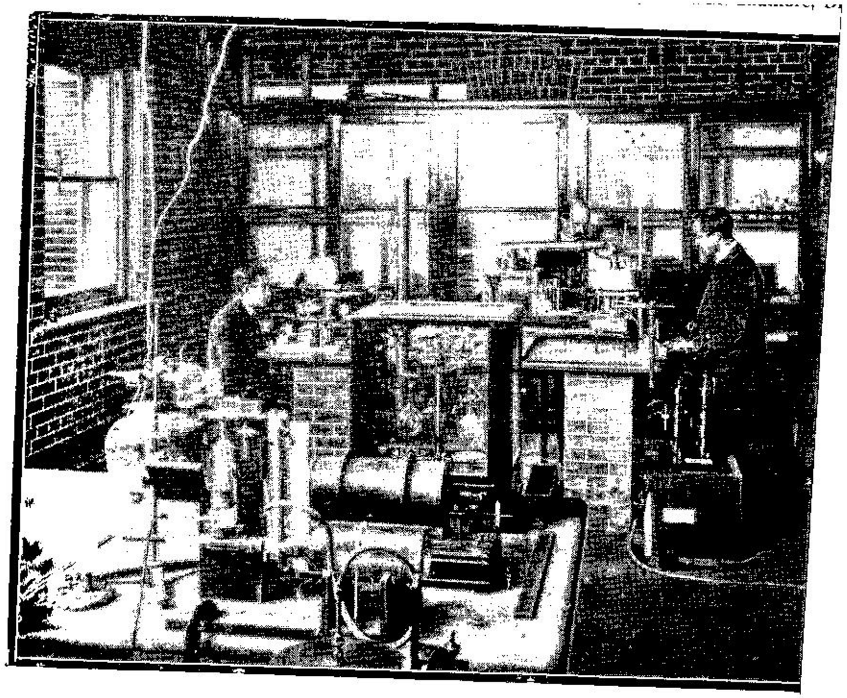

*H. Krischock, Critic photographer.*

**1. Professor Bragg's Research Laborator[y]**
**2. Electrical Engineering Laboratory.**

# W. H. Bragg and J. P. V. Madsen: Collaboration and Correspondence, 1905–1911

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Preserved in the Basser Library of the Australian Academy of Science there is a fascinating collection of letters from William Henry Bragg, then but recently returned to England after twenty-three years as professor of mathematics and physics at the University of Adelaide and only a few years away from being awarded the Nobel Prize, to his friend and former colleague in Adelaide, J. P. V. Madsen. From these letters, written during the years 1909–1911, we gain an intimate picture of the exciting developments then taking place in physics, from the pen of one of those most closely involved. They shed new light, in particular, on the famous controversy in which Bragg was then engaged with C. G. Barkla over the nature of X-rays and γ-rays. At the same time, they draw attention to an early and very striking episode in the development of physics in Australia—a subject on which, as yet, all too little has been written—and especially on the research carried out by Madsen, who was subsequently to become one of the most powerful figures in the Australian physics community. They are here published in full together with certain related items from various repositories in England, and accompanied by a discussion of the setting within which they were written. An extensive search has unfortunately failed to discover the letters Madsen evidently wrote to Bragg during the same period.

The story is well known of how Bragg himself first became seriously engaged in scientific research. Appointed Professor of Mathematics and Physics in the University of Adelaide in 1885, at the age of twenty-three, Bragg for almost two decades did virtually nothing in the way of research. However, when designated President of Section A, Astronomy, Mathematics and Physics, for the 1904 Dunedin meeting of the Australasian Association for the Advancement of Science, he found himself obliged to prepare a presidential address, and decided on this occasion to review recent work on radioactivity and the ionisation of gases—(Bragg, 1904a; Tomlin, 1976). After giving a competent and perceptive account of what was then known about ionisation (citing the work of, among others, J. J. E. Durack, one of the first of many Australian physics graduates to go to Cambridge to work at the Cavendish Laboratory), Bragg turned towards the end of his lecture to the question of the absorption of the α and β rays emitted by radioactive materials. Drawing particularly on some results obtained by the Curies, he argued that an α-particle should on account of its comparatively large mass pass undeflected through any molecules it encountered, being absorbed as a result of energy losses sustained in ionising the medium rather than through deflections. The absorption should therefore not follow an exponential law, as had been generally assumed; on the contrary, α-particles should have a definite range, provided they are all emitted with the same speed.

Following his return to Adelaide, Bragg sought out funds to purchase some radium and, assisted by his student R. D. Kleeman, proceeded to test and dramatically confirm his ideas. Indeed, by plotting a graph of the saturation ionisation current obtained at different distances of his ionisation chamber from the radioactive source, he was able to distinguish α-particles of four different ranges (and therefore velocities of emission) which he could correlate satisfactorily with the four α-emission steps delineated by Rutherford and Soddy in the decay series of radium (Bragg, 1904b; Bragg and Kleeman, 1904).

In further papers published during the next few years, several of them written jointly with Kleeman, Bragg refined and extended these investigations. An improved experimental arrangement, coupled with the use of a much purer sample of radium supplied by Soddy during a brief visit to Adelaide, yielded rather better values for the different α-particle ranges in air, and at the same time made it possible to study the relative stopping power for α-particles of other materials besides air. As early as 1905, Bragg and Kleeman jointly announced a quantitative law for this, namely that the loss of range of α-particles as a result of passing through different substances is approximately proportional to the square root of the atomic weight of the absorber, or to the sum of the square roots of the weights of the constituent atoms in the case of a chemical compound (Bragg and Kleeman, 1905a).

During this same period, Bragg and Kleeman also began a more detailed study of the processes going on inside their ionisation chamber, with the object of determining the total ionisation produced under various circumstances. Kleeman, however, left for England during the course of the investigation in order to take up an 1851 Exhibition research scholarship at the Cavendish Laboratory, and Bragg recruited first another student, H. J. Priest, and then his colleague J. P. V. Madsen to assist him instead (Bragg and Kleeman, 1905b; Bragg, 1906).

Madsen had been appointed Lecturer in Mathematics and Physics at Adelaide under Bragg in 1901, following a brilliant undergraduate career in physics and engineering at the University of Sydney (White, 1970). He and Bragg became close friends as a result of their collaboration, and this led in due course to the correspondence which is the subject of this paper.

The current passing between the electrodes of an ionisation chamber in which ions are being formed increases with the voltage applied until eventually a 'saturation' value is reached when all the ions formed in the chamber reach the electrodes. At voltages lower than that required for saturation, some of the positive and negative ions recombine in the chamber, and thus do not contribute to the current. Bragg and Kleeman had taken as their starting point in this phase of their collaboration the by then standard theory concerning this process of ionic recombination, according to which the rate of recombination should be simply proportional to the product of the existing numbers of positive and negative ions present per unit volume. Their results indicated, however, that another factor also needed to be taken into account, namely 'a process of recombination of newly-formed ions with the atoms from which they have just been separated'. This process, which they named 'initial recombination' to distinguish it from the better known 'general recombination', depended not on the numbers of ions present but on the rate at which they were being formed. It could readily be demonstrated by reducing the pressure in the chamber until the number of ions being lost by general recombination became negligible compared with the number being formed. Even in these circumstances, they found, a high potential was still required in order to obtain a saturation current.

Rutherford some years earlier had reported a result that clearly had some bearing on this new idea. Saturation could be reached at a much lower voltage, he had found, when air was forced over his radioactive source and then into a separated ionisation chamber than when it remained in contact with the source in the chamber itself (Rutherford, 1899). Bragg and Kleeman in their initial report took this as confirmation of their 'initial recombination' hypothesis. Rutherford's result followed, they said, because in the circumstances of his experiment the 'initial recombination' stage would be over by the time the voltage was applied; hence only the better understood 'general recombination' remained to be overcome in order to achieve saturation.

It is at this point that Madsen enters the story, for perhaps the first task Bragg suggested that he undertake when he became involved in the research was to verify Rutherford's result. This he was initially unable to do, and Bragg in reporting his results was reduced almost to bluster: 'It is . . . no essential feature of the initial recombination hypothesis', he wrote, 'that the act of recombination should take place within any set time. The one important point is that the recombination takes place between two ions originally forming parts of one molecule. It is quite conceivable that for a certain time the positive and negative may remain "semi-detached", their recombination in suspense until precipitated by some change of conditions. [Mr Madsen's] results point to a prolonged existence of these pairs' (Bragg, 1906).

The question was clearly of considerable interest: as Bragg pointed out, if 'semi-detached' ion pairs really existed, they might be expected to play a role in other phenomena such as phosphorescence. Madsen therefore embarked upon a more extended investigation, eventually bringing his results together in the form of a D.Sc. thesis which he submitted for examination in 1907. At the same time he drew up a report that was read to the Royal Society of South Australia a few months later, in April 1908 (Madsen, 1908a). In a sense, Madsen's results were disappointing, for instead of establishing the supposed new effect, they showed that his initial conclusion had been over-hasty: all traces of the initial recombination process quickly disappeared, and 'initial recombination is thus to be considered initial in respect to time'. It was presumably for this reason that no report of the work appeared in the *Philosophical Magazine*. Nevertheless, the research was useful, and skilfully carried out, and the examiners of the thesis, T. R. Lyle and J. A. Pollock, professors at the universities of Melbourne and Sydney respectively, had no hesitation in judging it worthy of the D.Sc. degree.

It was this initial research of Madsen's that gave rise to the first of the letters published below (Letter 1), one of only two from Madsen to Bragg from this period that appear to have survived. The date explains why the letter was written at all: Bragg was away for the Christmas holidays, probably by the sea, where he went each year with his family (Caroe, 1978). Madsen, however, was still busily working away in the laboratory. The experimental arrangement he was using was subsequently described in the paper presented to the Royal Society of South Australia (Madsen, 1908a).

Madsen in fact devised two different methods of separating the gas from the source of ionisation before testing it. In the first, a modification of Rutherford's arrangement, gas was drawn over the surface of some uranium oxide before passing between the electrodes of an ionisation chamber. In the second, the one with which Madsen is concerned in his letter to Bragg, an ingenious arrangement of pendulum-activated semaphores and switches interposed a lead screen between a radium source and the ionisation chamber immediately before a potential difference was applied to the chamber, after which the charge collected on the insulated electrode was measured with a quadrant electrometer. The numerical data Madsen sets out in his letter reveal the nature of his results in general: when the screen is in place the electrometer deflection changes very little with applied voltage, indicating that saturation has already been reached at the lower voltage as found by Rutherford.

During the same period as Madsen was pursuing this investigation, Bragg was launching the first salvos in a campaign that was soon to dominate both his own research and Madsen's as well as their subsequent correspondence, and that was to become for a time a *cause célèbre* among physicists everywhere. In two papers read before the Royal Society of South Australia on 7 May and 4 June 1907, and subsequently published as a single piece in the *Philosophical Magazine* for October of that year (Bragg, 1907a, 1907b, 1907c), Bragg opposed the generally accepted view that X-rays and γ-rays were pulses of electromagnetic radiation, and suggested that they consisted instead of material corpuscles, 'neutral pairs' made up of an electron in combination with a positively charged particle of some kind. Initially he suggested that the positive component might be an α-particle; later, when he realised that this was too massive, he suggested that it might be 'a positive counterpart to the negative electron' (Bragg and Madsen, 1908b).

The pulse theory had initially been developed by G. G. Stokes in Cambridge and Emil Wiechert in Königsberg shortly after X-rays were first discovered. The fundamental idea was simple enough: electrons presumably suffer a rapid deceleration when they strike the target of an X-ray tube, and therefore, according to classical electromagnetic theory, it is only to be expected that they will emit independent pulses of transverse electromagnetic radiation. These Stokes and Wiechert identified with Röntgen's mysterious rays.

The concept was subsequently taken up and extended by J. J. Thomson, who developed, though only tentatively at first, a typically vivid physical picture of the radiation. This might, he proposed, consist of pulses travelling along specific tubes of force ('Faraday tubes') and thus generating a highly structured distribution of energy in an advancing wave front. Thomson also pointed out that if electromagnetic pulses of the kind proposed passed through matter they should set the electrons in it vibrating and thus cause them to re-radiate or 'scatter' secondary pulses in all directions. This important suggestion was seized upon by C. G. Barkla, a former student of Thomson's at the Cavendish Laboratory, who promptly undertook a classic series of experiments on the scattering of X-rays in which, amongst other things, he obtained the first clear evidence that the rays could be polarised. His work was generally seen as providing strong confirmation of Thomson's ideas. (These and subsequent developments are discussed in McCormmach, 1967; Stuewer, 1971, 1975; Wheaton, 1978; and Jenkin *et. al.*, 1979.)

Bragg, however, was not persuaded. Coming to the subject from an entirely different point of view, namely a comparison of the ionisation produced by various kinds of ionising radiation, he emphasised properties of the rays which strongly suggested a particle-like character, and which seemed to him incompatible with any form of electromagnetic radiation.

Bragg's starting point was in fact the known behaviour of γ-rays, not X-rays, but from the beginning he clearly regarded the two as closely related species. The difficulty that had always previously been brought against a material theory was how to account for the great penetrating power of the radiation. This, Bragg thought, could in the light of recent research be seen to have been greatly exaggerated. The recognition that α-particles followed straight-line paths through matter showed that 'an atom ..., endowed with sufficient speed, can pass directly through another atom without appreciable deflection'. An α-particle lost energy chiefly on account of the electrical charge it carried: how much more penetrating, therefore, might an uncharged pair be in similar circumstances! Its lack of charge would also explain why it could not be deflected by electrical or magnetic fields. At the same time, 'it may at last suffer some violent encounter which will resolve it into a positive and a negative.... Of these the β particle would be the one possessed of much the greater velocity, and would appear as a secondary ray'. Here, perhaps, lay the answer to the major weaknesses Bragg perceived in the pulse theory:

> If the X-ray is an ether pulse it is difficult to understand, as Thomson has shown ..., why the spreading pulse should only affect a few of the atoms passed over, why the secondary cathode rays are ejected with a velocity which is independent of the intensity of the pulse which weakens as it spreads, and why it should be able to exercise ionising power when its energy is distributed over so wide a surface as that of a sphere of, say, ten or twenty feet radius. All these phenomena are capable of quite simple explanation if we suppose the ray to be a neutral pair which has only a local action, *i.e.* can only affect the molecules which it traverses, which can penetrate to great distances, which loses very little speed as it goes, and gives rise to a cathode ray when it is broken by impact (Bragg, 1907a).

Bragg was confident that Barkla's polarisation experiments could also be accounted for on his hypothesis by making appropriate assumptions about the way in which rotating pairs would be generated, and would subsequently interact with matter. After some initial hesitation, he also convinced himself that the idea could accommodate an experiment by Marx (Marx, 1905) which seemed to show that some X-rays, at least, travel at the speed of light. Marx's result was, he concluded, 'quite consistent with the hypothesis that the X-rays are complex, and consist in part of ether pulses travelling with the velocity of light, ... and in part of material particles, or pairs, travelling at a speed as yet undetermined ...' (Bragg, 1907b).

Bragg first set out these ideas in the course of a survey of the ionising properties not just of X- and γ-rays but of α's and β's as well, in which he argued that 'in all cases the bulk of the ionisation which the rays effect is of the same character, and consists in the displacement of slow-moving electrons ... from the atoms of the gas or other substance which they traverse'. High-speed electrons are of course sometimes produced when cathode rays pass through a gas or strike a solid target, but these, he maintained, whether scattered primary rays or true secondary radiation, themselves acted in turn as ionising agents producing slow-moving electrons in the gas through which they passed. 'It cannot be supposed', he said, 'that the bulk of the ionisation which is caused in the ionisation-chamber consists of high-speed secondary rays ...' A closer investigation of the secondary radiation was clearly desirable, and in a note added to the version of his paper published in the *Philosophical Magazine* Bragg welcomed some new results obtained by H. W. Schmidt which showed that in the case of β-rays striking aluminium this consisted of scattered primary rays (Bragg, 1907c; Schmidt, 1907a).

Soon afterwards, Bragg and Madsen reported the results of their own investigation of the secondary β radiation. Their by now highly-developed understanding of the way an ionisation chamber worked led them to question the experimental arrangement that had normally been used in studying this question. They devised a rather more satisfactory technique, but it did not in fact yield results substantially different from those that had been obtained by other workers. They found that the penetrating power of the secondary radiation (and hence, they inferred, its average velocity) varied with the atomic weight of the target, the radiation produced from substances of lower atomic weight being less penetrating. This, they said, was 'in general accordance with other experiments and with expectation'—an expectation based on their evident belief that the radiation was not truly secondary but in fact consisted of scattered primary rays. The paper describing the work was read before the Royal Society of South Australia on 1 October 1907 and subsequently reprinted in the *Philosophical Magazine* (Bragg and Madsen, 1907). Before they could take the investigation any further, however, the controversy engendered by Bragg's neutral-pair hypothesis erupted and became instead the focus of their attention.

The appearance of the October 1907 issue of the *Philosophical Magazine* containing Bragg's paper setting out his neutral-pair idea prompted an immediate response from Barkla, in the form of a letter in the issue of *Nature* for 31 October (Barkla, 1907). Barkla criticised Bragg's views and presented new data on the intensity of scattered X-rays in different directions with respect to the primary beam. These he regarded as strongly confirming the pulse theory while being at the same time incompatible with the neutral-pair hypothesis. Bragg, however, in his reply did not accept this, pointing out that Barkla's calculations with respect to the neutral-pair theory were based on an unjustified and intrinsically implausible assumption; hence 'the experiment has no value as a critical test' (Bragg, 1908a).

In this same response to Barkla, Bragg also gave the first news of an important new series of experiments upon which he and Madsen had been engaged. These experiments, which were subsequently reported more fully to the Physical Society and published in the *Philosophical Magazine* a few months later still (Bragg and Madsen, 1908a), yielded results which in Bragg's opinion utterly confounded the pulse theory.

In their new experiments, Bragg and Madsen, like Barkla, studied the distribution of secondary radiation in different directions with respect to the primary beam. They, however, used γ-rays rather than X-rays as their primary, arguing that the harder rays gave the simplest results 'for the obvious reason that such rays ignore atomic structure altogether even in the case of the heaviest atoms. The X-rays are soft, and therefore atomic structure influences and complicates the effects to a remarkable degree, as Dr Barkla's own work shows' (Bragg, 1908c). Their experiment amounted to comparing the secondary radiation in the forward and backward directions (the so-called 'emergence' and 'incidence' radiation respectively) produced when the γ-ray beam struck a thin target. They argued that if this secondary radiation were generated in an atom by a passing wave or pulse, it should according to well known principles be distributed symmetrically about a plane passing through the atom perpendicular to the direction of motion of the pulse: 'If we speak of the primary pulse as going forwards, the secondary radiation is just as likely to go backwards as forwards'. And they cited Thomson himself as their authority on this. In practice, however, they found a great want of symmetry: the 'emergence' radiation produced considerably more ionisation than that on the 'incidence' side. 'It seems to us', they wrote, 'that there is no escape from the conclusion that the γ rays are not aether pulses'. On the contrary, 'all our experiments so far show that, on the whole, the kathode radiations from a given stratum of matter traversed by γ rays possess momentum in the original direction of motion of the rays, and this shows that the rays are material', to wit, neutral pairs.

What happened in the target, they suggested, was that the positive component of the neutral pairs was stripped off while the negative component, or β particle, continued on its way with its speed virtually unaltered. Such a process would naturally give rise to an asymmetry in the forward direction. Furthermore, the emergence radiation would not then be expected to show the usual relationship between the amount of secondary ionisation and the atomic weight of the target which they had discussed in their previous paper, but would give the same results for all materials. The incidence radiation, on the other hand, should follow the usual law. This, too, they were able to confirm.

During the next few months, Bragg and Madsen sought to extend and perfect these results. Together they published a sequel to their initial account in which they set out the results they had obtained with an improved experimental arrangement, and discussed at some length the theoretical implications of these (Bragg and Madsen, 1908b). Once again they argued strongly for the material nature of γ and X-rays. This theory, they said, was 'much simpler and more complete than any explanation which the aether-pulse theory seems likely to afford, even in its latest form'. As for the latter, 'the difficulties of this theory are exactly those which would naturally arise in the attempt to transfer the properties of a material particle to an immaterial disturbance'.

Meanwhile Madsen undertook a difficult investigation of the secondary γ-radiation produced when γ-rays from radium are allowed to strike a thin target, a preliminary report of which he presented to the Royal Society of South Australia in July of the same year (Madsen, 1908b). As with the secondary β-radiation he had studied with Bragg, he found a marked asymmetry in intensity between the incidence and emergence radiations, and also in some cases a significant difference in quality between the two. Once again he found these results difficult to reconcile with the pulse theory, even taking J. J. Thomson's latest modifications to this into account, whereas he thought they could easily be explained on the material theory:

> A homogeneous bundle of hard γ rays ... in passing through matter suffer collision; the effect of such collision is to change the direction of motion of the incident primary ray—in other words, to scatter it; at the same time the scattered ray loses a certain amount of energy—it has become softened; this softening may be due either to a change in its speed or to a change in moment of the γ pair, or it may be both.

Supposing that his incident γ-ray beam was inhomogeneous, Madsen then went on to explain the asymmetry he had found in the quality of the secondary radiation in terms of a selective scattering effect whereby the softer primary rays were more readily back-scattered than their harder companions.

While Madsen was engaged on this work, Bragg kept up his running controversy with Barkla in the pages of *Nature*, refining and sharpening his earlier arguments, welcoming some results obtained by Cooksey (Cooksey, 1908) showing a similar asymmetry in the secondary β-radiation produced by X-rays to that which he and Madsen had found with γ's, and trying to take account of Barkla's discovery (for which he was later awarded the Nobel Prize) that among the secondary X-rays produced from a target were homogeneous or 'characteristic' rays whose hardness depended only on the material of the target (Barkla, 1908a, 1908b; Bragg, 1908b, 1908c, 1908d). Eventually, however, after allowing the controversy to continue a full twelve months, the editors of *Nature* decided to call a halt, at least so far as exchanges of letters in their journal were concerned. Unfortunately their axe fell not on the by now well established Bragg but on his junior partner, the inexperienced Madsen, who in October 1908 had sent them in all innocence a brief report on his work on the secondary γ-rays. Though this did get published (Madsen, 1908c) it appeared with an editorial note attached, as follows, which Madsen must have found rather discouraging:

> As there are few opportunities in Australia for an investigator to place his views quickly before a scientific public, we print the above letter, but with it the correspondence must cease. The subject is more suitable for discussion in special journals devoted to physics than in our columns.

Bragg, meanwhile, had enlisted the aid of another student, J. L. Glasson, in an investigation parallel to Madsen's in which however they studied not γ-rays but the distribution of secondary X-rays excited by primary X-rays striking a target. Here, too, the expected asymmetry was confirmed: 'We find that in general want of symmetry does exist, that it is sometimes very pronounced, and that is in keeping with expectation based on Madsen's study of the secondary γ rays' (Bragg and Glasson, 1908).

By now, however, the end of this very productive partnership was in sight, for Bragg had been appointed Professor of Physics at Leeds, and he left Adelaide to take up his new position in January 1909. Soon afterwards Madsen also left to take up a post as Lecturer in Electrical Engineering in his old university in Sydney. Before the year was out, Glasson, recipient like Kleeman before him of an 1851 Exhibition scholarship, sailed for England to pursue his studies in Cambridge. After a remarkable but all too brief flowering, physics in Adelaide reverted to the much more leisurely pace of earlier days.

The first of the letters from Bragg to Madsen published below (Letter II) was written shortly after his return to England, and reports in Bragg's usual lively manner the meeting of the Physical Society of London, held on 23 April 1909, at which he presented in person before some of the leading figures in British physics the results of the work he had carried out with Glasson. Bragg had evidently gone to the meeting prepared for criticism of his neutral-pair hypothesis but, as he records in his letter, this had amounted to no more than some inconsequential numerology from C. A. Sadler, Barkla's collaborator in his X-ray experiments. And even Sadler had admitted to him afterwards that the old-style pulse theory could no longer be maintained. Thanks in part to Bragg's onslaught, its inability to account for various well known features of the ionisation process was now widely recognised. 'J. J.' had therefore been driven to develop in much more detail his earlier suggestion that the pulses were confined to particular Faraday tubes. It was, however, difficult to reconcile this notion with traditional conceptions of the aether, and Bragg's letters reveal how he for one found it hard to take the idea seriously.

This first letter of Bragg's also makes clear his satisfaction that his former Adelaide student Kleeman had returned to the fold. Working under Thomson's supervision at the Cavendish

Laboratory, Kleeman had during the preceding few months done some important research on secondary γ-rays which he had interpreted very much in pulse-theory terms. Now, however, Bragg is able to report that Kleeman, too, agrees that the old pulse theory is dead and that the neutral-pair or material theory has much to be said for it. On the other hand, Bragg is careful not to say that Kleeman actually accepts the material theory. Almost certainly, in fact, he was one of those Bragg had in mind as he summed up for Madsen's benefit the attitude he had encountered among physicists in Britain to the debate their work had engendered: 'whilst there is no general assent to the material theory, there is no general opposition to it: on the other hand there is a feeling that some new theory has to be found, and that the material theory may be the right one'.

Mail took several weeks to reach Australia; Madsen's reply to Bragg (which we do not have) was dated 10 June, so it must have been written almost as soon as Bragg's letter arrived. The missing letter evidently gave news of how Madsen was settling in following his move to Sydney, and Bragg responded promptly in kind, on 1 August (Letter III). More importantly, he also sent news of the latest work on β-ray scattering, the subject to which, twelve months earlier, Madsen had turned following the successful completion of his work on the secondary γ-rays.

In that investigation, Madsen's starting point had been a paper by J. A. Crowther (Crowther, 1907) which revealed the possibility of studying the scattering of β-rays by very thin absorbing foils. Madsen conceived the idea of comparing the incidence and emergence radiation produced by β-rays in a manner analogous to the earlier experiments with γ and X-rays, in the hope of strengthening still further the parallels Bragg had drawn between the various classes of ionising radiation. That he succeeded in this was almost incidental, however, compared to the importance of some other results he obtained in the course of his experiment.

Madsen's apparatus was constructed in such a way that he was able to make a rough comparison between the amounts of small-angle and large-angle scattering, for different thicknesses of his absorbing screen. Astonishingly, he found that for thin screens the ratio of small-angle to large-angle scattering was practically constant, that is, that large-angle scattering was still significant in extremely thin screens where the likelihood of a β-particle suffering multiple collisions was remote. 'It would appear', Madsen concluded, 'that while the ratio remains constant we are concerned with only a single collision of any β-particle, that as the screen is further thickened, it becomes possible for a β particle to suffer more than one collision before emerging . . .'

These results were obtained while Bragg and Madsen were still together in Adelaide; Madsen presented a preliminary report concerning them at the same Brisbane meeting of the Australasian Association for the Advancement of Science at which Bragg delivered his farewell address to Australian science. Madsen's formal report was subsequently published in the *Transactions* of the Royal Society of South Australia before being reprinted after some delay in the December 1909 issue of the *Philosophical Magazine* (Madsen, 1909). Their significance became apparent only later, however, following the publication of J. J. Thomson's theory of β-ray scattering early in 1910 (Thomson, 1910), for this was predicated upon the assumption that the deflection of a stream of β-particles was a multiple-scattering phenomenon, that is, that it was the net effect of a large number of deviations each one of which was by itself insensible. Madsen's results directly contradicted this assumption.

At first Madsen's paper attracted little attention in Cambridge. Bragg, however, had confidence in his friend's results and fully understood their importance. In his letters from England, he urged Madsen to make haste in getting out his promised sequel on 'the effects of scattering and absorption for very thin films' (cf. Madsen, 1909, p. 913), at the same time letting him know that Rutherford's student William Wilson was also getting some unexpected results with β-rays. Though Bragg evidently did not yet know all the details, he knew enough to recognise the significance of Wilson's work, which in fact completely overturned the generally accepted view that the absorption of β-rays followed an exponential law, and set the stage for Thomson's reconsideration of the general theory of β-particle scattering (Wilson, 1909). Bragg also made a point of telling Rutherford of Madsen's results and, in addition, in a lengthy paper of his own published in the *Philosophical Magazine* in September 1910, he pointed out the implications they had for Thomson's theory, rendering this, he said, 'inapplicable to the actual case' (Bragg, 1910b). In subsequent discussions and correspondence with Rutherford, he continued to insist on the importance of Madsen's work, which by this means played a noteworthy role in the events leading up to Rutherford's publication in early 1911 of his nuclear theory of the atom, based on a belief in the importance of single rather than multiple scattering. (These events are described in detail in [Heilbron, 1967]).

In his letters to Madsen, Bragg included regular reports on the continuing saga of his controversy with Barkla over the nature of X and γ-rays. Barkla's homogeneous X-rays were clearly a worry, and Bragg in his letter of 1 August set out for Madsen's benefit his latest thoughts as to how they might be explained on the material theory. Though he here expressed himself very tentatively, his confidence in his idea subsequently grew, and twelve months later he published something very similar in the *Philosophical Magazine* (Bragg, 1910b, pp. 391, 415).

Bragg also took great delight in telling Madsen of Rutherford's sympathetic response to the material theory, and of the general scepticism—which in his enthusiasm he perhaps overstated—towards Thomson's 'energy blobs'. Likewise, the conversion to the material theory of Thomson's own assistant, G. W. C. Kaye, is gleefully reported in Bragg's next letter, dated 6 October 1909 (Letter IV). Nevertheless, coupled with Bragg's evident satisfaction with the successes his theory had achieved there may be discerned in his letters a growing recognition that it was probably not the last word on the subject. In particular, 'the "light quantum" of the Germans' seems to have given him considerable food for thought, though he remained generally sceptical about the idea. 'The neutral pair theory may or may not be absolutely true', he wrote on 12 December 1909 (Letter V), 'but I think nearly everyone thinks that its promulgation was absolutely justifiable at the time, and that it has led to several discoveries and encouraged several successful researches, which it alone prompted'.

Some of this work was in fact being done in Bragg's own laboratory. His letters make it clear that he had lost no time following his arrival in Leeds in establishing a vigorous program of research in his department, much of this inspired by the work he had previously been doing in Adelaide. Now, however, he had many more willing hands to set to the plough. Even though he himself published very little during his first year or two in Leeds, there is no sign in his letters to Madsen of the despondency his daughter later recalled from this period (Caroe, 1978). Miserable his wife may initially have been in the grime and poverty of an industrial city, but Bragg's enthusiastic involvement in his work is readily apparent, as is the stimulation he continued to draw from the long-running dispute with Barkla. Though he also complains of the work involved in getting his new laboratory properly organised, he nevertheless reports in serial fashion some research he himself had found time to begin on 'the conversion of X rays into cathode rays', the results of which he subsequently published jointly with one of his demonstrators, H. L. Porter (Bragg and Porter, 1911).

In Sydney, Madsen was evidently getting on with his investigation of β-ray scattering as repeatedly urged by Bragg. The latter in his letter of 6 October both applauded the approach Madsen had proposed and acknowledged his request for help in obtaining ultra-thin metal foils to use as his absorbers. The problem of obtaining suitable thin foils in Australia was one that Bragg himself had commented upon in an earlier publication (Bragg and Kleeman, 1905a); the situation appears to have improved in the meantime, however, for in his next letter Bragg reported that even after a diligent search, he could 'get nothing which we did not have in Adelaide' except for some purer copper foil which he was sending on.

For some reason, the surviving correspondence breaks off at this point. There is, however, no suggestion in the second group of letters which we have, dating from the first half of 1911, that the correspondence was then being taken up afresh. There is therefore every reason to believe that other letters were exchanged in between, of which no trace now remains.

The second surviving group of letters deals initially with Madsen's request to Bragg (the details of which are now lost) that he expend on Madsen's behalf a large sum of money, apparently £500, that he had been given to purchase radium. The bulk of this money appears to have been a donation from a wealthy Sydney tobacco merchant, Mr Hugh Dixson, though it probably also included an amount of £125 voted to Sydney University's Physics Department in January 1911 for such a purchase (personal communication from Mr Kenneth E. Smith, University

Archivist, University of Sydney, 8 April 1981).

On Rutherford's advice, Bragg made the purchase from Friedrich Giesel's famous radium extraction works attached to the quinine factory in Braunschweig (Letter VI and VII). Some years later, when Madsen no longer needed the radium Giesel supplied, it was transferred to Sydney's Prince Alfred Hospital, to be used for therapeutic purposes (*Prince Alfred Hospital Gazette*, 27 July 1917, p. 25).

In his long letter of 18 May 1911 (Letter VIII), Bragg also, as before, included the latest scientific news, and this Madsen must on this occasion have found exceptionally interesting. To begin with, Bragg sent news of C. T. R. Wilson's beautiful pictures of the tracks of ionising radiation taken with his famous cloud chamber. The X-ray tracks were especially significant: 'can't be anything else but the track of the cathode rays in the gas!' as Bragg remarked, and the pattern these formed was clearly not what one would expect if the X-ray were a pulse spreading out as it crossed the chamber. Bragg rightly took the track as evidence in favour of his material theory. On the other hand, he reported that Sommerfeld's reworking of the pulse theory (Sommerfeld, 1911) had produced 'lots of dissymmetry'; he remained unconvinced, however, that the theory could account satisfactorily for the by now notorious localisation of energy in a γ-ray.

From Madsen's point of view, a still more significant feature of Bragg's letter would have been his reference to Rutherford's recently published paper setting out his nuclear theory of the atom. In this justly famous work Rutherford had referred explicitly at one point to Madsen's paper on β-ray scattering, and, as Bragg hastened to point out to his friend, further investigations in this area were now urgently required in order to test the new theory. Knowing that Madsen had been working on precisely this subject for some time, Bragg had told Rutherford what he was doing in an effort to forestall his being cut out by a speedy resolution of the question in Manchester. The implication was clear, and had already been sheeted home to Madsen in a letter from Rutherford himself, also published below (Letter IX): Madsen needed to get out his results post-haste, or face the prospect of being beaten into print by others. Rutherford, while offering to stand aside in Madsen's favour, put the matter very clearly:

> 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 are going.

Rutherford's forbearance here is remarkable because, as Heilbron has shown in his account of the reception of Rutherford's ideas, one response to the paper was to accept Rutherford's theory in relation to the scattering of α-particles but to retain Thomson's idea of multiple scattering when it came to the scattering of β's (Heilbron, 1967, pp. 302–303). Nevertheless, Madsen did not succeed in taking advantage of Rutherford's generosity. Though in his reply to Rutherford (Letter X) he was optimistic that with the new batch of radium he had received from Bragg he would quickly be able to complete his investigation, he afterwards found that his results did not fall into place as easily as he had expected. He was still struggling to resolve the matter when he wrote to Bragg in the following November (Letter XI), the final letter to have survived from this fascinating correspondence: 'I cannot settle Rutherford's point from the scattering on the front side but hope to, by considering the ratio of scattered rays in a forward and backward direction for thin sheets'. In the end, the problem defeated him, and he did not publish anything.

Indeed, whether through disappointment at this turn of events or on account of other developments in his career, Madsen seems to have abandoned research at this time, and never again did he make original contributions to knowledge. Soon after writing the letter to Bragg from which we have just quoted, he was promoted to Assistant Professor in Electrical Engineering in his university, the first professorial-level appointment in electrical engineering in any Australian university, and doubtless his administrative responsibilities expanded as a result. With the outbreak of war in 1914, he was appointed Chief Instructor and Officer Commanding the Engineer Officers Training School in Sydney (White, 1970), a job that doubtless fully occupied his time. At the signing of the peace he returned to his university and soon afterwards, in 1920, was promoted again, this time to full Professor. Thereafter for many years he played a leading role in the promotion of Australian physics and engineering.

It has often been said that distance imposes special handicaps on those undertaking scientific research in Australia. The nature of those handicaps has, however, been insufficiently explored. The events discussed above allow us to be somewhat more specific. The successes achieved by Bragg and Madsen in Adelaide in the first years of this century show that even in the abstract sciences it was possible for Australian workers to reach and remain at the very forefront of world research, despite their remoteness from the major centres of western Europe. Though Bragg occasionally complained that 'new works often take some time to reach us here' (Bragg, 1908b) and though he and Glasson at one point acknowledged the possibility that in their work on X-rays they were out of ignorance merely duplicating work that Barkla had already done (Bragg and Glasson, 1908), the five or six weeks it took mail to travel between Britain and Australia was in fact no insuperable obstacle. Furthermore, Bragg's references in the articles he wrote in Adelaide make it clear that he had good if necessarily slightly delayed access to all the major journals in the field, including the continental ones, and to major new English-language books soon after they appeared.

The fact that Adelaide was still in some respects a frontier outpost did mean that some items of equipment were not easily procured. We have noted already how, early in his research, Bragg found suitable thin metal foils difficult to obtain; we have also seen, however, that soon afterwards virtually the same range of thin foils was available in Adelaide as Bragg was able to obtain in Leeds. More generally, Bragg and his collaborators seem to have had access to workshop facilities good enough to make up some quite complicated apparatus. The real problem in this regard lay with highly specialised pieces of equipment: when Bragg decided to purchase one of the new Dolezalek electrometers it took a very long time to arrive (Bragg and Kleeman, 1904), and the high-vacuum facilities evidently left a great deal to be desired when compared to those available to Bragg once he got to Leeds (see Letter IV below).

Remoteness had its effect in more subtle ways than these, chiefly, it appears, in engendering a feeling of intellectual isolation. Melbourne was the nearest city to Adelaide where other physicists were to be found, and it was over 450 miles away. During Bragg's early years in Adelaide when he was teaching himself physics in order to teach it, he discussed points of difficulty by mail with his opposite number in Sydney, Richard Threlfall (Moyal, 1975). So far as direct personal contact went, however, apart from the yearly or two-yearly meetings of the Australasian Association for the Advancement of Science and the perhaps once-a-decade year's leave travelling overseas, the group in Adelaide was very much on its own. (Bragg had but one year's leave, in 1897, during his twenty-three years in his Adelaide post.)

And a very tiny group there was in Adelaide. Throughout the period with which we are concerned, Madsen was Bragg's only colleague on the lecturing staff of mathematics and physics at the University. Sir Charles Todd, the Government Astronomer, was Bragg's friend as well as his father-in-law, and was no doubt used as a sounding-board on countless occasions. His field of scientific expertise was far removed, however, from that in which Bragg was doing his research. From time to time Bragg called upon his colleagues in the University's Chemistry Department for assistance, but this seems to have been limited to preparing samples of various gases for Bragg's experiments on the stopping power of different substances. He may also have discussed his work from time to time with R. W. Chapman, some-time lecturer under Bragg in mathematics and physics who in 1900 had transferred to a lectureship in the university's engineering department and had then in 1907 been appointed to the Chair of engineering. Finally, there was a handful of advanced students. Bragg acknowledges the help of only three, Priest, Glasson and, above all, Kleeman, during the five years in which he was active in research in Adelaide; and since he would almost certainly have set all his advanced students to work on his project, it seems that these three were the only ones he had.

At the time he left Adelaide, Bragg was reported as saying that his only reason for going was his interest in research (Caroe, 1978, p. 50). His letters to Madsen make abundantly clear what it was he hoped to gain by going to Leeds. Almost at once he was surrounded by a substantial group of researchers whom he could direct to questions bearing immediately on his own principal concerns. London and the Royal Society were within easy reach, and only a few miles away in Manchester was Rutherford. The opportunities for discussion were legion. The contrast with conditions in Australia, where

Madsen was now more intellectually isolated than ever, was complete. Though a gifted researcher, Madsen was yet no Bragg. Unable to fill the void created by Bragg's departure, he not only found it impossible to maintain his place in the vanguard of radioactivity research, his research output ceased entirely. While our story shows that continued front-rank research in physics was indeed possible in Australia, it also suggests that it was only possible for the most exceptional of individuals.

# APPENDIX

## Correspondence between W. H. Bragg and J. P. V. Madsen

## together with

## Related Correspondence between Madsen and Rutherford

Except where otherwise stated, originals are located in the Basser Library, Australian Academy of Science. The various items are published with the permission, as appropriate, of the Bragg family, Mr R. W. Madsen, the Australian Academy of Science, the Royal Institution, and the Syndics of the Cambridge University Library.

## 1. Madsen to Bragg, 28 December 1906.

Original: Bragg Papers, Royal Institution, London.

The University,
Adelaide
Dec. 28. 06

Dear Professor[1]

Just a line as I promised, to say how work was going on—the first trouble which cropped up was due to the rise of potential in the system which had afterwards to be connected to the electrometer, while the field was on—However this can be got over readily enough by using a sliding condenser, and as long as I use not more than 400 or 500 volts on a 4 cms chamber its effect can be eliminated for preliminary experiments.

But a more serious difficulty which has arisen, is that in order to get a large enough electrometer deflection the density of the ions must be very great and from some experiments which I have just completed, with the field on while the rays are acting, and pulling different thicknesses of mica over the radium—I find that with the fields strengths available initial recombination is obscured by general[.] To get over this the best I can think of is reduce the chamber to between one & two cms width, and to use instead of a circular aperture in the bottom plate of the ionization chamber as at present a long rectangular slit with the radium spread along a corresponding parallel groove[.][2]

The density of ionization should also be reduced to meet Kleeman's objection which would apply in this form of experiment viz—that the stronger fields may stop a number of ions from recombining, which are just on the point of doing so, at the instant when the field is applied.

However I do not feel at all sure that Kleeman's objection is quite sound when you consider Langevin's result "that the rate of recombination is independent of the field applied."[3]

I am giving you below a list of some of the results in terms of the first throw of the needle[.]

Chamber about 4 cms

| Volts | Rays acting while field is on | | Rays cut off before the field is applied | |
|---|---|---|---|---|
| | α | β | α | β |
| 400 | 1620 | 130 | 490 | 107 |
| 50 | 1070 | 123 | 447 | 100 |
| 400 | ----------- | | 537 | 118 |
| 50 | ----------- | | 490 | 110 |
| 400 | 2275 | 145 | 502 | 121 |
| 50 | 1305 | 145 | 465 | 116 |
| **Sheet of mica over radium** | | | | |
| 400 | 298 | 137 | | |
| 50 | 290 | 137 | | |
| 400 | 267 | 120 | | |
| 50 | 262 | 115 | | |
| 400 | ----------- | | 197 | 105 |
| 50 | ----------- | | 195 | 99 |
| **as above with Ethyl Chloride and Air** | | | | |
| 400 | 453 | 199 | 312 | 156 |
| 50 | 403 | 187 | 274 | 146 |
| **with air only** | | | | |
| 400 | ----------- | | 203 | 101 |
| 50 | ----------- | | 196 | 105 |

It seems rather cruel sending you this sort of stuff while you are on holiday so I hope you won't take it too seriously.

Wishing you a happy new year with kind regards to Mrs. Bragg

Your sincerely
J P V Madsen

**Notes:**

1. The work described in this letter eventually yielded results that were incorporated in (Madsen, 1908a).
2. Madsen finally settled on a chamber width of 1.8 cm., but retained the circular aperture. For a full description of his apparatus, see (Madsen, 1908a), pp. 27–30.
3. Langevin, 1903.

## II. Bragg to Madsen, 29 April 1909

The University,
Leeds

April 29, 1909

My dear Madsen

I told you I would write after the meeting of the Physical Society.[1] Well, I went up armed with every weapon I thought might be needed; but there was hardly any argument. There was a good attendance, and Lees[2] made the only sensible comment. He said that what was now wanted was greater precision: some absolute standard of hard and soft. Of course I agreed like anything and said that I thought the determination of the speed of the cathode ray which each X ray gave rise to was the first and most important determining factor as representing the energy: there might be a "quality" in addition. Barkla was not there: but his representative Sadler called attention to the fact that in our figures for the absorption by two tin foils the figures

| | Sn | Cu | Fe | Al |
|---|---|---|---|---|
| Emergence | 176 | 140 | 39 | 185 |
| Incidence | 122 | 119 | 15 | 60 |
| & four tin foils | | | | |
| Emergence | | | | |
| Incidence | | | | |

May 7. I had to stop here last week and was too late for the mail: now I can go on.

But now I have not the paper from which I was quoting, bother it! I am writing at home and the paper is at the University and I must post tonight. But if you will look at the paper (Glasson's & mine) you will see what I am going to tell you. Sadler [? worked out?] that several of the figures had constant differences e.g. 140–119 = 21, 39–15 = 24 and some more from the figures for the four tin foils & hinted that either I had got a constant error of addition to the emergence, or else there ~~was some~~ new radiation playing up with the experiments! Well that was about all, except that one or two made friendly & complimentary remarks. I had a long talk to Sadler afterwards and found him much more amenable and quite friendly. He admits that the old form of the pulse theory has gone and that JJ's mathematics is now no good. But the thing he clings to is that the $2^{ry}$ X rays from Cu Pb &c are quite distinct from the primary. And there is not much more. Kleeman came here a fortnight ago and spent a day or two. He says no theory explains everything so well as the material theory: and he also says that the old form of pulse theory has passed away. He has gone to Manchester to work now: and wants to come here in the summer, if he can find a scholarship to come on.[3] Kleeman & I talked things ~~hard~~ and we pretty well agreed right through. In fact whilst there is no general assent to the material theory, there is no general opposition to it: on the other hand there is a feeling that some new theory has to be found, and that the material theory may be the right one.

Crowther has a paper in the Proc Roy Soc[4] in which he finds that the primary X rays do all the ionisation in a gas, and the secondary cathode rays do nothing: but I think he has not taken sufficient account of the forward direction of the cathode rays after their production. I don't think the rays *would* hit the walls of his chamber to an appreciable extent, and he is wrong in saying that because they ought to and therefore some of their ionising effect should be lost, therefore they do not ionise at all. After all a thin stream of cathode rays can be traced for long distances in air at low pressure and show sharp sides, and I think the ionisation and illumination go together. Those Dublin people have a paper I see[5]: but they really are rather duffers, aren't they? Where they use the precautions we did, they get the right result, & where they don't they go wrong. Of course the rays (emergence) from Pb are softer than from Al when the γ rays are heterogeneous & contain both soft and hard. *We* know that. And they say that is against the material theory.

The people here are very good to me. They say Physics has not been properly supplied with apparatus & so on, and I am to have my turn now. Besides the £1000 for research I am to have say £500 for the ordinary lab. My lecturer & 3 demonstrators are jolly nice:[6] and the demonstrators are already starting on research, but we must get our cells. I will write again soon.

Yrs always
W H Bragg

The Ra arrived safely

**Notes:**

1. At this meeting, held on 23 April 1909, Bragg read a paper that he had written jointly with his last research student in Adelaide, J. L. Glasson (Bragg and Glasson, 1908).
2. Charles Herbert Lees, F.R.S. (1864–1952), professor of physics at East London College (later Queen Mary College, University of London).

3. Apparently Kleeman found a scholarship, because we learn from Letter III that he did spend the summer in Bragg's laboratory.
4. Crowther, 1909.
5. Hackett, 1909.
6. The lecturer was A. O. Allen, the demonstrators S.A. Edmonds, H. B. Keene and S. A. Shorter.

## III. Bragg to Madsen, 1 August 1909.

Bolton Abbey Aug. 1. 1909.
Wharfedale

My dear Madsen

I was glad to have your jolly letter of June 10th. Isn't Roseville on the same line as Turramurra; I was out there two or three times to my brother's house?[1] I am glad you are settled and are all well. Doesn't it take a long time rearranging all the work and getting things going one's own way? I like your idea of the comets[?] experiment and with your assent I will use it in a public lecture if I can.

I hope you are getting on with your β ray experiment, I want you to get those results out soon. There is a man at Manchester, W. Wilson working at β rays also:[2] I know he is getting unorthodox results which will rather run contrary to H W Schmidt & others.[3] I believe it is not actually the same experiment as yours, but you must not let things go on too long. By the way Rutherford said to me the other day that if the β rays went forward always, then a very thin sheet should show them nearly all going forward, since of course there would be little to turn them back. I don't remember that we actually attended to this point; but I must look up the figures, I wonder if they show it. I had Rutherford staying here for two days and it was great fun: he & his wife came. We have quarters in a jolly old farm house overlooking Bolton woods and with the moors at the back: a glorious place altogether. We have enjoyed it awfully even though it has rained most days & just now does not seem able to leave off. The moors belong to the Duke of Devonshire and he & the Prince of Wales are coming down to shoot next week. The heather is just coming out. Well, Rutherford and I talked hard, culminating on the last evening: when we both got excited and stamped about the room at intervals, to the amusement of our wives. He is very sympathetic to the material theory. He really had not gone in to it properly and I had a lot to tell him which was new to him both of the work of others and of ourselves. At one time he broke out with conviction "The old pulse theory (pause. .), the old pulse theory, Bragg, is as dead as mutton!" And he won't believe in J.J.'s energy blobs:[4] no one does, I think. H. A. Wilson[5] said to me that J.J. ought to be stopped by somebody. Rutherford quite understands now all out points. We talked a lot about Barkla's recent work,[6] which of course is awfully good. It is really simple on the whole, and seems just ripe for an explanation. We both agreed that it was something jolly simple if only we could hit it: and told me to hurry up or he would be having a shot himself. He was really awfully interested about it. The point is that each metal, at any rate from Cr to Zn has a special secondary homogeneous radiation and that this can be excited by the radiation from any heavier atom and not by that from a lighter. You will have read it of course. I don't mind telling you the theory I am testing. I am trying to find whether there is any connection between the velocity of the cathode ray and the power it has of exciting X rays in a given metal. Suppose for example that the rays that best excite X rays in Cu are faster than those that excite X rays in Fe, & that when they get too slow they don't have much effect. Then e.g. you will have cathode rays which act well on Fe and not at all well on Cu. Then all Barkla's results are explained. The rays from Zn (say) fall on Cu, they break up, cathode rays appear these knock about losing speed (it is certain now that this happens) until they get to the right speed for Cu: then off come the characteristic rays in quantity, and *in all directions* and *unpolarised*. But of course the Cu rays cannot excite X rays in Zn; because the cathode rays cannot *rise* in speed. In other word's Barkla's homogeneous secondary is really tertiary. But I have to test this theory, and I am only telling *you*: it may come to nothing of course. A man called Thirkill[7] of Clare College Cambridge is coming here to work for a few weeks and I am going to tackle it with him.

Geiger at Manchester has pretty well cleaned up the α ray problem. The particles do after all run right out in speed, but they get awfully scattered towards the end, so that Rutherford lost them by the photographic method.[8] Also the scattering $\propto$ atomic wt.

I put one of my demonstrators on the question I was last doing in Adelaide, viz the cathode rays made by primary X rays. He got the whole thing in working order after great labour, and then had to go to London without getting out much. I started in next day and got the results at once; reaped the fruit of his labour! But of course I will publish in the two names.[9] I think there is going to be something to publish viz that the *cathode rays* produced in a substance are proportional in number to the absorption of the X rays, and that the X rays do not ionise the gas directly at all. You know I got this roughly in Adelaide; but not nearly good enough to publish. I am working it with electroscopes, and I find that if you keep two going, one as a standard and reckon all the other readings as *ratios* to the standard you get marvellously consistent results even when the coil is not behaving.

Dr R. T. Beatty is working in my lab this summer.[10] He is trying to find the velocity of the cathode rays due to the secondary Cu rays by a direct magnetic method: so as to check Glasson's result.[11] He uses his quadrant electroscope, you remember it. He uses my big coil, 20" spark & a great X ray bulb. Kleeman is also here doing a diffusion of ions experiment.[12] Another of my demonstrators is proving the absurdity of H W Schmidt's "reflection" effect,[13] in fact has done so. I see you have had a shot at it long ago, but the methods are different: & he is tackling N R Campbell's anomalies of the absorptions by solutions.[14]

I have a Norwegian called Vegard coming soon, I don't know what I'll put him on to yet.[15] My senior demonstrator Shorter is doing initial recombination especially of CO.[16]

I have got a very nice house in Leeds now, with a gorgeous billiard room! and we are furnishing hard. Now I must stop. Remember me very kindly to my friends, David[17] and Pollock[18] Woolnough[19] and Warren[20], Maiden[21], if you see him. And write to me. I am trying to keep you posted up, you see! I wish I had Glasson's results as he gets them: I want them. I shall have to repeat some of them (for private information only), if I don't hear. He was good enough to send me one paper (unpublished). By the way I am going over Barkla's polarisation experiment very carefully: apparatus is nearly finished.

Kindest regards, old chap: we had great times together. I tell you, we made people look into things again: Kleeman says they thought X rays were about played out—at Cambridge, until we revived it all.

My wife sends her regards to you & yours

W H Bragg.

I have not too much time for long letters now, with all the racket of rearrangements. Would you let Glasson have a look at this? H. A. Wilson believes in the old pulse theory and that heaps of pulses at last start the particles off. But he has not really worked it out at all. He is going to McGill in Montreal: & Barkla takes his place at King's College.

**Notes:**

1. Bragg's younger brother, James W. Bragg, was engaged for many years in a successful import/export business between England and Australia (Caroe, 1978).
2. Wilson, 1909. The importance of Wilson's results is emphasised in (Heilbron, 1967).
3. Schmidt, 1907.
4. Thomson, 1907; cf. McCormmach, 1967.
5. Harold Albert Wilson, F.R.S. (1874–1964), professor of physics at King's College, University of London, and shortly to succeed to Rütherford's Chair at McGill University, Montreal.
6. Barkla, 1909; Barkla and Sadler, 1908, 1909a, 1909b.
7. Henry Thirkill (1886–1971), a Research Scholar and later Fellow at Clare College, had obtained First Class Honours in Part II of the Cambridge Natural Science tripos in 1908. He subsequently became University Lecturer in Experimental Physics at Cambridge and Master of Clare College, 1939–1958. He served as Vice-Chancellor of the University, 1945–1947, and was knighted in 1951.
8. Geiger, 1909, 1910a, 1910b.
9. Bragg and Porter, 1911.
10. R. T. Beatty (1882–1941), a Research Scholar at the Cavendish Laboratory, later a member of the Admiralty Scientific Staff. The work Bragg describes (cf. also Letter IV below) appears not to have led to a publication, but to have been a continuation of that described in (Beatty, 1910).
11. Glasson's result, here referred to, appears to have remained unpublished.
12. Cf. Letter II above. The experiment did not work; see Letter IV below.
13. Schmidt, 1907b.
14. Campbell, 1909. Neither of these pieces of research seems to have been published.
15. Cf. Letter IV below. Lars Vegard, Universitets-stipendiat of Christiania (later Oslo) University and, from 1918, professor of experimental physics at that university.
16. Also unpublished.
17. T. Edgeworth David, F.R.S., (1858–1934), professor of geology and physical geography at the University of Sydney.
18. James Arthur Pollock (1865–1922), professor of physics at the University of Sydney (F.R.S., 1916).

19. Walter George Woolnough, assistant lecturer in geology, University of Sydney.
20. William Henry Warren (1852–1926), since 1884 professor of engineering at the University of Sydney.
21. Joseph Henry Maiden (1859–1925), director of the Sydney botanic gardens and New South Wales Government Botanist (F.R.S., 1916).

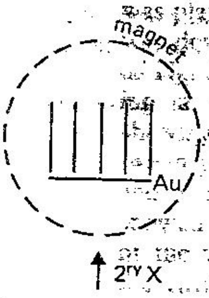
## IV. Bragg to Madsen, 6 October 1909.

Leeds, Oct. 6 1909.

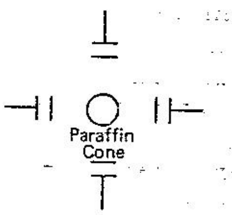
My dear Madsen

I got your letter yesterday and am answering it at once. Besides I want to tell you how things are going generally. First I will take the contents of your letter. You say you are trying to "derive a polar diagram of the intensity of scattered β rays". Quite right: what is wanted is a "polar diagram" for every sort of ray and every sort of atom. You don't say whether you are using γ or β as the primary rays, but it would be valuable in either case. By the way, talking of the intensity of emergence and incidence β rays produced by γ's: The ratio of emergence to incidence should tend to very large values when the plate is very thin, if all the rays go straight forward. Can this be shown experimentally? We got very large values I know. But could the point be made quite clear, for it stands somewhat in contrast to a probable effect of cathode and X rays. (I see I am losing my arranged sequence already). Kaye[1] has recently shown that when cathode rays fall on thin sheets the ratio of the emergence X rays to the incidence X rays is generally $> 1$ and is $> 3$ for aluminium. His paper is coming out in the Camb. Phil. Soc. Proc: I have seen the proof. I asked Kaye to send you a copy. Kaye was here half a day lately, and we got on splendidly together. I think this experiment converted him finally to the material theory, in spite of his being J.J.'s private assistant. He says he has shown the result to several, and no one can explain it on the pulse theory. Sometimes he starts with one foil & then goes on to two and three and so on: but he has not done many experiments altogether. He finds that R (emergence/incidence) grows a little with the thickness of the foil and then of course diminishes. I think this means that the X ray turns into the cathode ray at the moment of being swung round in the atom; and not that there is a chance of any cathode ray as it flies being turned into X without going out of its line of flight. For then R should become enormous when the layer is very thin, since there are far more cathode particles going forwards than backwards in a thin sheet. In fact does a pair get stripped in its flight without the $-^{ve}$ losing its direction, and a $-^{ve}$ pick up a positive only in the act of turning? We want to settle this: and we ought to find the distribution or "polar diagram" of cathode rays due to cathode rays, X due to cathode, and cathode due to X. Kaye is trying to do the second of these: you are doing the first: very likely we shall do the third here. And the comparison ought to show something! By the bye one of my demonstrators, Keene, who has gone to Birmingham Univ. wrote asking me to suggest a job and I told him to try the second of the above, the same as you are undertaking. This was before your letter came of course. But don't worry: his method is quite different, he is first making his K rays from X rays, you make them direct and he has not started yet and won't for a while I should think. Kaye quite sees all our points: he says the resemblance between the scattering of cathode rays and of X rays "is getting exciting" "So far as one could judge in the thin leaf experiments" he writes to me "by the phosphorescence of the glass walls of the tube, R for the secondary cathode rays seemed to follow any variation of R for the X rays. You could generally tell from the look of the tube whether R for the X rays was going to be considerable." I expect he is at it now, and that I shall hear from him shortly. To go back to your letter. You mention Kleeman's "polarisation". But he uses a wrong term here: he means distribution, and what he found was that R ($\frac{\text{emergence}}{\text{incidence}}$ i.e.) for secondary γ rays was considerable. I think I am right: he never touched polarisation in Barkla's sense.[2]

Dr. Beatty[3] has just left me to go back to Cambridge to keep his terms. His work came on with rather a rush just at the end and I think he was sorry to leave it. Still he got out some results. I asked him when he came to confirm Glasson's results by finding the velocity of the cathode rays due to the various homogeneous radiations, using a magnet. It turned out to be a very difficult experiment: and at one time he got rather sick of it. He could find hardly any influence at all due to the magnet. he had a little set of slits over some Au foil & tried to turn the cathode rays to one side. Finally it appeared that the effect only was plain when the pressure was down to less than 1 cm of Hg. Also he got to drawing a number of curves showing the relation between pressure & ionisation and then found that they showed him the penetration of the various radiations in the chamber. They seem to indicate a very soft X radiation from gold capable of crossing only a few cm of air. He showed that Cu rays actually do produce K rays in Ag: an effect which I think Sadler is trying to show is impossible. He wants to prove that X rays only excite cathode rays in metals whose homogeneous radiation is softer than their own. I think it is all rubbish, of course. Also he showed that the K rays excited in Au by Cu sec^y were slower than those due to Ag sec^y but he did not get good quantitative results before he had to stop. I think he was very well satisfied with his summer's work. Kleeman did not get on with his own exp^ts and then I asked him to find the ionisation in different gases due to K rays. By passing in the X rays first through gold, then card & subtracting, and he is getting on very nicely. He finds the ionisations in different gases due to the effects of the soft secondaries obtained in this way follow nearly the values for $\alpha$ $\beta$ or $\gamma$ rays.[4]




Vegard has been testing the polarisation.[5] He uses an apparatus which I designed to show the effect securely & it has four ionisation chambers symmetrically placed, and the whole thing can be turned round of course. I wanted him to see whether that which causes K rays is polarised as well as that which ionises the air and so is the subject of Barkla's experiments. He found it was; so polarisation must be accounted for on the material theory. He passed the X rays into the four chambers sometimes $\rightarrow$Au, Al, sometimes $\rightarrow$Al, Au and compared results. In the latter case he had a crowd of cathode rays, of course, and they showed the polarised effect i.e. different amounts of them were caused by the rays travelling in the two directions. He is still investigating the polarisation question with different forms of anticathode.




Kleeman has been and is doing the relative ionisations of different gases by the cathode rays.[6] He passes X rays into a chamber through $\rightarrow$Au, card screen: then from outside he reverses it to $\rightarrow$card, Au. The increase in the latter case is due to soft radiation from the Au. He gets figures much the same as for $\gamma$ rays, only they rise rather quickly for the heavy atoms. I think Ether is 1.23, $C_2H_5Br$ is 1.70 and $CH_3I$ is 3.00. I think however that he has a little soft X radiation with a range of only a few cm in addition to his cathode rays, and this may heighten the Br & I figures. You will see though that the figures are not enormous as for X rays, & fit in very nicely with the idea that the Br & I are great manufacturers of cathode rays: & that is where the big ionisations come from.

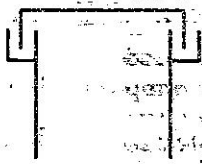
For myself I am trying to collect figures connecting the amount of cathode rays produced from each metal or substance with the absorption of the X rays in that substance, & showing the proportionality which exists pretty completely I think. I want to include Br & I if I can get suitable films: I am not sure I can, but I have got As and can get Sb. I can practically show I think that Br *does* produce clouds of cathode particles: I am also finding the absorption of all the cathode rays by Al foils.[7]

Crowther[8] must have had lots of soft radiation made in the ethyl bromide in his chamber & the reason why in spite of this his ionisation was proportional to the pressure must find some other explanation than that the ionisation by cathode rays or other soft radiation is negligible.

I have a man Thirkill from Clare College Cambridge who is doing Helium ionisation constants by $\alpha$ rays.[9] I never did it properly in Adelaide, only the argon.[10]

Now I think I have told you most of the doings here. You would see W. Wilson's paper on $\beta$ rays in the Roy. Soc. Proc.[11] also Eve's in the Phil. Mag.[12] on $\gamma$ rays confirming our results. I have not heard of anything more particular. I believe J J had a paper at Winnipeg in which he used the neutral pair idea to explain some vacuum tube phenomena.[13]

What cells have you got? I got 500 from Klingelfuss in Basle (Switzerland). They are test tube cells, pasted, with 1st. class porcelain insulation. They are small but doing well. I have a glorious air pump: Gaede's double rotary. A rotary oil does the 1st pumping down to about .01 mm of Hg: & a rotary Hg pump completes the job: all driven by a little motor. You just switch on the current & go off & do something else until the tube is ready. I find that X ray exp$^{ts}$ are quite easy if only you use a separate standard ionisation chamber all the time & work by comparison with it. E.g. the standard chamber gives 110 in a certain time, & the main one 172 then the current is called 172/110 = 1.56. I don't use a watch: just read the zeros turn on the current a convenient time & turn it off again. Beatty's electroscope is very sensitive, but tricky. For many exp$^{ts}$ I use the ord$^{y}$ electroscope: not even the tilted one. An inch each way is quite big enough: & allows the use of a less special microscope. If you want a sealed chamber to hold a vacuum & easily take down, try this:–



Pour in melted it rosin & beeswax round the joint. It is quite airtight. To take off warm with a flame, it is very quickly done.

I was called in as an expert last week to adjudicate on the claims of a man for a fellowship at Trinity College, Cambridge. I felt such a duke. Soddy is going to stay a night with me week after next. Now I must stop: this letter is all shop, meant to tell you what is doing so far as I know. I will write a more human one presently. Kindest regards to yourself & Mrs. Madsen.

Yrs always
W H Bragg

I will see about the foils

**Notes:**

1. Kaye, 1909.
2. Kleeman, 1909.
3. See Letter III above.
4. Kleeman, 1910.
5. Vegard, 1910.
6. Kleeman, 1910.
7. This work appears not to have led to a publication.
8. Crowther, 1909.
9. See Letter III, note 7 above. The project appears not to have led to a publication.
10. Unpublished.
11. W. Wilson, 1909.
12. Eve, 1908.
13. Thomson's paper was not published in the Report of the Winnipeg meeting of the British Association, but an account of his address subsequently appeared in *Engineering*, *88*, 1909, 374.

## V. Bragg to Madsen, 12 December 1909.

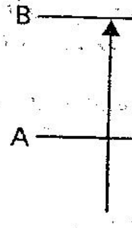
Leeds Decr. 12 [1909][1]

My dear old chap

I have been a round of labs lately, and have seen many people, pretty well all the men whose names we have discussed so often, so I guess I may as well write to you about it all whilst it is fresh.

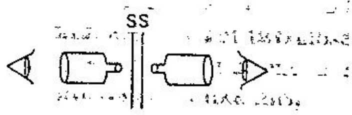
The points we used to talk about are very much to the fore and for your satisfaction and mine I may as well say at once that we have always been on the right track so far as I can judge. This is for your private ear! The neutral pair theory may or may not be absolutely true but I think nearly everyone thinks that its promulgation was abolutely justifiable at the time, and that it has led to several discoveries and encouraged several successful researches, which it alone prompted. Some, including Rutherford, have actually said as much to me, very positively, and wherever I go I find the theory and all our experiments treated with great respect. Also the newest work still fits in, and indeed strengthens our arguments. You would see Stark's work in the Phys. Zeit. of Nov. 22 (? I think, or thereabouts)[2] in which he finds the X rays from a C plate struck by cathode rays to be much more intense, and *much more penetrating* in the forward than in the backward direction. I spoke to you before about Kaye's work in the Camb. Phil. Soc. Proc: which covered part of the same ground.[3] You would also see Sadler's letter in a recent "Nature".[4] I saw him yesterday in ~~L~~iverpool and had a long talk with him about it, and saw his apparatus. He is quite clear that the different homogeneous 2$^{ry}$ X radiations cause cathode radiations from various screens; the vel$^{y}$ of which is quite independent of the nature of the screen but is (or rather the absorbability is) a linear function of the atomic wt of the radiator. He says there is little of it from a screen in which the radiator is unable to excite the 2$^{ry}$ X rad$^{n}$ which is characteristic of the screen: ~~but there~~ I am not sure that he has got his theory quite right, and I am going into the question. There was a point about which he was mixed and I tried hard to make him see it, but I had t[o] give it up. He thought that if the ionisation between two plates

A & B was prop! t[o] distance AB, then there was no cathode rad$^{n}$ from A or B. What it means is that the plate A contributes as much cathode radiation t[o] the space between A & B as would a block of air below A, the screen A being taken away.




I dined in hall at Trinity with J.J. the other day but we did not talk much science. Only I asked him as we walked home what he thought of the "light quantum" of the Germans, and of their practically abandoning the ether for the corpuscular theory of Newton (Einstein & Stark i[n] the recent Phys. Zeit).[5] He said it would not work at all. How could one explain reflection and refraction at the same surface? ↗ When I told Boltwood[6] this he said he did not think JJ would have much worry in explaining it if he wanted it for his theories! J.J. has a new theory every week. ✓ For myself I cannot see how they are going to explain the unique velocity of light in space. J.J. is much puzzled over a fact that he has recently discovered: he finds the velocity of the canal rays to be independent of the potential on the tube; its value is $3 \times 10^8$ cm/sec. the same as $\gamma$ rays: and he imagines, I think, that the atom emits a doublet which subsequently breaks up into +ve & −ve and that it is a sort of trigger action.[7] I don't know exactly how he explains all the X ray things: Glasson says he heard him say at a lecture that the X rays store up energy in the atom until the emission of the cathode ray takes place, but as you know there is any amount of argument against this theory.

Kaye is a great friend: he took me all round the Cavendish explaining everything. Beatty is continuing an experiment begun in Leeds: which is much the same as Sadler's. J.J. himself is at these canal rays: most of the others were on experiments which do not closely concern us. At Manchester they were all on radioactive work. There was a Russian[8] trying to find whether the absorption by $\beta$ rays depends on constitutive influences of the molecule. Of course he gets the negative: some of my own students have found the same; I cannot think what big mistake S. J. Allen has made in the Physical Review.[9] I am sending a letter to the Review.[10] Some one else is trying to measure the absorptions of $\beta$ rays of different speeds, if I remember right: but he was only starting. Boltwood is at the quantities of helium from all the different radioactive products. He is such a nice fellow. Geiger has been doing the *timing* of the $\alpha$ particles: he has some emanation between two fluorescent screens very close together like this, so that when an atom goes off its $\alpha$ particle is bound to hit one of the two small portions of the screen which are in view of the two microscopes. The observers register by keys pressing on the paper of the same chronograph. Then they don't get flashes *anyhow*, but in a sort of sequence with actinium eman$^{o}$ I believe they get two close together, and one a little later like this,




.. . .. .. .. . .

The Morse code, they call it! Isn't it extraordinary? Curiously enough they don't get it with Th.B & C. which you would rather expect.[11]

I have not got many results to chronicle myself. I have been so busy getting things straight. But as opportunities served I have been going on with the conversion of X rays into cathode rays, and my results are becoming more consistent. I want to measure also the absorptions of cathode rays of various speeds, and to trace the exact connection between the speed of the particle in the X ray tube and that of the 2ry cathode ray. I am getting a new workshop, and have taken a man from the Cambridge Scientific Inst. Co.

Dec. 17. Just heard that my man can't come for family reasons: what a nuisance! I have a second string in the shape of a Dutchman from Kamerlingh Onnes's lab. in Leyden.[12]

I have been much concerned to find you anything new in the way of foils, but I can get nothing which we did not have in Adelaide. The only thing is I can get real copper foil not Dutch metal and I have got a packet of that to send you. There is nothing else.

Have you heard of the new Snook apparatus for X rays? It has a ring induction coil, a real transformer with alternating primary and a commutator in the 2ry. which is mechanically reversed at the proper time. The voltage is 70,000 to 10,000 & you can get up to 60 milliamperes, perhaps more, but no tube will stand this for more than $\frac{1}{2}$ second: that is enough to take a photo thro the human body. The 2 Kw-size is £140! & the 4 Kw is £170.

Now I must stop. My kindest regards to your wife I hope you are all well and flourishing.

Yrs always
W H Bragg

**Notes:**

1. The date is established on internal evidence e.g. the presence of Boltwood in Manchester.
2. Stark, 1909.
3. Kaye, 1909.
4. Sadler, 1909.
5. Einstein, 1909.
6. Bertram B. Boltwood (1870–1927), Assistant Professor of Physics at Yale College, and from 1910 Professor of Radiochemistry. Boltwood spent the academic year 1909–1910 as a research fellow in Rutherford's department at Manchester.
7. Thomson, 1909.
8. W. A. Borodowsky on leave from the University of Dorpat, was preparing a thesis in Rutherford's laboratory on 'The Absorption of β Radiation of Radium'.
9. Allen, 1909.
10. Bragg, 1910a.
11. These remarkable results were never published.
12. Heike Kamerlingh Onnes (1853–1926), Professor of Experimental Physics at Leiden University.

## VI. Bragg to Madsen, 2 March 1911[1]

Rosehurst March 2 [1911]
Grosvenor Road,
Leeds.

My dear Madsen

Just a note to acknowledge the receipt of the cheque. My word! you are a lucky chap. I have seen Rutherford about it: & he says, as I expected, that the only thing to do is to write to Giesel[2] which I will do at once. I will write you a letter about β rays &c very soon. I am so glad your work is going on well.

My wife and I are so dreadfully sorry about the little baby that did not live. My wife was so distressed when I told her about it. Do tell her how sorry we are about it: my wife says only a woman can realise what it means.

I hope the rest of the family is well & happy.

Kindest regards
Yrs always
~~W H~~ Bragg

I will write again as soon as I hear from Giesel. I shall be delighted to see Dixson.[3]

**Notes:**

1. The dating of this and the succeeding letter is based on that established for Letter VIII.
2. Friedrich Otto Giesel (1852–1927), the chief chemist at a quinine factory in Braunschweig, produced purified radium bromide for sale as something of a hobby.
3. Mr. Hugh Dixson was a wealthy Sydney tobacco merchant who had donated the money for Madsen's radium purchase.

## VII. Bragg to Madsen, n.d. (March or April 1911).[1]

The University, Leeds.

My dear Madsen

Just a line to report progress. I have accepted, with Rutherford's approval, an offer from the Chininfabrik Braunschweig to supply 10 mmg now and 20 in May, at £16.5. a mmg. It seems an awful price, but Rutherford says it is right. I am thinking of making a small cup, with two divisions in it, one to hold the 10 the other the 20; & the radium must be put into 2 separate receptacles which will fit into the cup side by side, so that I can send you the 10 now in the cup & let the 20 follow. The activity is guaranteed 90%.

Yrs
W H B

**Note:**

1. The dating is based on that established for Letter VIII.

## VIII. Bragg to Madsen, 18 May 1911.

The University, Leeds. May 18. [1911]

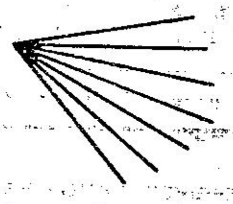
My dear Madsen

The radium for you has been posted and I hope will arrive safely. It was registered and I would have insured it but there are no insurance terms to Australia. I do hope you will find it all satisfactory.

I had rather a bother with the cup. The makers of the radium (Chininfabrik Braunschweig) refused to have anything to do with the cup: and I had to make it here. I did not know the dimensions very well, but I calculated that if 30 mmg were spread over 1 cm² or thereabouts your self absorption of β rays would be 15% which I thought a fair compromise. So I had a platinum cup made in the town here and fitted into a brass stand in my own shop: making the whole thing rather big and loose so that the radium people might pack it to suit. But they returned it: I think they do not like putting it in such an arrangement and I am not sorry myself that it should go out to Australia in a glass tube. Unpack it carefully, the glass tube is not likely to crack, but suppose it did! I think you might undo the last little square box on a clean sheet of glass so that if there had been an accident by any chance you would have everything in the one spot. I made the cup flat because the radium would be arranged more economically that way: and I am sending it on in the hope you will be able to use it or alter it. I dare say you could get it made a little shallower—it is platinum of course.

How are you getting on? How do you like Rutherford's new atom?[2] The situation is rather funny now. Crowther & Barkla were just now arguing in the Phil. Mag. about the X ray scattering in its relation to J.J.'s theory:[3] and Rutherford brings forward a theory which cuts the ground from under the feet of all of them if it is true. Rutherford's theory touches your β ray work very nearly: and indeed the law of scattering of the β particle is very much to be determined in order to test his theory. Knowing that you were working away at this and having your last letter explaining what you had got I thought it best to show it to Rutherford. I thought if he went at it hard he would with all his opportunities get ahead of you: he is a very generous chap and always ready to give everyone all he can. So I thought that if I told Rutherford exactly what you were doing and had done, he would take you in so to speak: your results agree with his theory very well, and you will see in his paper that he has made special reference to what you have published.[4] If you have anything more, now or in the future, I should write to him direct or at least through me if you prefer, and I know he would like to hear from you and build in anything you had to give.

You may have seen that C. T. R. Wilson has given a paper at the R.S. on a method of making visible the tracks of ionising particles.[5] He is very excited about it: has been working at it two years and just been successful. He flashes the rays (α β γ or X) through the gas and takes an instantaneous picture of the fog caused, by a simultaneous expansion. The ions have not had time to spread and so you see the tracks. The photos I believe are not as fine as the real thing which they say is gorgeous, especially for the α particles. My boy has seen them. CTR sent me two photos which Rutherford has just now or I would send them to you. The α particle is like this:

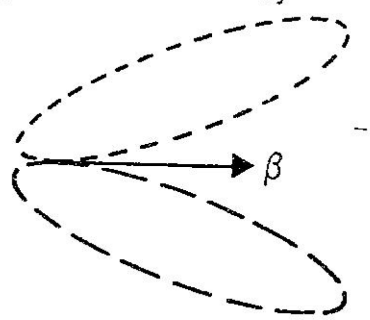


the shorter paths are of α particles that are not in the plane of the cell & have hit the walls I suppose. They are beautifully defined. The X ray one is "rather an effort" as my schoolboy Bob says.[6] It is like this

can't be anything else but the track of the cathode rays in the gas! The γ rays have not been photographed: but to the eye CTR says they show fine delicate straight lines right across the chamber which are no doubt the β rays from the walls.

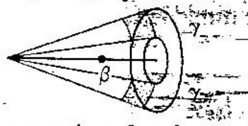
I am reading a paper at the RS next week:[7] just explaining the transformation of energy of the X rays: trying to account for the expenditure in secondary and in cathode rays. It has been awfully hard, because it is quantitative and there is so much to be taken account of. It is only approximate now, but I think it breaks the ice.

Sommerfeld has just carefully worked out ↗(Bavarian Academy)↙ what sort of a γ ray the electromagnetic theory gives from the starting of a β ray of different speeds.[8] He gets lots of dissymmetry for he finds that suppose the β ray goes off from RaC with a speed of 9/10th of light the γ ray goes out practically in a hollow cone, the dotted curve shows the intensity



in different directions: of course if the β ray began & ended in a certain small time the γ ray energy will be confined also between two spherical surfaces, which are not quite concentric. Thus the γ ray is like a spreading ring, the β ray lags behind of course.

The ↗ semi- ↙ ∢ of the cone is 15° for a 9/10th vely & 5° if the β ray gets up to 99%.




But this while it gives dissymmetry gives no clue at all as to how γ ray energy, which is ever widening gets back into one electron again. I have written to ask him & he is bound to reply, I think, that the γ ray has a trigger action, or else that there is a storing of energy.

I do hope you are all well. I am so glad to get something off to the RS: because it has been such a long job & the result seems so small for the labour which is great. Still it is a start in this line: & the X ray apparatus is now so good that consecutive readings differ by less than ½%: it has come on real well. I have got quite a lot in hand: I had a Royal Institution discourse,[9] & I have done the RS paper & a long paper for Armstrong for Science Progress.[10] Then I have a book (200 pages) to do for Macmillan who asked me some months ago,[11] & I have promised to write the article on radioactivity for Thorpe's new edition of his dictionary.[12] We are all well: summer has come in beautifully & the country is gorgeous. May it keep so! My very kindest regards to you old chap. & remember me to your wife & the University people Pollock Woolnough Vonwiller[12] & so on & Warren

Yrs truly

W. H. Bragg

I will pay for the cup & send back the rest of the money.

**Notes:**

1. The dating is based on internal evidence, in particular the fact that C. T. R. Wilson read his paper to which Bragg refers to the Royal Society in April 1911.
2. Rutherford, 1911.
3. Barkla, 1911.
4. Rutherford, 1911; p. 685.
5. C. T. R. Wilson, 1911.
6. Bragg's younger son, Robert Charles Bragg (born 1891) was killed at Gallipoli in 1915.
7. Bragg and Porter, 1911. Bragg read the paper to the Society on 25 May 1911.
8. Sommerfeld, 1911.
9. Bragg, 1911a. Bragg read his paper at a Weekly Evening Meeting on Friday 27 January 1911.
10. Bragg, 1911b.
11. Bragg, 1912.
12. Thorpe, 1912–13. Bragg's article, "Radioactivity", appeared in Vol. IV, pp. 534–544.
13. Oscar Ulrich Vonwiller (1882–1972), assistant lecturer and later (1923–46) professor of physics at the University of Sydney.

## IX. Rutherford to Madsen, 8 March 1911.

Original: Rutherford Papers, University Library, Cambridge.

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.[1] 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,[2] 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 distribution of electricity opposite in 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 abstract the main deductions from the theory which I find, as far as experiment 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.[3] Dr. Geiger is testing for me the correctness of the main assumptions, using the α rays and by the scintillation method.[4] As far as he has gone, he has found an extremely good agreement between the experimental and theoretical distribution of $\alpha$ 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[5] [f]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 $\alpha$ 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 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 cases 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 $\beta$ 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 are going.

Yours sincerely,
E. Rutherford.

Give my remembrances to Professor Pollock. I am hoping to visit Australasia at the time of the BA meeting.[6]

**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
$\phi$ = 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 $\alpha$ particle directed straight to the centre of the atom will be turned 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 $\alpha$ or $\beta$ 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 $\phi$ 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 between $\phi$ 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 $\phi$ is equal to

$$\frac{\pi}{4} b^2 n t \cot^2 \phi/2 \qquad (1)$$

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, supposed small (2) $nA^2$ (3) $\frac{1}{(mu^2)^2}$

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 corpusc[u]lar scattering is small compared with that due to the 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 $\operatorname{cosec}^4 \frac{\phi}{2}$ where $\phi$ 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 over a range of 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 cannot [be] due to accumulative small scattering.

E.R.

**Notes:**

1. Madsen, 1909.
2. Geiger and Marsden, 1909.
3. Rutherford, 1911.
4. Geiger, 1912.
5. Crowther, 1910.
6. It had been decided a few months before to hold the 1914 meeting of the British Association for the Advancement of Science in Australia.

## X. Madsen to Rutherford, 9 July 1911.

Original: Rutherford Papers, University Library, Cambridge.

The University of Sydney July 9 11

Dear Professor Rutherford

It was very good of you to let me know what you were doing on the scattering of rays & I can hardly thank you sufficiently for your kindness in delaying the β ray portion of the work. I must apologise for not having replied immediately to your letter, but I understood that portion of the Ra which Prof. Bragg was procuring for me would be sent out immediately & I hoped to be able to send you some results. However the 30 mg has just come to hand in one lot & I am now ready to go straight ahead. I have already been over the ground with a very weak sample but as the results are so poor quantitatively I thought it best not to publish them without verification. The polar diag. of distribution for Al, Au & C plates need correction for the effect of the plate before one can get at the probable distribution around an atom, and I should now be able to do this.

With regard to what you say with regard to the initial portion of the curve obtained by Crowther I think it is probably due to the comparatively large area of his active plate. I have experienced considerable difficulty in this respect owing to rays, which previously did not get into the ionization chamber, being brought in by scattering when the screen was in position near the Ra.

When the screen is further away from the Ra these oblique rays do not fall on it.

I hope I may be able to run over the work & let you have complete details before long now that my main difficulty has been overcome.

With many thanks for your kind consideration

Believe me

Yours faithfully

J P V Madsen

## XI. Madsen to Bragg, 7 November 1911

Original: Rutherford Papers, University Library, Cambridge.

*At the top of the first page of this letter, there is a note in Bragg's hand as follows, addressed presumably to Rutherford:*
*"I meant to put this with my letter of this morning. WHB."*

The University of Sydney Nov. 7 1911

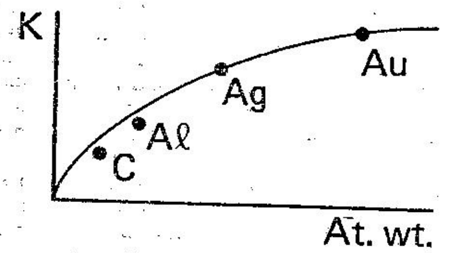
My dear Prof.

The figures you ask for are

| | |
|---|---|
| (1) for soft γ rays | 2200 |
| (2) " hard " " | 1560 |
| with 1.6 cm Pb plug | 1400 |
| " about 10 " " | 960 |

This last figure would be probably a fair measure of sundry scattered radiation & natural ionization in the chamber.

I am going on steadily at the scattering; have measured for thin foil of C. Al. Ag. Au. the scattered $\beta$ rays on the far side of thin sheets & am just about ready to tackle the near side. On the emergent side the curves shewing the relation between the rays scattered, and the mass per unit area of the screen appear quite similar for all these materials so that by plotting mass pr unit area × K the curves all agree. These K's seem closely related to the atomic weights but the relation is not quite so simple as I at first thought it.




A curve $K = \sqrt{at.wt}$ passes exactly thro' Au & Ag but it is about 15% above Al & C.

I cannot settle Rutherfords point from the scattering on the front side but hope to, by considering the ratio of scattered-rays in a forward & backward direction for thin sheets.[1]

I have just seen Kleeman—on his way to Adelaide[.] He tells me he is busy writing a book.[2]

We had Cox from Montreal over here a short time ago;[3] he is a most interesting chap & gave me quite a long account of his experiences with Rutherford.

Radcliffe is to come to Sydney permanently after Xmas to take charge of the Ra. Co's works.[4] He was across last week & brought up some of the RaBr he had extracted. It was about 1% purity but he says the final purification is straight forward. I have just been testing some of the specimens from Mawson's ~~new find~~.[5] They are much richer than the Olary, some seem to be almost pure Uranium compounds, so you may hear of Aus. becoming a big Ra producer before long. I am looking forward to seeing your book when it comes out.[6] With kind regards to Mrs. Bragg & yourself

Yours sincerely,
J. P. V. Madsen

I am sending Rutherford a copy of my results as soon as I get the first part together. I hope to go soon afterwards with fine powders deposited on foils, & obtain the detail of the "K" "At. wt." curve".

## Notes:

1. Madsen's hopes were apparently not realised, since the work he discusses here never led to a publication.
2. Kleeman, 1920.
3. John Cox (1851–1923), professor of physics at McGill University, Montreal, 1890–1909.
4. Sydney Radcliff, Principal of the School of Mines at Bairnsdale, Victoria, developed a process for extracting radium from carnotite ore mined by the Radium Hill Co. N.L. at Olary, South Australia. In 1911 he resigned from the school in order to develop his process to a commercial scale. The Company began commercial extraction of radium, using his process, in 1912, at Hunter's Hill, New South Wales. In 1916, however, it became insolvent.
5. Douglas Mawson (1882–1958), the celebrated Antarctic explorer, lecturer in mineralogy and petrology and later (1920–1952) professor of geology at the University of Adelaide. For Mawson's discovery, see Melbourne *Argus*, 22 November 1910, p. 6.
6. Bragg, 1912.

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