There is a memorandum in Newton's handwriting among the papers which formed the Portsmouth collection which has often been quoted. It was probably written in 1716, when Newton was seventy-three years old. One passage, in particular, I am sure you will remember:

In the same year I began to think of gravity extending to the orb of the Moon, and having found out how to estimate the force with which a globe revolving within a sphere presses the surface of the sphere, from Kepler's rule of the periodical times of the Planets being in a sesquialterate proportion of their distances from the centres of their orbs I deduced that the forces which keep the Planets in their orbs must be reciprocally as the squares of their distances from the centres about which they revolve: and thereby compared the force requisite to keep the Moon in her orb with the force of gravity at the surface of the Earth, and found them answer pretty nearly.

Thus Newton, in old age, undeterred by the inadequacy of ordinary language fully to expose the springs of his inspiration, set out soberly, and in logical order, the steps by which the hypothesis of universal gravitation formed in his mind and came to carry conviction.

In 1936, when Rutherford was sixty-four, he likewise looked back, though over a narrower span of years, to the time when he had put forward another hypothesis of no less generality, the hypothesis of the atom nucleus. He wrote:

It was then that I had the idea of an atom with a minute massive centre carrying a charge. I worked out mathematically what laws the scattering should obey, and I found that the number of particles scattered through a given angle should be proportional to the thickness of the scattering foil, the square of the nuclear charge, and inversely proportional to the fourth power of the velocity. These deductions were later verified by Geiger and Marsden in a series of beautiful experiments.

No one, I think, with an ear for cadences and a sense of form will fail to recognize the close similarity of these two statements: Newton, in retrospect,
seeking to give simple verbal expression to what was perhaps his greatest single triumph of intellect; Rutherford, in plain speech, attempting to rationalize, after twenty-five years, the processes of intuitive thought which led him to the nucleus. Let me set the statements in opposition, clause by clause:

Newton, 'In the same year I began to think of gravity extending to the orb of the Moon',
Rutherford, 'It was then that I had the idea of an atom with a minute massive centre carrying a charge',
Newton, 'I deduced that the forces which keep the Planets in their orbs must be reciprocally as the squares of their distances',
Rutherford, 'I worked out mathematically what laws the scattering should obey',
Newton, '[I] found them answer pretty nearly',
Rutherford, 'These deductions were later verified by Geiger and Marsden in a series of beautiful experiments'.

It is not my intention this afternoon to pursue this comparison farther at the level of textual criticism, nor to speculate whether the notion of universal gravitation required for its entertainment a genius of a different order from Rutherford’s: suffice to say that in Newton’s case it was the elliptical orbits of planets described according to the rule of constant areal velocity about the Sun which predicated an inverse-square-law force; with Rutherford it was the realization that hyperbolic orbits in an inverse-square-law field could reasonably explain the otherwise inexplicable large-angle scattering of $\alpha$-particles from thin metal foils—if only charge were concentrated within a sufficiently small domain within the atom.

So, with some sophistry, perhaps, a link may thus be forged between Newton and Rutherford, each in his own time adventuring on ‘strange seas of thought, alone’. I hope I shall not be charged with mere sophistry for attempting to forge that link. Among the collection of Rutherford papers assembled for display on this centenary occasion there is a student text of the first three sections of Book I of Newton’s *Principia*. Edited by Percival Frost, this was the manual from which undergraduates of many generations learnt the rudiments of particle dynamics in the latter part of the nineteenth century. Engaged in moving the library of my own department in Edinburgh this last summer, I came across several copies of this selfsame manual. The copy which is on display today I found in Rutherford’s own library after his death. Lady Rutherford had kindly offered me the choice of two volumes to take as mementoes. Hastily, I chose this as one of them. It has a simple inscription on the title page, ‘E. Rutherford,'
ON THE MOTION OF BODIES IN CONIC SECTIONS, UNDER THE ACTION OF FORCES TENDING TO A FOCUS.

PROP. XI. PROBLEM VI.

A body is revolving in an ellipse, to find the law of force tending to a focus of the ellipse.

Let $S$ be the focus to which the force tends, $P$ the position of the body at any time, $PCG$, $DCG$ conjugate diameters, $Q$ a point near $P$, $QT$, $PF$ perpendiculars on $SP$, $DCG$, from $Q$, $P$ respectively, $PF$ a tangent at $P$, $QR$ parallel to $SP$, $Qx$ parallel to $PR$, meeting $SP$ in $x$, and $FC$ in $v$, and let $SP$, $DCG$ intersect in $E$.

Then $F = \frac{2PQ}{SP^2} QR$ ultimately, when $PQ$ is indefinitely diminished.

But, by similar triangles $QT$, $PFE$,

$$QT = PF \cdot \frac{PF}{BC}$$

Now, $Qx = CD^2$, by the properties of the ellipse,

$$Qx = CD^2$$

The investigation is exactly the same as in the last proposition, employing the subjoined figure.

Also, repulsive force from $C \propto CP$, and by Prop. vii. Cor. 3, force from $C$; force to $S : OP \cdot SP^2 : PE$,

whence force to $S \propto \frac{1}{SP^2}$, since $PE$ is constant.

PROOF XI. PROBLEM VI.

and $P_e = CP, QR = PE, Tx = TP$, by similar triangles;

$$Qx = \frac{CD^2}{AC^2}$$

Hence $QR = DC^2$,

and $vG = 2CP$, $Qx = Qe$, ultimately;

$$Qx = \frac{2BC}{AC}$$

if $L$ be the latus rectum of the ellipse;

$$F = \frac{2PQ}{SP^2} \cdot \frac{1}{SP^2} \cdot \frac{1}{SP^2}$$

All that.

Since the force tending to the centre of an ellipse, under the action of which the ellipse can be described, varies directly as the distance $CP$ from the centre $C$; let $CE$ be drawn parallel to the tangent $PQ$ to the ellipse; then if $S$ be any point within the ellipse, and $SP$, $CE$ intersect in $L$, force tending to $C$; force tending to $S$;

$$CP \cdot SP^2 : PE \cdot \frac{1}{SP^2}$$

since $PE$ is constant.

PROF. XII. PROBLEM VII.

A body is revolving in a hyperbola, to find the law of force tending to a focus of the figure.

The investigation is exactly the same as in the last proposition, employing the subjoined figure.

Also, repulsive force from $C \propto CP$, and by Prop. vii. Cor. 3, force from $C$; force to $S : OP \cdot SP^2 : PE$,

whence force to $S \propto \frac{1}{SP^2}$, since $PE$ is constant.

FIGURE I. Pages 220 and 221 of Rutherford's copy of Frost's, Newton's Principia, First Book, Sections I, II, III, with marginal notes.
Trinity College’. Rutherford, it seems, had bought it, at second- or third-hand —for the fly-leaf is cut away, and the bookseller’s price has twice been marked down—as one of his first purchases on his arrival in Cambridge as an advanced student. In the margins throughout the text there are various annotations, some of them in the same black ink as was used on the title page. The most copious markings are on page 220, where the book lies open on display (see figure 1). The proofs which the young Rutherford annotated here establish the inverse-square law of force for bodies moving in elliptical or hyperbolic orbits under forces acting from a focus. Some fifteen years later, in Manchester, he ’worked out mathematically what laws the [large-angle] scattering [of α-particles] should obey’. He said so, in so many words, in the testament which I have quoted already.

We are fortunate that much of Rutherford’s working on this problem has survived in manuscript form. Next to Frost’s Newton, this surviving material is on display: a bundle of thirty-five manuscript sheets, written at different times, with different pens, and on different batches of paper. (There are three sheets of headed notepaper, and a number of unheaded sheets, from the Macdonald Physics Laboratories at McGill.) Obviously, when a fresh opportunity to devote serious thought to the problem presented itself, Rutherford took whatever writing materials lay at hand, even McGill notepaper, already mellowed with age—though it is strange to find this on his desk in Manchester in 1910. Almost certainly all the calculations on these sheets date from the autumn of that year or the winter months that followed. Very probably Rutherford assembled them together, and put them in some kind of order, when he was writing the paper ‘The scattering of α and β particles by matter, and the structure of the atom’ which appeared in the Philosophical Magazine in May 1911. The manuscript of this classic paper is alongside the bundle of more-or-less rough calculations in our present exhibition. On the top sheet of the bundle there is the laconic title ‘Theory of structure of atom’.

I have wandered slightly from my Newtonian theme: let me return to it briefly to make my point. One group of papers in the bundle, separately labelled 1 to 5 (they are in fact sheets 11 to 15, as the bundle is arranged), are entitled ‘Deflection of a particle through a large angle’ (see plate 7). There are photocopies of these sheets alongside the bundle. Here, for the first time, Rutherford is getting to grips with his problem. He takes an atom in which there is negative charge distributed within a sphere of radius \( R \), and at the centre a balancing positive charge of negligible size. An α-particle crosses the boundary of the atom with specified velocity. If it were to enter radially, it would be turned back at a distance \( b \) from the centre. If it enters in any other aspect, it...
Deflection of $\alpha$ particles through a large angle.

Initial velocity $V_0$. Current charge $q$. 

$$\frac{1}{2}mv_0^2 = \frac{NqE}{v}$$

$$v_0^2 = \frac{2mE}{q} \quad \therefore \quad \mu = \frac{NqE}{2m}$$

Let $\alpha = \alpha$. The $\alpha$ particle is suddenly brought down. The charge + then be deflected by current charge, coming with $\mu$ which comes $\frac{1}{2}$. The $\alpha$ is now a heavy bullet with centre, about which are forces.

\[ P = \text{unit force} \]
\[ S = \text{centre of atom (unfused)} \]
\[ S' = \text{the force} \]
\[ \theta s = R = \text{radius of atom} \]
\[ PL = \text{distance from cathode} \]
\[ \theta s' = \text{the new cathode} \]
\[ \theta s - s' P = 2a \quad \therefore \quad \mu = \text{unfused atomic radius} \]

Suffering no forward. Then what? unremarkable or $s$.

One of a bundle of 35 manuscript sheets (Cambridge University Library, Rutherford Collection, PA 194) containing various calculations on $\alpha$-particle scattering made in 1910. 

Courtesy, University Library, Cambridge.
Plate 8

Page 39 of Rutherford's laboratory notebook of 1917 (ibid., NB 22) recording observations made by Kay on 6 November on the 'anomalous effect in nitrogen', and registering an 'important' conclusion on the opposite page. Courtesy, University Library, Cambridge.
moves, so he assumes, in a hyperbolic path with the centre as external focus. The problem is to calculate the deflexion. Making no reference to the fact that the path would not be strictly hyperbolic throughout the whole volume of the atom on the basis of his precise assumptions, he proceeds directly with his analysis. On any estimate, his choice of variables is unusual. One is simple enough: it is the ratio of the radius of the atom to the closest distance of approach, namely $R/b$. Clearly this parameter has a simple direct relationship with the energy of the incident $\alpha$-particle. However, his other variable has no such simple significance: it is the angle, at the point of entry of the $\alpha$-particle across the boundary of the atom, between the lines joining that point to the centre and to the other focus—the internal focus—of the hyperbola on which the $\alpha$-particle is assumed to move. In the end he obtains a simple, albeit approximate, expression for the deflexion of the $\alpha$-particle in terms of these two variables. In the Newtonian context, what I ask you to do is to compare the calculations on these five sheets with the calculations on pages 220 and 221 of Frost’s students’ *Principia*. I think you will agree that the approach is essentially the same in the two cases: it is basically the geometrical approach to particle dynamics which pervades the whole of the first Book of that great work as Newton originally wrote it.

Leaving Newton on one side, now, there are two further matters in relation to Rutherford’s calculations concerning $\alpha$-particle scattering and the structure of the atom to which I would like to refer. The first concerns the calculations themselves, the second concerns the nuclear hypothesis which resulted from them. Let me take them in sequence.

I have already referred to the fact that in the first calculations of large-angle scattering the radius of the whole atom is retained as a parameter in the analysis. The initial conditions are specified as those describing the entry of the $\alpha$-particle into the neutral atom. Essentially, Rutherford is feeling his way into the unknown territory which lies within that boundary. Earlier in his investigation he had been concerned to compare the predictions of his central-charge model with those of Thomson’s model, with its uniform distribution of positive electrification, in relation to small-angle scattering. For that purpose he used the impact parameter as variable, as the textbooks use it today. The values of this parameter which were important for this particular calculation were smaller than the radius of the whole atom by no more than a factor of ten. It appears strange, indeed, that when he proceeded to calculate the large deflexions he abandoned the use of this simple parameter, for a time, in favour of the angular variable that I have already described. Ultimately, of course, he returned to the use of the impact parameter—it was the only direct way of estimating relative
probabilities. Then, when he finally realized that the values of the impact parameter of interest for his main problem were less than one-thousandth of the radius of the whole atom, that constant, also, disappeared from his calculations, he referred the impact parameter instead to the closest distance of direct approach, the extranuclear electrons were left out of account, and the derivation of the scattering formula took on its now familiar form.

Obviously, we cannot at this distance in time place the various pieces which make up the thirty-five manuscript sheets of the bundle that I have been discussing, with perfect confidence in strict chronological order—and we cannot be sure that other pieces of equal interest have not been lost—but, if we accept the present arrangement of the material as broadly sequential, as I have done, we are surely presented with clear evidence of the torment and tumult in Rutherford’s mind as the novel concept of the atom nucleus slowly emerged from his long deliberation in its stark simplicity. No analogy is wholly convincing, but one thinks inevitably in this connexion of the final version of Maxwell’s electromagnetic theory, of which Whittaker wrote that it ultimately appeared 'stripped of all the scaffolding by the aid of which it had been first erected'.

So much for the calculations: a word now concerning the nuclear hypothesis itself. It is worth comment that Rutherford did not commit himself seriously to print on the subject of his new ideas for nearly three years after the original paper of May 1911 was published. Not more than three pages out of 670 in that most massive of his original publications, *Radioactive Substances and their Radiations*, which appeared in the spring of 1913, refer to the problem of a-particle scattering and the nuclear atom. It was as if the generality of the new ideas and the wide range of their significance had not been fully appreciated; almost as if there were a lingering suspicion in Rutherford’s mind that they were not at that time sufficiently well-proven to qualify for inclusion in a students’ textbook. Whatever may have been the reason, it was not until the March 1914 issue of the *Philosophical Magazine* that there appeared a substantial paper under Rutherford’s name giving ‘a brief account’ as he wrote, ‘... of the later investigations which have been made to test [the “nucleus” theory of the atom]’ and dealing with ‘certain points in connexion with the ... theory ... which were purposely omitted in my first communication’.

In the same March issue, the fourth of the now-classic series of five papers which Niels Bohr wrote on the constitution of atoms and molecules also appeared. Rutherford had been the communicator of all these papers, yet his own account of them in his survey of ‘later investigations’ may, in retrospect, be thought to do them scant justice: ‘there may be much difference of opinion...
as to the validity of the underlying physical meaning of the assumptions made by Bohr', bespoke his lack of conviction at the time.

When his vision was darkened with doubt, the generally transparent clarity of Rutherford’s prose tended to desert him. Here was a case in point: surely it is a troubled and tortuous phrase, ‘the validity of the underlying physical meaning of the assumptions’. Such phrases, however, are rare enough in his writings, and they do not disclose the true Rutherford. Possibly, too, I have over-emphasized the doubts of 1911 to 1914: whatever may be said by the most cantankerous critic, Rutherford alone seized on the implications of the large-angle scatterings, and discovered the nucleus—and, equally alone, Niels Bohr built around that nucleus an atom model which for a long season served to bring order out of chaos in the physicist’s world, whether or not it was possible at the time to appreciate the underlying physical meaning of the assumptions on which the model was built.

It is to Bohr’s great credit, also, that he saw more clearly than others what were the implications of the empirical displacement law of radiochemistry: the phenomenon of radioactivity in all its aspects was essentially a nuclear phenomenon. Once Rutherford had accepted the cogency of this conclusion, he can have had no more doubts. For nearly twenty years he had devoted the major part of his energies to the problems of radioactivity. Almost incidentally, in the process, he had discovered the nucleus. Now it was clear that the property of the spontaneous emission of particles was a property of the nucleus itself. It was indeed clear that his latest discovery had opened up a new realm of nature, and he accepted the challenge of its exploration without further demur.

I chose to set three names at the head of my discourse today—the names of Rutherford, Faraday and Newton. I have yet to refer to the second of them, though, in passing, I have mentioned Maxwell, Faraday’s champion and expositor (if expositor he needed!). If my choice had been restricted to one name to set beside that of Rutherford, then surely I must have chosen Faraday’s. ‘The greatest experimental physicist since Faraday’—that is the common assessment of Rutherford, and the juxtaposition is singularly appropriate. Look at the early notebooks of the first Cambridge period when he was studying the properties of gaseous ionization, determining coefficients of mobility and recombination, or look at the notebooks of 1917 and 1918 (see plate 8), when alone with Kay in Manchester he was convincing himself of the reality of the ‘anomalous effect in nitrogen’, and you will get some idea of the intensity of Rutherford’s attack on his chosen target. Day by day, even many times a day, something is changed in the conditions of experiment; a new slant is given to the question being asked of nature, or a new question is asked. This is precisely Faraday’s method in a later
setting. Re-read his *Experimental Researches* or his *Diary* and remind yourselves just how intense Faraday's questioning was in his own time. Granted the development of the art of the experimenter in the last seventy years of the nineteenth century, surely it is common ground with all of us that in the first decade of the twentieth Rutherford was Faraday's direct inheritor. However, in setting Faraday's name in the title of my discourse this afternoon, it was not my intention merely to remind you of this obvious and general affinity: there were two matters in particular to which I wished to draw your attention. The first relates to the use of the word 'nucleus'; the second concerns the elementary unit of charge.

Rutherford, as perhaps you know, did not use the word 'nucleus' in his 1911 paper 'On the scattering of α and β particles . . . and the structure of the atom'. It appeared first in his writings, as far as I can discover, on page 184 of *Radioactive Substances and their Radiations*—and even there only as a convenient synonym of 'central charge'. Faraday, however, had used the word, in the context of speculations on the structure of the atom in 1844. On the basis of a comparison of the densities of the alkali metals, sodium and potassium, with the densities of their compounds with elements of smaller atomic weight than themselves, Faraday had identified large regions of empty space in the atoms of the two metals (the regions occupied by the valence electrons according to the Bohr model) and had been led to speculate that the atom model of Boscovich had much to commend it. 'If we must assume at all . . . then the safest course appears to be to assume as little as possible', he wrote, 'and in that respect the atoms of Boscovich appear to me to have a great advantage'. Referring to the ordinarily held view that an atom consists of a material nucleus having 'a system of powers in and around it', he concluded 'To my mind, therefore, the . . . nucleus vanishes, and the substance consists of the powers'.

You must not accuse me of attempting to assign to Faraday any vestige of credit for the nuclear hypothesis of Rutherford, but he did indeed use the word 'nucleus' in the way that I have described—and, much more significantly, he did identify regions of empty space within atoms on the basis of simple facts set one against another with the sure insight of genius—seized of their importance in much the same way as Rutherford was seized of the importance of the slight blurring of the edges of the image of the slit which had been covered by a thin sheet of mica in his experiments on the magnetic deflexion of α-particles in Montreal in 1906. With utter directness he concluded at that time '[to produce the observed change of direction in a distance equal to the thickness of the mica] would require . . . an average transverse electric field of about 100 million volts per cm. . . . Such a result brings out clearly the fact that the atoms of matter
must be the seat of very intense electrical forces'. In those sentences of 1906, rather than in anything that Faraday wrote, is to be found the germ of the nuclear hypothesis of 1911.

The second matter that I wished to draw to your attention in coupling together the names of Rutherford and Faraday relates to the magnitude of the elementary unit of charge. Faraday, you may remember, wrote a very considerable dissertation, in December 1833, entitled 'On the absolute quantity of electricity associated with the particles or atoms of matter'. Seventy-five years later Rutherford wrote an account of experiments carried out in collaboration with Hans Geiger on 'The charge and nature of the α-particle'. The manuscript of the paper containing that account, together with some of Geiger's experimental notes, is among our exhibits today. There is no denying the underlying unity of interest in these two investigations—Faraday's almost entirely speculative, Rutherford's largely grounded in quantitative observation. What I wish to emphasize is the novelty of the conclusions in each case. Faraday wrote 'What an enormous quantity of electricity, therefore, is required for the decomposition of a single grain of water: I have endeavoured to make a comparison, but the proportion is so high that I am almost afraid to mention it. It would appear that 8,000 such charges of the Leyden battery as I have referred to above would be necessary to supply electricity sufficient to decompose a single grain of water—or, if I am right, to equal the quantity of electricity which is associated with the elements of that grain of water, endowing them with their mutual chemical affinity'. Rutherford, uninhibited by the inveterate modesty of his predecessor, summed up the results of his investigation more directly: 'the positive charge $E$ carried by an α-particle from radium C is $9.3 \times 10^{-10}$ E.S. units', and he later added, with equal conviction, 'the value of $e$, the charge on a hydrogen atom, becomes $4.65 \times 10^{-10}$. It is the novelty of this latter assertion which I wish to set in the same context as Faraday's estimate of the charge, in Leyden jar units, associated with one Troy grain of water, holding the atoms of hydrogen and oxygen in chemical combination in the molecule.

Rutherford's value for the fundamental unit of electric charge was novel in that it was some 50% greater than the then accepted value, based on the experiments of Townsend, Thomson and H. A. Wilson. Their determinations had centred around $3.1 \times 10^{-10}$ e.s.u. When Rutherford and Geiger obtained $9.3 \times 10^{-10}$ e.s.u. for the charge on the α-particle, there could have been nothing easier than to have concluded that the α-particle in fact carries three units of positive charge. Rutherford, however, rejected this facile conclusion. To have accepted it would have been to identify the α-particle with an entity of mass
number 6. For many years he had known in his bones that dead $\alpha$-particles were helium atoms, of mass number 4, though he had not, at that time, proved it beyond dispute. The $\alpha$-particle just could not carry three fundamental units of charge, whatever the numbers might suggest!

At some length Rutherford sought to justify his opinion by appeal to other quantitative results in radioactivity, but at best there were four stages in his argument and limits of error were uncertain, though he did not deign to quantify them. Indeed, it was an argument that could have appealed only to the converted. However, Rutherford himself was of that number, and that was all that mattered to him at the time. His intuitive conclusion was that the mass number of the $\alpha$-particle was 4; from the value of the specific charge it was then clear that the charge number was 2, ergo the magnitude of the fundamental unit of charge must be $4.65 \times 10^{-10}$ e.s.u., as he asserted it to be.

We all know that he was, indeed, right, but it is interesting to observe the source to which he appears to have looked for support, and for the saving of his conscience. In the paper as published there is a footnote which does not occur in the manuscript copy on display today. It reads 'It is of interest to note that Planck deduced a value of $e = 4.69 \times 10^{-10}$ E.S. unit from a general optical theory of the natural temperature radiation'. Rutherford, the master experimenter, appears in this footnote to be appealing for support to an arch theorist, the originator of a theoretical viewpoint which at the time in question was still not fully accepted by theorists generally. Advisedly, I say it appears as if Rutherford was appealing for support from a speculative theorist, for such an interpretation is inherently suspect: to accept it uncritically would be to ignore altogether the character of the man. I take the view, rather, that the footnote was inserted at Geiger's instigation. On the page of Geiger's notes headed 'Measurements on June 11th 1908'—the page which is open for inspection in today's exhibition, pencilled, in his own hand, presumably as a reminder against the time when the experiment should be written up for publication, we find the single word 'Planck' and the two words 'probable error'. If, at the end of the day, Geiger did not prevail upon his professor to say much on the subject of probable error in the final publication, at least he seems to have been successful in persuading him to refer briefly to Planck in a footnote. After all, the value for the elementary charge deduced from the established values of the universal gas constant and the faraday, with the help of Boltzmann's constant, obtained on the basis of Planck's formulae for the radiation constants of Stefan and Wien, was in excellent accord, as the footnote testifies, with the new value which Rutherford was proposing as a result of a direct determination. Rutherford may not have been anxiously looking for support for his new value from anyone, or
feeling any acute need to salvage his own conscience, but even he was not prepared to look a gift horse in the mouth, when Geiger presented it to him.

And so I have come full circle, and I am left only with my title, 'Rutherford—Faraday—Newton'—and with some obligation to knit together my scattered observations and give them coherence. On one level, assuredly, there can be no comparison of the achievements of these three. It is not the least of Rutherford’s legacies to the future advancement of science that in Montreal, in Manchester and in Cambridge, successively, he gathered around him a team of young men dedicated to the pursuit of new knowledge, and that he provided them prodigally with profitable subjects for research. So was the modern era of coordinated attack on a restricted field of physics inaugurated. It was immensely productive: possibly never again, over a period of thirty years will one man be in a position to lead the world, in this way, in any field of scientific endeavour of equal significance. However, if you will consider for a moment, I think you will agree that in my discourse today I have hardly referred to this aspect of Rutherford’s achievement: I have hardly mentioned any of his ‘boys’. I have pictured him alone in his study struggling to give acceptable mathematical form to his thoughts on a-particle scattering, or alone in his laboratory gaining such familiarity with the properties of ionized gases that he came to think of the ions as ‘jolly little beggars’ that he almost fancied he could see, or examining a minute piece of photographic plate with a dark streak across it slightly more fuzzy at one end than the other and being seized with the idea of electrostatic fields within the atom of enormous magnitude, or, finally, standing almost alone against the scientific world in asserting, on the basis of his own conviction, that all previous determinations of a fundamental constant had been seriously in error.

Truly, genius is an attribute of the quality of a man’s achievement in the times of his solitariness. In these terms my comparison of Rutherford and Faraday and Newton is surely a valid comparison. Faraday the lone investigator, and Newton, engaged in fashioning a new language and finding no one able to converse with him in it, these men are in every way fit companions for Rutherford in his moments of solitary inspiration. In such moments Rutherford did not need to ‘stand on the shoulders of giants’ to see farther than the rest of us—he never said that he did—neither did Newton, in spite of what his mock-modesty compelled him to write. Rutherford, Faraday, Newton, each in his own way unique, each in some way unsurpassed in achievement, each secure among the company of the giants in our Society’s history!