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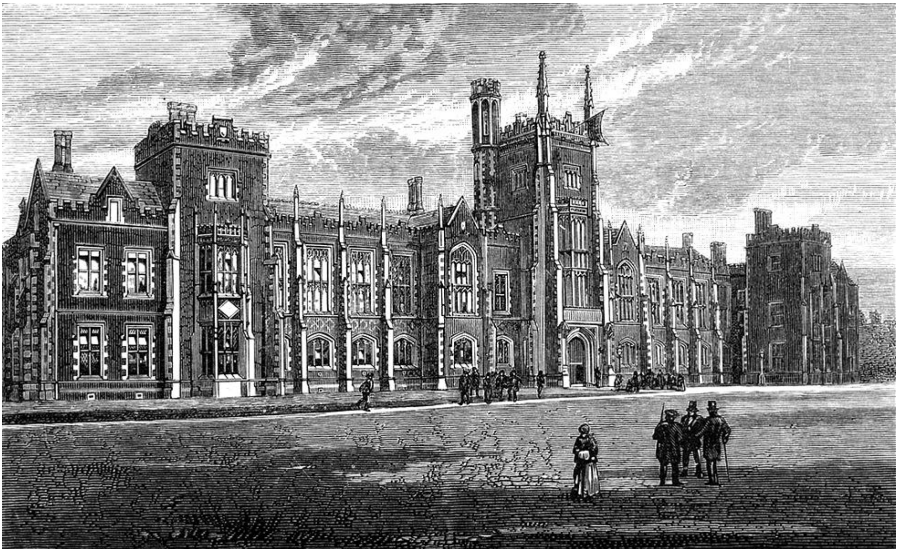
Newsletter

# Physicists and Mathematicians of Belfast



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## Foreword

The Physicists and Mathematicians of Belfast is the 4th special issue of the newsletter following others on William Stroud, Lord Rayleigh and Rutherford & Chadwick. It represents a record of the meeting of the History of Physics Group at Queens University, Belfast on 25th June 2014 which was organised by Andrew Whitaker and Mark McCartney.

I should like to thank the speakers for providing written versions of their talks which enables us to bring these presentations to those members who are unable to join us on the day.

The cover picture is of Queens University in 1888.

M J Cooper
Editor

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## Contents

## Introduction

It is fair to say that physics and mathematics in Belfast date from the foundation of the Belfast Academical Institution (later given the title Royal, so BRAI or ‘Inst’) towards the beginning of the nineteenth century. This had school and College departments, the College department being designed as a Scottish University on a small scale. Inst was liberal theologically, and it was designed to provide a useful education for boys who would have to make their own way in the world. Science and technology were stressed as well as, for example commerce.

The leading spirit among the teaching staff was James Thomson (Senior) who had risen from working as a farmhand to be Professor of Mathematics, as well as writing several best-selling textbooks. Among his children were the exceptionally able James (Junior) and William – the latter would in due course become Lord Kelvin. However in 1830 James (Senior)’s wife died and he moved to Glasgow University, where he was again the most prominent member of staff, though he had a long hard struggle against the power of privilege and nepotism.

When the Queen’s Colleges at Belfast, Cork and Galway were founded in 1845, it might have been expected that the College department at Inst would form the nucleus of Queen’s College Belfast (QCB). However it was decided that a fresh start would be made, and with the founding of the new College, Inst itself relaxed over a period of time – the Medical Faculty lasting at Inst for several years – into a school, as it is today.

An interesting suggestion at the time was that James Thomson Senior might return to Belfast as President of QCB. He planned that, if this were to happen, William, whose studies at Cambridge were ending, and who was already regarded as a mathematician of great ability, could succeed his father as Professor of Mathematics at Glasgow.

However this was not to be. While James Senior was the choice of the liberal element of Belfast society, a Presbyterian clergyman of the firebrand type was favoured by the more conservative element, and in the end a more moderate clergyman who was well-known to the government, Pooley Henry, was chosen. James was offered the Vice-Presidency at roughly half the salary of the President, and turned it down angrily.

His ambitions for William now became focussed on the vacant Chair of Natural Philosophy [Physics] at Glasgow, and this was achieved in 1846. It might be felt that this was a prime example of the nepotism James Senior had been determined to stamp out, but it must be admitted that the results were particularly successful. William was to hold the position until 1899, and certainly became the most-respected physicist in Britain, perhaps in the world, as well as having major technical achievements to his name, in particular the Atlantic cable and a great advance in compasses for naval service.

His brother, James Junior, had a fairly desperate time in the 1840s, suffering a number of injuries and then a serious illness. However towards the end of the decade he played an important and largely unrecognised, part in his brother’s seminal studies founding thermodynamics, and James himself has gained credit for the discovery of the depression of the freezing point of water under pressure.

Meanwhile James Senior’s refusal to take up the Vice-Presidency of QCB had a silver lining in the appointment of Thomas Andrews to the post. Andrews, a medical doctor, had previously been Professor of Chemistry in the Medical Faculty of Inst. His was an inspired appointment. With Henry he achieved steady if slow progress in the new College, but more importantly he was responsible for a series of scientific results of the greatest importance.

He is best-known for his establishment of the continuity of the vapour and liquid states, and his concept of critical temperature and pressure, ideas crucial in the understanding of the liquefaction of gases. He also did very important work on the constitution of ozone.

In this work he was assisted by two men who started their academic careers in Belfast before moving on to Chairs in Scotland. Peter Guthrie Tait was appointed to the Chair of Mathematics in Belfast in 1856, and, despite the title of his Chair, worked closely with Andrews in his experimental studies of ozone. Indeed his interests moved so far in the direction of experimental science that he applied for, and in 1860 gained, the Chair of Natural Philosophy at Edinburgh. In the contest for this position he actually beat James Clerk Maxwell; Tait was obviously not nearly as renowned a research worked as Maxwell, but what must have swayed the appointment panel was his famed excellence in teaching.

At Edinburgh, as well as his more scientific achievements he worked to great effect on a wide variety of theoretical topics including knot theory and quaternions. Perhaps his most lasting work was his collaboration with William Thomson on their text *Treatise on Natural Philosophy*, which revolutionised the teaching of physics, centring the conceptual development around the conservation of energy, rather than force and motion, as in the Newtonian analytical structure used until this point.

In his work on liquids, Andrews was assisted by James Thomson Junior, who had returned to Belfast in 1850, and who became Professor of Civil Engineering at QCB in 1873. Andrew's talent lay primarily in experiment, while Thomson excelled in mathematical and theoretical work, so they made an excellent team. Unfortunately Thomson left Belfast in 1873, because the Chair at Glasgow became vacant and William insisted that his brother joined him in Glasgow. If the joint work on liquid had continued in Belfast, it is possible that they might have anticipated the work of the Dutchmen Johannes van der Waals and Heike Karmerlingh Onnes, which was to win Nobel Prizes for them early in the next century.

Towards the end of the nineteenth century, the most important physicist with a strong Belfast link was undoubtedly Joseph Larmor, who was born in 1857 and went to school at Inst, before obtaining the highest honours at QCB. He then moved to Cambridge where he was Senior Wrangler (first place in the mathematics examination) in 1880. He then became Professor of Natural Philosophy at Galway. In 1884, he applied unsuccessfully for the Cavendish Chair at Cambridge, JJ Thomson being appointed, but he took up a lectureship in Cambridge which he held until 1903 when he became Lucasian Professor at the University.

He was involved in all the most important branches of physics for several decades, but his most important work was done in the 1890s as one of the 'Maxwellians'. James Clerk Maxwell's theory of electromagnetism was, of course, one of the greatest achievements in the history of physics, but his early death in 1979 meant that the theory required substantial work to make it a fully articulated research programme. Much of this work was achieved in the 1880s by the Maxwellians, Oliver Lodge, Oliver Heaviside and George Fitzgerald, while Heinrich Hertz demonstrated the existence of Hertz's electromagnetic waves.

The achievement of Larmor and also Hendrik Lorentz was to embed into Maxwell's work, which was itself centred around waves, the idea of the particle. Larmor worked in terms of the ether, but he did not think of the ether as a material medium. Rather 'electrons' moved in the ether according to Maxwell's Laws, but they were not material either but nuclei of intrinsic strain in the ether. An electron moved in the ether rather like a knot sliding along a rope. In the course of his work Larmor was the first person to write down the Lorentz transformations, though he only showed that they were correct to second order, not, as Lorentz showed later, to all orders.

In the new century, QCB gained its independence in 1908 as Queen's University Belfast, QUB, but the greatest achievement of a physicist from Belfast was not linked with the University. Ernest Walton had schooldays had been at Methodist College, just opposite the University, from where he moved to Trinity College Dublin, and thence to the Cavendish Laboratory at Cambridge, where he worked under Lord Rutherford. As is well-known, working with John Cockcroft, he built the first particle accelerator in the world, and split the lithium nucleus, an achievement for which the two scientists were awarded the Nobel Prize for Physics in 1952. Walton remains the only Irish winner of a Nobel Prize in Science. He was to move to Trinity College Dublin but lack of finance there prevented him from carrying out much research.

For much of the middle period of the twentieth century, the Physics Department at QUB was in the capable hands of Karl George Emeleus. Emeleus was a productive research worker, but if he did not quite reach the heights of his brother, Harry Julius, who became Professor of Inorganic Chemistry at Cambridge in 1945, it was certainly because he devoted so much effort to his students. He was an inspiring teacher, and much of the development of the Department from the 1960s may be put down to his earlier work.

In the years before the Second World War, it so happened that there was a remarkable flowering of talent in other Departments at QUB which would contribute greatly to physics after the war.

William McCrea was Professor of Mathematics from 1936; after the war he was to have many major achievements in astronomy and cosmology. Harrie Massie was in charge of Mathematical Physics from 1933. After the war he was to be one of the leading physicists and scientific administrators in

Britain, being most influential in the fields of atomic physics and space science. In Belfast in the 1930s, a number of excellent scientists were attracted to work with him; James Hamilton, Samuel Francis Boys, James Hamilton and David Bates were all to have careers of the greatest distinction after the war.

While these all dispersed around the outbreak of war for war-work, an important incomer was Peter Paul Ewald, who had lost his prominent position in Germany with the coming of Hitler, and, in John Bell's phrase, was 'washed up on the shores of Ireland'. He replaced Massie. Ewald had been crucially influential at the outset of X-ray crystallography, making a crucial suggestion to Max von Laue, who gained the credit for the discovery, constructing the mathematical basis of the subject, and being one of its most important practitioners for several decades. He was able to continue this work in New York after the war.

In Belfast after the war, David Bates returned to Belfast to develop a large and exceedingly flourishing research centre in theoretical atomic and atmospheric physics, which lasts to this day. Phil Burke and many other well-known scientists have been members of this group.

On the experimental side, Brian Gilbody and Dan Bradley built up important groups in atomic physics and laser physics respectively. Gilbody stayed in Belfast, but Bradley, one of the major pioneers working with lasers, moved to Imperial College London. Later he moved back to Ireland to Trinity College but unfortunately his career was soon ended by serious illness, and he died in 2010. Both these research groups are still prominent, and more recently research in astrophysics and condensed matter have also been successful.

Among physicists with Belfast links who have been prominent in recent years are two who are similar, not only in name but in the fact that, in slightly different circumstances they might have been awarded Nobel Prizes.

John Bell was a student at QUB where he profited greatly from the encouragement of Emeleus and Ewald. He has carried out much important work in quantum field theory and elementary particle physics, but, of course, is most famed for his work on the foundations of quantum theory – Bell's Theorem and Bell's Inequalities.

Jocelyn Bell, who was born in Belfast, is best known for her part in the discovery of pulsars, but she has many other achievements in physics and its promotion.

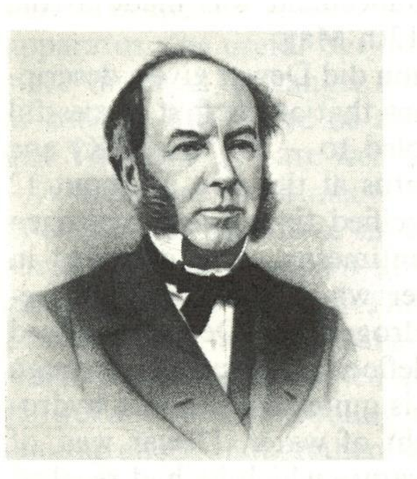
Amongst recent Belfast educated mathematicians, similarly to the physicists we can name two who are linked – this time not by name, but by the fact that they were fellow undergraduates at Queen's in the 1960s. After his time at Queen's. Raymond Flood, went on to a career at the University of Oxford, has published widely on the history of mathematics, and now holds one of the oldest and most famous mathematics chairs in the U.K. – the Gresham Chair of Geometry. His classmate, John Toland, who in 1978, proved Stokes' conjecture on the existence of gravity waves of maximum height on deep water, is now Director of the Isaac Newton Institute, Cambridge, the U.K.'s national hub for research in the mathematical sciences .

Across the years it may be said that physicists and mathematicians from Belfast have made substantial contributions to their disciplines, certainly at least commensurate with its size and status. Its present healthy state leads one to have confidence that this success will certainly continue.

Mark McCartney
Andrew Whitaker

## Thomas Andrews (1813-1885)

***Peter Ford***
***University of Bath, UK***



Thomas Andrews

If you read the Dictionary of National Biography or the Dictionary of Scientific Biography the first thing that you see is that Thomas Andrews was a chemist. This is an unpromising start for anyone preparing to give a talk at a meeting devoted to the History of Physics. Another biography that I consulted said that he was a physical chemist, which is a bit more optimistic. My personal interest in Thomas Andrews stems from the fact that he was the first person to determine the critical temperature of any gas, in this case carbon dioxide. Knowledge of the critical temperature is vital for the liquefaction of a gas, since one must reach a temperature below this in order to liquefy a gas by applying pressure, a technique which was widely used to liquefy gases. Low temperature physics stems from the ability to use liquefied gases to obtain the necessary low temperature and this of course in turn relies on the ability to liquefy the gas in the first place. Andrews obtained the critical temperature by studying the pressure-volume relationship for carbon dioxide for different temperatures, the P-V isotherms. This is essentially studying Boyle's law an experiment which many of us carried out at school.

Thomas Andrews was born on 19th December 1813 at 3, Donegall Square, Belfast. He was the eldest of six children of Thomas Andrews, a linen merchant of Belfast, and his wife Elizabeth. The address interests me. Donegall Square is regarded as the centre of Belfast and in the middle of it is the very fine City Hall. The City Hall in Durban, South Africa, is almost an exact copy of the one in Belfast. Also on Donegall Square is the Linen Hall Library, the most important independent library in Belfast, which has a huge archive of pamphlets and material relating to the unrest in Northern Ireland forming a valuable research source. I am speculating as to whether the first Linen Hall Library was built on the site where Thomas Andrews spent his childhood, since it was originally built on a disused warehouse for storing linen.

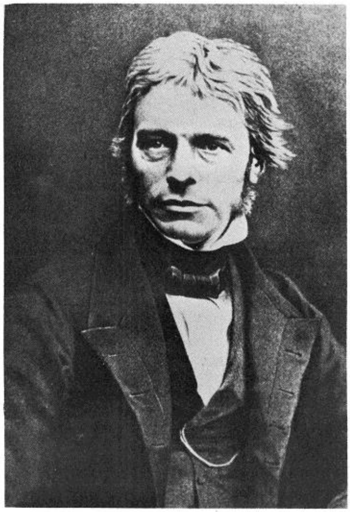
Thomas Andrews attended the Royal Belfast Academical Institute or “Inst”. This is a most important school in Belfast, which still flourishes today. Among its alumni are William Thomson, Lord Kelvin, and another Thomas Andrews, who was the chief architect of the ill-fated Titanic, which was built in the nearby Harland and Wolff shipyard. Inst was opened in 1814 in a building originally designed by Sir John Soane who had also designed the Bank of England in London in 1788. Andrews then moved to the University of Glasgow in 1828 to study chemistry under Thomas Thomson (1773-1852). It should be pointed out that Thomas Thomson was no relative of William Thomson, Lord Kelvin. Thomas Thomson came from St Andrews and studied in Edinburgh before coming to Glasgow. He was no mean chemist and his life is also briefly described in the Dictionary of National Biography.

In the Summer of 1830, Andrews travelled to France arriving in Paris in time to hear the Winter lectures given by several of the leading French chemists and spending a short time working in the laboratory of Professor Dumas. Andrews spent the following years in studying medicine. He passed the four-year undergraduate course at Trinity College, Dublin, followed by time at Belfast and finally Edinburgh, where in 1835 he received the diploma of the Royal College of Surgeons of Edinburgh and graduated with a doctor of medicine, the MD. He declined chairs of chemistry at two schools of medicine in Dublin instead setting up a medical practice in Belfast while at the same time was appointed to teach chemistry at the Royal Belfast Academical Institute. He was employed like this for the following ten years during which time he published several scientific papers becoming quite well known in scientific circles. In 1842 he married

Jane Hardie and they had four daughters and two sons.
In 1845 Thomas Andrews was appointed the vice-president of the Northern College, which later became Queen’s College, Belfast and so he resigned both his teaching post and medical practice. In 1849 there was the opening of the Queen’s Colleges the organisation of which Andrews had engaged in since 1845. It should be appreciated that since 1800 there was the Union of Great Britain and Ireland. The whole of Ireland sent members of parliament to Westminster although in Ireland there was some devolution. Ireland was ruled over by Queen Victoria. By 1845, the population of Ireland was some 8 million and only had one established university, namely the important and prestigious Trinity College, Dublin, which had a high Anglican tradition. This was not particularly favoured by the people of Belfast who tended to be Presbyterian. A quick glance at the map can make people appreciate that a more suitable and well established university relatively close to Belfast was over the North Sea Channel to Glasgow in Scotland and this accounts for so many academically gifted people from the Northern Ireland area studying in Glasgow or Edinburgh. It was to improve the university situation in the island of Ireland that three new “Queen’s Colleges” were created namely in Belfast, Cork, and Galway.

1846 saw the beginning of the great the potato famine, which had a devastating effect on the country, resulting among other things in widespread emigration from Ireland, especially to the United States of America. This gave rise to a dramatic decrease in population from which it has never fully recovered. Agitation for Irish Home Rule increased from the 1870s. After a lot of bloodshed, the partition finally took place in 1920, the six northern counties remaining in union with Great Britain while the rest formed Ireland. Because of this, Belfast was the only one of the three universities created in the 1840s to retain the name Queen’s University.

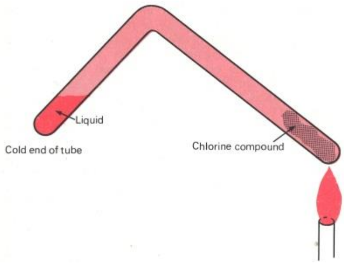
Thomas Andrews was appointed the professor of Chemistry at Queen’s College, Belfast, a position which he held until 1879. During this time he carried out a lot of teaching and administration as well as a steady stream of useful research. However, far and away the most significant of this research was that involved in studying the liquid-gas transformation with a meticulous series of experiments on carbon dioxide, carried out between the years 1861-69. These demonstrated the chief features of the liquid-gas transition and firmly established the concept of the critical temperature.



To understand the importance of Andrew's determination of the critical temperature of carbon dioxide, it is necessary first to look at some work of Michael Faraday at the Royal Institution in London.

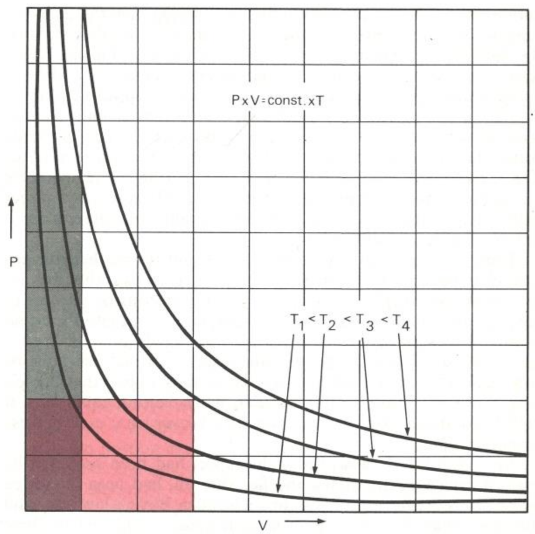
One of the lesser known achievements of Faraday (right) is his seminal work on the liquefaction of gases. He was working as a scientific assistant to Humphry Davy, who discovered the element chlorine in 1823. That same year Faraday heated a compound of chlorine in a sealed tube.

He observed an oily liquid at the cold end of the tube, which he quickly identified to be liquid chlorine. Faraday realised that by heating the compound in a sealed tube he had created an overpressure. In addition, the liquid collected at the cold end of the tube. Hence, in order to liquefy a gas, one required both a high pressure and a low temperature. Using this technique he was able to liquefy a variety of gases including carbon dioxide.




Faraday's liquefaction of chlorine

However, Faraday and many others were unable to liquefy nitrogen and oxygen, the two chief constituents of air, as well as hydrogen. There were heroic attempts by several people to do so applying huge pressures to their apparatus sometimes resulting in catastrophic explosions. Hence these three gases became known as the "permanent gases" apparently resisting the idea that all materials should exist as solids, liquids and gases. Faraday suspected correctly that he had not obtained a sufficiently low temperature to liquefy these gases but did not specify the concept of critical temperature. This was left to a Frenchman Charles Cagniard de la Tour who in 1822-3 studied the effect of heat and pressure on several liquids and concluded that there was a certain temperature above which it was not possible for it to remain a liquid no matter how much pressure was applied. This he called the critical temperature.

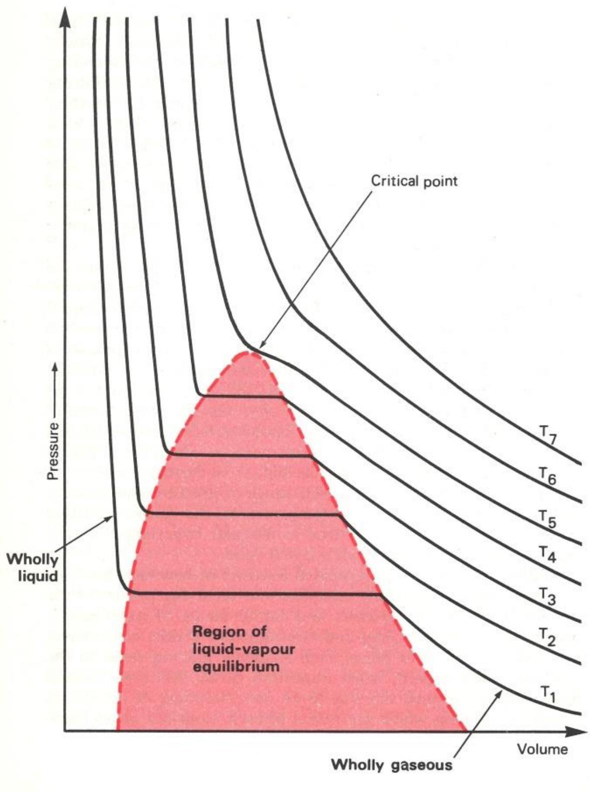



Isotherms for an ideal gas at four different temperatures

Andrews measured the pressure-volume relationship of carbon dioxide for a series of different temperatures in a similar manner to the much earlier work of Robert Boyle in which he studied air. Robert Boyle (1627-1691), son of the Earl of Cork and the "father of chemistry", is another eminent scientist whose roots lay in Ireland. Late in 1655 or early in 1656, Boyle came to Wadham College, Oxford, where he joined an active group of natural philosophers, who together formed the nucleus for the foundation of the Royal Society of London in 1662. Meetings were frequently held in Boyle's lodgings in the High Street where, working with that outstanding experimental scientist Robert Hooke, he developed a remarkable air pump, which allowed him to study how the volume V of a gas varied with applied pressure P. Boyle obtained the series of curves for air shown in Slide 4, which correspond to the P-V isotherms. Boyle established the well known relationship:

Pressure P X Volume V = constant

Today this is universally known as Boyle's Law and is among the first topics of physics which one learns about at school. There is currently a plaque in the High Street at Oxford commemorating Boyle's work there.
Andrew's master stroke was to study carbon dioxide, which is a gas at room temperature but can be converted to a liquid at a relatively low pressure. Hence the whole range for studying the liquid-gas equilibrium takes place at relatively low pressures, thereby avoiding explosions to the glass apparatus, which can occur when very high pressures were used when other gases had been studied.

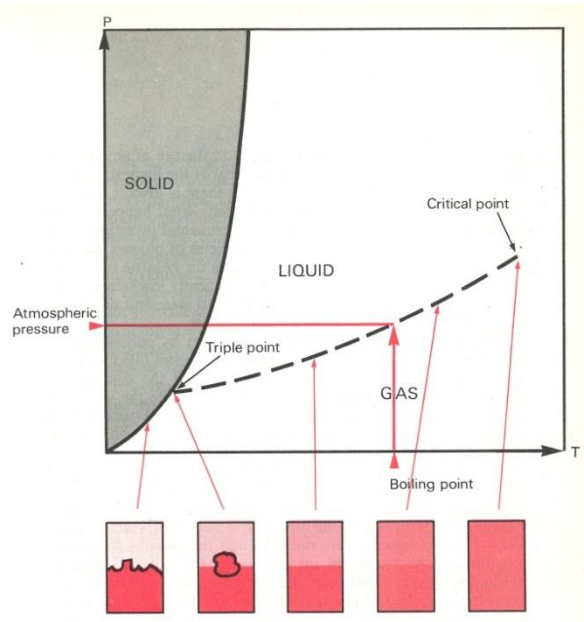



Isotherms of a real gas as measured by Andrews

From his results one can appreciate that these isotherms only approximate to Boyle's Law at high temperatures (T7). At lower temperatures one can clearly distinguish the regions where one has wholly liquid or wholly gas. The flat portion of each isotherm shows a region where one has a situation which is partly liquid and partly gas and the two phases are in equilibrium. This region becomes smaller on moving to higher temperatures until one arrives at a situation (T5) where this gas-liquid equilibrium situation ceases altogether. The point at which this takes place is the critical point and the temperature corresponds to the critical temperature.

Andrews obtained a critical temperature for carbon dioxide of 31C. The importance of Andrew's work is that he had obtained a universal curve, one which is applicable to all matter. It helped pave the way for liquefying, oxygen and nitrogen and later hydrogen culminating in 1908 when Kamerlingh Onnes in Leiden liquefied helium having first determined its critical temperature at just over 5K.

The universal nature of the solid-liquid-gas transition can be seen in a plot of the pressure- temperature diagram of state.

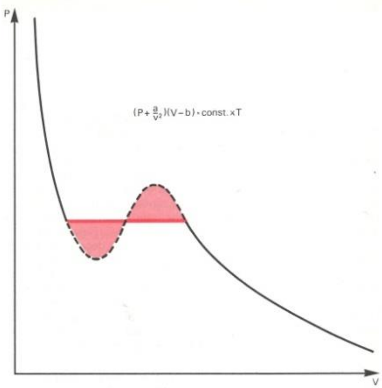



The pressure – temperature diagram of state

This shows the curves at which the solid, liquid and gaseous phases of a substance are in equilibrium. It should be noted that the liquid and gaseous phases merge into each other above the critical temperature. The triple point is where the solid, liquid and gaseous phases are in equilibrium. The work of Thomas Andrews gave rise to the field of physics called "critical phenomena", which nowadays forms an important aspect of condensed matter physics. It is quite a wide ranging area. For example, there is a close analogy between the liquid-gas transition at the critical temperature and the ferromagnetic-paramagnetic transition at the Curie temperature. The determination of the critical temperature of carbon dioxide reflects the careful and painstaking working of Andrews and his skill as an experimentalist, something which characterised all his investigations.

Understanding the behaviour of real gases, and of Andrew's results, came in 1872 from the Dutch scientist Johannes Diederik van der Waals. Boyle's law is explained in terms of kinetic theory. This assumes that a gas can be regarded as an ensemble of molecules whose size is negligible compared with the volume they occupy and which undergo elastic collisions with the walls of the vessel in which they are contained. In reality, a real gas does not satisfy these criteria. The molecules occupy a finite volume and they exert forces upon each other. Van der Waals modified Boyle's Law to take account of these two criteria. He modified the volume V by assuming that V → (V-b), where b is the volume occupied by the molecules themselves. He took account of the second criterion by replacing P → ($P+a/V^2$), where the letter "a" is a constant. The attractive forces between molecules will tend to bring them closer together thus effectively corresponding to an additional pressure. This "pressure" will become larger as the molecules get closer together, resulting in the $a/V^2$ term. Hence, instead of Boyle's law for an ideal gas, for a real gas at any temperature T one should use the Van der Waal equation:

$$(P + a/V^2)\,(V-b) = \text{constant}$$



Van der waals modificaton of Boyle's Law

The diagram above shows that the van der Waal equation has important features which occur in real gases as first demonstrated by Andrews. At low pressures and large volumes, one has a situation which approximates to Boyle's Law for an ideal gas. At high pressures and low volumes there is a liquid, which is very incompressible. The complex region bounded by the dotted line is the liquid–gas equilibrium situation. The van der Waal equation cannot describe this region since it is far too simplistic to accurately take into account the nature of the inter-molecular forces.

Van der Waals work is highly regarded and was particularly valuable in supplying theoretical back up for the first liquefaction of helium by Kamerlingh Onnes in 1908. Van der Waals obtained the Nobel Prize in Physics for 1910 while Kamerlingh Onnes obtained it in 1913. This reflects the strength of Dutch science in the early part of the twentieth century since Lorentz and Zeeman jointly received the Physics Prize in 1902 and Van't Hoff the Chemistry Prize in 1901.

Although nowadays Thomas Andrews is chiefly remembered for his work on the liquid – gas transition, he did carry out other important research. One of these in collaboration with Peter Guthrie Tait was their work on ozone, where they helped establish that it was an allotropic form of oxygen. Although a superb experimentalist, Andrews was no theoretician. This was supplied by Tait and working together they formed a powerful combination.

Andrews played a very important role in establishing the new Queen's University in Belfast. He was vice-president and professor of chemistry from 1845 until his retirement in 1879. He then retired to Fort William Park, Belfast and died there in 1885. He was elected a Fellow of the Royal Society of London in 1849 and of Edinburgh in 1870. He was offered a knighthood in 1880 but declined the honour. Apparently, the reason for this was that his scientific hero Michael Faraday had also declined a knighthood and as a result Andrews thought that it was not appropriate that he should accept one*

Thomas Andrews has been described as a man of great kindness and humanity who played an important role in helping to establish Queen's University in its early days. The impressive University that exists today owes quite a lot to Andrews pioneering work and the standard that he set. In addition, he was concerned about many social issues of the day and wrote several articles relating to these. As a scientist he showed considerable experimental skill and flair and made important contributions in several areas. In my opinion Thomas Andrews richly deserves to be included in the pantheon of great physicists connected with Queen's University, Belfast which we are celebrating today.

* I am grateful to Professor Colin Latimer for pointing this out to me at the Conference.

**References**

In writing this article I have made use of accounts of Andrews' life in the Dictionary of National Biography and the Dictionary of Scientific Biography. In addition I have used material from the book: The Quest for Absolute Zero by K. Mendelssohn, World University Library, 1966.

I have written about the history of the liquefaction of gases in:

The Rise of the Superconductors, P.J. Ford and G.A. Saunders, CRC Press (2004), Chapter 1.

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# James Thomson 1822-92

***Andrew Whitaker***
***Queen's University Belfast***

By any criterion but one, James Thomson (Figure 1) would justifiably be remembered as one of the very most creative and important engineers and physical scientists of the nineteenth century. He became a Fellow of the Royal Society and was appointed to give the prestigious Bakerian Lecture in the year of his death. He produced a substantial number of important inventions and discoveries in engineering, physical science and what might now be called geophysics. He spent 36 years as a Professor of Civil Engineering, 20 in his home city of Belfast, and 16 in Glasgow, in the latter occupying what had been the first such Chair in the world and was still one of the most prestigious. He was universally regarded as a dedicated and excellent teacher. He was awarded three honorary degrees, from Belfast, Glasgow and Trinity College Dublin.

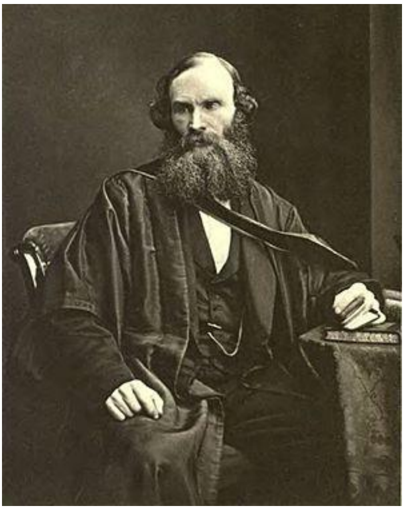

Figure 1 (above)

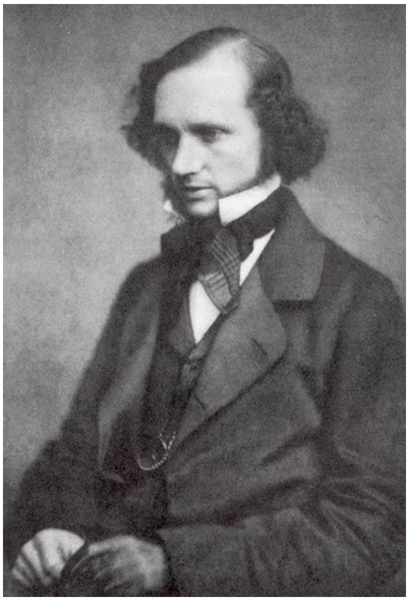

Figure 2 (right)

Unfortunately the only criterion by which he usually is judged is in comparison with his younger brother William. William (Figure 2) was a scientist of spectacular achievement; in particular he is regarded as one of the three founders, with Rudolf Clausius and MacQuorn Rankine, of the fundamental scientific discipline of thermodynamics. He did much important technical work, especially with regard to the Atlantic cable, where he showed both practical expertise and a certain physical courage. He worked with government on many committees and inquiries, and became Baron Kelvin of Largs (as much for his political stance as for his technical achievements). He built up a reasonable wealth, and owned an ocean-going yacht and a modest stately home. Even when he died he was buried in Westminster Abbey.

James could not compete in this public way, but he had his own strengths, which in some ways complemented those of William. Smith and Wise contrast James' 'determined single-mindedness, manifested in compulsive involvement with engineering problems' with 'William's impulsive attacks on a variety of loosely related problems'. For Joseph Larmor, James was 'the philosopher who plagued his pragmatical brother'.

In particular it will be argued here that James' contributions to the studies on thermodynamics were sufficient that William's part in the discovery might justifiably be shared by the two brothers. John Perry, a pupil of James, and later a well-known physicist acknowledged this: 'James… may be said to have really shown to Clausius, Rankine and his brother the method of stack which has led to the development of thermodynamics'. In this area James is best known for the discovery of the lowering of the freezing point of water under pressure, though on this point the contributions of the brothers to this discovery will here be re-assessed.

**Sketch of the life of James Thomson**

James and William were the sons of James Thomson Senior (1786-1849), in his own way as remarkable a man as either of his sons. He had begun life as a farmhand on a family-owned farm, he had obtained some education from a local Presbyterian minister, and he was then able to study at the University of Glasgow, keeping himself by teaching at home in the summer when the University was closed.

When he graduated, a great opportunity presented itself. Following the failed rebellion of 1798, led in the north of Ireland mainly by Presbyterians, there was a quarter of a century or so of religious liberalism in Belfast. A result of this was the founding of the Belfast Academical Institution, always known as Inst, and later given the title of Royal - BRAI. This was a religiously liberal school delivering practical education. It had school and college departments, and James Senior had appointments in both, as teacher and Professor of Mathematics.

Life was good – he married Margaret Gardner in 1817, and they had six children – two sisters, Elizabeth and Anna older then James and William, and two younger brothers, John and Robert. James Senior rose early to write best-selling textbooks and he was able to buy two houses opposite Inst – one where the family lived, and on the site of which there is now a plaque as the birthplace of William, and the other to rent out.

Sadly in May 1830 Margaret died, and by December 1831 James Senior had become Professor of Mathematics in the University of Glasgow. The family found that they were not as well-off as expected, and the result was that James and William were taught at home rather than at school, greatly, of course, to their advantage. They attended University lectures even before they matriculated, which they did when James was 12, William 10, early even for those days. They were consistent prize-winners, but William, the younger was always first, James second, which may have dented James' confidence somewhat. James graduated with a BA in 1839, and with an MA in Mathematics and Natural Philosophy in 1840. Meanwhile William concluded the course for the MA but did not graduate because he proceeded to study in Cambridge.

James entered the Dublin office of John McNeil, professional engineers, but this was the start of an unfortunate decade for him. He had to return home with a knee injury in 1841, though he did take the opportunity of studying engineering at Glasgow University under Lewis Gordon, the first Professor of Engineering in the world.

In 1842 he worked with John McClean on canal building, but in the following year he moved to work in the drawing office of the famous Horseley Iron Works in Staffordshire. Later in the same year he took up a position as a premium apprentice at Fairbairn Shipbuilding in Millwall, learning about heavy engineering techniques, marine engines and iron ships,

but business problems meant that in 1844 he had to move to Manchester to work with Fairbairn on steam-engines.
James was decidedly unsettled. He wrote to William as follows: ‘I am not getting settled in any works. Just when I begin to take root I have to leave the place… I wish my apprenticeship was as nearly done as yours – but when it is done, I fear I shall have no comfortable berth to step into as that which is probably waiting for you.’

Worse was to follow. In 1844 he had to return to Glasgow seemingly seriously ill, with an exceptionally irregular pulse. His Calvinist doctor told him that he might die at any moment. There must have been a period of despair, but strangely this period of living at home in Glasgow was extremely positive. James developed most of the research interests that were to occupy him for the rest of his life. He discussed the relationship between work, temperature and heat with William and thought about the topic continuously. Conscious that he was living off his father, he designed engines that he hoped to patent.

In 1846 William was appointed to the Chair of Natural Philosophy at Glasgow. James would have been pleased for him, but the contrast with his own position, living at home without a job and perhaps likely to die, must have been stark.

By 1846, in fact, James’ health had been restored. He must previously have been totally run down by working and living in the noisy dirty factory environment. Though he was to return to engineering, he worked in consultancy and lecturing, steering well clear of factories. However his troubles were not over. In 1847 his younger brother John died of typhus and two years later James Senior, on whom James had depended so much, died of the same disease.

Even before this, James, distressed by the severity of much of the Christianity he was exposed to, became a Unitarian, and felt that he had to move away from his family: ‘However painful the effort was, and gloomy the way before me, I tore myself away from my friends.’ He moved to London to work with Lewis Gordon to patent his vortex turbine.

At this point his sister Anna did a wonderful thing for him; she had married and returned to Belfast, and in 1851 she pleaded with James to come to Belfast and start an engineering business. This was to be the end of his unhappy period and the start of a successful life. Sadly Anna herself was to die in 1857.

In Belfast James' business grew; he became resident engineer to the Belfast Water Commissioners, making efforts to improve the standard of sanitation in the rapidly growing city. He made friends at Queen's College, in particular Thomas Andrews, with whom he was to carry out joint work later, and Neilson Hancock. In 1853 he married Neilson's daughter; they were to have a long and happy marriage and had three children.

Then in 1853 he was asked to stand in for the Professor of Engineering at Queen's, the position becoming permanent in 1857. This was clearly the ideal position for him; he dedicated himself to teaching and research, the latter to be described shortly.

After James had had a long and successful period at Queen's, in 1872 Rankine, Professor of Civil Engineering at Glasgow died. James was not immediately keen to apply for the post – he was doing good work in Belfast and his family was settled, but William, now Sir William because of the Atlantic cable, insisted that he did so. His references were from the great and good of physical science – James Clerk Maxwell, Andrews, James Joule, Hermann van Helmholtz and P.G. Tait. Joule wrote that: 'In the event of his seeking the chair of Glasgow College, it is impossible that there can be any serious competition, inasmuch as [James's] European fame as a discoverer combined with his eminent success as a teacher of Engineering, are such as to place him altogether without a rival.'

James was just as successful at Glasgow as at Belfast until deterioration of his eyesight led to his retirement in 1889. He was to die three years later.

**Achievements in engineering and science (a summary)**

James' most significant engineering achievement was his vortex turbine, a horizontal water-wheel, which was patented in 1850. It was carefully designed to make full use what we would now call kinetic energy, avoiding jarring on the way in, and the water leaving still with kinetic energy. The turbine was manufactured by Williamson of Kendal, and examples were working for up to a hundred years. His vortex pump was used for drainage from land bellow high tide. It contained a whirlpool to make full use of the energy of the water leaving the pump.

James invented the friction brake dynamometer. He also improved the theory of river meanders, and spent much of the last years of his life studying atmospheric circulation, the subject of his 1892 Bakerian lecture.

He took part in the study of the melting of ice and the movement of glaciers, interacting with such scientists as James Forbes, John Tyndall and Michael Faraday.
He investigated the ‘parallel roads’ of Lochaber, which he was able to explain in terms of the presence and movement of glacial lakes, and he was also able to explain many of the features of the Giant’s Causeway in detail.
He studied the liquefaction of gases with Thomas Andrews, and also pointed out the existence of the triple point, the point on a graph of pressure against temperature where gas, liquid and solid may coexist.

He constructed integrating machines to predict the tides, and lastly he invented several terms that are still in use today – radian, interface, ergometer and torque.

**James and William and the search for thermodynamics**

One of the main concerns of the brothers was the long-term future of the Universe. Was it like a clockwork model, going on for ever unaltered, as suggested by Halley’s comet? On the other hand, it was discovered in 1832 that the period of Encke’s comet decreased each revolution, suggesting friction and so an arrow of time.  James was convinced that tides must lead to retardation of the moon or the earth.

Was there conservation or decay? Believing that only God could destroy, the brothers argued for conservation. And yet, William also felt that: *‘Everything in the material world is progressive. Change and decay in all around I see.’*

Around 1842 the brothers were watching barges go through locks. Sometimes ‘power’ led to useful ‘work’ – the barge rose. Sometimes not – water just spilled over. What had happened to this potential work? Similarly in a steam engine heat travels from the boiler to the condenser and work is done. Yet in other circumstances heat may travel from hot to cold merely by conduction and in this case no work is done. Again – what has happened to this potential work?

It must be stressed that in these examples heat is not being transformed to work. At this time, and roughly since 1780, following the ideas of Laplace, the favoured view of heat was that of the caloric theory. Today the usual view of this theory is that heat consisted of particles of a subtle, weightless, highly elastic solid. Many of the properties of heat may easily be explained by the theory, particularly when it is recognised that caloric may be defined as consisting of free caloric which affects a thermometer, and latent caloric which doesn't.

Actually, though, the central point of the caloric theory is that heat is conserved. Since it is conserved it certainly cannot be transformed to work! This assumption is clear, though usually implicit, in the work of the Thomsons and also Sadi Carnot and Clapeyron who we shall meet shortly. Clifford Truesdell wrote: *'For Laplace, heat is never created nor destroyed. We shall refer to this assumption by the traditional term Caloric Theory of heat.'*

In the caloric theory there is a yet more potent way of talking about heat. We can talk about the 'amount of heat' in a system. If a system goes through a cycle and returns to its starting-point, it must have the same amount of heat. So if we have all the appropriate information about a system, for example for a gas the pressure, volume and number of molecules, we may know how much heat it possesses.

In the late 1840s, the brothers discovered and rescued from obscurity two theories about heat. They realized that each had profound things to say, but the theories were in disagreement and they found it impossible to reconcile them.

The first was the 1824 theory of Sadi Carnot (1796-1832). Carnot's aim was to obtain the maximum amount of work from a fixed amount of fuel, which was associated with a 'hot reservoir'. Of this work William was to say that: *'Nothing in the whole range of Natural Philosophy is more remarkable than the establishment of general laws by such a process of reasoning.'*

Carnot had many crucial insights, nowadays not usually noticed because they are taken for granted. First, since we have a continuous process, we must have a cycle.

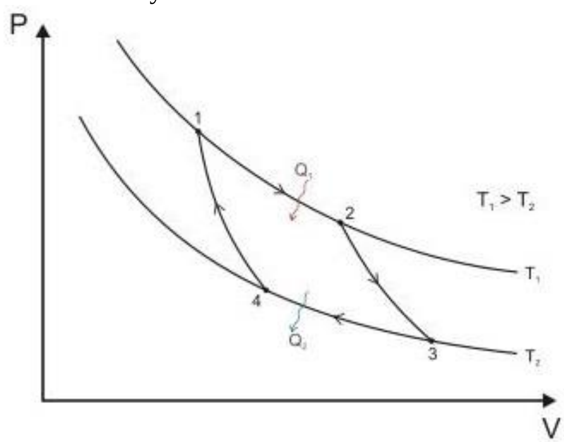

Figure 3, actually produced by Clapeyron not Carnot.)

As a result of this there must be a cold reservoir at temperature $T_2$ into which heat is deposited as well as the hot reservoir at temperature $T_1$. Since Carnot is using the caloric theory and so no heat is transformed to work, in fact all the heat gained from the hot reservoir must be deposited in the cold reservoir. In Figure 3, $Q_1 = Q_2$.

As mentioned above, efficiency will be lost if heat is exchanged between the system and either of the reservoirs with system and reservoir at different temperatures. Thus Carnot chose as his processes isothermals, 12 and 34, where the system remains at the temperature of the reservoir, and adiabatics, 23 and 41, where there is no flow of heat – this is the definition of the word 'adiabatic.

On the diagram the work done is the area inside the cycle. The Carnot cycle was reversible, and he was able to show that this was connected to its being the most efficient cycle possible. The requirement for two reservoirs would later be translated into the Kelvin statement of the Second Law of Thermodynamics.

Carnot's work was almost totally ignored. Emile Clapeyron rewrote it to make it more accessible, and provided the well-known diagram – Carnot had used only equations. It was translated into English in 1837 and German in 1843, but the Thomsons did not obtain a copy until 1848, when they obtained one from Lewis Gordon. They were highly impressed and William wrote an important paper on Carnot's work in 1849. In fact when Clausius produced his later crucial work, he had not read Carnot's paper, only William's report on it.

William and James were convinced that Carnot's work was an essential component of the true theory yet to be produced, but they were also impressed by the work of James Joule, which disagreed with it fairly drastically.

James Joule (1818-89) was convinced that, contrary to the caloric theory, heat was a form of energy, and he performed many experiments designed to demonstrate this, in particular rotating paddle-wheels in water to cause an increase in temperature. He criticized Carnot as follows:- What had happened to the work that could have been produced by the fall in temperature between that of the fuel and that of the boiler (the hot reservoir)? Joule found it difficult to promote his views. His papers were rejected by the Royal Society, and at the meeting of the British Association for the Advancement of Science in 1847 he was given a very short time for his presentation. However William found his comments interesting and discussed them in detail with James. Joule was later to say that William's support had lifted him from obscurity. James wrote to William: 'Some of his views have a slight tendency to unsettle one's mind… If some of the heat can absolutely be turned into mechanical effort, Clapeyron may be wrong.'

The brothers were bemused. Carnot could not deal with the problem Joule had identified, but Joule predicted that heat could be transformed to work, which the brothers did not believe. William added a footnote to his Carnot paper praising Joule's work and adding that Joule called for abandonment of Carnot's axiom of conservation of heat. But he added that: 'If we do so, we meet innumerable other difficulties – insuperable without further experimental investigation, and an entire reconstruction of the theory of heat from its foundation.'

In the meantime the brothers came up with and solved a problem that rather strangely was an important result of thermodynamics even before the theory itself had been produced. They considered a Carnot engine acting in reverse with both reservoirs at the freezing point of water. Because there is no difference in temperature, the area inside the cycle is zero and so no work is required. In the cycle water will gradually be turned to ice, but the problem is that water expands when it freezes so it seems that the cycle could be used to produce work and hence perpetual motion!

The solution is that the two temperatures must be at different temperatures; the temperature must be lower during the expansion at higher temperature than for the contraction at lower pressure. Thus the freezing-point of water must be lowered under pressure.

But who deserves the credit for this solution? Certainly James performed and published the calculations; William arranged for the highly accurate thermometer to be constructed and the measurements to be performed in Glasgow. But whose was the eureka moment? James wrote that 'it occurred to me ', but in a letter to James Forbes, William said 'it struck me'.

Smith and Wise suggest that (i) William was being generous, (ii) William had forgotten, or (iii) Both had the idea simultaneously. Suggestion (i) seems unlikely – even if William had been prepared to be generous, surely James would have not been willing to accept credit for an idea that was not his. Suggestion (ii) also seems unlikely, so it seems certain that (iii) is correct – the brothers came up with the idea in discussion and at the same moment.

In connection with James' paper, a large claim was made over rather a small point. After its initial publication, his paper was published in another journal but with a small change. In the original publication, James follows Carnot by using caloric and conservation of heat, though with a footnote questioning the point.

However in the reprint he dispenses with conservation of heat. Point 4 in the Carnot cycle is reached not when the heat given out to the cold reservoir equals that obtained from the hot reservoir, as on a caloric theory, but when point 1 may be reached by an adiabatic. This argument was stressed in James' obituaries, and when William prepared his own collected papers for publication in 1881, he added a note to his Carnot paper pointing out James' 'advance' and thus giving him credit form the first break from caloric theory.

Back in 1850 Clausius came up with a neat solution to the brothers' dilemma. (Rankine had similar ideas but his work was restricted to rather a special molecular model so he obtained little credit for his work.) Clausius retained Carnot's theory except that he rejected conservation of heat and, following Joule, replaced it by broad equivalence of work and heat. He said that heat consists of motion of 'the least parts of bodies'.

We must not talk of 'the amount of heat in a system'. There IS a new quantity 'internal energy' which represents the sum of the kinetic and potential energy of the system. However internal energy may be increased by heating the system OR by doing work on it, so it is impossible to say how much 'heating' and how much 'working' has been done. This is the First Law of Thermodynamics. The Second Law is essentially the Carnot theorem as modified by Clausius.

The loss of potential work that had so worried the brothers was explained in term of it being not 'destroyed' but 'lost to man irrecoverably' as low temperature heat.

William wrote a very general paper in 1851 and continued to make important contributions to thermodynamics for many years, though it was Clausius who came up with the idea of entropy.
But what about James? His name slipped out of history despite, as Crosbie Smith saying: ‘Much of the early interest in Carnot and Clapeyron lay with James, and it appears to have been his enthusiasm which inspired William to go beyond a mere passive awareness of Clapeyron’s memoir’ and ‘James as the “theoretical engineer” had rather more specialized and concentrated interests relating to the motive power of heat than William.’

It seems appropriate to inquire whether James should indeed be ‘promoted’ to being regarded as a founder of thermodynamics.

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# Joseph Larmor (1857-1942)

*Colin Latimer, Queens University, Belfast*

Joseph Larmor by Frank McKelvey, 1940. QUB

## 1 Background and early years

Joseph Larmor and his seven siblings were born on a small farm at Ballycarrickmaddy, Magheragall, approximately ten miles northwest of Belfast. His mother, ambitious for her children's education, arranged for the family to move into Belfast where they stayed with an uncle and opened a grocery shop around 1863. Joseph attended a famous local school, the Royal Belfast Academical Institution, commonly known as "Belfast Inst", which had previously been attended by James and William Thompson (the future Lord Kelvin). He was a thin delicate black haired boy who exhibited a precious ability in maths and classics. At the age of 14 he advanced with a top scholarship to Queen's College Belfast where he graduated with high honours BA degree at 17 in 1874. After graduating Larmor stayed on at Queens as a senior scholar in mathematics for an additional year and was awarded his MA in 1875.

In 1876, a year later than planned due to ill health, he was elected a Minor Scholar at St John's College Cambridge, becoming a Foundation Scholar in 1878. He took the Mathematical Tripos in 1880 and was senior Wrangler and First Smith's Prizeman, J J Thomson being Second Wrangler. These two were destined over the next twenty years to complement one another in building up the electromagnetic theory of matter. He was elected a fellow of St John's before returning that same year to Ireland as Professor of Natural Philosophy at Queen's College Galway where, to his evident delight, he discovered "a nest of scholars, like one of those little old German Universities". There he continued his studies of Maxwell's *Treatise on Electricity and Magnetism* as well as investigating physical problems by applying Hamilton's principle of least action - a principle to which was to remain central to his scientific thought all his life. His first scientific papers were written in Galway. Interested in returning to Cambridge, Larmor applied in 1884 for the Cavendish professorship of experimental physics. He was not successful and J J Thomson got the job – an event which no doubt further contributed to the long lasting uneasy relationship between the two men. However in 1885 Larmor was appointed to succeed him in one of the recently created university lectureships in mathematics. By 1892 Larmor had published over thirty papers in applied mathematics and theoretical physics and he was elected to the Royal society. He remained at St John's for the rest of his working life, succeeding Sir George Stokes as Lucasian professor, the famous chair of Newton, in 1903. His lectures were "ill ordered and obscure but…even the most examination obsessed student could perceive that here he was coming to an advanced post of thought, which made all his previous teaching seem behind the times". Sir Edward Bullard reminisced in later life that Larmor was one of only three lecturers he bothered with during his undergraduate days at Cambridge.

## 2 Electrodynamics and *the Ether and Matter* 1892-1901

Larmor's most important contributions to scientific theory are contained in three memoirs all entitled *A dynamical theory of the electric and luminiferous medium* published in the Philosophical Transactions of the Royal Society (1894-96-97). These papers presented his theory of the electron, which of course gained increased appreciation when in 1897 when J J Thompson experimentally observed the electron. Larmor submitted a summary of his main conclusions as an essay for which he was awarded the Adams Prize (founded in memory of the discoverer of Neptune – another member of St John's College) The essay was published in 1900 under the title *Aether and matter: a development of the dynamical relations of the Aether to material systems on the basis of the Atomic constitution of matter, including a discussion of the influence of the motion of the earth on optical* *phenomena*. Despite being no easy read, this book is still available for purchase today, including a digital version for Kindle! First editions fetch over £500. This *magnum opus* has been placed in context by Buchwald:-

*'Between 1873 and 1894 British and American physicists were proponents of a theory which they almost all learned directly from Maxwell's Treatise on Electricity and Magnetism ...During the three years 1894-97, the most basic principles of Maxwell's theory were abandoned, and the entire subject was reconstructed on a new foundation – the electron – by Joseph Larmor in consultation with George Fitzgerald .. [ He proposed that] the only source of charge is a particle, that the flow of such particles uniquely constitutes the current of conduction, and that the aether must be strictly separated from matter….*'

Larmor thus saw clearly that the material aether of Stokes, Kelvin and others must be pictured not at all as some strange kind of solid, liquid or gas, but as something so distinct we have no analogy by which we may describe it – it is simply a scheme of mathematical equations. In this sense Larmor stands out as one who helped destroy the attempts to explain all natural phenomena in terms of the laws of mechanics as laid down by Newton. Larmor's electronic theory of matter also enabled him to predict the apparent absence of motion of the earth through the Aether as shown by the famous Michaelson – Morley experiment of 1887 – indeed their null result became central evidence for the validity of his theory. By 1900 he had solved the problem of correlating the electromagnetic fields observed by an earthbound observer with one who was stationary with respect to the Aether and so formulated and published the space time transformations in *Aether and Matter,* fully two years before Lorentz and Einstein, and now known as the Lorentz transformations. It is interesting to speculate in this context why people like Larmor, Lorentz and Fitzgerald did not come up with the theory of relativity before Einstein as they already had the required basic equations. Dirac, speaking mainly of Lorentz, has said that

*'He preferred to stay on the solid ground of his mathematics. So long as he stayed there his position was unassailable. If he had gone further he would not have known what criticism he might have run into.'*

While historians of science have concentrated and agonised over Larmor's work on the foundations of electromagnetism already discussed, he is mainly remembered by the working scientists of today for several much used formulae and theorems. In particular his descriptions of the behaviour of an electron and other charged particles in a magnetic field, which enabled him to explain the Zeeman effect, have secured him a lasting place in

physics terminology. *The Larmor radius*, a term widely used in plasma physics, is the radius of the circular motion of a charged particle in a uniform magnetic field. *The Larmor precession and frequency* describes the precession of the magnetic moment of a particle in a magnetic field. This is the basis of NMR and modern MRI machines and had led in recent years to Larmor being feted in the field of radiography. *The Larmor Theorem* allows the effect of the magnetic field to be neglected to first order by transforming into an appropriate rotating frame of reference. *The Larmor formula* gives the total power radiated by a charged particle as it accelerates or decelerates. This is the basis of how radio waves radiate from an antenna.
With the publication of *Aether and Matter* in 1900 Larmor's work on the electronic theory of matter, the most productive period of his research career, came to a natural conclusion. Larmor, and his contemporary, Lorentz, had paved the way for 20$^{th}$ century non-classical physics incorporating relativity ad quantum mechanics, and in which theories are seen as an essential part of the picture rather than an attempt to analyse it in terms of other concepts.

## 3 The 20$^{th}$ century, 1902-1932

After the turn of the century Larmor's productivity decreased considerably. This can be attributed to his increasing workload as an administrator and his public and university service.

### 3.1 Scientific research

Larmor's scientific work, in this second period, mainly concerns the study of general dynamics and thermodynamics including the dynamical history of the earth, formal optics and geometry. He, in collaboration with Hills, produced a new kind of analysis of the irregular motion of the earth's axis of rotation as given by the determinations of latitude at the chain of International Latitude observatories. He also studied the protection from lightning and the range of protection provided by lightning rods. He studied the effects of viscosity on free precession, which Jeffreys later used in his argument against mantle convection. He is still remembered for two major pieces of work.

As early as 1884 he had first considered the effects upon the earth of a conducting layer in the upper atmosphere of the earth and how such a layer could screen the earth from external magnetic fields. A younger colleague at St Johns was performing experiments bouncing radio signals from the ionosphere in an investigation as to how radio waves could be transmitted round the earth. Larmor became interested in the bouncing mechanism. Was it by reflection or refraction? He developed a theory of refraction of short waves by free electrons and published his conclusions in 1924. Appleton was later able to verify this conclusion in his Nobel prize winning work on the Heaviside layer.

Larmor also was to become a leading authority on geomagnetism. In 1919 at the BA Meeting in Bournemouth he presented a short paper in which he presented his dynamo hypothesis according to which the electric currents responsible for the magnetic fields of the earth and the sun arise in the swirling of molten metal in their interiors. This theory was largely ignored at the time and even later was opposed by Cowling with his famous anti-dynamo theorem. It was not easy to resolve this problem especially since it was extremely difficult to recreate the effect in the laboratory. However quite recently (2007), almost 90 years after Larmor's paper, the process has now been observed experimentally by Fauve and colleagues at the Ecole Normale Superieure in Paris and Larmor is now undisputedly acknowledged as one of the founders of geomagnetism.

3.2 Public and university service

Although an unassuming and diffident man, Larmor to the detriment of his scientific work, took on an enormous work load of public and academic service.

Larmor served on the council of the London Mathematical Society from 1887 until 1912 and acted as vice-president (1890-91), treasurer (1892-1912) and president (1914-1916). He was awarded the De Morgan medal of the society.

He became a fellow of the Royal Society in 1892 and acted as secretary from 1901 until 1912. He was awarded the Royal Medal in 1915 and the Copley Medal in 1921 and he was elected to honorary membership of many other learned societies including The Royal Society of Edinburgh, the Royal Irish Accademy, The American Academy of Arts and Sciences, the French Institute, and Accademia dei Lincei. . Ten universities awarded him honorary degrees.

He was knighted in 1909 and received the freedom of the City of Belfast in 1912. He entered parliament as a Unionist MP for Cambridge University in 1911 and served until 1922. In addition to contributing to the debate on Irish Home Rule his major concern was for education and the universities. In later years he was fond of reminiscing of how he secured the defeat of the alternative vote by a long speech leading the bewildered house deeper into mathematics until the whip gave him the signal that the wanted absentees had arrived. He was also wont to tell of how he had voted for the abolition of the carrying of a red flag in front of motorcars, and had been haunted by remorse ever since.

Larmor was active in St John's College affairs and was a member of the college council for many years. However according to D'Arcy Thompson it is an open secret, which he never told, that with all his honours he was a disappointed man – all for the want of one honour which he would have prized above all the rest, the Mastership of his College". His relationship with the Master, Sir Robert Scott, was poor to say the least and he refused to permit the University to hold a retirement dinner in his honour. Significantly the College does not possess a portrait of him – the only one known was commissioned by his alma mater, Queens University Belfast where it hangs in the Great Hall. However Larmor was not fond of travel. During his whole career it would appear that he only went abroad three times. He attended the BA meeting in Winnipeg in 1909, where his talk was rudely criticised by J J Thompson – no wonder Chadwick records "…I can't remember [Larmor] ever coming to the Cavendish Physical Society" He turned down invitations to attend the first two Solvay Conferences but did attend the third in 1921. He also attended an International Congress of Mathematicians held in Strasburg in 1920. He always returned the Ireland for the summer vacations.

Larmor was greatly interested in the history of his subject and edited the collected papers of Kelvin, Stokes, Fitzgerald and James Thomson. His memoir of Kelvin was a labour of love, and he remained proud of it as one of the best things he had ever done.

### 3.1 Retirement years

On retirement in 1932 at the age of 75, he retired from Cambridge, suffering from pernicious anaemia. He lived in a large house "Drumadiller" overlooking Belfast Lough where his four unmarried siblings lived. Here he continued studying and writing on a wide range of subjects and sending letters to friends. His writing, never good, was now even worse (in earlier years a compositor had rendered “it would be” as “t cosec C”). He submitted short comments, mainly to Nature, on a wide range of topics, including the potency of dilute medicines relating to his own experiences. As old men often do, he came to resent change. In a letter to nature in 1935 on the replacement of cgs units with the MKS system he laments “Why cannot it be let alone?”

After his last surviving brother died, he remained alone in the care of an old family servant for his last year. He died in May 1942 and is buried in the local priory graveyard. The major beneficiary in his will was his old school The Royal Belfast Academical Institution who received £10,000 (now worth over £1million) to provide scholarships for deserving boys. Queen’s University and St Johns College also received bequests for the same purpose.

According to Eddington, Larmor had been “An unassuming diffident man, who did not readily form close friendships and whose numerous acts were performed without publicity….a baffling personality”. Appleton concludes

*‘To many people in Cambridge, Larmor appeared an aloof and remote figure. Perhaps his own generation tended to be awed by his great reputation. But to us younger folk he was friendly, considerate and generous in the attention he gave to us, and our association with him will always remain a proud and fragrant memory’*

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This talk was compiled with the aid of the Oxford Dictionary of National Biography, the Complete Dictionary of Scientific Biography, encyclopedia.com, the Dictionary of Irish Biography, the Encyclopaedia of Geomagnetism and Paleomagnetism and references therein.

# Ernest Walton 1903-1995

***Brian Cathcart***

Ernest Thomas Sinton Walton shared the 1951 Nobel Prize for physics with Sir John Cockcroft for, in the words of the citation, 'the discovery of the transmutations of atomic nuclei by artificially accelerated particles'. This feat had been achieved at Cambridge University twenty years earlier when, by harnessing high voltages, they were the first to break down nuclei using beams of particles that were under the experimenters' control. The two men thus opened the way for the development of modern particle physics – or as the citation put it, their work 'may be said to have introduced a totally new epoch in nuclear research'.

Educated at Methodist College, Belfast ('Methody') and Trinity College, Dublin, Walton was an experimental physicist praised for his 'exceptional ability' by no less an authority than Lord Rutherford. He was twenty-eight years old when he and Cockcroft achieved their breakthrough after three years of preparation, beating several rival teams in the United States to become the first, in the journalistic shorthand of the time, to 'split the atom' under controlled circumstances. In doing so they also provided the first experimental proof of the already world-famous equation, $e=mc^2$, a development that delighted Einstein.

Walton had arrived at Rutherford's Cavendish Laboratory as an 1851 Exhibitioner in 1927, having completed an MSc in hydrodynamics. He had little knowledge of, and no training in, the field of experimental atomic physics, of which Rutherford was the acknowledged world leader and to which, as director, he applied almost all the resources of his laboratory. Walton had recognised that this was the most exciting and promising research field of the time and was determined to enter it despite pressures to stay with hydrodynamics. He soon proved that he was a quick learner with good instincts.

Rutherford was at that time coming to terms with the obsolescence of experimental techniques which had served him well for two decades, involving the use of natural sources of fast particles, normally from radium products, and zinc sulphide scintillation screens. With the former as the projectiles and the latter as the means of detecting outcomes, he and his colleagues and students had conducted many groundbreaking bombardment and scattering experiments that had hugely expanded the understanding of the atom and its nucleus. By 1927, however, after several years of frustration, he was coming to accept that the returns from these techniques were diminishing and that a new approach was needed. In his presidential address to the Royal Society that November he evoked the possibility of accelerating particles artificially through the use of high voltages. This might provide a flexible, adjustable and reliable tool for the experimenter. He spoke in terms of millions of electron volts, and though this reflected the state of understanding at the time – it seemed that nothing less would suffice – it also implied a transformation of laboratory culture. To his audience his vision must have seemed remote, if not far-fetched.

Within days of this address, the young Walton was in Rutherford's office pitching an idea for a short-cut to the production of fast particles. It was, the Irishman would always insist, pure coincidence: he had not read or heard about Rutherford's Royal Society speech and his idea was simply something that had come to him during his period of training in the Cavendish 'nursery'. What he suggested was a means of accelerating electrons by spinning them in a circular electrical field, something that might be achieved on a workbench and without the industrial scale apparatus needed for millions of volts. Rutherford was intrigued and approved the project, but Walton soon met obstacles he could not overcome and so switched to a linear rather than a circular approach. Here too he struggled – in fact both of his ideas would subsequently be proved viable by others, and to a degree Walton was simply ahead of his time. Frustrating though such setbacks must have been, the experience was to bear fruit in three important ways. First, he impressed Rutherford, who saw in him 'an original and able man' who had 'tackled a very difficult problem with energy and skill'. Second, he had begun to acquire expertise in what was a very new field. And third, he had made the acquaintance of John Cockcroft, who was his laboratory neighbour and who was then having the ideas that led to the Nobel experiment.

Cockcroft, the son of a small-time Lancashire mill-owner, was six years older than Walton, had fought in the First World War before taking a PhD in mathematics, and had experience in the electrical power industry, at Metropolitan-Vickers. His mathematics background had equipped him to follow the development of quantum mechanics over the previous couple of years and at the end of 1928, drawing on the insights of the Ukrainian theoretician George Gamow, he detected a possible short-cut to particle acceleration that was rather different from the work Walton had been doing. Applying quantum mechanical principles, Cockcroft found there was a measurable probability that protons would penetrate the nucleus if they were subjected to acceleration by as little as 300,000 electron volts. This would involve a large apparatus by the standards of the time – much larger than Walton's projects – but it was still nothing like the ten-million-volt equipment Rutherford had discussed at the Royal Society. The director immediately gave Cockcroft the go-ahead to begin putting his idea to the test and at the same time designated Walton as his partner.

The challenges were considerable, albeit they were not of a kind likely to be encountered in a modern laboratory with adequate funds. Their apparatus would have several components, notably a transformer, rectifiers, a proton source and an experimental tube, and what they sought from each of these lay beyond the state of the art at the time. Only the transformer could be acquired externally (from Metropolitan-Vickers), though even that had to be of a unique design; the rest was designed and built in the laboratory, almost every component being the result of exhaustive trial and error. The anodes and cathodes in the rectifiers, for example, could not be ordered from a catalogue, nor was there much in the way of literature to provide guidance on their design; instead the best materials had to be identified by long and wearisome testing, and then the best shapes, with everything at every stage crafted by hand. And on those designs depended the shape, size and composition of the glass tubing, which again had to be the subject of both theoretical research and experimental work. The corona effect was a constant concern, and, since the rectifiers operated under vacuum, so were leaks. 'The rate of progress in the lab has been zero lately,' wrote Walton during this period. 'I spent the whole of last week looking for a very small leak in a complicated piece of apparatus. In the end I had to take it all to pieces.' Such setbacks were common.

Nor was the rhythm of life in the Cavendish laboratory well suited to slow, long-term work of this kind. This was a place where the day started gently between 9am and 10am and ended abruptly and without fail, by order of the director, at 6pm, with researchers under instruction to read at home if they were determined to carry on working. In between there could be many interruptions, from the routine afternoon tea-break to symposia, colloquia and tours by eminent visitors – not to mention the disruptions caused by Rutherford's occasional explosions of rage. And the director's instinctive preference for thrift and simplicity could cause problems too. This was a man who boasted that he could do research at the North Pole if he had to, and he ensured that most of the work in his laboratory was done on a shoestring. On top of all this, Walton struggled with academic and funding regimes that normally expected more frequent publication than he could manage while working on a project so large and complex.

He and Cockcroft, none the less, enjoyed some advantages. Notably, they had the wholehearted backing of Rutherford – even though there is evidence that he was not quite convinced by the possibility of penetrating the nucleus with just 300,000 electron voltes. A powerful figure both in the university and on the national scientific scene, the director was ultimately able to ensure that Walton received the grants he needed, and his endorsement secured funding for expensive items such as the transformer. Cockcroft's connections at Metropolitan-Vickers were also helpful: the transformer was acquired at a good price and the pair were also the first to use, in prototype, a revolutionary new vacuum pump that undoubtedly saved them many, many hours of struggle with the liquid air machines that were then the laboratory standard.

By May 1930 they had a working apparatus capable of operating, if not at 300,000 volts then perhaps at 280,000, though they were painfully aware of its fragility. Simply keeping it working absorbed a good deal of time, but they were able to publish a first account of what they were doing and to conduct the first of the experiments for which their cumbersome machine was designed. Target materials were placed beneath the accelerator tube and bombarded with intense beams of protons, and in keeping with the concerns and expectations of physicists at that moment, they looked for gamma ray emissions. In some cases they found nothing and in others there were faint rays of uncertain character, but just as they began to look more closely into this the transformer failed. In the ensuing delay Cockcroft and Walton made the decision to start again and build a bigger, better and more reliable apparatus, even though it would take them at least another year. They did not have confidence in the machine they had built and it's likely that they had lingering doubts that 300,000 volts would not be enough after all. They also knew they would have to move to a new lab space which gave them room to expand – and Cockcroft had come up with an ingenious idea for a circuit that would increase the power at their disposal.

This redesign allowed them to exploit much of what they had learned in the previous couple of years, but it none the less plunged them back into the laborious business of research, trial and error for their many components. This work had been and would be done largely by Walton. Cockcroft was not a natural experimenter and had few gifts when it came to apparatus, moreover he had many other commitments both in the laboratory, where he operated as a kind of manager and consultant, and in the university, where he taught and was the bursar of his college. There can be little doubt that Rutherford had this in mind when he put the two men together: if Cockcroft had been left to do the job alone he would have had neither the time nor the aptitude and it would never have been completed. Walton's particular skills and single-mindedness provided an excellent complement to Cockcroft's managerial ability and mathematical powers. Rutherford would write of Watlon and the project: 'I feel it of great importance that the work on which he is engaged with Dr Cockcroft, viz. the production of high speed atoms, should be developed as rapidly as possible, and Walton is the only student we have who has the ability and technique to carry it out.'

By now Rutherford and his two protegés were aware that they had competition. In Berkeley, California, Ernest Lawrence and Stanley Livingston were developing their cyclotron and breathlessly publishing claims of ever-higher voltages achieved. At Caltech in Pasadena Charles Lauristen had constructed a fine accelerator tube and had access to large power supplies, and at the Carnegie Institution in Washington D.C. Merle Tuve had developed his own tube design and was aiming to link it with the power source offered by his friend Robert van de Graaff's new generator. Every month a private letter or a published articled brought news of further progress.

Early in 1932 Walton and Cockcroft were satisfied that their new apparatus was complete and testing was under way when Rutherford unceremoniously instructed them to get on with some experiments. On 14 April, therefore, with Cockcroft busy elsewhere, Walton installed a target of lithium at the foot of the accelerator tube, and a zinc sulphide screen and microscope to observe what happened. He then fired up the machine, a process that involved turning on an array of air pumps, motors and transformers and gradually increasing the power. After half an hour he left the control desk and, crawling across the floor to avoid electrocution, installed himself in a little observation chamber resembling a tea chest. Putting his eye to the microscope he observed the screen alight with scintillations. After doing some basic tests he summoned Cockcroft, who in turn summoned Rutherford, who summoned James Chadwick. All were in awed agreement about what was happening: protons (atomic weight 1) were entering lithium nuclei (atomic weight 7), which were then dividing into alpha particles (atomic weight 4). The machine, in short, was splitting nuclei in two. More than that, as they soon discovered, it was capable of doing this at voltages as low as 125,000 volts, which no one at that time would have dreamt possible. And some simple measurements revealed that the escape of energy and loss of mass in each reaction corresponded precisely to the equation $e=mc^2$. In its very first hours of operation the cumbersome apparatus in the converted lecture theatre had produced results that would astonish the scientific world.

The news made headlines around the globe, though in those days very few papers were equipped to interpret a scientific breakthrough of this kind. Cockcroft and Walton were suddenly famous as the young men who had split the atom, and they were obliged to tolerate a good deal of wild speculation about the implications of what they had done. For their parts, they carried on with their machine for another couple of years, making new discoveries and attending conferences. Such was the obvious potential of the new technique, however, that very soon they were competing not only with Berkeley, Caltech and the Carnegie but with a dozen other laboratories. Particle physics was born.

Walton soon stepped out of the front line of research, returning to Trinity College, Dublin in 1934 and remaining there in relative quiet for the rest of a long career. Three times during the Second World War he was asked to join the allied scientific effort, first to improve radar and then to develop the atomic bomb, but each time his college insisted he could not be spared. He stepped back into the limelight briefly in 1951 to accept the Nobel Prize. Rutherford had nominated the pair in 1937 but we should not be surprised that it took time to come, for Nobel prizes often arrive late: Max Born (1954) had to wait nearly thirty years for his. In the case of Cockcroft and Walton, moreover, it was explicitly awarded on the basis that the 1932 experiments had ‘opened up a new and fruitful field of research which was eagerly seized upon by scientific workers the world over’. In other words, time had revealed the true importance of the work. And it continues to do so, for particle physics now engages tens of thousands of scientists around the world. In the age of the Large Hadron Collider it is hardly necessary to illustrate or explain its importance and value, but it is worth recalling that Cockcroft-Walton accelerators also have many applications in industry.

The Cockcroft-Walton experiment, as the Nobel citation made clear, was a landmark, and as always with such achievements, others made contributions. It could not have been done, for example, without the theoretical insights of George Gamow. More importantly, it could never have happened without the vision, support and oversight of Ernest Rutherford, whose determination to understand the nucleus was a force of nature in its own right. Rutherford was not only a brilliant scientist; he was also a brilliant judge of men, choosing collaborators and subordinates – Soddy, Hahn, Geiger, Chadwick, Blackett, Oliphant and more – whom he could inspire and guide to outstanding achievements. Walton, the Methody and Trinity boy with the supple mind and clever fingers, was one of those, and it was Rutherford's inspiration to pair him with Cockcroft, knowing as he did that their abilities would complement one another. Walton had dazzled his teachers at school and university, had insisted on going to the Cavendish to conduct atomic research and had swiftly won Rutherford's admiration for his skill and ingenuity. His reward was to be given a very long and extremely difficult challenge to which, in the teeth of strong American competition, he rose with complete success. Neither Cockcroft nor Walton could have done it alone; nor could they have done it without Rutherford.

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# David Robert Bates 1916-1994

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All parents eventually have to decide on the first school which each of their children should attend. Sometimes it is an easy choice: the nearest school to their home, or the school which either or both of the parents attended themselves. For others, the parents will seek out the school which provides the best facilities, as they see them, for the educational development of the child, hopefully bearing in mind the child's natural abilities and aptitudes.

Mr and Mrs Bates chose to send David, and his sister, to a local school in Omagh known as "Miss Quiglie's". It was a single-room, single-teacher school, and it provided David with a grounding in basic skills during the first four years of his educational journey. But Mrs Bates realized the limitations of such an environment for a boy who was showing considerable promise. The family moved to Belfast where David could enrol in Inchmarlo, the preparatory school of the Royal Belfast Academical Institution ("Inst") to which he subsequently progressed. It was not without sacrifice, for David's father continued his business as a pharmacist in Omagh, travelling home to Belfast twice a week.

At Inst, David blossomed academically, excelling particularly in Mathematics and the Science disciplines, and developing a great enjoyment from Chemistry enhanced by the small laboratory he built in an outhouse at home. And so it was that he came to Queen's University in 1934 to study Chemistry and Physics. His first year required the study of four subjects: Chemistry was one, Pure Mathematics was another, and Physics was divided into two – Experimental and Mathematical. It was with this latter pair that he was to find his true academic home, influenced by the superb lectures of the outgoing Harrie Massey in Mathematical Physics and the equally excellent but more measured style of the lectures of George Emeleus in Experimental Physics. It is as true now as it was then that time and effort spent by eminent researchers in a thorough preparation of their lectures and classes can have a profound effect on their students' future development. And so it was with David Bates.

It was no surprise when David took first class honours in Experimental and Mathematical Physics in 1937 and he elected to stay on for a further year, working towards an MSc degree in Mathematical Physics. At that time, Massey was interested *inter alia* in the nature of atomic and molecular processes in the ionosphere. David and another student, JJ Unwin (who sadly died in a flying accident during the Second World War), were jointly set the task of calculating the rate coefficient for the radiative recombination of electrons and atomic oxygen ions which was considered by Sydney Chapman possibly to be an important loss process in the ionosphere, and also to calculate the photoionization cross section of atomic oxygen which it was hoped would solve the problem of the origin of the different ionospheric layers. The work was published in 1939 as David's first research paper[1], with Bates and Unwin joined on the author list by Massey and RA Buckingham, the last named having undertaken checks of the work, always a wise precaution because the calculations had been so extensive and hugely time-consuming, there being only a set of seven-figure logarithm tables to facilitate the arithmetic.

It had been David's intention to follow a career in school teaching, with the ambition of becoming head of a science department, possibly in due course at Inst. But he had been infected by the research bug and he decided, much to Massey's delight, to stay on further and work towards a PhD. His new research task involved the study of a number of atomic and molecular processes which were also of importance in the study of the ionosphere.

They included:

the rate of radiative attachment of electrons to oxygen atoms

$$O + e \rightarrow O^- + h\nu \qquad (1)$$

the rate of photo-detachment from atomic oxygen negative ions by solar radiation

$$O^- + h\nu \rightarrow O + e \qquad (2)$$

and the rate of mutual neutralization of atomic oxygen ions

$$O^+ + O^- \rightarrow O + O \qquad (3)$$

Again, hugely time consuming calculations were required. To assist in this laborious and often tedious work, David had available to him a differential analyzer, constructed in the QUB Physics labs by J Wylie and others[2]. It had four integrators, making possible the solution of radial wave equations which included an electron exchange term. Such an analogue machine still required a great deal of man-power. It did however reveal to David the possibility of machine (eventually electronic) help in undertaking large-scale calculations of atomic and molecular properties, especially those relevant to atmospheric research.

By the end of 1938, David's time in Belfast was coming to an end, fortunately for Queen's of temporary duration. In 1939, Massey moved to the Goldsmith Chair at University College London, and David went with him so as to continue his researches. Sadly the war intervened and both Harrie Massey and David Bates were assigned to the Admiralty Research Laboratory in London where David worked on determining how the magnetic field of a ship, which activated mines, could be reduced by a suitably placed coil carrying an electric current. This work, while being very important, was to David and Harrie rather dull and repetitive and they kept up their spirits by continuing their discussions in an air raid shelter on how to solve problems relating to the ionosphere.

The war over, David and Harrie returned to UCL to continue their research, with David taking up a Lectureship in Mathematics. The war had taken its toll on the buildings of UCL and the work conditions were much less than ideal, but nevertheless the calculations continued unabated. The loss mechanism in the ionosphere had still not been solved, in spite of the calculations of mechanisms (1)-(3), though much had been learnt from them. Bates and Massey proposed a further mechanism – dissociative recombination:

$$O_2^+ + e \rightarrow O + O \qquad (4)$$

In parallel with this work, David began the role of superviser of PhD students. His first such student was Agnete Damgaard, from Denmark. They worked on the determination of oscillator strengths (*f*-values) of radiative absorption lines (or, equivalently, transition rates for the reverse emission process) of ions, which could be described approximately by writing the effective potential seen by an electron in Coulomb form as

$$V(r) = -(Z-N_c)/r \qquad (5)$$

This permitted the solution of the ensuing radial equation to be solved analytically in terms of confluent hypergeometric functions. This simplified form of the potential neglected other inter-electronic effects which were incorporated into the more general potentials used by Douglas Hartree, but was adequate for the treatment of ions with a single electron outside closed shells, and was effective as a first approximation for the calculation of oscillator strengths for an even wider range of elements. This *Coulomb approximation* has stood the test of time, and was still being used at least for comparison purposes over two decades later. “Bates and Damgaard” became a famous paper[3].

David attracted fame via a different source. In 1950, he spent a period of time in the USA. While there, he made the suggestion of seeding the upper atmosphere with sodium vapour from a rocket. He predicted that under suitable conditions a yellow glow would result. The airglow would be caused by sodium atoms: when they are exposed to solar radiation, sodium D-line photons would scatter. It worked, when sodium metal was evaporated into the upper atmosphere at twilight. The brightness was closer to that of the full moon than of Venus at its brightest. David bemoaned to Harrie Massey that such an experiment could not be achieved in London because of a lack of facilities. But before long, it became possible through the newly formed British Space Research Programme. The fame, even notoriety, came when the press got hold of the story. They wrote of Artificial Moonlight. But more scurrilously, when David and Barbara wed quietly in 1956, the headline screamed: “MOONLIGHT PROFESSOR WEDS IN SECRET”!

David’s work on upper atmosphere physics was to form the core of the research undertaken following David’s return to his native Northern Ireland in 1951. He had been appointed to the post in Queen’s which Massey had held prior to his departure in 1939, but now upgraded to the Chair of Applied Mathematics. He and Massey had informally agreed that electron collisions would form the dominant theme at UCL while heavy particle collisions would be the backbone of the research in Belfast. This division was not of course intended to be rigidly fenced, and both groups felt able to work in each other’s area from time to time, especially as the group in Belfast began to grow. Initially, David enlisted Alex Dalgarno, Benno Moiseiwitsch and Alan Stewart who had just completed their PhDs. They formed his “dream team”, and so they acted, not only to further their own research interests, but to nurture a new generation of atomic and molecular physicists who would carry the skills they developed under the careful eye

of David Bates to the far corners of the world. While upper atmosphere processes remained the dominant theme of this small group's research, Benno Moiseiwitsch diversified into theoretical aspects of variational principles, Alan Stewart undertook many pioneering calculations of the electronic structure of atoms and ions, and Alex Dalgarno seemed to diversify into everything.
David took advantage of the growth in the university sector in the UK and slowly the team began to grow, into one of the largest groups of theoretical atomic and molecular physicists in the world. One of the professors in Queen's, from a quite different discipline, once told me that David's group was widely recognized as the jewel in the university's crown. Many of those who joined the group stayed in AMO physics for the rest of their professional lives. Some took positions in the expanding Department of Applied Mathematics in Queen's while others made their mark further afield. It is perhaps invidious to single out individuals from such a group of scientists, but I will mention just three who have gone on to have a major influence in the USA.

Michael McElroy received his PhD from Queen's in 1962 and immediately left for the States, where his key research over a long period has been in Atmospheric Physics. The embryonic development of this work was begun in Belfast, and led to seminal work on the abundance of ozone in the atmosphere and changes in the atmosphere caused by human activity.

Ray Flannery took his PhD in 1964 and remained on the staff in the department for a short while before moving to Georgia Institute of Technology. His research focused on recombination processes in the ozone layer and also in planetary and stellar atmospheres. He was also instrumental in forwarding the reputation of Queen's University in general and David's department in particular, in the United States.

Neal Lane came to the Department in QUB for a short period in the late 1960s, as a postdoctoral research fellow before returning to the United States. In due course he became President of Rice University and during the Clinton administration, he was Assistant to the President for Science and Technology and Director of the White House Office of Science and Technology Policy.

Important though these men were in the scientific and political communities in the United States, they never forgot their time in David's group in their formative years. They held him in high regard, both as a scientist who had

imaginative and innovative scientific ideas and as the leader of a group which had nurtured their early development, setting them on the right road to successful and influential careers. David's influence was very far-reaching.
In 1967, Alex Dalgarno himself moved to the United States, to a chair at Harvard and in due course set up ITAMP, a major centre for theoretical atomic and molecular physics, wide-ranging in its interests and output. In creating this centre, Dalgarno was in many senses modeling the pattern he had observed David Bates follow in the early 1950s in Belfast.

Phil Burke arrived in Belfast in 1967, to fill the chair vacated by Dalgarno. I arrived the same day, but as a postdoc. Phil's interests at that time were in electron-atom or electron-ion scattering but using different methods from those being employed at UCL. Nevertheless, strong collaboration between Queen's and UCL ensued.

The arrival of Phil Burke also heralded an upsurge in computing effort and facilities in the department. In David's early career, the differential analyzer had been a boost to progress through its ability to facilitate the solution of differential equations. David had experienced at first hand the laborious nature of so many of his early calculations, both in Belfast and then in London. He had acquired the QUB department's first electronic computer in 1962 – a DEUCE machine, funded by a research contract from the US Office of Naval Research – which had speeded up many of the group's calculations from that time. Indeed, it was the first computer housed in QUB. It represented a major advance over the differential analyzer, but still required long hours of time, often put in by research students, in order to benefit from this advance. David encouraged the use of electronic computers by members of the group, and the computing needs of the group quickly grew. Phil Burke encouraged application for time and funding to use the best national computing resources. This has enabled the group to remain at the forefront of theoretical AMO research.

I have deliberately avoided discussing David's research in any detail. This is partly because of the breadth of that work, and any description of it in a few pages would be highly selective and unrepresentative. Suffice perhaps to say that his leadership in AMO physics, and particularly in the physics of the upper atmosphere, has been of profound significance for the development of this field. But I wish to mention one piece of work, not because of its significance (which is relatively small) but because it was the one piece of work I did (in 1980) in conjunction with David and it gave me some insight into how he undertook research.

There is an unusually strong feature in the spectrum of an aurora, at 214.5 nm. Several possible explanations of this feature had been proposed previously and had been rejected following calculations of the rates and cross sections of the collision processes. In 1978, a different source of this feature was proposed, the dissociative ionization of $N_2$ by electron impact[4]:

$$N_2 + e \rightarrow N^+(^5S^o) + N + e + e \qquad (6)$$

A critical parameter in determining the likelihood of this process being the source of the feature is the lifetime of the metastable $N^+$ level. In 1980, the only value in the literature for this lifetime was 4.4 $\mu$s[5] , which seemed remarkably short. My calculations gave a figure of 6.4 ms, three orders of magnitude greater. David wrote up his conclusions on the basis of my result, but hesitated to submit the paper for publication. He was wary about rushing into print for such a large change from earlier, supposedly established work. I checked my calculations but could find no reason to change the result I had obtained. Several weeks passed, until one day David came into my office with a smile on his face. He had had a letter from Alex Dalgarno whose value for this lifetime was 5.8 ms, only 10% different from my own. David submitted the paper[6] and Alex separately wrote up his results[7]. Our results tended to rule out (6) as the real source of the feature, although they did slightly leave the issue unanswered. I mention this work largely because it showed me how one should not rush into print without checking tone's work, and how that checking is often best done by means of an independent determination. It is a trait which David picked up from his MSc work, when Massie encouraged Buckingham to check out the results which Bates and Unwin had obtained. It is how science is best done and how scientists can maintain their credibility.

We were all shocked when David Bates suffered a heart attack in 1974. He had coped with a tremendous amount of administrative work, both in maintaining the research thrust and the funding applications which even then were needed for such extensive work, and as Dean of the Faculty of Science which he did with his characteristic thoroughness. Nor did he let his own research diminish through such "outside" tasks. It was clear though that such a heavy load had to be shared, and he passed over the headship of the department to Phil Burke, with Benno Moiseiwitsch taking responsibility for the organization of the teaching. David was appointed to a Special Research Chair, which he filled until he retired in 1982, to become Emeritus Professor.

He was recipient of the Hughes Medal of the Royal Society (1970), the Chree Medal of the Institute of Physics (1973), the Gold Medal of the Royal Astronomical Society (1977) and the Fleming medal of the American Geophysical Union (1987).

David's international reputation was demonstrated by his election as a member of the International Academy of Astronautics (1961), a senior member of the International Academy of Quantum Molecular Science, an honorary foreign member of the American Academy of Arts and Sciences (1974), an associate member of the Royal Academy of Belgium (1979) and a foreign associate of the National Academy of Sciences in the USA (1984). He was also the recipient of honorary degrees from nine universities, both home and abroad.

Two prizes are awarded in his honour, in addition to that awarded within the department to the best final-year QUB student who stays on to undertake a PhD in the research group. The Institute of Physics now awards their David Bates Prize to a young scientist who shows particular promise in AMO physics. This is most fitting, in view of his support and encouragement for young colleagues, over many years. In addition, the Division of Planetary and Solar System Sciences of the European Geosciences Union also awards annually a David Bates Prize and Medal to a scientist who has made exceptional contributions to planetary and solar system sciences.

While his illness in 1974 had been to his family, friends and colleagues a great shock, it did not compare with the much greater shock when the news of his death 20 years later was announced. I happened to be in the departmental office late in the afternoon of Monday 5 January 1994 and took the fateful telephone call. It seemed unbelievable. We knew he was in hospital, but we understood it was for a relatively minor procedure and we had not expected complications. But complications there were, and the very sad news had to be communicated to many people who had known him and worked with him so closely over the years. It was a difficult time for Barbara and their children Adam and Katherine, but in public at least they

remained calm. The funeral was hard for us all, but it was well organized and that helped to ease the sorrow and pain. Yet we knew that an era had passed and we would no longer have his wisdom and knowledge to guide us into the future.
What of his legacy? The research field that he had ably led for so many years was in good shape and was flourishing in many parts of the world, as well as at home. Our understanding of the physical processes of the upper atmosphere had developed considerably over the years, thanks to his pioneering spirit and deep physical insight. But perhaps his greatest legacy is the people whom he influenced and guided in their own careers. As he remarked[8]:

*"Whereas research papers are sadly ephemeral, research students who have passed through one's department normally outlive one. Their successes are therefore a continuing source of gratification."*

He was generous with his time and with his encouragement to younger scientists, always ready to give them every opportunity and due credit for their work. He was characterized by kindness, and though inherently shy, he was outgoing when it came to his relationships with those around him. He was a strong family man, but the family of David and Barbara widened out beyond their nuclear family to embrace all who came to work in his group. They too formed part of a family which has spread far and wide, and carried with them the atmosphere of a unique group led so ably by David. We are fortunate to have known him, to have worked with him, and to have been able to call him our friend.

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# John Stewart Bell 1928-90

*Andrew Whitaker*
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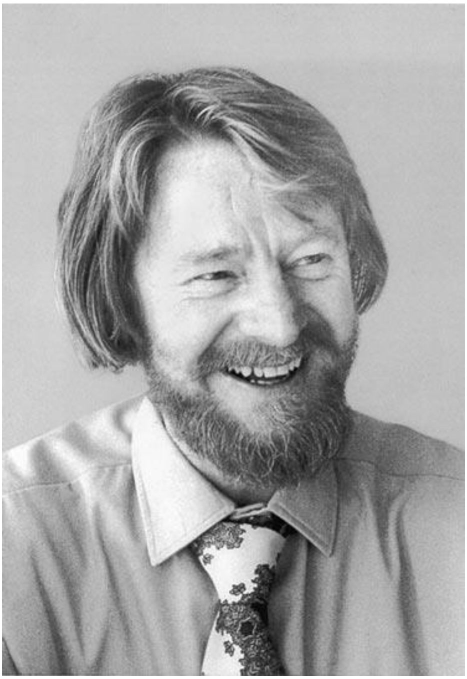
John Stewart Bell investigated the already mature field of quantum theory, and succeeded in achieving a depth of understanding of the theory and its implications for our understanding of the physical Universe that had been previously unsuspected. Indeed Henry Stapp, a distinguished physicist from the Lawrence Berkeley Laboratory in California, has described the famous Bell's theorem as 'the most profound discovery of science'.



Bell was born in Belfast, son of Annie and John Senior; he had an elder sister, Ruby, and two younger brothers, David and Robert. His family was not well-off, but Annie made sure that they did not go without any of the essentials, and, living close to the hills surrounding the west of the city, life was quite pleasant. However one is brought back to earth when one realizes that John was the only one of the siblings who was able to stay at school much after the age of 14. This was not because of lack of ability. All had successful careers, and in particular Robert qualified as an electrical engineer by part-time study, became a college lecturer in Canada and wrote several textbooks.

John himself passed the qualifying examination for grammar school, but to be able to afford to go he would have had to obtain a scholarship, and, though he attempted to do so at every grammar school in the city, he was unsuccessful. Only at the last moment a small amount of money was obtained which was enough for him to take a course at Belfast Technical High School. Though this course was not as academic as those in grammar schools, it qualified him to attend Queen’s University Belfast.

It was not quite clear that he would have the financial backing to enter Queen’s, but in any case he was still only 16, too young to enter the University, and he was fortunate enough to obtain a post as physics technician for the year 1944-5. During this year the two mainstays of the physics department, Professor George Emeleus and Dr Robert Sloane were very kind to Bell, lending his books and enabling him to attend the first-year lectures and take the end-of-year examinations. This enabled him to have a year over at the end of the course, which, as we shall see, he was to put to good use.

It was only just in the nick of time that Bell obtained a small grant from the Cooperative Society, which enabled him to enter Queen’s as a student. (In subsequent years he obtained awards from both Queen’s itself and Belfast City and was comparatively well-off.)  He was an outstanding student and obtained first-class honours in Experimental Physics in 1948.

The only untoward event was a clash he had with Dr Sloane on he fundamental aspects of quantum theory. We shall return to this later.

Bell was now able to use his extra year to take a further degree in Mathematical Physics. Here he came into close contact with Peter Paul Ewald, a refugee from Germany who was in charge of this academic area. Ewald had made crucial contributions at the very beginning of X-ray crystallography thirty years before, and in the inter-year wars had been a world leader in the area, as indeed he was to be again after the war in the United States. Ewald was extremely friendly with Bell and must have encouraged him to aim at a career in research after graduation. Bell obtained another first-class honours degree in this subject in 1949.

He would have loved to work for a PhD but the requirement to earn some money meant that he needed to get a job, and he was successful in obtaining one at probably the most prestigious centre for physics in the UK at the time – Harwell, where efforts were centered around the production of power by the use of nuclear energy.

The Director of Harwell was John Cockcroft, who, with Ernest Walton, had constructed the first particle accelerator. Naturally he was very keen to build up a strong accelerator interest in the establishment, and Bell became a key member of a theoretical group helping with the design of these instruments. The two main types of accelerator were linear accelerators and cyclotrons – actually because of relativistic considerations, pre-war cyclotrons had to be upgraded to synchrotrons. Bell worked on both types though mostly linear accelerators.

Bell's work was to trace the paths of the particles through the accelerators, and to propose arrangements of electric and magnetic fields so as to maximize the focus of the beans. Only desk-top calculators were available, so the work required a sound knowledge of electromagnetism and the ability to use approximations effectively and consistently. His work was excellent, and indeed he was one of the first to study the new technique of strong focusing which consisted of applying both axial and radial focusing. Indeed in 1952 Bell became a consultant to the team designing the proton synchrotron at CERN.

A happy personal event at this time was that he married Mary Ross, who was also working on accelerators and indeed continued to do so for the rest of her career. She had been brought up as a vegetarian, while John had become one in his early teens.

In 1953 he was given an exciting opportunity, a year's leave to carry out some research. His obvious choice was to spend this year at Birmingham working under the celebrated theoretical physicist Rudolf Peierls. During this year he was able to prove the famous CPT theorem, an important theorem in quantum field theory. It says that if a proper physical event at the particle level is subjected to the following operation: C (particles replaced by antiparticles); P (reflection); and T (viewed reversed in time), it is still a proper event. The theorem was also proved by Wolfgang Pauli and Gerhard Luders, and is known as the Pauli-Luders theorem, but Bell had impressed Peierls markedly.

When he returned to Harwell, Peierls arranged for him to join a group working on elementary particles and fields. With another piece of work toadd to the CPT theorem, he was able to obtain a PhD, and for the rest of his life his official work was in this area and he wrote many important papers.

However by the end of the decade, John and Mary thought that Harwell was moving away from genuine research towards contract work, and they moved to CERN, the Centre for European Research in Nuclear (and particle) Physics in Geneva.

Perhaps his most important work here was in so-called anomalies. With Roman Jackiw, he demonstrated the ABJ or Adler-Bell-Jackiw anomaly. (Analogous work was carried out by Stephen Adler.) A simple statement of such an anomaly is that quantization leads to breaking of the symmetries of the classical [i.e. pre-quantum] model. This has been the stimulus for an enormous amount of further work, and indeed the Bell-Jackiw paper is Bell's most-cited paper, it has been cited more times than the famous ones on quantum theory.

A detail of this argument was that the sum of the charges in each generation of fundamental particles must be zero. Now in the first generation are the electron, the electronic neutrino, the up quark and the down quark, with charges -1, 0, 2/3 and -1/3 respectively. These do not sum to zero! However each of the quarks comes in 3 'colours' so multiplying the 2/3 and the -1/3 by 3 gives the required answer. This was not the first proof of 'colour' but it was a nice check.

Another important idea of Bell was that the weak nuclear interaction should be described by a gauge theory. Martinus Veltman and Gerard t'Hooft developed the idea and went on to share the Nobel Prize for Physics in 1999. Today it is know that all the fundamental interactions of physics are based on gauge theories.

The importance of Bell's work was rewarded by his being elected a Fellow of the Royal Society in 1972. Though this date was after the publication of his important quantum papers, it is before they were at all well-known so the election clearly relates to the work on elementary particles and fields.

Now we turn to his work on quantum theory. He called this his 'hobby'. In part this was a joke, but in part very serious. Bell was paid for by CERN to work on particles and fields, and, being a man of great conscience, was determined to carry out, and be seen to carry out, a full day's work in this area. He also spent a great deal of effort on the quantum area, and actually also a lot of time answering letters on quantum matters. This overwork may well have contributed to his early death.

To understand Bell's contributions, it is necessary to look at the history of the conceptual problems of quantum theory. These had been argued about by Niels Bohr and Albert Einstein since the 1920s and 1930s. Bohr was generally though to have solved all the problems with his 'Copenhagen interpretation' of the theory, his main supporters being Werner Heisenberg and Pauli. Einstein strongly disagreed and Bell unashamedly became a follower of Einstein.

We shall examine these conceptual problems by use of a simple example using the spin of an electron. We may say that the spin may have two states that we shall call (+) and (-). If the state is (+), a measurement of the spin gives the answer ½, but is it is (-) the measurement gives the answer -½ .

So far this is straightforward. But a fundamental aspect of quantum theory is that the state may be more complicated. It may be $c_1(+) + c_2(-)$, where $c_1$ and $c_2$ are constants. Experiment then tells us that we will always get the answer ½ or -½, but we do not know which. The probability of getting the result ½ is $(c_1)^2$, while that of getting the result -½ is $(c_2)^2$.

We can immediately see that this violates two central aspects of classical (pre-quantum) physics. It violates determinism because the same state before measurement may lead to either of two different results at measurement. It also violates realism because prior to measurement it does not seem that the spin has a value at all!

It is important to stress that for Einstein, and for Bell following him, physics should be a search for the understanding of a real physical Universe, not just a means of predicting the result of experiments.

One obvious way of solving these problems would be to surmise that the state as given by quantum theory, $c_1(+) + c_2(-)$, may be supplemented by additional information, called a hidden variable. In this simple case, the hidden variable could just be a value of (+) or (-) which would determine the result of the measurement.

But Copenhagen was adamant that there could be no supplementation of the wave-function or the state of the system with hidden variables, and in 1932 the famous mathematician John von Neumann produced a mathematical 'proof' that there could be no such variables.

Through the 1920s and 1930s Einstein had been endeavouring to show that the Copenhagen ideas were flawed. His best attempt was in 1935, the famous EPR-paper which he wrote with Boris Podolsky and Nathan Rosen.

They examined an ENTANGLED state of 2 spins [1 and 2]. This had the form $c\{(+)_1(-)_2 + (-)_1(+)_2\}$ where c is again a constant. This is an extraordinary state, as neither spin has its own properties. If particle 1 is (+), then particle 2 is (-), but if particle 1 is (-) then particle 2 is (+).

Now we allow the particles to separate, and we then measure the spin of particle 1. The result may be (+) in which case the state of particle 2 must at once be (-). Alternatively the result may be (-) in which case the state of particle 2 must at once be (+).

It appears that there are two possibilities.

A The measurement on particle 1 has an immediate effect on particle 2. This is contrary to the third classical virtue – locality, because it seems that a 'message' is being sent at infinite speed. [N.B. This is not actually prohibited by relativity because we cannot use it to send information; nevertheless Einstein would not like it!]

B Each of the particles had a value all along, but this is contrary to Copenhagen. It is a hidden variable and we call this restoring realism.

Einstein had to choose between (no locality) and realism. He was very keen on locality and realism so naturally he chose *local realism*.

It should be mentioned that Bohr produced a 'refutation' of EPR, based on his ideas which he called 'complementarity'. Few read it but it was assumed by all but a very few that he had defeated Einstein. Bell later was highly critical of Bohr's argument and today it is difficult even to see what Bohr meant. But it should again be stressed that very few physicists were at all interested, and even those who were interested felt that it was 'armchair philosophy'. It seemed inconceivable that an experimental test could be suggested and carried out.

We now turn to John Bell, and first discuss his argument with Dr Sloane. It may be mentioned that Dr Sloane had clearly taught himself quantum theory. He was quite capable of explaining to the run-of-the-mill student that if the uncertainty in measurement was large, that in momentum was small, and vice versa – Heisenberg's notorious uncertainly principle. But when Bell asked him what determined whether either uncertainty would be big or small, he really had no answer. Bell accused him of being dishonest!

At least in retrospect this encounter may have been of great benefit to Bell. It showed him that the ideas of a highly intelligent man who claimed to understand the Copenhagen approach to quantum theory were actually rather incoherent. Perhaps the approach was not as sound as everybody appeared to think!

Bell's views on Bohr and Einstein were: 'I felt that Einstein's intellectual superiority over Bohr, in this instance, was enormous; a vast gulf between the man who saw clearly what was needed, and the obscurantist'.

Bohr insisted that the measuring system must be treated as classical, but the measured system must be quantum. Bell asked: 'But where is the division?' [rigorously, not just (as he termed it) FAPP – for all practical purposes]. Bell called this 'the shifty split' and hoped to eliminate it. Of course it disappears if the measured system has hidden variables and so also becomes classical.

Then in 1952, David Bohm produced a hidden variable theory. Normal quantum theory has wave OR particle; Bohm had wave AND particle. Bell was entranced. Much later he wrote: 'In 1952 I saw the impossible done.'

This led Bell to his two great papers published in the 1960s. In the first paper, written in 1964 but not published until 1966, he was able to show that von Neumann's theorem was wrong. In fact, when looked at in the right way, it was 'silly'. Thus the log-jam that had halted the study of the foundations of quantum theory for thirty years had been removed.

Bell had noted that, although Bohm's theory obeyed realism and determinism, it was rather flagrantly non-local. He wondered whether all hidden variable theories were non-local, and in his second great paper, published in 1964, he studied an EPR-type experiment but analyzed the spins in each wing of the experiment along different directions. He was able to show that IF quantum theory were true, there were no local hidden variable theories i.e. Einstein's wish had not come true. Local realism was ruled out. It is in this sense (only) that Bell is sometimes called 'the man who proved Einstein wrong'.

This work was the stimulus for Stapp's remake quoted at the beginning of this article. He argued that until this moment it was assumed that physics consisted of 'things' [i.e. realism] interacting in a local way. Bell's work showed that this was untrue.

But was quantum theory true for these exceptional cases? Bell had produced the famous 'Bell inequalities' that were obeyed by local realistic theories but disobeyed by quantum theory. These could be tested, and this process was called 'experimental philosophy' because ideas of philosophical depth were being tested in the laboratory.

The first person to do so was John Clauser in 1972 using entangled photons; he obtained results agreeing with quantum theory and so disagreeing with local realism. While that conclusion is certainly correct, the experiments had loopholes, which have been very difficult to remove.

The detector loophole is that the detectors register comparatively few of the photons incident on them. The locality loophole is that the detectors in the two wings of the experiment could communicate with each other. Alain Aspect made considerable strides to removing the latter in experiments using lasers in 1980-82, and further progress on removing the loopholes has been slow but steady. Finally in 2014, on the fiftieth anniversary of Bell's paper, it seems that a single experiment removing all the loopholes is very close.

It may be said that Bell's work has stimulated the emergence of two major scientific fields. The first is quantum foundations – very much contrary to the wishes of the upholders of Copenhagen, the interpretation of quantum theory is very much open to discussion. The second is quantum information theory; the ideas of Bell, particularly his stress on entanglement, have encouraged an enormous amount of work on work on quantum computation, quantum cryptography and quantum teleportation.

Sadly John Bell himself died rather suddenly of a stroke in 1990.

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# Tait's Belfast Appointment: Professor of Mathematics at the Queen's College (1854–1860)

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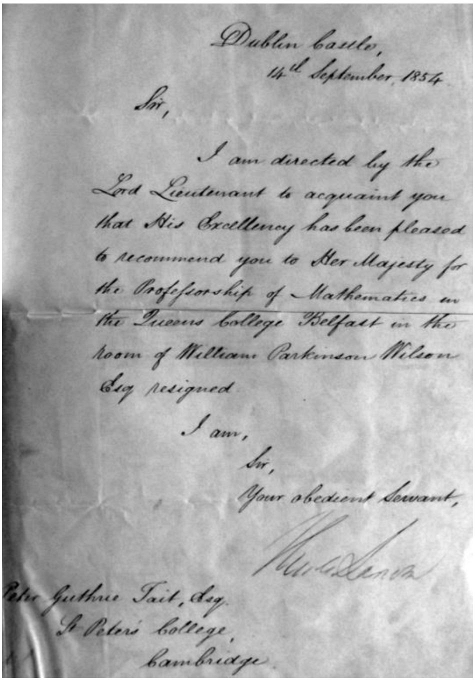
## Appointment to the Queen's College, Belfast in 1854

Peter Guthrie Tait (1831–1901), F.R.S.E., Professor of Mathematics at the Queen's College, Belfast (Q.C.B.) and Professor of Natural Philosophy at the University of Edinburgh, was one of the leading physicists and mathematicians in Europe in the nineteenth century. He was appointed to the mathematics professorship at the Q.C.B. on 14 September 1854 in succession to William Parkinson Wilson (1826–1874), the founder Professor of Mathematics in the college. See Figure 1.



Dublin Castle,
14th September, 1854

Sir,

I am directed by the Lord Lieutenant to acquaint you that His Excellency has been pleased to recommend you to Her Majesty for the Professorship of Mathematics in the Queens College Belfast in the room of William Parkinson Wilson Esq resigned

I am,
Sir,
Your obedient Servant,
[illegible]

Peter Guthrie Tait, Esq.
St Peter's College,
Cambridge

**Figure 1.**

Letter (dated 14 September 1854) sent to Tait on behalf of the Lord Lieutenant of Ireland (Edward Granville Eliot), confirming Tait's appointment to the mathematics professorship at the Q.C.B. in succession to Wilson. At that time only the Crown had the authority to appoint and dismiss professors in the Queen's Colleges. The Lord Lieutenant of Ireland was the British Monarch's official representative in Ireland. The original letter is preserved in Tait's scrapbook at the birthplace of James Clerk Maxwell, 14 India Street, Edinburgh. Courtesy of the James Clerk Maxwell Foundation.

Tait had come from Peterhouse, Cambridge: he was two years into his Fellowship. In 1852 he had been Senior Wrangler and First Smith's Prizeman. This means Tait had placed first amongst graduates taking first-class degrees in mathematics at Cambridge that year and that he had also placed first in special examinations in mathematics and natural philosophy. Figure 2 shows a portrait of Tait which dates from his Cambridge days. Prior to Cambridge, Tait had spent one session at the University of Edinburgh where he had studied mathematics under Philip Kelland (1808–1879) and natural philosophy under J. D. Forbes (1809–1868). When Forbes went to St Andrews as Principal of the United College in 1860, Tait left Belfast and returned to Edinburgh to take up the Chair vacated by Forbes.

**Figure 2**. P. G. Tait from the album of *Forty-six portraits of pupils of William Hopkins*, created by Thomas Charles Wageman between 1829 and 1852. Courtesy of the Master and Fellows of Trinity College, Cambridge.

**Professors of Mathematics at the Queen's College, Belfast**

The Queen's Colleges at Belfast, Cork and Galway were founded in 1845; the first professors were appointed in August 1849 and lectures began in October, with the formal opening of the Q.C.B. in December. Figure 3 shows the Q.C.B. as it was around 1861.

**Figure 3.** The Q.C.B. c.1861 from Moody T W and Beckett J C 1959 *Queen's Belfast 1845–1949: The History of a University* vol 1 (London: Faber & Faber) frontispiece.

The first few professors of mathematics at the Q.C.B.—Wilson, Tait and the Scotsman, George Middleton Slesser (1834–1862) who succeeded Tait—all fit a common profile: all three had been Senior Wranglers at Cambridge and Fellows of Cambridge colleges; two (Wilson and Tait) had also been First Smith's Prizemen; and all were remarkably young when they were appointed to the Q.C.B. During the first one hundred years of its history, the average age of professors in the Q.C.B. at the time of their appointment was just over thirty-eight; and during that period only four professors were appointed at the age of twenty-four or younger (Wilson and Tait being two of them). Another coincidence is that Wilson and Tait—who had both arrived at the Q.C.B. without having had any prior publications—published, during their professorships at the Q.C.B., a textbook on dynamics. Tait's book, *Dynamics of a Particle*, which was written with his Cambridge friend William Steele(1831–1855), was published in 1856. Slesser's death in 1862 affected the end of the succession of young Cambridge Senior Wranglers appointed to the mathematics professorship at the Q.C.B: after Slesser the professorship was taken over by John Purser (1835–1903) who was born in Dublin and had studied at Trinity College, Dublin. Purser dedicated thirty eight years of service to the Q.C.B. as Professor of Mathematics.

Mathematics was an important part of the undergraduate curriculum at the Queen's Colleges: it was compulsory for all first-year Arts students and for all first- and second-year engineering students. Second-year Arts students had the option to choose between a higher course of mathematics and the classics, namely Latin and Greek. From the evidence gathered by a Royal Commission of 1857—which looked into the performance of the Queen's Colleges and the Queen's University (their degree-granting authority)—we have information on Tait's professorial duties and the nature of the instruction he gave in mathematics. From this source we know that in his first year at the Q.C.B. Tait gave 330 lectures; that he had thirty-three first-year students and eleven second-year students; and that he lectured for two hours a day, every weekday, between 9 and 11 a.m. (first-years at 9 a.m. and second-years at 10 a.m.).

Tait's first-year students were taught in two divisions because it was evident to Tait at matriculation examinations that some entrant students had a much better knowledge of mathematics than others. In the teaching of the lower division, Tait was assisted by a senior mathematics scholar. This enabled Tait to devote his attention to the more able students in the higher division. Tait's lower mathematics division studied: Euclid (Books I-IV, VI), algebra up to progressions and plane trigonometry up to the solution of triangles. The higher division followed 'a complete Course of Algebra and Trigonometry, with the principal historical properties of the Conic Sections, treated geometrically'.[i] In his lectures for second-years, Tait covered: analytical geometry, the differential and integral calculus, and the first three sections of Newton's *Principia.* And if quick progress was made, Tait would also cover: the solution of differential equations (those of an elementary nature) and the principles of analytical geometry in three dimensions. For the best insight into the material covered in Tait's lectures, consult past examination papers. Those sat in 1857 can be found in the Commissioners' *Report*.

In addition to mathematics, Tait also taught an advanced natural philosophy class in place of the Professor of Natural Philosophy, John Stevelly (1794/5–1868) who was in poor health. This class was a new initiative in 1854. Attendance at the class was voluntary: it was intended for students hoping to take honours. During the first year it ran, the class was attended by six students and it took place twice weekly. Tait instructed these students in 'the applications of Analytic Geometry and the Differential and Integral Calculus to Statics, Dynamics, and Physical Optics, and those portions of Astronomy that are particularly Physical'.[ii]

**Tait's Belfast associations**

During the six-year period that Tait spent at the Q.C.B. he established a number of happy and profitable associations, especially with those professors belonging to the science division of the Arts faculty. The science division occupied the northern wing of the college.

**Thomas Andrews, Vice-President and Professor of Chemistry**

Through Thomas Andrews (1813–1885) Tait was introduced both to experimental work and to Sir William Rowan Hamilton (1805–1865), who Benjamin Pierce described as 'the immortal author of quaternions'.[iii]

When Tait arrived in Belfast in 1854, Andrews was already engaged in his researches on ozone and had published important work on the subject. Andrews usually worked alone but at the end of 1856 he asked Tait to join him in his investigations to determine the volumetric relations of ozone. These researches occupied Tait and Andrews until the start of 1860, when they began a new line of investigation concerning the compression of gases. Tait and Andrews worked together on this until the summer of 1860 when Tait resigned his Belfast Chair. Tait's role was principally to help Andrews with the calculations and to construct the apparatus used in their investigations. Tait's biographer writes: '[Tait] proved such an apt pupil in the art of glass blowing that ere long Andrews gave that part of the manipulation over to his eager and energetic companion.'[iv]

Tait greatly benefited from working alongside Andrews. Andrews gave Tait his first real introduction to experimental work and fostered in him a love of chemistry. Tait said that it was in Andrews' laboratory that he 'first learned properly to use scientific apparatus' and that it was Andrews' 'sage counsel [which] impressed upon [him] the paramount importance of scientific accuracy, and, above all, of scientific honesty'.[v] Tait's collaboration with Andrews led to Tait's first published papers: three papers on ozone, written jointly with Andrews, were presented to the Royal Society of London and printed in Andrews' *Scientific Papers*.[vi]

Tait was very fortunate to have come into contact with Andrews so early on in his career: Andrews took Tait under his wing and acted as his mentor. Andrews– who was eighteen years Tait's senior – was patient, approachable and knowledgeable. Tait said Andrews was 'thoroughly trustworthy and warm-hearted; an excellent example of the true Christian philosopher'.[vii]

Andrews' temperament contrasted but complemented Tait's impatient zeal and youthful exuberance. Tait's biographer writes:

Tait used to speak with intense admiration of the extreme care and patience with which Andrews carried out all his researches. […] At times indeed the patient care of the skilled experimenter must have chafed somewhat the brilliant young mathematician ever eager to get to the heart of things; but no amount of argument or theorising on Tait's part could move the master from the steady tenor of his way. Years after when Andrews in his failing health visited Edinburgh Physical Laboratory to inspect a set of his own apparatus for the liquefaction of gases it was at once a privilege and an inspiration to witness the deep affection and admiration with which Tait regarded his whilom colleague.[viii]

Tait expressed his gratitude to Andrews in a letter of condolence he wrote to Andrews' widow following the death of Andrews in 1885. Tait wrote:

For my own part, I feel that I cannot adequately express my obligation to him whether as instructor or example. I have always regarded it as one of the most important determining factors in my own life (private as well as scientific) and one for which I cannot be sufficiently thankful, that my appointment to the Queen's College at the age of 23 brought me for six years into almost daily association with such a friend.[ix]

Another immeasurable benefit of Tait's association with Andrews was that Andrews facilitated the correspondence between Tait and Sir William Rowan Hamilton. Tait had bought a copy of Hamilton's *Lectures on Quaternions* in 1853 while he was still resident at Peterhouse. He quickly understood the significance of Hamilton's quaternions but was having trouble with the physical applications of the quaternionic method, which was his primary interest in the theory. In August 1858 Andrews wrote a letter of introduction to Hamilton on Tait's behalf. Andrews had studied at Trinity College, Dublin where he had attended Hamilton's twice-weekly lectures on astronomy. Andrews wrote to Hamilton:

Our Professor of Mathematics (author of a work on Dynamics and formerly a senior wrangler at Cambridge) has been directing his attention of late to Quaternions, and is anxious to be allowed to correspond with you on that subject. Mr. Tait is a young man of excellent abilities, and, I believe, a very good mathematician; and I have therefore no hesitation in introducing him to you.[x]

Hamilton was happy to correspond with Tait on the subject of quaternions but he warned that he might be out of practice as he had since moved on from quaternions and at present was engaged in other researches.

The subsequent correspondence between Tait and Hamilton proved mutually beneficial. Tait, having the benefit of correspondence with Hamilton, could start to make real progress with quaternions. In 1859 Tait's first papers on quaternions were published in the *Quarterly Journal of Mathematics*.[xi] That same year Tait and Hamilton met for the first time at the British Association meeting in Aberdeen. Encouraged by Tait's enthusiasm, Hamilton resumed his interest in quaternions and began work on a new book, his *Elements of Quaternions*.

The relationship between Tait and Hamilton was not without its tensions as a misunderstanding arose over Tait's plans to publish his own treatise on quaternions which threatened their cordial relationship. Hamilton had given Tait permission to publish a set of examples on the application of the quaternionic method but he believed that Tait was to publish, instead, a full exposition of the theory which would appear before his own *Elements*. Hamilton misunderstood the planned scope of Tait's project and became suspicious of his motives; he feared that he had been deceived by Tait. Fortunately, Tait was able to put Hamilton at ease by agreeing to delay the publication of his book until Hamilton's *Elements* had appeared. And so it is that Tait's *Elementary Treatise on Quaternions* was finally published in 1867; one year after the posthumous publication of Hamilton's *Elements*.

Hamilton's quaternions captivated Tait and inspired a lifetime of mathematical research. Tait became 'Hamilton's chief disciple'[xii]; the leading expounder of the quaternionic theory and the foremost advocate for its use in physics. It was his blind loyalty to Hamilton which led Tait into conflict in the 1890s with the Englishman, Oliver Heaviside (1850–1925) and the American, J. Willard Gibbs (1839 – 1903), whose vector calculus threatened Hamilton's quaternions.

**Sir Charles Wyville Thomson, Professor of Mineralogy and Geology**

Tait's Belfast association with Charles Wyville Thomson (1830–1882) led Tait on to important scientific work which he conducted in Edinburgh in the 1870s, in connection with the *Challenger* Expedition. The *Challenger* Expedition was a four year voyage of scientific discovery of the world's oceans, conducted between 1872 and 1876. It marked the beginning of the modern science of oceanography.

Charles Wyville Thomson was a Scotsman: he was born in Linlithgow. He was only one year younger than Tait and had started at the Q.C.B. the very same year as Tait. Wyville Thomson was a 'brilliant young zoologist'[xiii] who had come to Ireland in 1853 as Professor of Natural History at the Queen's College, Cork. In 1854 he was appointed to the professorship of mineralogy and geology at the Q.C.B., where he remained until 1870 when he went to the University of Edinburgh as Professor of Natural History.

When HMS *Challenger* returned from its voyage in 1876, Wyville Thomson—who had been appointed as the scientific leader of the Expedition—had to oversee the analysis of the specimens and the publication of the results in the Expedition *Reports*. He carried out these duties from an office which he had set up in Edinburgh. He enlisted the help of the very best specialists—in diverse disciplines and from across the globe—to analyse the specimens and interpret the results.

Wyville Thomson asked the following of Tait: to work out the errors that would need to be applied to the temperature readings which had been recorded by the self-recording deep-sea thermometers, in order to compensate for the high-pressure conditions; Tait was given the set of observations and the *Challenger* thermometers (Figure 4). The *Challenger* thermometers were constructed using Six's thermometer design**:** in 1782 the Englishman, James Six (1731–1793) had invented a thermometer capable of recording the maximum and minimum temperatures reached over a period of time at a particular location.

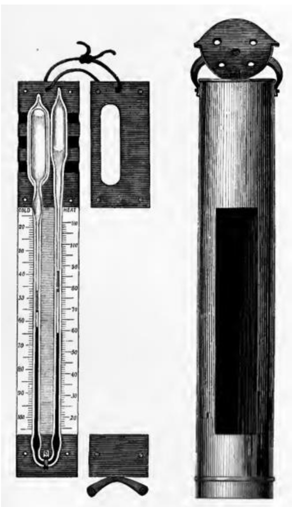


**Figure 4.** The *Challenger* thermometers from Tait P G 1898 *Scientific Papers* vol 1 (Cambridge: at the University Press) p 461.

Before the expedition, Captain J. E. Davis of the Admiralty had conducted some experiments and determined that a correction of at least half a degree Fahrenheit for every mile depth under the sea had to be applied to the temperature readings which had been recorded. He believed that pressure would have a bearing on temperature. Tait, having examined the thermometers, was convinced that only a small correction, if any, would need to be made; and so he set about retesting the thermometers, so as to understand how Captain Davis had arrived at his correction figure. To recreate the high-pressure conditions in his Edinburgh laboratory Tait used a hydrostatic press.

After exhaustive research, conduced over a period of three years, Tait arrived at five possible causes of temperature change; the principal source of temperature change being the heating of the vulcanite mounting of the thermometers due to compression. This, as Tait understood, would be of no consequence in the deep-sea experiments, so Tait ultimately decided that no correction of the recorded thermometer readings was necessary.

In his experiments on the *Challenger* thermometers Tait had to have a way to accurately measure the pressure inside the hydrostatic press. Tait, considering all the pressure gauges then available to be untrustworthy, was led to design a simple, yet ingenious, device known as the Tait Gauge (Figure 5). He went on to use the Tait Gauge in further important scientific work, which followed on from his work on the *Challenger* thermometers. He engaged in research on: the compressibility of water, glass, mercury; the physical properties of fresh water and sea water; the effects of pressure on the maximum density point of water; and so on.

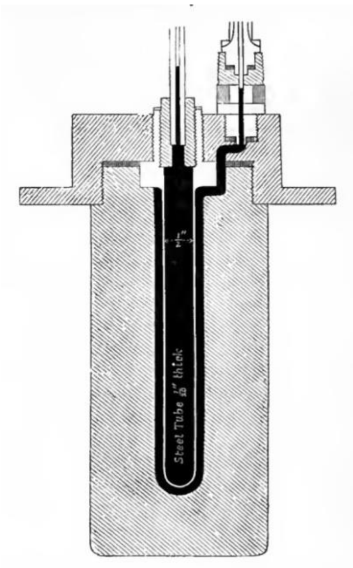


**Figure 5.** The Tait Gauge from Tait P G 1898 *Scientific Papers* vol 1 (Cambridge: at the University Press) p 476.

## James Thomson, Professor of Civil Engineering

During his career Tait published 365 papers and twenty-two books; but he remains best remembered as the co-author, with William Thomson (1824–1907), of the epoch-making *Treatise on Natural Philosophy*, which was a contemporary treatment of the subject in terms of the new physics of energy which emerged in the 1850s. When Tait met William Thomson for the first time in 1860, he had already known his brother, the Belfast-born engineer, James Thomson (1822–1892) for six years.

James Thomson was appointed Professor of Engineering at the Q.C.B. in 1857, though he had been Acting Professor of Engineering since 1854. In 1873 he went to the University of Glasgow to take up the Engineering Chair in succession to the late Macquorn Rankine (1820–1872). James, though remarkable in his own right—for his original work on the dynamical theory of heat, especially on the lowering of the freezing-point of water by pressure, and for his Turbine and Vortex Pumps inventions—has been somewhat overshadowed by his younger brother, William. William Thomson, later Lord Kelvin, was Professor of Natural Philosophy at the University of Glasgow.

Soon after meeting, Tait and William Thomson decided that they ought to produce a book together on natural philosophy. Theirs was an ambitious project, which demanded eighteen years of collaboration; and in that time only the first of a series of planned volumes ever saw publication. In fact, it is said that the work would never have been published had it not been for Tait's 'dogged persistence'[xiv]: Thomson disliked book writing and over the years he became much wearied by the project and his enthusiasm waned. For a time Tait used *Treatise on Natural Philosophy*, also known as "*Thomson and Tait*" or *T and T′*, as a teaching guide at Edinburgh. To the students it must have seemed a formidable text, for they referred to it as the 'Student's First Glimpse of Hades'[xv]. Tait's biographer Knott referred to the work in more positive terms, writing: 'The publication of Thomson and Tait's *Natural Philosophy* was an event of the first importance in the history of physical science. No more momentous work had been given to the world since the days of the brilliant French mathematicians, Laplace, Lagrange, and Fourier.'[xvi]

## The Porter family

At Cambridge Tait had made the acquaintance of the Porter brothers, William Archer Porter and James Porter (1827–1900). James went on to become Master of Peterhouse and Vice-Chancellor. William and James' father was a presbyterian minister in Drumlee, Castlewellan, Co. Down and a father of twelve. During his time in Belfast Tait grew close to the Porter family; and in October 1857, shortly before the commencement of the new academic year, Tait married one of Porter girls, Margaret Archer Porter (1839–1926). They married in Shankill, Antrim. Tait was twenty-six and Margaret was just eighteen. Tait's friend and collaborator William Thomson once recalled the personal happiness that Tait had found in Belfast. He wrote: 'During those bright years in Belfast [Tait] found his wife, and laid the foundation of a happiness which lasted as long as his life.'[xvii] Tait and Margaret went on to have six children and their first child Edith was born in Belfast in 1860.

## Reasons behind Tait's resignation

The fact that Tait had made the transition from bachelor to family man in Belfast probably played a part in his decision to resign from his professorship at the Q.C.B.: Tait now had a family to support and the professorial income at the Queen's Colleges (Table 1) was significantly lower than in the Scottish universities.

| Subject | Fixed salary | Class fees | Total |
|---|---|---|---|
| Anatomy and Physiology | £200 | £444 | £644 |
| *Mathematics (Tait)* | *£250* | *£332* | *£582* |
| Natural Philosophy | £250 | £317 | £567 |
| Midwifery | £100 | £128 | £228 |
| ... | ... | ... | ... |
| Celtic Languages | £100 | — | £100 |
| Natural Philosophy | £250 | £317 | £567 |

**Table 1.** Average total income of professors at the Q.C.B. calculated for the 6-year period of Tait's professorship (1854–1860) using data from Moody T W and Beckett J C 1959 *Queen's Belfast 1845 – 1949: The History of a University* vol 2 (London: Faber & Faber) pp 703–704.

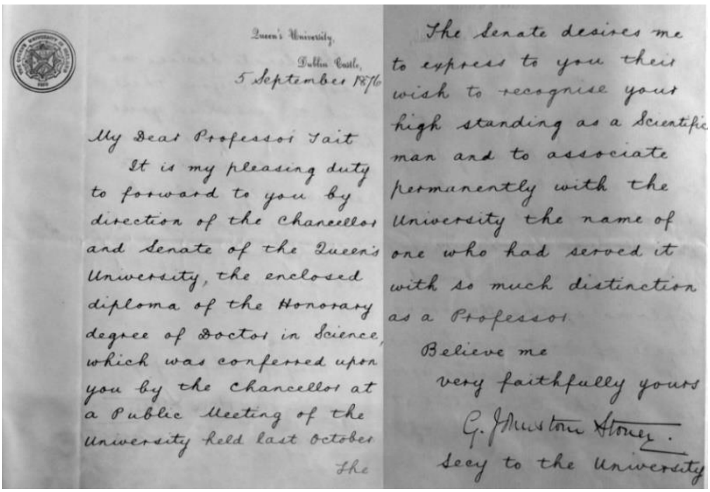
At the Queen's Colleges, as in the Scottish universities, a professor's income came from two sources: a fixed salary and class fees paid by the students who attended lectures in that subject.

Of course, there were other factors at play, besides income, in Tait's decision to return to Edinburgh: the position at Edinburgh was a more prestigious position; and Tait's interest lay in the application of mathematics to physics, rather than in pure mathematics, so the professorship at Edinburgh was more suited to his own particular interests than his Belfast professorship.

## Conclusion

At the Q.C.B. Tait benefited from a nurturing environment. He had arrived at the college with a proven record of academic excellence but he was still only twenty-three and at the very beginning of his career. Six years later when he resigned from his professorship, Tait left Belfast well-equipped for life as a successful professor in Edinburgh: he had gained experimental experience through Andrews; he had published his first scientific papers on ozone and quaternions, and his first book, *Dynamics of a Particle*, and he had honed his natural abilities in original scientific investigation and as a gifted expositor. Indeed, the reason that Tait succeeded over other excellent candidates for the Edinburgh Chair—including Maxwell, Routh and Fuller—was because he had already established at Belfast a reputation for having remarkable powers of exposition in the lecture theatre. But most important of all, Tait had established at the Q.C.B. a number of associations, indeed friendships, which would lead to future collaborations and important scientific work.

So, initially, Tait did not have strong Belfast connections, he spent only a short time in Belfast and his real achievements were made in Edinburgh, not in Belfast; however, the role that the Q.C.B. played in Tait's formation and the strength of the relationships that Tait forged there, the scientific networks that had been established, make Tait's appointment as Professor of Mathematics at the Q.C.B. not only a significant period in his own life but, in a wider context, in the history of science. In recognition of his scientific achievements, Tait received an Honorary D.Sc. (1875) from the Queen's University in Ireland (Figure 6) and Honorary Fellowship of the Royal Irish Academy (1900).



Queen's University,
Dublin Castle,
5 September 1876

My Dear Professor Tait

It is my pleasing duty to forward to you by direction of the Chancellor and Senate of the Queen's University, the enclosed diploma of the Honorary degree of Doctor in Science, which was conferred upon you by the Chancellor at a Public Meeting of the University held last October

The Senate desires me to express to you their wish to recognise your high standing as a Scientific man and to associate permanently with the University the name of one who had served it with so much distinction as a Professor

Believe me
very faithfully yours
G. Johnstone Stoney
Secy to the University

**Figure 6.** Letter (dated 5 September 1876) sent along with Tait's diploma from the Queen's University in Ireland. The original letter is preserved in Tait's scrapbook. Courtesy of the James Clerk Maxwell Foundation.

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[ii] Ibid.

[iii] Hankins T L 1980 *Sir William Rowan Hamilton* (Baltimore: The John Hopkins University Press) p 323.

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[v] Andrews T, Tait P G and Crum Brown A 1889 *The Scientific Papers of the Late Thomas Andrews* (London: Macmillan and Co.) p xxxiii.

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[vii] Andrews T, Tait P G and Crum Brown A 1889 p xxxvi.

[viii] Knott C G 1911 p 13.

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[x] Andrews T, Tait P G and Crum Brown A 1889 p xxviii.

[xi] 'Quaternion Investigations Connected with Fresnel's Wave-Surface' (1859) *Quart. Journ. Math.* 'Note on the Cartesian Equation of the Wave-Surface' (1859) *Quart. Journ. Math.* ***3***. Reprinted in Tait P G 1898 and 1900 *Scientific Papers* 2 vols (Cambridge: at the University Press).

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[xiv] Campbell N and Smellie R M S 1983 *The Royal Society of Edinburgh (1783–1983): The First Two Hundred Years* (Edinburgh: The Royal Society of Edinburgh) p 35.

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[xvi] Knott C G 1911 p 176.

[xvii] Thomson W 1911 Obituary Notice of Professor Tait *Mathematical and Physical Papers* vol 6 ed J Larmor (Cambridge: at the University Press) p 364.