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Chapter 4

Who Got Moseley's Prize?

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Henry Gwyn Jeffreys Moseley (1887-1915) made prompt and very skilled use of the then new technique of X-ray scattering by crystals (Bragg scattering) to solve several problems about the periodic table and atoms. He was nominated for both the chemistry and physics Nobel Prizes by Svante Arrhenius in 1915, but was dead at Gallipoli before the committees finished their deliberations. Instead, the 1917 physics prize (announced in 1918 and presented on 6 June 1920) went to Charles Glover Barkla (1877-1944) "for discovery of the Röntgen radiation of the elements." This, and his discovery of X-ray polarization, were done with earlier techniques that he never gave up. Moseley's contemporaries and later historians of science have written that he would have gone on to other major achievements and a Nobel Prize if he had lived. In contrast, after about 1916, Barkla moved well outside the scientific mainstream, clinging to upgrades of his older methods, denying the significance of the Bohr atom and quantization, and continuing to report evidence for what he called the J phenomenon. This chapter addresses the lives and scientific endeavors of Moseley and Barkla, something about the context in which they worked and their connections with other scientists, contemporary, earlier, and later.

Introduction

Henry Moseley's (Figure 1) academic credentials consisted of a 1910 Oxford BA with first-class honors in Mathematical Moderations and a second in Natural Sciences (physics) and the MA that followed more or less automatically a few years later. His real education, however, occurred in Rutherford's lab at Manchester, and he clearly thought of himself as a physicist. But the impact of his 1913-14 papers on chemistry was profound.

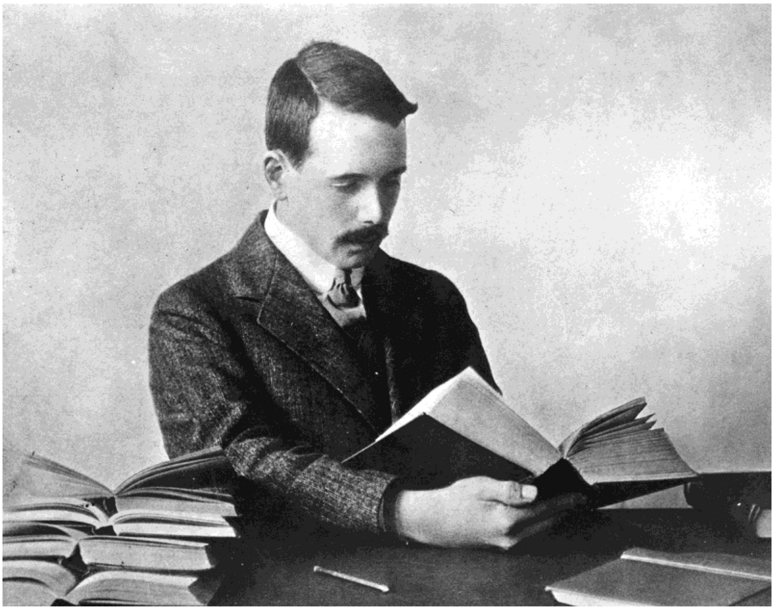

Figure 1. Henry G. J. Moseley. (National Bureau of Standards Archives, courtesy AIP Emilio Segre Visual Archives, W. F. Meggers Collection) (Reproduced with permission from reference (1).)

To quote from the nearly-contemporary *History of Chemistry* by F. J. Moore (*2*):

> *Three conclusions stand out prominently, and are worth emphasizing: first, the order of the existing elements is the same as that already adopted on the basis of chemical analogy, even where this contradicts the strict order of the atomic weights, as in the case of argon and potassium [also cobalt and nickel]; second, the elements of the rare earth group all find separate places upon the curve, and are therefore entitled to similar recognition in the table, and cannot all be grouped in one place as has been done by some theorists; third, the fact that the elements in this arrangement are equidistantly spaced shows more clearly than has hitherto been possible, exactly the number of new elements whose discovery may be expected and their character. As a matter of fact there are now but three vacant spaces . . . [between Al = 13 and Au = 79]*

As a matter of fact there were four, but Moseley had counted thulium twice, shoving ytterbium and lutetium forward. He later corrected these mistakes (*3*, *4*), so that the gaps appeared at 43 and 61 (Tc and Pm, both radioactive) and 72 and 75 (Hf and Re), both quite difficult to separate from their neighbors to provide a pure enough sample for Moseley to measure.

In any case, chemistry indeed, and surely Nobel-worthy chemistry, as very many of Moseley's contemporaries and successors have said and written. Moore's last sentence on the work of Moseley reads (*5*):

> *the atomic number of an element is a more fundamental index of its quality than its atomic weight. This value must depend upon something closely allied with mass, but not identical with it, and we must now restate the periodic law in the terms: The properties of the elements are periodic functions of their atomic numbers.*

But Moseley was shot and died at Gallipoli on 10 August 1915, along with about 44,000 other British, Australian, and New Zealand troops, and some 84,000 from the Ottoman empire (*6*).

What prizes might Moseley have won, besides the 1919 posthumous Carlo Matteucci Award? Table 1 displays the Nobel Physics and Chemistry awards 1913 to 1924. It is instantly clear that the division between chemistry and physics might well have put radioactivity, atoms, and nuclei on the chemistry side (*7*, *8*), and Moseley was in fact nominated for both in 1915 by Svante Arrhenius (Chemistry Nobel 1903) (*9*, *10*), though the committees had not completed their work prior to his death. Some years one or the other prize was not awarded, but the obvious answer to "who got Moseley's Prize?" seems to be Charles Glover Barkla for 1917, though the actual presentation in Stockholm did not take place until June, 1920. The rest of this chapter focuses on a comparison of their lives, their work, and their legacies, with an appendix exploring more complex connections among them, their predecessors, contemporaries, and successors.

I was prepared to dislike Barkla for "taking" Moseley's Prize when I started looking into the lives of these two physicsts, but Barkla fell more and more out of the scientific mainstream from 1916 onward; all three of his sons served during the Second World War, though Barkla himself had avoided war work in the First; and the youngest of those sons, Michael, a promising surgeon, died in North Africa in 1943.

The most important sources on Moseley are the biographies by Jaffe and especially the later and more complete one by Heilbron (*13*, *14*). Additional Moseley minutiae have been gleaned from a number of obituaries, reminiscences, and celebrations (*15–32*). Barkla has been chased through a comparable number of short biographies, obituaries, and descriptions of his work (several of the form "what Barkla did wrong") (*33–45*).

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Table 1. Nobel Prizes in Chemistry and Physics, 1913-1923

YEAR	*CHEMISTRY (11)*	*PHYSICS (12)*
1913	Alfred Werner, in recognition of his work on the linking of atoms in molecules by which he has thrown new light on earlier investigations and opened up new fields of research especially in organic chemistry	Heike Kammerlingh Onnes, for his investigations of the properties of matter at low temperatures which led, interalia, to the production of liquid helium
1914	Theodore W. Richards, in recognition of his accurate determinations of the atomic weight of a large number of chemical elements	Max von Laue, for his discovery of the diffraction of Röntgen rays by crystals
1915	Richard M. Willstätter, for his researches on plant pigments, especially chlorophyll	William Henry Bragg, William Lawrence Bragg, for their services in the analysis of crystals by means of X-rays
1916		
1917		Charles Glover Barkla, for his discovery of the characteristic X-rays of the elements
1918	Fritz Haber, for the synthesis of ammonia from its elements	Max Planck, for his discovery of energy quanta
1919		Johannes Stark, for his discovery of the Doppler effect in canal rays and the splitting of spectral lines in electric fields
1920	Walther H. Nernst, in recognition of his work in thermochemistry	Charles-Edouard Guillaume, for the service he has rendered to precision measurements in physics by his discovery of anomalies in nickel steel alloys
1921	Frederick Soddy, for his contributions to our knowledge of the chemistry of radioactive substances and his investigations into the origin and nature of isotopes	Albert Einstein, for his services to theoretical Physics and especially for his discovery of the law of the photoelectric effect
1922	Francis W. Aston, for his discovery by means of his mass spectrometer of isotopes in a large number of radioactive elements and for enunciation of the whole number rule	Niels Bohr, for investigation of the structure of atoms and of the radiation emanating from them

Continued on next page.

Table 1. (Continued). Nobel Prizes in Chemistry and Physics, 1913-1923

YEAR	*CHEMISTRY (11)*	*PHYSICS (12)*
1923	Fritz Pregl, for his invention of the method of microanalysis of organic substances	Robert Andrews Millikan, for his work on the elementary charge of electricity and on the photoelectric effect
1924		Karl Manne Georg Siegbahn, for his discoveries and research in the field of x-ray spectroscopy

Something About the Lives of Moseley and Barkla

Tables 2 and 3 have most of the bare facts, those about Moseley largely from references (*13*, *14*), those about Barkla from references (*33–45*). Complementary, and occasionally contradictory, Moseley items appears in references (*15–32*). The Heilbron biography of Moseley includes all the letters of his (either direction) that have been found (*14*), from 1897 to 1915, the large majority of them to his mother or his sister, Margery (born 1883). The eldest child, Betty, died when Henry was about 12. The oddest thing about these letters, in my eyes, is that, while many include "loving" or "love" in the closing line, they are nearly all signed "H. Moseley," or just "H. M." I am very sure I never signed a letter to my mother that way. Did you? Was it the custom of the time and place? (Several colleagues have mentioned to me their recollection of Einstein letters to his sister signed "A. Einstein," and, in hunting down a different topic, I found the complete scientific papers of John James Waterston (*46*), who anticipated Kelvin and Helmholtz in attributing the heat inside stars to gravitational contraction, and sometimes signed off "Your affectionate brother, J. J. Waterston;" and the artist Camille Pisarro signed many of his letters to his wife C. Pisarro.) There are no letters to Henry senior, who died when H. G. J. was two weeks short of his 4th birthday, in 1891. His mother Amabel remarried, in December 1914, William Johnson Sollas, a widowed professor of geology at Oxford.

Both men came from families in the social stratum where it was customary to send one's sons away to boarding school. That the Moseleys outranked the Barklas (at least on the academic ladder) is clear in that Eton rates higher than the Liverpool High School for Boys, and in that both father Henry Nottidge Moseley and maternal grandfather John Gwyn Jeffreys received multiple obituaries in scientific journals (*49*), while father Barkla and maternal grandfather Glover apparently did not. Both men carried their mother's maiden names as middle names, common then and even now.

Table 2. Henry G. J. Moseley and Charles G. Barkla, Personal Information

	MOSELEY	*BARKLA*
Date of Birth	27 November 1887	7 June 1877
Place of Birth	Weymouth, Dorset (south)	Widness, Lancaster (north)
Mother	Amabel Gwyn Jeffreys daughter of biologist and conchologist 1913 English woman's chess champ	Sarah Glover daughter of watchmaker
Father	Henry Nottidge Moseley Prof of anatomy, Oxford went on Challenger expedition	John Martin Barkla Secretary, Atlas Chemical Co.
Schools	Summer Field School Eton	Liverpool Institute High School for Boys
First degree	1st in Mathematical Moderations, 2nd in Physics (BA, Trinity College, Oxford, 1910)	BSc, Univ. Liverpool, Honours in Mathematics (1898); 1st in Physics (1898)
Marriage		1907 - Mary Esther Cowell, daughter of John T. Colwell, Receiver General, Isle of Man (when CGB promoted with larger salary)
Children		3 sons (2 medical, 1 physics); 1 daughter; youngest son (promising surgeon) died North Africa on duty 1943 (Flight Lieutenant Michael)
Religion	Church of England; stood godfather to a nephew	Methodist, very devout
Recreations	Rowing, other school and college sports but not cricket; hillwalking, alpinist, gardening	Singing (fine baritone); golf later in life
Death	10 August 1915, shot at Gallipoli	23 October 1944, Braidwood, Edinburgh; gradually declining health, probably accelerated by death of his son

Table 3. Post-Baccalaureate Careers of Moseley and Barkla

	MOSELEY	*BARKLA*
Further Education	Univ. of Manchester with Rutherford (1910-1913); demonstrator and assistant lecturer; John Harling fellowship (1912-1913); MA Oxford (*47*)	MSc 1901 Liverpool; Trinity and King's College Cambridge, with J. J. Thomson at Cavendish 1899-1902
First Jobs	J. S. Townsend provided lab space at Oxford (not a job)	1902-09 Univ. of Liverpool, DSc 1904 1909-1913 Wheatstone Professor, London
Subsequent Jobs	Applied for professorships at Oxford and Birmingham 1914-1915; Lieutenant, Royal Engineers, October 1914-August 1915	1913-1944, Chair of Natural Philosophy, Univ. of Edinburgh; 1931 LL.D. (h.c.) Liverpool
First Research	short half-lives of decay products (with Fajans)	speed of electrical waves in wires of various thicknesses and materials (*48*)

Barkla (Figure 2) was the elder by almost 10 years, a scientific generation, though much less in human lifetimes. Just then, it was a longer ten years than at many times before and since, both because the physics of radioactivity, atomic structure, and radiation was advancing very rapidly and because of the impact of The Great War (see next section). Virtually all sources mention that Barkla took his Methodism very seriously, regarding science as a part of the search for God the Creator. At Cambridge, he moved from Trinity College to Kings to sing with the chapel choir, which must have meant Church of England music. He preserved a fine baritone voice and a correspondingly good lecturing voice through most of his life. Moseley in his letters comes perilously close to saying that he thinks religion is a good thing for children and the lower classes (*50*).

Moseley died unmarried at 27, and, according to his mother and sister, had just begun taking notice of young women (in very short supply at both Eton and Oxford). Barkla married at 30, when a promotion increased his income sufficiently to make this realistic, by the standards of the time, when upper middle class women did not work outside the home. The War made a difference in this, too! Of Barkla's children, one is described as a musician (the daughter, presumably).

Much more is easily found about Moseley; not so much about Barkla. One thinks of trying the *Encyclopedia Britannica*, only to discover that the editions around their time (mine is the 14th Edn., 1929-30) weren't very interested in science. Each gets a paragraph (Moseley's with précis of accomplishments (*52*); Barkla just affiliations and "numerous papers about radioactivity and X-rays in many scientific journals") (*53*), but pianist Ignax Moscheles in Vol. 15 gets as much, and the Moscow Narody Bank an entry more than equal to the sum of Barkla plus Moseley. On the other hand, Quantum Theory, a very brief entry by Barkla in 1926 (*54*), has been upgraded to 13 pages in the 14th Edn. Entry (*55*), by William Wilson, Ph.D., D.Sc., F.R.S., the Carlile Professor of Physics at the University of London, who cites, of our peripheral heros, Planck, Andrade, H. S. Allen, James Jeans, Max Born, and Schroedinger.

Figure 2. Charles G. Barkla. (Photograph by Gen.Stab. Lit. Anst., courtesy of AIP Emilio Segre Visual Archives, Weber Collection, E. Scott Barr Collection) (Reproduced with permission from reference (51).)

If one wanted more information about Barkla the person, perhaps Edinburgh would be the place to start. R. J. Stephenson was there when he wrote the scientific critique referred to below.

And there might well be living Barkla grandchildren or great-grandchildren, with family records, like those Mr. A. Ludlow-Hewitt (descendent of Moseley's sister Margery) provided to Heilbron, though we are now 40 years further downstream from the people and events. Wynne (cited below) acknowledges interviews with Barkla family members and family letters etc. provided by daughter Mrs. Cecile Paterson, but he addresses almost entirely "works" rather than "life."

The records of Moseley's and Barkla's scientific works are much more comparable in completeness. There are really only about eight relevant ones by Moseley (dated 1911-14) and enormous numbers by Barkla (dated 1903-33), including many short notes to *Nature* and *Philosophical Magazine*.

And Then There Was a War

Earilier in the 20th century, a few minor Balkan Wars had flared and died, leading the British foreign secretary in spring, 1914 to say that he had never seen Europe so peaceful. He was soon proven wrong, and the literature on the causes and battles of the first world war is said to comprise some 20,000 volumes (of which I own about 0.35 %, beginning with S. McMeeki (*56*); one recommended by a reviewer is by F. Morton (*57*)). But this is not our story here, nor is the impact of the war on science, engineering and technology (my current research, enormous, and perhaps first addressed by E.B. Poulton (*58*)).

Rather what affected the Nobel Prizes was that Barkla, somewhat sympathetic to the German point of view, stayed in Edinburgh and continued work on X-ray scattering, while Moseley, who had been in Australia for the 1914 meeting of the British Association for the Advancement of Science hurried back to England immediately after his own presentation and set to work to get a commission, preferably in the Royal Engineers.

Unfortunately he succeeded, and though Rutherford and others tried to have him called back for war work in England, it was too late. He sailed out with the British Mediterranean Force in June, 1915, via Malta and Alexandria, and reached Gallipoli in August. The campaign was doomed from the start, having been called "a lunacy that could never have succeeded (*59*)," for which Winston Churchill must bear much of the blame. The most that can be said is that, by the time of the D-Day landings in World War II both technology and planning had improved.

Killed instantly by a shot on 10 August 1915, Henry Moseley was buried there, in keeping with the British rule that no bodies should be returned. British, Australian, New Zealand, and French deaths there amounted to about 40,000 and those on the Ottoman side perhaps twice as many.

My current list of physicists and astronomers whose lives were profoundly affected by the First World War (death, serious wounds, prolonged career interruption, destruction of institution . . .) runs to about three single-spaced, typewritten pages. A subset connected with the stories of radioactivity, atomic structure, and X-ray scattering in the United Kingdom (UK) includes: Rutherford's glass blower, Otto Baumbach, interned in UK; Rutherford, himself engaged in work on underwater detection of submarines; James Chadwick, interned near Berlin; Hans Geiger, served as an artillery officer; Charles G. Darwin, served in Royal Engineers, later Royal Air Force (to which Moseley had aspired at one moment); William L. Bragg, a volunteer, sent to France and tested sound ranging to locate enemy artillery (was doing this near Ypres when the letter telling him of his Nobel Prize arrived); his brother Robert, died of wounds suffered in the Dardanelles; Arthur Schuster's son, wounded in the Dardanelles; Edward Andrade, served in France as junior artillery officer for nearly 3 years; Harold Robinson and Walter Makower, (Moseley co-authors) volunteered around 1916 (when conscription was in any case introduced in England); Ernest Marsden, returned from New Zealand to serve in same time frame; Otto Hahn, called up in Germany but reassigned to work on gas warfare; Francis W. Aston, (Chemistry Nobel 1922), G. P. Thomson (son of J. J.) and Frederick Lindemann, engaged in war work at the Royal Aircraft Establishment at Farnborough; Patrick M. S.

Blackett, (who received the second Moseley studentship) served on active duty at the Battle of Jutland. And so forth. Some of these are well known, and my list has been augmented by service mentioned in Jaffee (*60*).

In a broader picture, the columns of *Nature* from 1914 August onward gradually record the colleges and classrooms of Oxford and Cambridge emptying out as large numbers of the undergraduates volunteered (about one half in many cases). German war news published in *Nature* was somewhat restricted (though not nearly so much so as you might suppose), but German college men also flocked to serve; and I was told at a recent meeting that the same was true for college students in Turkey, so that their deaths at Gallipoli could well have included potentiel future scientists as gifted as Moseley.

About the leakage of information, the best-known story is Einstein's pioneering papers on general relativity reaching Eddington in England via de Sitter in the Netherlands. Bohr in neutral Denmark performed the same function for other colleagues. Austrian scientists were frequently able to receive scientfic journals from Britain with some delay. German astronomical observations continued to appear in *Nature*, and so forth. This is all part of yet another story.

The Great War echoed down time. Fritz Haber was the one German scientist whom Rutherford never forgave (though poison gases were responsible for only about 1% of the war deaths). In 1920, the Braggs refused to go to Stockholm to pick up their Nobel medals, so Barkla was the only British physicist there. And it is now widely said that the main cause of World War II (WWII) was World War I (WWI) and the impossible demands in the peace treaties.

Impossible I can attest to, having purchased at auction an original copy pf the Versailles treaty, which concerned only Germany; others bound Austria, Turkey, and Bulgaria. Not just enormous sums of money to be handed over, but niggling restrictions, like the precise numbers of cows to be handed over to Belgium and the permissions needed to dredge out their own canals. Only about 20 previously existing international organizations and treaties were allowed to continue. The ones concerning chemistry, physics, and astronomy were not among them, again another story.

Interlude

The basic physics of X-ray production, scattering, interference, diffraction, reflection, and spectroscopy discussed below can be skipped by readers who remember their junior level physics.

Georges W.W. Sagnac (1869-1928) appears to have been the first to observe X-ray scattering as part of his thesis work (1896-1900) at the Sorbonne. He observed that secondary radiation is produced when X-rays fall on heavier metals and this radiation contains lower frequency X-rays and negatively charged rays (*61*). He was a supporter of the ether wind theory (also adopted by Barkla at times) in preference to special relativity and in preference to general relativity for gravitational redshifts.

The relevant work of the Braggs, Barkla, and Moseley all started with an evacuated glass tube held at a high potential difference between a cathode that emitted electrons and an anode (called anti-cathode) into which they smashed. The electrons stopped in the metal anode emitted mostly Bremsstrahlung radiation, with X-rays up to a maximum energy (minimum wavelength) set by the potential difference.

But some of the electrons knocked electrons out of inner orbits of the material in the cathode (what we now call the K and L shells). Those electrons (then called corpuscular radiation) could be counted. Electrons from higher orbits (most often just the very next higher one) then fell in to fill the gaps, emitting as they went one X-ray each, with a definite, discrete energy equal to the difference between the energies of the two shells involved. Neither Moseley, let alone Barkla, understood the situation in quite these terms.

In the case of a thin, evacuated glass tube, most of those X-rays got out, emerging at an angle set by the face of the solid being hit by the electrons (Figure 3). What was different is what Barkla and Moseley did with those X-rays.

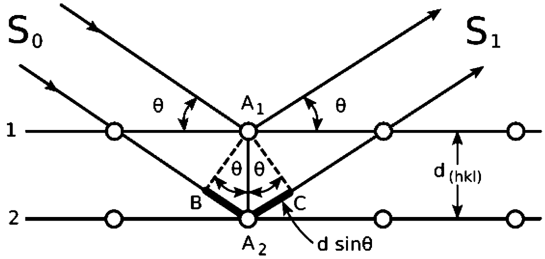

Figure 3. The reflection of X-rays by crystal planes, illustrating the derivation of the Bragg equation. (Reproduced with permission from reference (62).)

Barkla used the full cone of X-rays and measured their penetration power, generally in aluminum. This provided sufficient energy resolution for him to recognize the continuous ("heterogeneous" or later "heterochromic") spectrum from the Bremsstrahlung and one or more of the homogeneous (or homochromic or line) features produced by the second process, now associated with the name of Auger. But his papers date from 1925 (*63*, *64*), which was too late to matter to Moseley and too late to dig Barkla out of his J-swamp (next section). Note that Barkla's detector had initially been a gold leaf electroscope and later an ionization chamber.

Moseley, in contrast, "wasted" most of those X-rays by feeding them through a narrow slit of impenatrable material (the far left of Figure 4), to give him a narrow beam which he then passed into what we now call a Bragg spectrometer (Figure 4), where that beam was "Bragg scattered, or reflected, or diffracted, or interfered" off a crystal (like that of Figure 3) that could be rotated to sweep a narrow, nearly monochromatic X-ray beam across a detector. Moseley's first detectors were ionization chambers (far right in Figure 4), like the ones Barkla eventually used, but Moseley soon adopted photographic plates, moving the plate as the beam from the spectrometer swept, to provide a permanent record on which wavelengths could be measured accurately at leisure.

By choosing NaCl (whose atomic spacings were known) for the crystal in the spectrometer, Moseley was able to measure calibrated wavelengths for his X-rays, as indicated by the geometry of Figure 3, though at the price of needing quite long exposures to record anything. In a perfect system, one would get exactly one electron out for each of those X-rays (or rather the other way around, if your image of the process is one where things happen in a classical order).

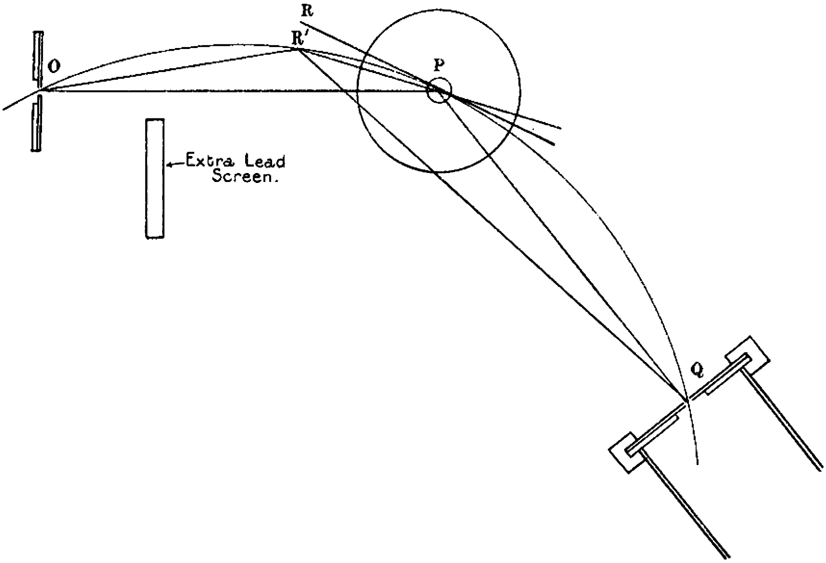

Figure 4. Diagram of spectrometer; O, bulb slit; P, axis of instrument; PR, PR', two positions of crystal face; Q, slit of ionization chamber. (Reproduced with permission from reference (65). Copyright 1913 The Royal Society.)

The end product in both cases was a spectrum like that shown in Figure 5, but with much sharper features of better defined wavelengths or frequencies or energies for Moseley. Features called α are electrons falling in from the next orbit up, ß from two up, and so forth. K and L are the orbits they fall into.

A countably infinite number of elementary/intermediate physics texts explain all these matters similarly, some perhaps more clearly and some less so than my version.

Two other sorts of scattering will enter in before we are through with the next two sections on just what each did. The first has a cross section $\sigma_t = 6.65$ X 10^{-25} cm^2 per electron, given the name Thomson scattering. It is a purely classical, electromagnetic (EM) wave event. An EM wave comes in, wiggles an electron, and the electron sends the wave back in some random direction, its energy unchanged. This is a good approximation provided that the X-ray wave has an energy very small compared to the electron rest mass; Barkla thought the word “scattering” should be applied only to unchanged energy events;and he was, in any case, intellectually very attached to Thomson, saying once that his papers were written with Thomson as his primary intended audience.

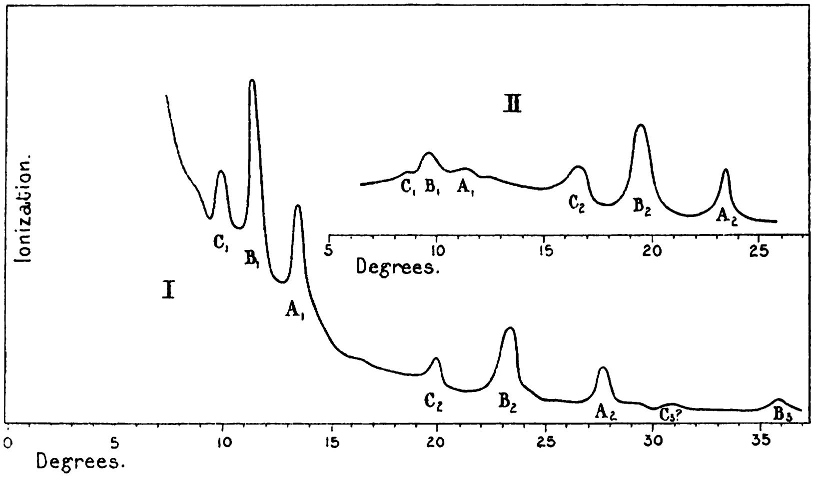

Figure 5. The first X-ray spectrum. (Reproduced with permission from reference (65). Copyright 1913 The Royal Society.)

The second is called Compton scattering, for Arthur Holly Compton (*66–68*), though it was first observed by J. A. Gray (*69*). In this case, the X-rays come out with less energy than they brought, but the loss does not depend on the substance they are hitting. Instead, said Compton, a photon ("quantum") and an electron undergo an elastic collision, conserving both energy and momentum, so that the energy of the outgoing X-ray depends on what went in and on the scattering angle θ, λ = the initial wavelength, λ' = the wavelength after scattering, m_e = the rest mass of the electron, thus (in wavelength units),

$$\lambda' - \lambda = \frac{h}{m_e c}(1 - \cos\theta) \qquad \text{(Eqn. 1)}$$

Astronomers, incidentally, are more used to the inverse Compton effect, in which a soft photon gains energy from an energetic electron. Barkla was eventually forced to accept that something stole energy from X-rays besides his "fluorescent" process, but he refused the quantum mechanical explanation (*65*, *70*).

Rayleigh and Raman scattering also exist (often for molecules), but we will leave them for after tea.

Moseley's Scientific Career and Accomplishments

It is fairly easy to summarize what Moseley did, both because of the excellent biographies and because he published only 10 real papers (from Moseley and Fajans' paper in 1911 on "Radio-active Products of Short Life (*71*)," to Moseley and Robinson's paper in 1914 on "The Number of Ions Produced by the Beta and Gamma Radiations from Radium" (*72*)).

All references to the *Philosophical Magazine* are Sixth Series, unless otherwise indicated. And the work for which Moseley is still (somewhat) honored and remembered is contained in just two sole-author papers "The High Frequency

Spectra of the Elements" and "The High Frequency Spectra of the Elements, Part II," (*73*, *74*), reporting work done almost entirely alone (letters from E. Rutherford and John S. Townsend) (*75*).

I read them both with some care. They are clear and concise, and, apart from a few changes in technical terminology, could have been written by an intelligent, literate Manchester/Oxford man of 2013-14.

He took up the technique of Bragg scattering, (truly the idea of the younger Bragg, as has not always been clear) (*76*), added a second slit so that a very narrow, monochromatic beam of X-rays came out, and recorded intensity as a function of angle (hence wavelength of the X-rays) on photographie plates.

Moseley did this for samples (many only a few milligrams) of all the elements from Al to Au (atomic numbers 13 to 79) that he could get hold of. Some were necessarily alloys or mixtures, and he explains why. A sample supposedly of Celtium or Keltium turned out to be a mixture of rare earths, for the excellent reason that C/Keltium does not exist (*77*). He lined up many samples that could be moved sequentially to become his cathode and X-ray emitters, without having to take the whole apparatus apart each time he wanted to study a different one. They were carried along on a sort of aluminum train (and the drawings of the apparatus in his two key papers are not as "crystal clear" as the text).

There were a few stumbles along the way, miscounting the atomic numbers at one point, thinking that Tm was two elements, and so putting the gaps in slightly the wrong places in his much-improved periodic table. These are mentioned in letters to Rutherford and to von Hevesy (*78*).

The atomic numbers he actually examined were 13, 14, 17, 19, 20, 22, 23, 24, 25, 26, 27, 28, 29. 30, 39, 40, 41, 42, 44, 45, 46, 47, 50, 51, 52, 58, 60, 62, 63, 66, 68, 73, 74, 76, 77, 78, 79, other known elements not providing suitable solid samples. (Remember, Moseley did use brass and resolved celtium/keltium into other rare earths, so samples didn't need to be pure but they did need to be solid.) The gaps with neither X-ray data nor other, known unsuitable elements were at numbers 43, 61, and 75, his N and our Z. He was sure that at least the α, β and γ lines existed for all, both K and L, though some were outside the wavelength region he could access and some were too faint to see.

Moseley's photographie plates recorded wavelengths, which he turned into frequencies (assuming speed = c) and energies, assuming Planck-like quanta. Moseley's law can be expressed in any of those units. Frequency perhaps looks simplest, shown in equation 2:

$$f^{1/2} = K\,(N - b) \qquad \text{(Eqn. 2)}$$

where K and b are constants. For the K alpha lines, b came out very nearly 1.0, and for the L alpha lines b was approximately 7.4.

We take this as a confirmation of the Bohr shell or ring model of electron distribution in atoms. If you pull out one K electron, the nucleus is still shielded by the other one. If you pull out an L electron, the nucleus is shielded by some larger number (not always 7.4, and there are more modern ways of counting inner electrons) when e.g. K-alpha arises from an L shell electron falling in to refill the

K shell. The concept came from Walther Ludwig Kossell (1888-1956) after the start of the war (*79*, *80*), too late for Moseley to incorporate the ideas in his last, *Nature*, paper; and Barkla had no use for anything based on Bohr atoms.

The key papers also say firmly that his number N is the correct position of each element in a periodic table, with H = 1, He = 2, Li = 3 . . . Zn = 30, and so forth. In addition to tidying up the rare earths and, putting Ni and Co in their right, chemical positions, this leaves no space before or between H and He for coronium, nebulium, aether, or any other super-light elements you might have wanted to postulate to explain optical spectra or other properties of light.

Moseley also declared that what he was doing would have little or no relevance for how outer electrons might behave and so would not explain optical spectra or valency. For traditional chemists of the time, including some of those who heard Rutherford's and Moseley's presentations at the summer-1914 Australian meeting of the British Association for the Advancement of Science, this was a fatal objection to taking atomic number and other aspects of the X-ray work seriously (*81*). It was from that meeting (to which he had traveled with his mother) that Moseley hurried back to England to volunteer for active duty in the blossoming war.

Over the next few years, experiments along similar lines by the Braggs, William Henry (1862-1942) and William Lawrence (1890-1971), (Physics Nobel 1915), Karl Manne Georg Siegbahn (1886-1978) (Physics Nobel, 1924, who used a "Swedish" device), Frederick Soddy (1877-1956) (Chemistry Nobel 1921, who concentrated on radioactive elements and their isotopes), and a few others, filled in many of the other elements, and clarified details of the K, L, M, alpha, beta, gamma, delta, epsilon (in order of decreasing wavelength and decreasing intensity) lines, and generally completed the program on which Moseley had declared his intention to work after 1914.

Lingering doubts arising from N (or Z) being close to one half the atomic weight but exactly equal to the number of electrons outside the nucleus yielded to Chadwick's 1932 discovery of the neutron. In some parallel universe, Moseley would have retired from his Oxford or Birmingham professorship (the Oxford one went to Lindemann in 1919) in about 1957, having lived to cheer on the discovery of elements so radioactive that you cannot make a sample large enough to scatter X-rays and must study them from their decay products, as Moseley had briefly studied radium and uranium. With the filling in to Z = 118 and the naming of 113, 115, 117, and 118, his chances of being eponymized in that way have probably dropped close to zero.

Of course, you can also imagine some other, much less happy, parallel universe, in which Moseley led a long life, but hung up at some point on which later work diverged from his (counting electrons in outer shells by quantum mechanical calculations rather than his "b" parameter?) and so served out most of the years of his distinguished professorship out of step with most of the physics and chemistry communities. I have no argument against that, except that Moseley was careful about citing other people's papers (including Barkla's 1911 paper on the spectra of fluorescent Röntgen radiation (*82*)) in his, that he had worked very productively with four co-authors (Fajans, Makower, Darwin [who appears in some of his letters], and Robinson) in his three short years of research, and that nearly everyone quoted in the biographies seems to have found him agreeable, hard working, and always ready for something new.

And obviously, in any reasonable parallel universe, Moseley won a Nobel Prize in physics or chemistry some year between 1916 and 1924, filling in one of the blanks in Table 1, or perhaps sharing, or perhaps shoving Barkla out of the golden spotlight.

Several of Moseley's papers thank Prof. Rutherford for his "kind interest," and the ones from Oxford express gratitude to Prof. Townsend for providing facilities.

Barkla's Scientific Career and Accomplishments

It is a good deal harder to summarize what Charles Glover Barkla did. There seems to be no actual biography, though many have written on aspects of his career and his own publications stretch from 1903 to 1933 (*33–45*, *83–93*) Those he thought most important appear in the *Proceedings of the Royal Society* and *Philosophical Transactions of the Royal Society (London)*, but there are also many in *Nature*, the *Proceedings of the Physics Society of London*, *Philosphical Magazine*, and *Jahrbuch der Radioaktivität und Elektronik*. I have read nearly all the ones in *Nature* (including some single-paragraph rebuttals to Bragg and Compton), and in the *Philosophical Magazine* from 1913 onward, that is, the volumes available in the University of California Irvine Ayala Science Library. According to Stephenson (*37*), there are 26 papers from the Cambridge period alone, and the total number, according to the bibliography compiled by Allen (*94*), is seventy.

For both Moseley and Barkla, articles about them are sometimes careless about what their degrees were called. Both habitually put MA after their names as authors, and having one seems to have been roughly the criterion for submitting one's own papers rather than having them submitted by the professor. (Moseley crossed that line between his two major publications; I have not seen any of Barkla's early enough to have a "submitted by" line, though they may exist.) Even in my days at Cambridge (1969-71) an MA was the standard second degree (not an MSc for scientists) and allowed one to use the library and wear a particular sort of academie gown to dinner on special occasions. In the Oxford University system, the MA is awarded seven years after matriculation without further examination, upon payment of a small amount (*95*). Oxford DSc gowns are bright red. I do not know about Liverpool, but it is somehow hard to imagine Barkla in anything except black.

The bare bones of the discoveries attributed to Barkla appear in all the obituaries and many of the texts mentioned in the legacy section. First is the demonstration that X-rays can be polarized, and so must have some at least of the wave-like properties of ordinary light (*96–102*). Second was the recognition that samples of any element zapped with high energy electrons or broad band high energy radiation (X- or gamma-rays) will emit both a broad-band continuum and one or more emission lines or bands, with the emission features unique to each element. Third, the naming of two of the sets of emission features K (which he associated with electrons close to the nucleus of a Rutherford atom) and L (further out electrons). Naming of M and N clearly followed, but the measurements came from others.

Barkla had originally called his two series of characteristic Röntgen rays A and B before Rutherford put forward a nuclear atom. Had he stuck with A and B, A being the hardest, he would not have had alphabetical space for J rays, and might well have kept out of a good deal of trouble! Rutherford actually proposed the existence of J rays (in 1913/14) to save his theory of the origin of beta and gamma rays (*103*).

Barkla described the broad band part as primary X-ray, or heterogeneous, and the narrow bands as fluorescent (by analogy with the UV/optical phenomena) or secondary or characteristic and homogeneous. He characterized X-rays through essentially his whole career by their ability to penetrate successive thin sheets of aluminum. Today we would say that the X-rays are exponentially attenuated, with absorption coefficients a function of energy or wavelength or frequency of the X-rays.

Barkla's first scientific investigation is described in many places as measurements of the speed of electrical waves in wires as a function of their size and composition (*104*). But the Astrophysics Data Service (ADS) picks up his paper trail only in 1904, "Energy of Secondary Röntgen Radiation (*105*)."

The titles of a subset of his papers over the next few years indicate the direction of his work and thinking. All but two are single author. Sadler was a fellow MA at least; most of his later papers are again single author or collaborations with students. "Polarization in Röntgen Radiation (*106*, *107*)," "Secondary Röntgen Radiation (*108*, *109*)," "Secondary Röntgen Rays and Atomic Weights (*110*)," which indeed found that there was a correlation that he was, however, unable to make quantitative. "The Nature of X-rays (*111*)," in which he wrote that they are correctly described by the "ether pulse" theory that had been put forward by George Stokes in 1852 (*93*), though by the time of his Bakerian lecture he spoke of his own "spreading wave" theory of X-rays; Barkla and C. A. Sadler, "Homogeneous Secondary Röntgen Radiation" and "Classification of Secondary X-radiators (*112*, *113*)."

There were other student co-authors during his Liverpool days, including J. Nicot, G. H. Martyn (on "Interference of Röntgen Radiation, preliminary account (*114*)," though the "secondary account" seems never to have appeared), V. Collier, and A. J. Philpot ("Ionization in Gases and Gaseous Mixtures by Röntgen and Corpuscular (electronic) Radiation") (*115*).

Philpot was a London BSc, and the paper concludes that there is no evidence that velocities of ejected electrons depend on the element from which they have been ejected and that the ionization is due to complete absorption of corpuscular radiation set free by homogeneous X-rays which have been completely absorbed. The gases range from H_2 to C_2H_5Br (ethyl bromide).

At this point, Barkla is beginning to slip into a series of firm opinions, starting with the idea that true scattering cannot change the penetration power of X-rays, presumably deriving from his deep admiration for J. J. Thomson, whose classical scattering indeed does not change X-ray energies measurably. One student co-author from 1907, A. L. Hughes, was later a professor at Washington University, St. Louis (US). Stephenson and other writers on Barkla's career do not tell us what became of the others (*37*), and this will be more worrisome for the Edinburgh years and students.

Barkla tried two of the techniques pioneered by others, but never really used them for anything. First, he did indeed see fringes using rock salt as a diffraction grating ((*116*) and earlier papers in *Philos. Mag.* and *Proc. Phys. Soc. London).* Second, he used photographie emulsions as detectors (*117*), and in this paper acknowledges a grant from the Solvay Institute, but instead of using photographic images to make precise measurements of X-ray wavelengths, as Moseley was doing the same year, the paper ("The photographic effect of X-rays and X-ray spectra") uses the K series secondary X-rays from Mn, Cu, Zn, Br, Mo, Ag, Sn, Sb, I and Ce to calibrate the X-ray equivalent of an H and D curve, how much darkening of the emulsion do you get from X-rays of a given penetration power, exposure time, and so forth. Using a silver bromide emulsion, he also recorded the bromine secondary spectrum. But still his real measure of the X-ray flux coming out in any given experiment is the rate at which it would discharge a gold leaf electroscope, that is, produce ionization in air.

Two other early oddities deserve to be noted before going on to the J phenomenon and Barkla's years at Edinburgh. First he gave a talk at the British Medical Association on physical and therapeutic action of secondary X-rays (*118*), in which he assured them that the necessary process was the conversion of Röntgen ray energy into corpuscular radiation, with the latter being absorbed by the target organ. The session chair introduced the next speaker by saying that the same kind of scientific precision could not be expected in a discussion of anaphylaxis and anaphylatic shock (and indeed, Dr. W. E. Dixon presented three theories, ferment, side-chair, and colloidal as preferred, classical, and speculative in that order, much, we would now say, like Barkla's changing views on the nature of X-rays).

Second, he engaged in a spat with the senior Bragg on precisely that topic, the nature of X-rays. Each sally falls on a single page of *Nature* **1908**, *78*. Barkla appears on pp. 7, 245, and 665, and W. H. Bragg on pp 271, 293, and 665, that last page being the only thing they share. Barkla is promoting a wave theory (called aether pulse) on the of basis of his detection of polarization and the details of the angular distribution of primary and secondary X-rays. Bragg is selling a particle theory, which he calls dual particle or neutral particle, meaning a bound pair, either of alpha + beta rays or of an electron and a corresponding positively charged particle. Upon striking matter, the pair would sometimes break apart, leaving the negative charge as a detectable electron or beta ray, while the positively charged particle "becomes ineffective (*119*)." He addresses six pieces of experimental evidence better explained by particles than by waves. "Wave-particle duality" covers the dispute for us, but the waves are not Barkla's ether-pulse waves, and the particles are not Bragg's dual particles. One could (though I do not) argue that Barkla was the less wrong of the two.

Barkla also takes on Arthur Holly Compton over the effect that bears the latter's name (*120*), and the response (*121*). In due course he allows that X-rays hitting heavy things can lose energy, but concludes that the explanation is different from Compton's explanation, which requires quanta of radiation (see

Interlude section). Indeed, Barkla never comes to terms with quantum mechanics, retaining a holistic view of wholeness of physical systems that Wynne described as somewhat akin to the German "naturphilosophie (*39*)." An earlier physicist, Johannes Ritter, under the influence of the naturphilosophie concept of polarities was led by William Herschel's discovery of infrared radiation to find evidence for ultraviolet radiation.

This brings us to the J series, later the J phenomenon. Barkla wrote early on that he had called the two series of characteristic X-rays he found K and L to leave room for others with either less or more penetrating power. Indeed M and N appeared. But, starting some time before his 1916 Bakerian lecture, he thought he had seen evidence for more penetrating homogeneous X-rays, which he called the J series in the lectures (*91*), and for the first time in a journal paper the next year (*122*). The Bakerian also noted that the number of X-ray "quanta" kicked out per electron was of order unity (0.81-1.09 in his experiments). He reported evidence for J electrons from (1) X-ray abosrption, (2) ionization of gas by X-rays, and (3) intensity of electron radiation from plates exposed to X-rays, all three methods yielding the same wavelength, shorter than K X-rays. The J electrons would have to be inside the nucleus, could not be accomodated in a Bohr atom, and so he rejected the Bohr model. His views had, if anything, hardened by the time he gave his Nobel lecture in 1920, which, he said, would focus on two items: the absence of evidence for quantization except in the single case of an atom with one electron pulled out and falling back and the J features. At that point he was also saying that, while the K features are clearly homogeneous lines, the L's seemed to be heterogeneous. No spectral feature contains literally only a single frequency or wavelength. L features in general are broader than K's, because there is natural (finite lifetime) broadening of both upper and lower states and typically more l and m values that contribute, but neither are truly sharp. Yes, of course the details are more complicated than this, but what is not? Even hydrogen Balmer lines have L and M components that, in a strong magnetic field, like that of some white dwarfs, go wandering in wavelength beyond recognition.

Barkla was not, at least initially, alone in his rejection of the Bohr atom (*123*). Otto Stern had a particularly negative reaction (*124*). But (of course) the periodic table/law/system itself did not receive universal acceptance or accolades (*125*).

From 1921 on, he writes of the J phenomenon (or effect) rather than J lines (*41*), and his view of them gradually evolved from an absorption phenomenon (1920) to the condition of the exciting X-ray tube (1925) to the state of the absorbing and scattering substances (1930).

According to Stephenson (*37*), Barkla had 14 Ph.D. students at Edinburgh who worked on some aspect of the J phenomenon. Theses before 1935 included seven where the student said yes, he had seen the phenomenon, and one that said no. Those after 1935 include one "yes" and five "nos." Apparently even his own students were coming to doubt what Barkla regarded as among his most important discoveries. An interesting case was R. T. Dunbar who reported a qualified yes while Barkla's student (*126*), but then moved on to Cardiff, built new apparatus, and reported no J effects (*127*). Other students whose names appeared as co-authors included R. R. C. Sale (*128*), A. E. M. M. Dallas (*129*), G. I. Mackenzie (*130*, *131*), W. H. Watson (*132*), and S. R. Khastgir (*133–137*).

His last papers were in *Nature* (*138*), disagreeing with Backhurst from the National Physical Lab, Teddington, and others who said they had found no evidence for any J phenomena (*139*), and in *Philos. Mag.* Barkla and J. S. Key on "Determination of the J-discontinuity by a condition of Matter (J-phenomenon-Part X) (*140*)." This promises "further communication will shortly be made," which never happened, although Barkla continued to have X-ray experiments performed by his technician Stevens until very late in life.

This last J-paper addresses two topics, laws governing the J-phenomenon when it occurs and investigation of conditions favorable to the occurrence. Figure 6 shows what, by this time, was meant by J: a discontinutity in the ratio of fluxes of primary to secondary X-rays as a function of their penetration power, as determined by the thickness of aluminum through which they had passed, in the range 0.01 to 0.1 cm.

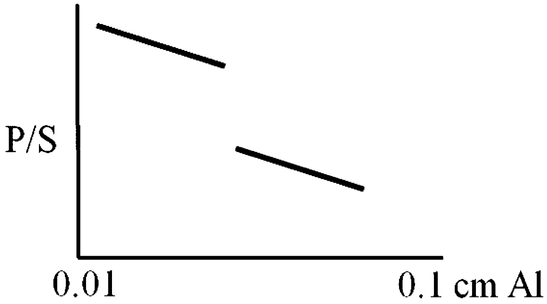

Figure 6. Primary/Secondary (P/S) X-rays as a function of their penetration power as determined by the thickness of aluminum through which they passed.

The apparatus (in Figure 7) is similarly simple. The oblique line is the scatterer, so that primary X-rays are the ones that go through and secondaries the ones that get kicked down. It looks a bit like a simplified diagram of the 2016 Laser Interferometer Gravitational Wave Observatory (LIGO) or the Michelson-Morley experiment of 1887. And in this last paper, there were samples that recorded the discontinuity; others that didn't; and some that recorded it only part of the time, depending on the X-ray tube, substances used to scatter, and perhaps other conditions. The phenomenon had become "fussy."

Long before these last papers, Barkla had, in effect, been drummed out of the mainstream of British physics. No one ever traveled up to Edinburgh to look at or participate in his experiments. He won no more prizes, though he was apparently asked quite often to be the examiner on theses from other universities, and was said to be very good at it, and one would really like to know what became of most of those 14 Ph.D. students, several of whom were women, apart from Pal, who returned to India, and Dunbar who went on to Cardiff.

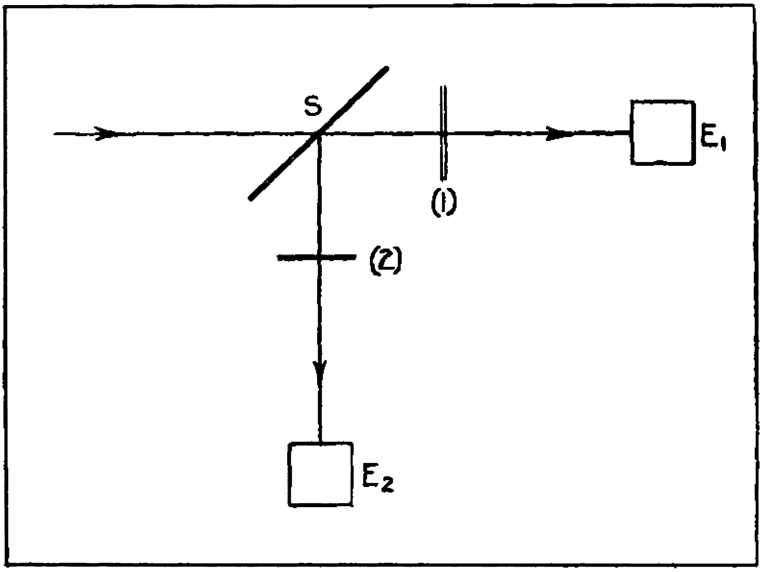

Figure 7. Experimental Apparatus. A beam of primary X-radiation falls upon the scattering substance (S); lead screen (1, with small aperature) allows some primary radiation to fall upon electroscope (E1); scattered radiation goes through lead screen (2, with larger aperature than 1) and falls upon electroscope E2. (Reproduced with permission from reference (140). Copyright 1933 Taylor and Francis.)

Reputation, Honors, Recognition, and Legacy

Both Moseley and Barkla have lunar craters named for them. There is an asteroid Moseley, but it was named for Terence J. C. A. Moseley, a former president of the Irish Astronomical Association, clearly not a close descendent, since Henry died *sine prole* and had only sisters. In their lifetimes, Barkla, of course, received "his" Nobel and also, in 1916, the Hughes Medal and Bakerian Lectureship of the Royal Society. Moseley had been awarded several fellowships and studentships that carried both some honor and some money, but had time for no more.

In 1917, the Nobel Physics Committee reserved the Prize (meaning that the money went back into the endowment). But in 1918, they chose Charles G. Barkla for the reserved 1917 Prize (and Max Planck for the 1918 Prize), responding to the 1918 nomination by Rutherford. Both the nomination and the official report were less complete and less focused on the nominee's work than usual, and included a good deal about how Moseley's work rested on Barkla's results. The choice of Barkla may have been guided by a desire to avoid yet another German award and to placate Britain. In the event, Barkla was the only prizewinner from an "Allied" nation to attend the delayed ceremony in Sweden in June, 1920 (*92*).

Moseley's death was described by Lord Rutherford, Isaac Asimov, and others as one of the greatest human and scientific tragedies of the first war (though we cannot know what others might have done had they survived). It has been claimed that his loss almost immediately affected British policy on appropriate wartime service for scientists and other promising young people.

Barkla's obituary writers Wilson and Allen were clearly attached to him personally (*36*, *40*), but struggled to describe the latter part of his career in even remotely complimentary terms. Strangely, I think, Barkla was asked to write the paragraphs called "Quantum Theory" for the 13th, 1926 edition of the Encyclopedia Britannica (p. 268-270), in which he said (*54*):

> *Quantum mechanics . . . a new theory . . . has been formulated by Heisenberg, which, whether fruitful or not, promises to put quantum mechanics in a much more logical form. Its physical significance is not, however, apparent.*

In contrast, Bohr, in the same *Brittanica* edition describes Heisenberg's quantum mechanics (*141*):

> *which constitutes a bold departure from the classical way of describing natural phenomena. It has in particular allowed the Balmer formula to be derived.*

The Wilson obituary stresses work by von Laue, the Braggs, Moseley, Bohr, and Rutherford that was going on at the same time, leaving the slight suggestions that, if Barkla had not found X-ray polarization, the K and L series, and so forth, someone else soon would have. This is unlike the situation for, say general relativity, which, if not Einstein in 1911-1915, Berlin, would have had to wait a very long time to be discovered (*124*).

The Verdict of History

What of "the verdict of history?" The biographies and other discussions of Moseley's work invariably praise his enormous creativity in both designing and building experiments and his devoted hard work in carrying them out. Much of this was written soon after his death and needs to be evaluated accordingly, but Urbain, who had spent time with him in Manchester, without their having a spoken language in common, wrote "Vive la loi de Moseley!" before his death (*142*).

In contrast, nearly everything that has been written about Barkla from his time to ours attempts to explain what he did wrong, why, and why he received a Nobel after his work was no longer on the forefront (*37–39*, *41*, *88*, *92*, *143–145*).

Friedman suggests that the award to Barkla was some combination of a posthumous honor to Moseley and a desire to honor British physics (*92*), as the war was winding down. Barkla reciprocated by speaking highly of German physics in his acceptance address. In addition, the Royal Swedish Academy, selectors of the physics winners, included a number of conservative physicists who continued to find quantum mechanics (and relativity) unattractive.

The Future

Finally, what are we leaving for the next generation? The following text books, in roughly chronological order, came from the University of California Irvine Library and Department collection, my own shelves, and offices of colleagues (special thanks to Arnold Guerra and Edward Gerjuoy, whose books were honorably returned to them). A book with no mention of Bragg anything was deemed not relevant to the search, and the pattern is from Barkla being perceived as at least as important as Moseley to his disappearing completely.

1) Floyd K. Richtmyer, Earle H. Kennard, Thomas Lauritsen, *Introduction to Modern Physics,* 5th Edn, McGraw-Hill, 1955 (*146*). Remember that Richtmyer, who had been firm in not finding J effects, died in 1939, so this must be a late edition or reprinting. The Braggs as expected. Moseley's law as described in two important papers. And five Barkla references concerning the discovery of X-ray polarization, discovery of K characteristic secondary radiation, discovery that number of electrons is about half the atomic weight (references to others as well), the first method of producing homogeneous (monochromatic we would say) beams, naming the K and L fluorescent radiation, and discovery that the hardness of secondary beams increases with A of the secondary emitter as measured by the penetrating power in aluminum.
2) David Halliday, *Introductory Nuclear Physics*, Wiley, 1956 (*147*). Bragg, yes, as expected. Moseley the 1913 *Proc. Roy. Soc.* paper on obtaining high potentials from uranium. Barkla no.
3) Robert B. Leighton, *Principles of Modern Physics,* McGraw-Hill, 1959 (*148*). This is a text I used in graduate school. Friedrich, Knipping, Lauer 1912, X-rays diffracted so we can analyze X-ray beams into monochromatic components and deduce structure of crystals. Barkla 1906 X-rays polarizable. Bragg 1913 X-rays from tube = "white" plus bright lines characteristic rays polarizable (although this discovery was Barkla's, not Bragg's). Moseley wave number of characteristic radiation increases with A, with Z roughly half of A, unique sequence in Z. Established ordering of the elements and important confirmation of Bohr-Rutherford atom. Moseley's law.
4) Richard T. Weidner, Robert I. Sells, *Elementary Modern Physics*, Allyn & Bacon, 1968 (*149*). Bragg law, plane, reflection, W. H. and W. L. Moseley first systematic survey of characteristic X-ray frequencies, equation, atomic number, and ordering of elements, e.g., Ni/Co. No Barkla.
5) Irving Kaplan, *Nuclear Physics, 2nd Ed.* Addison-Wesley 1963 (*150*). I taught out of this book in early 1970s. Considerable details with references. Bragg diffraction and reflection of X-rays. Van den Broek as suggesting the position of an element in periodic system = nuclear charge = number of electrons "could not be considered proven until after the work of Moseley". No Barkla.

6) Mentzer Russell Wehr, James A. Richards, Thomas W. Adair, *Physics of the Atom, 3rd Ed*, Addison-Wesley, 1978 (*151*). Bragg planes, scattering, reflection, diffraction. Moseley's law and quoted "we have here a proof that there is in the atom a fundamental quantity which increases by regular steps as we pass from one element to the next. This quantity can only be the charge on the central positive nucleus." Barkla in timeline as 1908 for polarization and characteristic secondary x-rays.
7) Hans C. Ohanian, *Modern Physics,* Prentice Hall, 1987 (*152*). Bragg planes, Bragg's law, Moseley's law, plots of K and L series for atomic numbers. Barkla showed X-rays are waves with double scattering experiement using two blocks of carbon. X-rays that came away from first perpendicular to iincident direction, scattered a second time gave zero intensity perpendicular to that. Also Nobel for characteristic x-rays.
8) John R. Taylor, Christ D. Zafiratos, Michel A. Dobson, *Modern Physics for Scientists and Engineers*, 2nd Ed. Pearson Prentice Hall, 2004 (*153*). Bragg law, spectrometer. Moseley characteristic X-rays and Bohr atom. K alpha line vs. atomic numbers. No Barkla
9) Raymond A. Serway, John W. Jewett, *Physics for Scientists and Engineers,* 6th Edn, Thomas-Brooks/Cole, 2004 (*154*). Only Bragg.
10) Hugh D. Young, Roger A. Freedman, A. Lewis Ford, Francis W. Sears, *Sears and Zemansky's University Physics,* 13th Edn, Pearson Addison Wesley, 2004 (*155*). Bragg reflection, Moseley's law. No Barkla.
11) Douglas C. Giancoli, *Physics for Science and Engineering with Modern Physics* 4th Edn, Pearson Education International, 2009 (*156*). Bragg scattering, equation, peak, Moseley plot. No Barkla.

Appendix 1. Unreconstructed Physicists

We have met Kazimierz (Kasimir) Fajans (1887-1975), who from 1943 to his death in 1975, held on to his "quanticle theory" of atomic structure and Barkla's 1916-1944 attachment to his J-phenomenon. Historians of physics, or perhaps more often physicists *per se* have thought in similar terms about Albert Einstein's 30 year quest for a unified theory of gravity and electromagnetism (*157*), while Niels Bohr, in the years after his major accomplishments, did not attempt to hold on to the earliest versions of atomic structure, but strongly encouraged younger colleagues moving foward (*158*).

Barkla had a double problem, of being wedded not only to a particular, J, idea but alse to a particular form of experiment, which determined the energy of X-rays by their penetrating power through aluminium. This made it possible for Pal to conclude that there were five contributors to his erratic results (*90*), (1) Compton scattering, (2) interference in the scattered radiation, (3) absorption of various constituents of the beam, (4) narrowness of the primary beam in constrast with the width of the scattered beam (which sounds like the problem in optical spectroscopy of matching a slit width to an image size), and (5) ionization provided by the radiation received in the ionization chamber. Stephenson (*37*), who had access to Barkla's lab notebooks from 1921-22 and 1944 found therein neither any persuasive J-discontinuities nor any great repeatability of the experiments.

At some point in compiling the information for this chapter I began to be reminded of what sociologist of science Harry Collins had written about the first-ever searches for gravitational waves (*159*), carried out by Joseph Weber after the funded technology had moved on to interferometers. You cannot expect entire impartiality from me on this topic; I was married to Joe Weber for the last 28-½ years of his life. My "take" on the situation is to be found in V. Trimble (*160*). Collins has perhaps mellowed a bit over the years (*161*), pointing out that the whole field of searching for graviatational waves would have started much later (perhaps even later than 2016) without Weber's pioneering work and staunch defense of it. Science is said to be (and probably is) a self-correcting process, but it is not necessarily a kindly one. The harshest words to date on Weber came from Janna Levin (*162*, *163*). And the words here are not kinder to Barkla than ones written closer to his time.

Einstein and his attempts at unified field theory need no introduction from me. Probably less well known is the long period in which Erwin Schrödinger, mostly in Ireland by then, struggled for a "theory of everything" and failed (*157*).

An accidental discovery, while prowling for Barkla in the *Dictionary of Scientific Biography* was L. Hoddeson's entry on John Bardeen (*164*), winner of two physics Nobels, for the transistor and for the Bardeen-Cooper-Scieffer theory of superconductivity. Through his last decade or so, he put forward, and held onto, a "novel quantum mechanical theory of charge density waves," while the community moved on to some other point of view. Though new to me, this late deviation from the mainstream is well known in the condensed matter community. In a brief conversation (mostly about other topics) with the second most-senior female member of the University of California Irvine Physics Department, a condensed matter physicist 15 years my junior, I mentioned having come across this strange aberration, and she immediately knew what I meant, even though I gave the phenomenon a slightly wrong name.

One of the reviewers has suggested that the general territory of initially very successful physicists getting hung up on one of their pioneering ideas and declining to move on when the rest of the community does might be worth more extensive investigation. Informally, obviously, this has already been done, giving rise to a Max Planck quote generally bowdlerized into "Science progresses one funeral at a time (*165*)."

Appendix 2. Connections

The beginning may be a very good place to start, but there is a tangled web of who knew whom, who worked with whom, what the war did to them all, and repercussions down to the present. Let's then start with Kasimir (Kazimierz) Fajans, born in Warsaw in 1887. With a first degree from Leipzig and a 1909 Ph.D. from Heidelberg, he came to Rutherford's lab for 1910-11, where he became Moseley's first co-author (*166*). Fajans appears at his best as the author of the 1916 obituary of Moseley in *Die Naturwissenschaften* (*167*). Karl Schwarzschild, an astronomer who died of service-contracted pemphigus in 1916, received British obituaries but much less gracious ones (*168*).

Since there are a couple of good Fajans sources, let's chase him both forward and backward in time (*169*, *170*). Important influences on him were Wilhelm Ostwald at Leipzig, Phillip Lenard at Heidelberg, Richard Willstätter (1915 Chemistry Nobel) at Zurich and also lectures by Albert Einstein there; at Manchester, Chadwick, Darwin, Geiger, von Hevesy, and Moseley; and at Munich, where he founded in 1930 an Institute for Physical Chemistry with money from the Rockefeller Foundation, Röntgen, Sommerfeld, and Heisenberg, and visitors Pauli and Bethe. He had made American friends in a visit to Columbia in 1930, and this turned out to be fortunate. Like many Polish-born Jews, he left Germany in 1934, returned briefly to Cambridge, and then on to the University of Michigan (Ann Arbor) as professer in succession to the recently-retired Moses Gomberg. During WWII, he interacted with Samuel K. Allison, A. H. Compton, Fermi, Seaborg, and H. Urey. He retired in 1957 but remained active almost to his death, 18 May 1975 at Ann Arbor.

Now comes another sad part (*171*). The story has some echoes of Barkla's later career. In 1943, Fajans conceived of a theory of chemical bonding, which he called the quanticule theory. A quanticule was a precisely defined group of electrons that would interact with a nucleus collectively. It was not a quantum theory, and Fajans was suspicious of any application of quantum mechanics to chemistry, finding the mathematics impenetrable. It might, 30 years before, have been a competitive theory, but by 1943 was "a useless attempt to turn back the course of science." Fajans thought this his most important work and involved at least two students in it, Theodore Berlin and Oliver Johnson on how and why molecular fluorine differs from the other halide molecules (*172*, *173*). The University of Michigan archives preserve many letters exchanged with Linus Pauling (1954 Chemistry Nobel for his theory of the chemical bond).

Fajans passed unscathed through the first war, but Ostwald Helmuth Goehring, his co-author on the papers reporting the discovery of what is now called protoactinium (*174*, *175*), was called up in 1914 and does not appear again in the chemical literature (*176*). Fajans yielded credit for the elemental discovery to Meitner and Hahn five years later (*177*).

Out of curiosity, I chased Fajans' scientific pedigree backward for several generations. His Ph.D. at Heidelberg, on stereochemical analysis, was earned under Georg Bredig (1868-1944), who worked with Arrhenius (who appears in several other chapters in this volume), but his official advisor for his 1894 Leipzig Ph.D. was Wilhelm Ostwald (1883-1932), the 1909 Chemistry Nobelist. Keep going, and you will come to other chemists whose names you recognize, eventually reaching J. H. Schulze (1687-1744), who was the first to demonstrate that silver iodide is darkened by sun light, not sun heat. Additional work by Thomas Wedgewood (a Darwin connection), Humphrey Davy, Niecephore Niepce, and Henry Fox Talbot turned this revelation into the photographic medium (with negatives) that enabled Moseley to record his X-ray lines accurately (*178*).

Another plunge into the web of connections brings up Karl Herzfeld (1892-1978) and Friedrich Hasenöhrl (1874-1915), the former as someone working on X-ray scattering in our time frame and author of "Uber ein

Atommodel, das die Balmer'sche Wasserstoffserie aussendet (*179*).” So? Well, Herzfeld was the student of Friedrich Hasenöhrl, receiving his Ph.D. from the University of Vienna. Herzfeld was briefly an assistant to Fajans in Munich, while Linus Pauling was briefly his postdoc. Both quickly volunteered to serve for Austria. Herzberg’s Wiki entry says that Hasenhörl was a conscript (*180*), but he was 40 years old and linked military documents say that he received, as Oberlieutenant, the Order of the Iron Cross 3rd class with war decorations after he was killed by a grenade, having been previously wounded and returned to service. Fritz (as he appears in the military document linked to his Wiki) had studied with Stefan and Boltzmann in Vienna, and with Kammerligh Onnes and Lorentz in the Netherlands. He was actually Boltzmann’s successor after the latter commited suicide in 1906.

Herzfeld had written six papers before receiving his Ph.D., and six more from the front, these latter on statistical mechanics applied to both chemistry and physics. He came to the United States as professor of physics at Johns Hopkins University (JHU), where his best-known student was John A. Wheeler (Ph.D. 1933, students in turn including Kip Thorne and Charles Misner, in turn scientific great-grandfathers by now).

In about 1946, Herzfeld moved from JHU to the Catholic University of America, (CUA) in Washington D.C., where he taught a large fraction of the graduate physics courses, largely in evening sessions, for the sake of returning military personnel, financed by student veterans’ benefits. A CUA Ph.D. student named Joseph Weber took some of those classes in 1949-51, and was led thereby to look into amplification of microwave radiation by inverted populations of moecules. He is currently better known for not discovering gravitational waves and we were married from March 1972 until his death in September 2000. You met him briefly in Appendix 1. Two of Hasenöhrl’s children later lived in Rockville and Silver Spring, Maryland, close to Herzfeld’s home in D.C.

If the game “shaking hands with Shakespeare” appeals to you, before you read further, get out a pencil and paper and think how many steps it takes to connect you with Moseley or Barkla, or any of the other chemists and physicists who have appeared so far. As a postdoc in Cambridge (summer 1968 and 1969-71) I just barely met the younger Bragg and so can reach Moseley at one remove. I also met Herzfeld, shortly after Joe and I were married, which opens up the German connection to Hasenöhrl, Boltzmann, and all.

John Heilbron reports that he had some correspondence with Fajans and once spoke to Bohr, so we can all consider ourselves at one remove’ from them. I met the younger Bohr (Physics Nobel 1975). Since Fajans spent his last decades in Ann Arbor, which has and had a plentiful coterie of astronomers going back well before his death in 1975, that path is also multiply connected.

While we are at it, Gerald Holton was the last student of Percy Bridgman (Physics Nobel 1946) who is part of a purely American lineage, going back to Nathaniel Bowditch, the first American to be elected a foreign associate of the Royal Astronomical Society (*181*).

Cecil Powell sneaks in as a single sentence, both because he also worked with Wilson and Rutherford, at the Cavendish, received a Hughes Medal, was elected

F.R.S. in 1949, and is a plausible answer to the question: "Who got Marietta Blau's Prize?"

But we dive in a bit deeper once more to meet Herbert Stanley Allen (author of the F.R.S. Barkla obituary). Allen (1873-1954) was strongly in favor of quantum mechanics, (unlike Barkla) but a devout Wesleyan Methodist (like him). He was both F.R.S. and F.R.S.E., had worked under J. J. Thompson at Cambridge and alongside Barkla at the University of Edinburgh. He was later professor at St. Andrews (and died there). The report of his retirement appeared in the report of the Senatus Academia for 29 June 1944, in minutes taken and reported by D'arcy Wentworth Thompson, the first edition of whose *On Growth and Form* was written and published during the first war and the second edition during the second war.

Starting as early as 1915, Allen latched on to the idea of the ring electron or Parson magneton as a model for electrons in Bohr atoms, another idea well out of the mainstream, in the later years when he still defended it. What about Alfred Lauck (his mother's maiden name) Parson (1889-1976)? He was in the US at Harvard and the University of California Berkeley for a couple of years, 1913-15. Gilbert N. Lewis (another of our "should have beens") was then chair of the Berkeley Chemistry Department, and his 1916 model of chemical bonding, a shared pair of electrons (as in "The Atom and the Molecule") had some input from Parson. Lewis's atoms, unlike Bohr's, tended to be cubical or rectangular. As for Parson, he served in the Great War, suffering severe shell shock, and gave up accademia.

Charles Galton Darwin (1887-1962) should perhaps have come first, since he was probably the most influential of Moseley's co-authors (*182*, *183*). He was the grandson of THE Charles (Robert) Darwin, via George Howard, was born (and died) in Cambridge, and received an honors degree in the math tripos from Cambridge (Trinity College) in 1910, going on to work with Rutherford at Manchester. Two papers following his collaboration with Moseley provided "the foundation for subsequent interpretation of X-ray diffraction by crystals [and] anticipated by many years classic work by E. P. Ewald (*184–186*)." Both papers were communicated to *Philos. Mag.* by Rutherford, and between them they cite Barkla, Moseley, and Bragg, and thank Moseley and Rutherford (the latter for his interest). One of the conclusions was that "Barkla's results depend in some way on an intermediate secondary electron." He also thanks Mr. G. H. Hardy, who later discovered Ramanujan, for advice on whether a series converges and, despite the better formulae thus achieved, finds that his calculations of reflected intensity are not very satisfactory. Because C. G. Darwin outlived both Moseley and Barkla, it was he who got to pontificate on "The Discovery of the Atomic Number (*187*)."

During the war that killed Moseley, Darwin was first a censor in France, then with the Royal Engineers (at the request of W. L. Bragg), working on sound ranging for artillery, and finally with the Royal Airforce studying aircraft noise. Then after two years as a lecturer at Christ's College, Cambridge and one at the California Institute of Technology (where he interacted with Richard Chase Tolman, better known for work in general relativity), Darwin took up the Tait professorship at Edinburgh (1924-36). Several of his papers from 1927 to 1940 deal with the quantum mechanics of electrons and make use of Tolman's results. Darwin thus overlapped Barkla at Edinburgh for his full 12 years there.

Back he went to Christ's College as Master, 1936-38. and then (perhaps with some regret, seeing the advent of another war), took up the directorship of the National Physical Laboratory, as the successor to Bragg (1938-49). Meanwhile, the next Tait professor was Max Born, who had left Germany of necessity, and also of necessity had sold many of his physics books (to the University of Maryland, where I read a few of them) just before he won his belated Nobel Prize in Physics in 1954 (*188*). After his retirment, C. G. Darwin devoted most of his attention to population issues and eugenics, as a Neo-Malthusian. Perhaps it was the Galton strain coming out in him (Francis Galton (1877-1911) was his half-cross-first cousin twice removed). You can listen to his BBC radio broadcast "This I Believe (*189*)," in which he denied any mysticaI sense of religion, but worried about the population explosion.

And who was C. G. Darwin's successor at the National Physical Laboratory? Why, it was dear old Teddy (Edward Crisp) Bullard (1907-1990), the marine geologist. He had studied nuclear physics and electron scattering with Rutherford and P. M. S. Blackett at Cambridge (Ph.D. 1932), but thought career opportunities would be limited there. Bullard is the author of the profound remark that the most important thing scientists learned from World War II was the difference between a thousand dollars and a million dollars, and what you could do with each (well, anyhow I heard it from him). We now circle back to George H. Darwin. One of his important calculations showed that tidal drag is slowing the earths rotation and moving the moon away. Extrapolating backwards, he suggested that the Pacific Ocean basin was the scar left by the moon flying off (*190*). Why do we no longer consider this likely? Plate tectonics and continental drift; that is, the Pacific basin wasn't there when the moon formed many billions of years ago. Among the many items of evidence for plate tectonics is the excellent fit you get between the continents when you look at the edges of their continental shelves, rather than their images on maps, as Wegener and Helmholtz before him had done? Who did it? Why Teddy, of course (*191*)!

Acknowledgments

An unknown reviewer has pointed out that a forthcoming book on Moseley being edited by R. Egdell and R. MacLoed has information on Moseley not covered in Heilbron's biography, namely that there was an additional sister. A recent book by Eric Scerri makes a strong case for possible influences by Van den Broek on Moseley's discoveries.

I am indebted to the organizers of the ACS session for "giving" me Moseley and to the editors for including this chapter. John Heilbron and Gerald Holton generously read and provided input to the first draft; Russell Egdell did the same for the second draft. Their comments, corrections, etc. have been fully incorporated (even where they slightly disagreed). Harry Collins and Allan Franklin told me about the Friedman and Wynne references. Eric Scerri and the other two referees provided helpful comments and corrections. Editor Vera Mainz bravely undertook the challenging task of turning a typescript that cited sources by surname and year into an e-text with citations numbered. Her comments and

changes (mostly deletions) have been treated with equal seriousness to those of the reviewers, in some cases with what I hope is an appropriate compromise, where I think that there is something relevant to be said, but not necessarily in the way I first said it. She has also been instrumental in getting all the various reviewer's comments accommodated in the chapter. Vera has done more than the average editor does to get my chapter into print, and I thank her for all her efforts.

My introduction to the periodic table came in about 1950, from my father, Lyne Starling Trimble, who was a very fine chemist, though a rather poor business man.

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