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Readers will have noticed that this is the first newsletter to appear this year. The reason is simply that I have had insufficient material to justify one. For various reasons it was unfortunately not possible to use any of the lectures from the 'Superconductvity' meeting in July and these normally provide the core of most of the newsletters.

Financial constraints on our budget have meant that this issue has been hastily produced in order to meet the financial year end requirements and consequently even this includes only one of the talks given at the 'AGM meeting' last November.

Another, Dr. Anthony Constable's fascinating talk entitled 'Kaye and Laby - a Centenary', celebrating the first year of that ubiquitous publication - 1911 - clearly belongs in this issue but has had to be held over until next year when I'm sure it will provide some enjoyable springtime reading.

I do hope you will continue to support the group and its newsletter by way of sending in articles, long or short, news of events etc. concerning our subject as I'm sure we all are dedicated to Physics and its History.

After all as Ernest Rutherford once said in his inimitable modest way:

'Science is divided into two classes – Physics and stamp collecting.'

Malcolm Cooper

Editor

AGM report

The Chairman reported that as well as today's very successful meeting, entitled "Some anniversaries in Physics", there had been an equally successful meeting held at Cambridge on 11 and 12 July 2011, organised jointly with the IOP Superconductivity Group and the Low Temperature Group, to mark the centenary of the discovery of Superconductivity by H Kammerlingh Onnes in Leiden, and the 25th anniversary of High-Temperature Superconductivity. The first day's talks had reviewed the history of superconductivity, while the second day had concentrated on current research.

Future meetings under consideration include a meeting at the Bath Royal Literary and Scientific Institute (BRLSI) in June or July, to celebrate the legacy of the Braggs. 2012 has been designated "International Year of Crystallography", as it is the centenary of Bragg's Law.

The special issue of the newsletter on Lord Rayleigh had been a great success but sadly there had not been an issue recently, for lack of material but it is hoped to have a special issue on Rutherford as well as the normal issues.

The current Chairman (Peter Ford) and Hon Sec (John Roche) were elected to continue in office and Malcolm Cooper was persuaded to continue as Hon Treasurer, and was elected *nem con*. Two members of the committee, Peter Borcherds and Stuart Richardson retired. The Chairman thanked them for their valuable service and two new members were elected to replace them: Chris Green and Peter Rowlands.

In addition to the official Group website at www.iop.org/activity/groups/subject/hp/ Kate Crenell now maintains her own website, which Group "members may be interested to see", at www.physicshistory.org.uk This contains a generous quantity of archive material, for which there is no room on the official website.

It is hoped to arrange an International Conference on the History of Physics jointly between the IOP and EPS. This will be a considerable undertaking and will require a great deal of planning. *

It was suggested that the Brian Gee legacy could be put towards a booklet of Blue Plaques, which could also include what material IOP already has. Further suggestions included plaques to Alan Turing at Sherbourne School and to William Prout, in a Gloucestershire churchyard.

It was proposed that the Group should broaden its remit and concern itself not only with the history of British physics but also with that of Europe. This would, of course, require a change to the constitution.

John Roche

*Any suggestions, comments to Editor

The History of X-Rays

Professor Angela Newing

On 8th November 1895, Professor Wilhelm Röntgen was carrying out experiments in his laboratory in the University of Würzburg. On passing a high voltage current from an induction coil through an evacuated Crookes tube covered with black paper, he noticed a glow on a fluorescent screen on his bench nearby. He noticed that the glow was strongest nearest the anode of the tube and concluded that some radiation which was invisible to the human eye was being given off by the anode. He called this 'X-rays' because he had no idea at the time of the exact nature of the radiation.
The necessary properties for the production of x-rays are, high vacuum, high voltage and a discharge tube with suitable geometry, and all these were available to Röntgen at this time.

Having realised that his x-rays went through both glass and paper, Röntgen set out to find what else could be penetrated by them and he spent the next month experimenting with various bits of wood and metal. The x-rays were generated from the glass of the tube and Röntgen's work was mostly done by passing the rays into his zinc lined darkroom through a small aperture in the wall. This, unknowingly, provided him with personal radiation protection. He tried deflecting the cathode rays with a magnet and found that the x-rays then came from a new spot on the tube wall. At this time Röntgen's wife was concerned about his health because he spent so much time in the laboratory and had little interest in food or in conversation. She persuaded him to tell her about his discovery and on 22nd December he demonstrated it to her by getting her to put her hand on a photographic plate in the beam for 15 minutes. The subsequent picture was the first human radiograph.

Röntgen's first paper 'Über Eine Neue Art von Strahlen' was published on 28th December and was notified to scientific colleagues in Germany and elsewhere. Various physicists repeated the experiments and the popular press took an interest. The 1896 Great Exhibition At Crystal Palace, London included an x-ray demonstration as did the Exposition of the

Electric Light Association organised by Thomas Edison in New York in the summer of 1896.
Röntgen was awarded the first ever Nobel Prize for Physics in 1901.

Diagnostic x-ray equipment was provided to the various fighting units of the army in South Africa and elsewhere to help to locate foreign bodies such as bullets as well as showing broken bones. Soon, diagnostic x-ray departments were set up in several hospitals. By 1898, dense substances such as barium were introduced into the oesophagus to show abnormalities of the alimentary tract. All this without knowledge of the harmful effects of x-rays until the operators of the apparatus began to develop radiation induced injuries.

The next major development of the use of x-rays for diagnosis occurred in 1968. Godfrey (later Sir Godfrey) Hounsfield was an engineer working for EMI in London when he felt that conventional x-ray methods of tomography (the production of cross sectional information) were very primitive and could be improved by computer methods. He set up a lathe bed with a slice of pickled human brain and rotated a narrow beam of gamma rays around it with a detector opposite. This arrangement produced information of attenuation coefficients for each ray which, when analysed by computer, was able to give a cross sectional reconstruction. This was a very time consuming experiment, but he was able to cut down the time taken by using a fan beam of x-rays and the first CT scanner was produced for the Atkinson Morley Hospital in London in 1972. Hounsfield and Allan Cormack, a South African Medical physicist, shared the Nobel Prize for Physics in 1979, by which time CT Scanners were widely available.

The first attempt to use x-rays for treatment (radiotherapy), made only six months after their discovery, was by Dr Leopold Freund in Vienna. He had heard that a man working with x-rays had contracted dermatitis accompanied by hair loss. He then read of a man in Berlin who lost hair on his head after it had been irradiated. He had a young female patient with a large hairy naevus on her back which he treated with a series of x-ray exposures. She lost the hair and the discoloured skin but developed long standing dermatitis because of over dosage. Later, various other skin conditions were treated successfully.

Radioactivity, discovered by Henri Becquerel in 1896, (Nobel Laureate 1903 with Pierre and Marie Curie) gave another way of providing external radiation using radium tubes spaced within plasticine giving successful results for skin lesions.

All these early treatments had a major disadvantage that the patient's skin always received a high radiation dose leading to burning, even when radiation fields were combined at different angles around a tumour site. Great improvements were developed from about 1960 when higher energies were available. These energies gave skin sparing because the radiation did not reach its 100% value until secondary electron production within the patient's body had built up. Firstly, treatment machines using sources of cobalt-60 were used and later, linear accelerators, which did not have the same disadvantage of a decaying source of radioactivity with a half life of 5.2 years.

Developments continue in the production of machines and accessories both for diagnostic and therapeutic work. X-rays also have many and varied industrial uses.

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Footnote: Professor Newing's book 'Light, Visible and Invisible, & its Medical Applications' ISBN 1-86094-164-8 Imperial College Press 1999, contains all this information and much more. It is still in print and costs £20 from Imperial College Press (57, Shelton St, Covent Garden London WC2H 9HE)

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Disclaimer

Superconductivity - the first hundred years

Dr. Peter Ford
University of Bath

Heike Kamerlingh Onnes

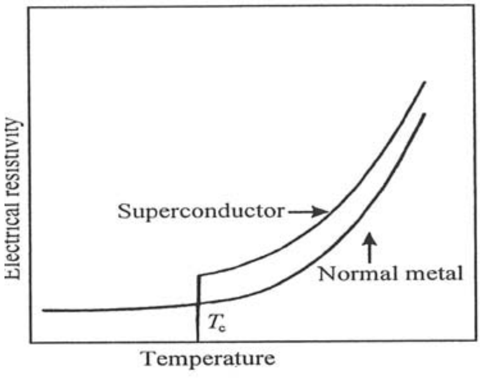
On the 11th and 12th July a highly successful meeting was held at the Cavendish Laboratory in Cambridge marking "100 years of superconductivity and 25 years of high temperature superconductivity". The meeting was organised jointly by the History of Physics, Superconductivity and Low Temperature Groups of the IOP. The first keynote lecture was given by Professor John Waldram of Cambridge who described the discovery of superconductivity and the subsequent events up to around 1957. Waldram had been a research student of Sir Brian Pippard who among his many distinctions was a highly regarded Chairman of the History of Physics Group. John Waldram has made important contributions in superconductivity as well as writing very readable text books both on thermodynamics and superconductivity. It is hoped that the present article captures some of the flavour of Waldram's lecture as well as that of the meeting.

The discovery of superconductivity

In 1895 Lord Rayleigh and Sir William Ramsay jointly announced their discovery of a new element, which they called argon. The following year Ramsay examined the gases emitted from the material pitchblende using a spectroscope and observed some characteristic yellow lines, which he concluded were due to a further new element helium, whose existence had been inferred many years earlier in 1869 when a spectroscope was first turned towards the sun during a solar eclipse and similar yellow lines were seen. Both argon and helium are members of a new group in the Periodic Table, called the Inert or Noble gases, the elements of which are characterised as being unreactive chemically and having zero valence. Studying the properties of these new elements showed that helium had a very low critical temperature and would therefore almost certainly liquefy at a lower temperature than hydrogen, which at that time held the record as the element with the lowest boiling point. This was 20K and was first liquefied by Sir James Dewar in 1898 at the Royal Institution in London. In the attempt to first liquefy helium Dewar was challenged by the Dutch physicist Heike Kamerlingh Onnes, who over many years had built up superb low temperature facilities in his laboratory at Leiden in Holland. In the end it was Onnes who won the race first liquefying helium during an epic experiment in 1908. He found that helium liquefied at 4.2K and by pumping on this it was possible to obtain temperatures just below one degree above the absolute zero of temperature. It was a truly remarkable achievement.

Once such a low temperatures could be achieved it was natural to examine the behaviour of metals in this region. Work by Dewar and others had already shown that the electrical resistance of metals decreased with lowering temperature going towards 20K, the boiling point of liquid hydrogen, but there was controversy as to what would happen as the absolute zero was approached.

Kamerlingh Onnes examined both gold and platinum but his results were inconclusive as can be seen from Figure 1 (left).



Comparison between the electrical resistivity of a superconducting and normal metal at low temperatures (less than 20 K).

At temperatures below about 10K the electrical resistance appeared to flatten off to reach a so called “residual resistance”, which was due to impurities and strains in the metal. Such an effect had already been investigated at high temperatures and was known as “Matthiessen’s Rule”. It was suggested that mercury would be a suitable metal to study since, being the only liquid metal at room temperature, it was possible to distil it many times and obtain an extremely pure sample. The results obtained are also shown schematically in Figure 1 and the sudden drop of the resistance around 4K to reach a value which appeared to be zero caused consternation in the laboratory. The actual results are shown in Figure 2 (below).

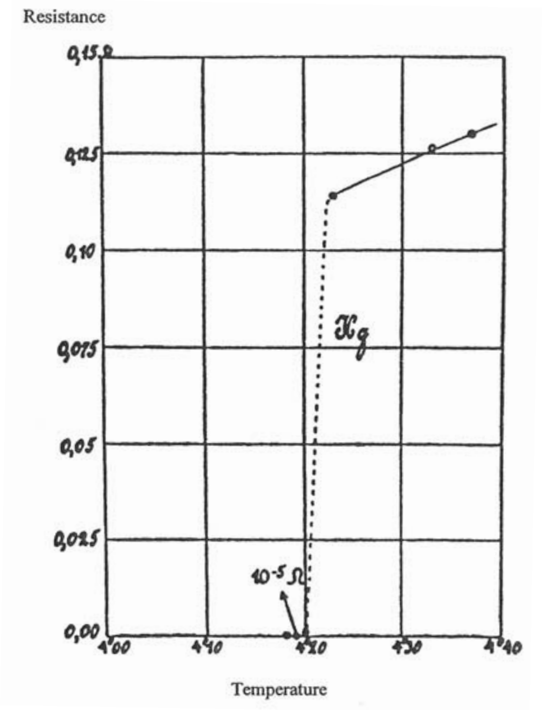


Figure 2 The electrical resistance in ohms of mercury in the vicinity of 4.2K, indicating a sharp drop at the superconducting transition. (After Kamerlingh Onnes.)

The likely reason for this sudden drop in resistance was believed to be an electrical “short” in the sample at low temperatures and extensive investigations were carried out to find its likely source. However, after some while it was gradually realised that perhaps it was not a “short” but an exciting and completely new effect which became known as “superconductivity”.

Further complications occurred in understanding what was going on, arose because the sudden disappearance of the electrical resistance seemed to appear at the exact temperature that helium became a liquid. It was difficult to understand how this could be having such an effect.

Although the date for the first liquefaction of helium is well documented as 10th July 1908, the date for the discovery of superconductivity is somewhat obscure. This is partly due to some poor record keeping by Onnes combined with his near illegible handwriting. However, some careful studies of his original notebooks by Dirk van Delft and Peter Kes [1] have pointed to the 8th April 1911. It is also unclear as to how much credit should be given to his research student Gilles Holst, who took the experimental measurements of the electrical resistance using a Wheatstone bridge and a sensitive galvanometer, equipment which is probably familiar to many readers of this article. In an article for Physics World by Paul Grant [2], in a series marking the centenary of superconductivity, he emphasises the contribution made by Holst and states that it would be inconceivable today that Holst would not have been co-author in the publications by Onnes which announced the discovery. By contrast Dirk van Delft and Peter Kes in EuroPhysics News [3] state that Holst never expected or asked to be co-author. Whatever is the truth, Holst later had a distinguished scientific career becoming the first director of the Philips Research Laboratories at Eindhoven in 1914. Mention should also be made of the outstanding experimental skills of Gerrit Flim, who was the technical manager in the cryogenic laboratory at Leiden, and Oskar Kesselring the master glassblower. Both men and their assistants contributed enormously to the success achieved by Kamerlingh Onnes, who was awarded the 1913 Nobel Prize in Physics for his work leading to the first liquefaction of helium.

After the initial discovery of superconductivity in 1911, further proof was obtained by observing similar effects in both lead and tin at 7.2K and 3.7K respectively, which are both above and below the boiling point of liquid helium at 4.2K. The superconducting transition temperature, T_C, for mercury at 4.2K was coincidental. Onnes then went on to examine in an ingenious manner whether it was possible to measure the residual electrical resistance in the superconducting state. In 1820 in a seminal experiment the Danish scientist Hans Christian Oersted had shown that passage of a steady electrical current generated a magnetic field, thereby showing the connection between electricity and magnetism, which until then had been considered to be two distinct subjects. Onnes tried to measure the decay time of the magnetic field for a current which was flowing in a closed superconducting loop and found that there no decay whatsoever. This demonstration of a "persistent current" meant that in the superconducting state the electrical resistance is effectively zero.

Kamerlingh Onnes was a visionary and realised quickly that the passage of an electrical current in the superconducting state could be of great benefit for the electrical generation and distribution industries, which were rapidly gaining momentum and becoming widespread in the first decades of the twentieth century. In the normal state there are energy losses due to Joule heating (I^2R) caused by the resistance R of the transmission wires. This is clearly not the case for transmission wires in the superconducting state where the electrical resistance is zero. However, here he hit upon a severe problem. Passage of a large current generated a sufficiently high magnetic field which in turn destroyed superconductivity. This critical magnetic field H_C before superconductivity was destroyed was very small, only a few hundredths of a Tesla. The situation is shown in Figure 3, (below) which is an example of a phase diagram.

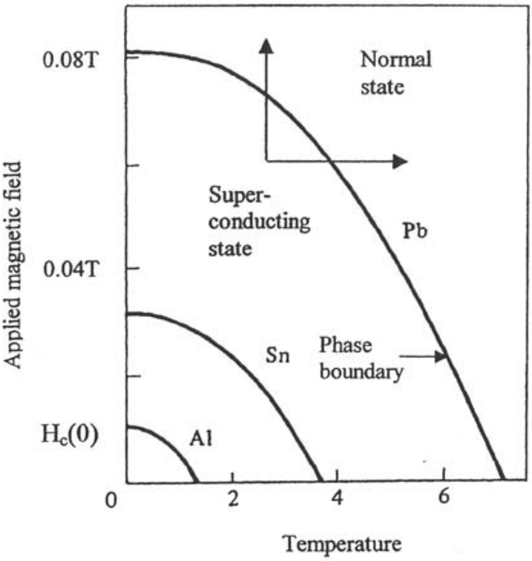


Figure 3 The way in which the critical magnetic fields of superconducting lead (Pb), tin (Sn) and aluminium (Al) depend upon temperature. The phase boundary defines the transition from the superconducting to the normal state.

The parabolic curves are the phase boundary, which mark the transition from the superconducting to the normal state, for the three elements aluminium, tin and lead. The normal state is reached if either the temperature rises above a critical value T_c or the magnetic field exceeds a critical value H_C. Kamerlingh Onnes appreciated that in order for superconductivity to have much practical value both the critical temperature T_C for the onset of superconductivity and the critical magnetic field Hc had to increase dramatically. Much of the subsequent history of superconductivity has revolved around trying to realise these two conditions.

For many years Onnes' laboratory at Leiden was the only one in the world with facilities capable of reaching down to one degree above the absolute zero of temperature and therefore he had a monopoly of research into superconductivity. However, progress was slowed due to the outbreak of the First World War in August 1914 [4]. Although Holland was neutral throughout the War, the German violation of the neutrality of Belgium, and the human atrocities and destruction that they caused there, resulted in a large number of Belgians becoming refugees and flooding into Holland. The violation of the neutrality of Belgium also caused Britain and the Commonwealth to enter the War to come to the assistance of Belgium and France. During the War, Onnes devoted much of his time helping refugee children from Belgium. Not surprisingly, in writing about the history of science, we concentrate on the scientific achievements of people. It is easy to forget that frequently they lived their lives through turbulent historical events which impacted greatly upon them. During this period Onnes revealed himself as a man of great humanity and compassion as well as being an outstanding scientist.

Developments in the 1930s

By the early 1930s other laboratories such as those in Berlin, Breslau, Bristol, Cambridge, Kharkov, Moscow, Oxford, Toronto and Washington had developed low temperature facilities and were able to study superconductivity. The superconducting transition appeared to be extremely sharp (to within a milli-Kelvin) as well as occurring in a surprisingly large number of elements, alloys and compounds. Superconductivity also attracted the attention of theoreticians including such luminaries as Werner Heisenberg, Niels Bohr and Felix Bloch but they made little progress in understanding it. Kamerlingh Onnes himself was interested in finding an explanation and remained so until his death in 1926.

Until the early 1930s it was believed that superconductivity was solely the sudden loss of electrical resistance at a well defined critical temperature Tc, although it was also appreciated that the magnetic field caused by passage of an electrical current could destroy superconductivity. The picture changed dramatically in 1933 through the experiments carried out in Berlin by Walther Meissner and Robert Ochsenfeld who made an unexpected and fundamental discovery in the magnetic behaviour of a superconductor.

The essence of their discovery is shown in Figure 4. They observed that when pure tin is cooled down in the presence of a magnetic field, as soon as the superconducting transition temperature is reached, the magnetic flux is suddenly completely expelled from the interior of the sample. It was nearly a century earlier that Michael Faraday at the Royal Institution in London had examined the magnetic behaviour of materials and introduced the concept of magnetic lines of force or flux lines.

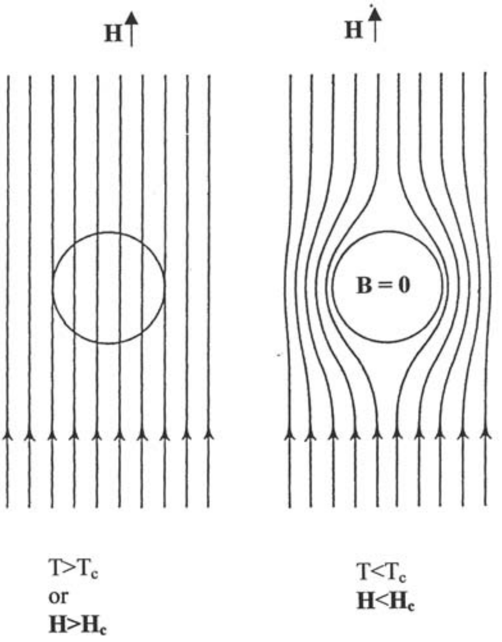


Figure 4 The Meissner effect.

He was the first person to make the distinction between paramagnetic and diamagnetic materials depending on their response to an applied magnetic field. A paramagnetic material is attracted towards an applied magnetic field and the lines of magnetic flux tend to crowd together in the material. By contrast, a diamagnetic material is repelled from such a field and the magnetic lines of flux tend to move apart. The observations by Meissner and Ochsenfeld revealed a unique situation in which the magnetic lines of flux are completely excluded from the superconducting state, so that the magnetic flux density **B** within the superconductor is zero.

This extreme condition corresponds to perfect diamagnetism. It was soon recognised that this situation is a fundamental property of a superconductor which became known as the Meissner effect. It thus became clear that, in addition to having zero electrical resistance, a superconductor also possesses perfect diamagnetism. Observation of the Meissner effect in a material is generally considered to be a better and more clear cut test than zero electrical resistance, because the latter can be caused from all sorts of spurious reasons unrelated to superconductivity.

Perfect diamagnetism cannot be explained by starting with zero electrical resistance and applying Maxwell's electromagnetic equations. These would require that the magnetic flux density **B** inside a superconducting material cannot change and that any flux present inside the superconductor would remain frozen in. As such, the magnetic flux inside a superconductor would depend on its previous magnetic history and this would mean that the superconducting state is in a metastable condition rather than being in true thermal equilibrium. The observation of the Meissner effect resolves this problem and the complete exclusion of magnetic flux on entering the superconducting phase means that a true thermodynamic state exists, which is reversible. Raising the temperature above the critical value T_C, or applying a magnetic field greater than H_C, causes flux penetration to take place and a sample to return to its normal state.

The fact that the superconducting state is in thermal equilibrium means that thermodynamic relationships can be applied to the transition from the normal to the superconducting states. This was first carried out by the Dutch scientist Cornelius Gorter, who was a successor to Kamerlingh Onnes as the director of the low temperature physics laboratory in Leiden. As a result of Gorter's analysis, it was predicted and observed experimentally that a sharp but finite discontinuity would occur in the specific heat at the transition temperature Tc. Gorter made a further important contribution in 1934 when he put forward a two fluid model in which the electron gas within a superconductor can be thought of as having two components. One component consists of "superelectrons", which are totally ordered, and therefore have zero entropy, and are responsible for the superconducting behaviour. The second component behaves like a normal electron gas. Below the superconducting transition temperature, the superconducting electrons short out the normal electrons giving rise to the zero electrical resistance. A similar two-fluid model was put forward in 1938 by the Hungarian scientist Lazlo Tisza to explain the strange behaviour observed in superfluid helium. The similarities between the two models suggest the striking analogies between superconductivity and superfluidity, both of which are nowadays referred to as quantum fluids.

Advances in the 1950s & 60s

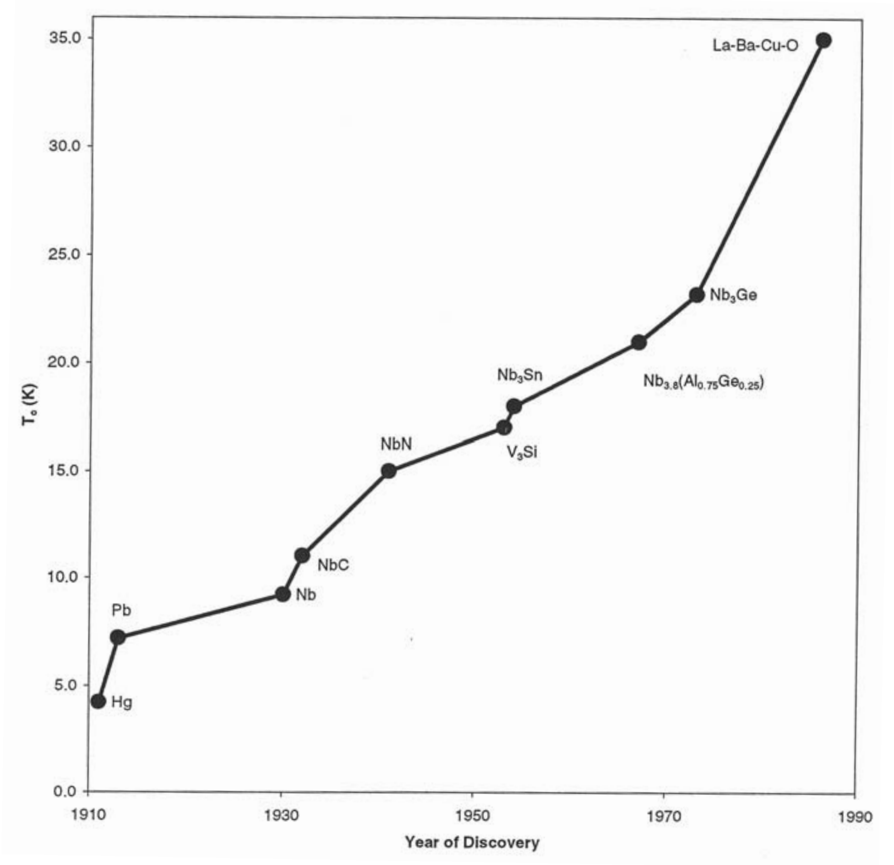


Figure 5 A plot of the highest superconducting transition temperature obtained against the year for the period between the discovery of superconductivity in 1911 and the advent of the high temperature superconductors in 1986.

Since the discovery of superconductivity in 1911 there has been a slow increase in the superconducting transition temperature. This is illustrated in Figure 5 (above). An important development, especially for commercial applications, was that of certain intermetallic compounds such as Nb_3Sn and NbTi. These were mainly fabricated in the large research based industrial laboratories in the United States such as Bell Labs, General Electricity, IBM and Westinghouse.

Such intermetallic compounds showed Type II behaviour enabling them to remain superconducting to much greater magnetic fields than the earlier or Type I materials. The difference between Type I and Type II superconductors is illustrated in Figure 6, which shows the behaviour of their magnetization versus applied magnetic field.

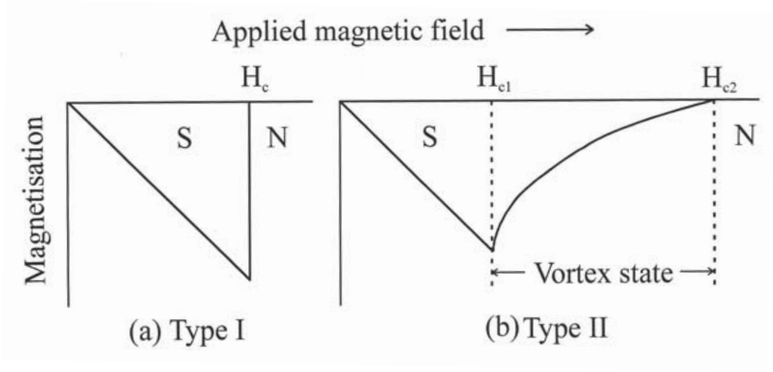


Figure 6 Magnetization versus applied magnetic field for (a) Type I and (b) Type II superconductors.

Both the Type I and Type II superconductors show the Meissner effect at sufficiently low applied magnetic fields, whereby the magnetic flux is completely excluded from the interior of the specimen, so that the flux density there remains at zero. For a Type I superconductor, when the applied magnetic field strength reaches the critical value H_C, the superconducting state no longer exists and magnetic flux penetrates the material producing the normal state. At higher applied magnetic fields the material behaves as a normal conductor. The elements and some simple compounds show Type I behaviour.

The situation is rather different for a Type II material. In this case the Meissner effect is displayed up to a certain value H_{C1}, in a similar manner to Type I materials. However, above H_{C1} partial flux penetration takes place but the bulk of the material still remains superconducting. This occurs up to a second critical value of H_{C2}, which is very much larger than H_{C1}. Above H_{C2} the material behaves as a normal conductor. The region between H_{C1} and H_{C2} is the unique and important property of Type II superconductors, which enables these materials to be utilised for high field magnets, since H_{C2} can be as large as several Teslas. The region between H_{C1} and H_{C2} is variously known as the intermediate, mixed or vortex state, whereby the bulk of the superconductor remains superconducting although partial flux penetration has taken place.

It has been subject to extensive study which is outside the scope of this article. Nevertheless, the introduction of Type II materials has enabled a small but important industry to develop starting in the 1960s manufacturing compact high field magnets for a variety of pure and applied applications.

The 1950s saw important advances in the theoretical understanding of superconductivity. The subject had intrigued theoreticians since its discovery although little progress had been made. In the 1930s a significant development was made in Oxford by the London brothers, Fritz and Heinz, who like other colleagues at Oxford, such as Franz Simon, Kurt Mendelssohn and Nicolas Kurti, were refugees from Nazi Germany. They believed that of the two basic properties of superconductivity, namely the zero electrical resistance and the Meissner effect, theoretically it was the latter which was the more important. They found that in order to explain this effect it was necessary to modify the classical Maxwell electromagnetic equations relating the electric current density to magnetic fields. They derived new equations whereby they were able to predict that the magnetic flux is excluded from the bulk of the superconductor, except for a small surface effect in which the magnetic flux does penetrate the sample but the depth to which this occurs rapidly decays in an exponential manner. This became known as the London penetration depth and its existence was soon verified experimentally and measured for a variety of superconductors.

Unlike their colleagues, neither Fritz nor Heinz London remained long at Oxford [5]. Fritz moved to Paris and then gravitated to the United States where he spent the rest of his life at Duke University in North Carolina. Heinz moved to the University of Bristol and then spent many years at Harwell. I was fortunate to get to know Heinz slightly while I was a research student at the University of Manchester in the early 1960s and was working on what became the first successfully operating helium dilution refrigerator. This was an ingenious idea of Heinz London, involving the isotopes of helium (helium-3 and helium-4), whereby such a refrigerator was predicted to reach and maintain temperatures well below 0.1K. In this work we were advised by Hugh Montgomery*, who was then also at Harwell, and who for many years was a prominent and highly regarded member of the History of Physics Group.

In 1950 Fritz London produced the first volume of his classic two part book *Superfluids*, which was devoted to superconductivity. In this he emphasised that superconductivity should be regarded as a quantum mechanical state observable on a macroscopic scale. This means that a superconductor can be thought of as behaving like a gigantic atom.

* See Newsletter 24 - Editor

1950 also saw important theoretical work from Soviet Russia where the two theoreticians Vitaly Ginzburg and Lev Landau extended the earlier work of the London brothers. The work of Ginzburg and Landau was based on Landau's theory of second order phase transitions which introduced the concept of an order parameter. For superconductivity, the order parameter distinguishes the superconducting and normal phases. They introduced the idea of a superconducting wave function as well as that of the coherence length, which gives a measure of the distance over which the superconducting wave function varies in a zero magnetic field. In 1957 another Russian, Alexei Abrikosov, used the Ginzburg-Landau theory as the starting point for his own work which led to the prediction of Type II superconductivity. The 1950s was the height of the "Cold War" between Russia and the West, so that for many years the seminal work carried out by the Russian scientists was virtually unknown in the West. Both Ginzburg and Abrikosov obtained the Nobel Prize for Physics in 2003 for their work on superconductivity, while Landau, who died in 1968, had obtained the prize in 1962. The knowledge of the relative magnitude of both the penetration depth and the coherence length is required in order to assess whether a given superconductor will exhibit Type I or Type II behaviour.

Another important advance was made in 1950 by the distinguished physicist Herbert Fröhlich, who spent many years at the University of Liverpool having also come to this country as a refugee from Nazi Germany. He based his work on the belief that the interaction between the conduction electrons and the atomic lattice was crucial to understanding the mechanism for superconductivity. His theory was not successful in explaining some of the properties of superconductors but could account for the apparent paradox that the best conductors of electricity, namely the noble metals copper, silver and gold, appeared not to go superconducting whereas poorer conductors such as lead (T_C=7.2K) and niobium (T_C = 9.5K) had the highest transition temperatures for all the elements. In the noble metals the electron-lattice or electron-phonon interaction is small, resulting in them being good conductors of electricity, but is not conducive to them becoming superconductors. By contrast, in lead and niobium the electron-phonon interaction is much stronger accounting for its superconductivity as well as its relatively poor conductivity in the normal state. Fröhlich's work was also able to explain the isotope effect, which was discovered shortly afterwards by Emmanuel Maxwell in the United States. It was found experimentally that the critical temperature Tc depended on the atomic mass M of the sample. For example, with isotopes of mercury T_C decreases from 4.185K to 4.146K as the atomic mass increases from 199.5 to 203.4. This shows that the observed change in T_C is small and requires great care and sensitive equipment to measure. Analysis of the data showed that the transition temperature T_C is inversely proportional to the square root of atomic mass, M.

It was well known by 1950 that the atomic vibrations in a lattice were also inversely proportional to the square root of the atomic mass M and this similarity in behaviour suggests that the lattice vibrations are likely to play an important role in understanding superconductivity. The fundamental contributions made by Fröhlich in the area of superconductivity may well have not been sufficiently recognised as can be seen from a recent letter to Physics World [6].

The important breakthrough in understanding superconductivity came in 1957 when John Bardeen, Leon Cooper and Robert Schrieffer published their famous BCS theory. A year earlier Cooper had suggested that superconductivity might be associated with a bound pair of electrons, each having an equal but opposite spin and angular momentum, travelling through the metal. These are the famous "Cooper pairs". In formulating his ideas Cooper has paid a fulsome tribute to the earlier work of Fröhlich [6]. The idea that electrons in a metal can form pairs is itself remarkable since they both have a negative charge and so would be expected to repel each other. However, a very weak attractive interaction is possible via the lattice. As an electron passes through a lattice of positively charged ions, the motion of the ions is disturbed near the vicinity of the electron and they tend to crowd round the electron forming a screening cloud of positive charge. Under certain circumstances a second electron can also become attracted to this cloud of positive charge and hence the two electrons are effectively coupled to each other. This attractive interaction is very weak and can easily be destroyed by lattice vibrations. As such, superconductivity is confined to very low temperatures, a few degrees above the absolute zero. Together Bardeen, Cooper and Schrieffer were able to build on the idea of "Cooper pairs" and develop a comprehensive theory of superconductivity which could account for many of the experimental observations. Their theory has been highly influential in the subsequent developments of superconductivity and they were awarded the 1972 Nobel Prize for Physics for this work. For Bardeen, this was his second award for physics. In 1955 he received the prize together with William Shockley and Walther Brattain for his work on the invention of the transistor. He is the only person ever to be awarded the prize twice in the same discipline.

Powerful evidence for the idea that pairs of electrons play an important role in superconductivity came from the first determination of the flux quantum. The idea that flux quantisation is a quantum effect associated with superconductors originated around 1950 from the fertile mind of Fritz London. He proposed that the magnetic flux passing through the hole from a current carrying superconducting ring, cannot take any arbitrary value but instead must be some multiple of the basic quantum mechanical unit h/e, where h is Planck's constant and e the electronic charge. This is an extremely small quantity and at the time was considered to be unmeasurable with existing equipment. However, by 1961 this flux quantum was measured independently, but virtually at the same time, by Bascom Deaver and William Fairbank at Stanford University and Robert Doll and Martin Näbauer in Munich. Both sets of authors showed that the measured flux quantum corresponded to a value of h/2e instead of h/e as predicted by London. This is consistent with the idea of bound electrons as "Cooper pairs" in superconductors, in which the charge on the current carriers is 2e, rather than the single charge e, which was assumed by London. This year therefore marks the 50th anniversary of the important discovery of the flux quantum.

Around this time Brian Josephson, who was a postgraduate student at Trinity College, Cambridge, was considering what might happen when two superconductors were separated from each other by a thin insulating barrier. In 1962, in a seminal publication, Josephson pointed out that, in addition to ordinary single electron tunnelling taking place, there should also be an additional contribution from the tunnelling of "Cooper pairs". He derived an equation for this resulting tunnelling current. Although this work might appear to be highly esoteric, what became known as the Josephson effects have found important applications in technology, notably as an extremely sensitive detector of magnetic fields in the so-called SQUID devices. A detailed discussion of the Josephson effects is outside the scope of this article but some idea of its significance can be appreciated by the fact that Josephson was one of the recipients for the 1973 Nobel Prize in Physics.

The discovery of high temperature superconductors

The year 1986 marked the seventy-fifth anniversary of the discovery of superconductivity. Any celebrations taking place were distinctly muted. Although much had been understood about superconductivity, and a small industry, especially in the area of compact high field magnets, had been formed, the dream of room temperature superconductivity seemed as far off as ever. During the seventy five years the superconducting transition temperature T_C had only risen from 4.2K for mercury to 23 K for an intermetallic compound Nb_3Ge, which was discovered in 1973 (Figure 5). In the intervening years thirteen years no superconductor with a higher T_C had been found. Even worse, theory suggested that superconductivity was unlikely to occur above about 25K.

All this changed in 1986 when a rather innocuous looking paper appeared in the journal *Zeitschrift für Physik* which described measurements of the electrical resistance on a complex ceramic metal oxide containing the elements Lanthanum (La) – Barium (Ba) -Copper (Cu) oxide, which showed evidence for the existence of superconductivity above 30K. The two authors, Alex Müller and Georg Bednorz, were working at the IBM Laboratories outside Zurich and they described their observations of a dramatic decrease in the resistance of this material starting at around 30K. The title of their paper was very diffident referring to "Possible High T_C Superconductivity in the Ba -- La – Cu – O System". The reason for this tentative wording is that the observation of a sudden decrease in the electrical resistance could arise from a variety of causes, not least an electrical short, and many people have made extravagant claims from which they have had to subsequently recant. They realised that in order to be much more confident about their claim it was necessary to measure the susceptibility and demonstrate the Meissner effect, but at the time of the submission of their paper the apparatus for this was not in operation.

Within a short period of the publication of their paper the world of physics began to hum. Many research groups throughout the world abandoned their ongoing research and began to investigate the claims of Bednorz and Müller and extend their work. Prominent among them was that led by Paul Chu at Houston, Texas. His research speciality was to examine materials under very high pressure. His work on the new lanthanum based ceramic oxide showed that the superconducting transition temperature Tc increased with applied pressure. The implication of this observation was that it might be possible to find a new compound consisting of smaller atoms that could simulate at atmospheric pressure the "squeezing effect" due to the application of high pressures and thereby produce a new superconductor with a greatly enhanced transition temperature at ambient pressure. This led Chu and his colleagues to begin a frenzied effort to find such a material. Speed was of the essence since Chu realised that other research groups would have the same idea and the rewards for the first person to obtain a superconductor with a Tc greater than the boiling point of liquid nitrogen (77K) would be enormous. Chu decided to enlist the help of one of his former graduate students Maw-Kuen Wu, who headed a research group specialising in preparing and studying oxide materials at the Huntsville campus of the University of Alabama. The concerted and frenetic effort from the two groups rapidly led to success. The decisive change was to replace lanthanum with the element yttrium. In the March 2nd 1987 edition of *Physical Review Letters* the two groups announced the synthesis of a new ceramic oxide Yttrium (Y) – Barium (Ba) – Copper (Cu) oxide, with a superconducting transition temperature at ambient pressure of 93K. In contrast to the diffident title and style of the article by Bednorz and Müller, this article left little room for doubt that they were quite confident about their claims and that they had indeed made this discovery. The oxide is best known by its acronym YBCO and it must be one of the most studied materials ever. Although Chu is frequently credited as having discovered YBCO, it would appear that it was Wu, and particularly his graduate student Jim Ashburn, who made the important breakthrough of suggesting using yttrium.

The discovery of YBCO following so shortly after that of the lanthanum superconductor led to a redoubling of effort throughout the world to verify and extend the work. At the time, I was working in the Physics Department of the University of Bath and well recall the excitement caused by all of these observations and members of the Physics and Materials Science Departments joined forces also to examine the new materials. It was probably the most remarkable time ever in the history of condensed matter physics with seemingly every latest edition of a journal containing articles with significant new results. Indeed, to cater for all the papers being submitted, new journals were created and established journals began to run supplements. The atmosphere of the time can be gauged from the now famous Extraordinary Session of the March 1987 meeting of the American Physical Society, which was held in New York and took place barely two weeks after the appearance of the article by the groups from Houston and Alabama. The session went on into the small hours of the morning and the atmosphere was euphoric with a widespread belief that the recent discoveries were likely to mark the start of a revolutionary new era in which superconductors would underpin much of everyday technology. The event subsequently became known as the "Woodstock of Physics". The following month I attended a similar meeting of the European Physical Society held in Pisa, in which there was a similar euphoric atmosphere. The year 1987 ended triumphantly for Bednorz and Müller with their award of the Nobel Prize for Physics. This was an extremely rapid award of such a prize but was one of the rare occasions that the original stipulation of Alfred Nobel's will was fulfilled exactly in that the award should be made to those who *"during the past year, shall have conferred the greatest benefit on mankind"*. The impact of their work was also recognised in the Nobel Prize citation which noted that their discovery inspired a great number of scientists to work with related materials.

Within a very few years, similar materials to YBCO were discovered which possessed even higher superconducting transition temperatures. These were based on: Bismuth (Bi)- Strontium (Sr) – Calcium (Ca) - Copper (Cu) oxide; Thallium (Tl) –Barium (Ba) – Calcium (Ca) – Copper (Cu) oxide and Mercury (Hg) – Barium (Ba) – Calcium (Ca) – Copper (Cu) oxide. The very rapid increase in the superconducting transition temperature Tc, within a very few years of the discovery by Bednorz and Műller, can be seen in Figure 7. However, the holy grail of room temperature superconductivity still appears to be a long way off.

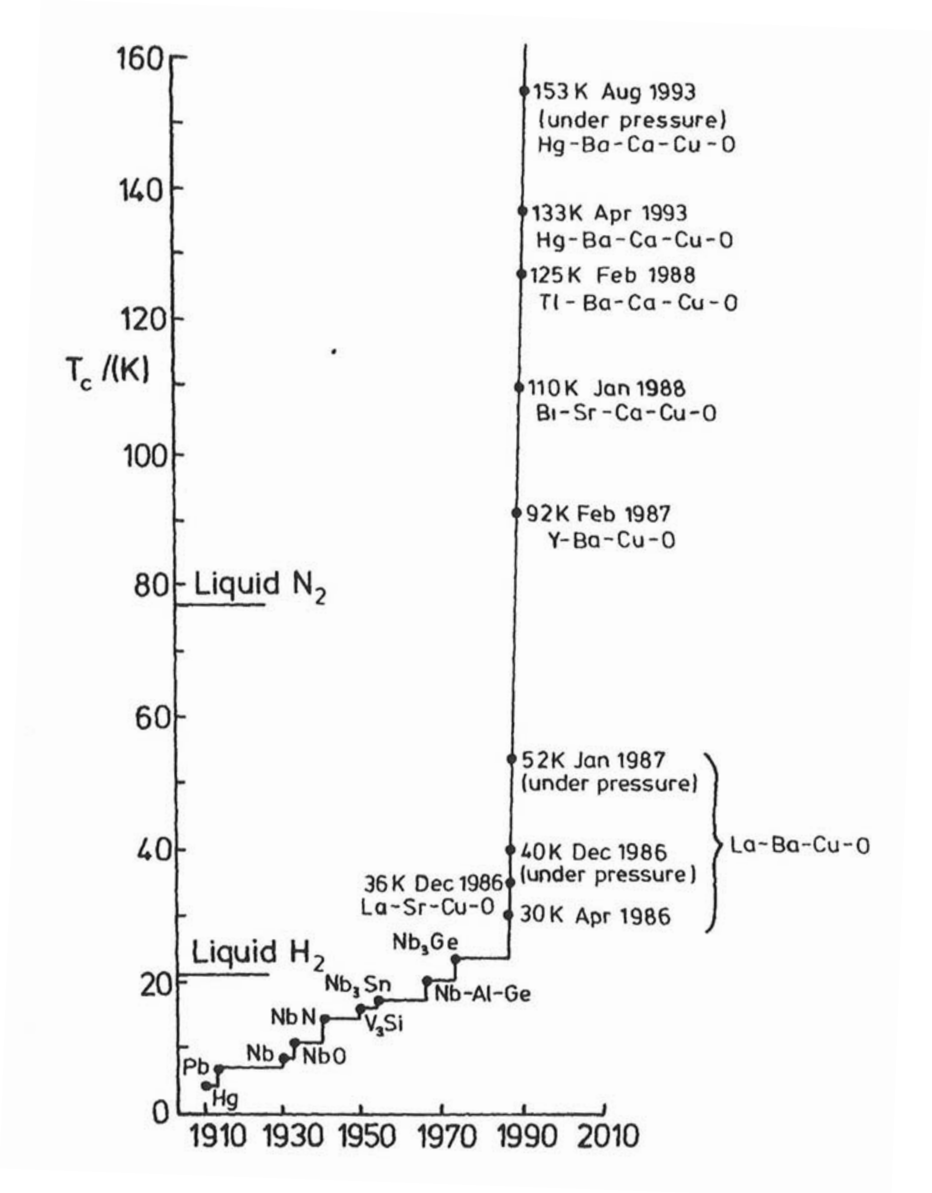


Figure 7 How the maximum superconducting transition temperature T_C has changed with time, showing the dramatic increase resulting from the international scientific response to the discovery of the high T_C materials. (After Bednorz and Müller)

The various high temperature superconductors mentioned in this section are all derivatives of the basic perovskite structure named after the Russian mineralogist Count Lev Alekseevich von Perovski in 1830. They are naturally occurring minerals and in the form of silicates are the most abundant materials in the earth's crust. In solid state or condensed matter physics, materials with this structure are found to possess a variety of properties: insulators, semiconductors, metals, superionics, ferroelectrics, piezoelectrics and others. Many of these different properties result from the possibility of introducing a large number of modifications and defects into the basic perovskite structure. For much of his career, Müller had been fascinated by perovskites and this instilled in him a belief that materials with such a structure could generate new and exciting physics. It was a viewpoint which has served him well.

The aftermath

The twenty five years since the initial discovery of the first of the high temperature superconductors have yielded an enormous body of information about them. However, the euphoria, which erupted in 1987, immediately following the discovery of YBCO, has not been fulfilled. An elegant experiment carried out by Colin Gough and his co-workers at the University of Birmingham established that superconductivity in these materials involved pairs of electrons as in the Type I and Type II materials. However, the situation with the high temperature superconductors is much more complicated than the straight forward electron-phonon interaction, which gave rise to the successful BCS theory for Type I superconductors, and is currently still imperfectly understood.

Other new superconductors have been discovered. These include magnesium diboride (MgB_2), first made by the group led by the Japanese scientist Jun Akimitsu in 2001, and certain iron based compounds, found in 2006, by another Japanese group led by Hideo Hosono. Magnesium diboride looks to be a particularly attractive material for technical applications since it is easy and inexpensive to fabricate and the starting materials magnesium and boron are both easily available in large quantities. However, it is only superconducting at 39K, although this is not regarded as an insurmountable problem. Over the last fifty or more years there has been enormous progress in the development of relatively inexpensive and reliable Stirling cycle engines capable of reaching temperatures down to about 10K.

There is still enormous interest in the potential for using superconducting materials in technological applications. On the small scale there are a variety of possible applications in superconducting electronics involving thin films, which appear promising. The large scale applications include the possibility of having a superconducting grid network, frictionless bearings and flywheels and a variety of applications for high field magnets. The first two are very much in the prototype phase of development whereas for the last fifty years there has been a small industry manufacturing superconducting magnets. The most important application is the manufacturing of superconducting magnets for use in Magnetic Resonance Imaging (MRI) scanners for medical purposes. This has proved to be an extremely successful and powerful form of non-invasive medical diagnostic, which has very few harmful side effects. Two recent applications of high field superconducting magnets have attracted media attention. The first is in the Large Hadron Collider (LHC) at CERN in Geneva. This has been the largest project involving superconductors to date in which a total of some 1200 superconducting magnets have been constructed from many centres around the world and rigorously checked and tested at CERN. The LHC is a truly awesome project and at the time of writing (August 2011) the prospect of finding the elusive Higgs boson appears to be hopeful. The second is the ITER fusion project in Southern France. The possibility of obtaining power from fusion has been a dream of scientists and engineers for many years. The technological problems needed to be overcome are formidable but the rewards for success would be enormous. The ITER fusion project is the most important attempt to obtain fusion power to date. It will involve the construction of a large number of high precision superconducting magnets.
It would seem likely that the high temperature superconductors will play an increasingly important role in several superconducting applications as can be seen from an upbeat letter from two of the principal organisers of the recent Cambridge Meeting [7].

The old Chinese proverb exhorts “May you live in interesting times!” The first hundred years of superconductivity has yielded a large amount of interesting and important physics which has led to some useful applications. It is likely that this will continue during the second century.

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Two of the references given above are from the excellent series of articles appearing in Physics World for April 2011, which were written to mark the centenary of the discovery of superconductivity.

In writing this article I have made use of my notes that I took during the Cambridge Meeting as well as from a book which I have written with my colleague George Saunders of Bath University: The Rise of the Superconductors, P.J. Ford and G.A. Saunders, CRC Press, Boca Raton, USA (2004).

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Meeting Notice

The prescience of Hutchie Synge

TCD April 19th 2012

E H Synge (1890-1957), older brother of J L Synge and nephew of the playwright J M Synge, chose to abandon his undergraduate course in Mathematics and Old Irish at Trinity College Dublin and conduct his further studies in seclusion. His visionary proposals in physics, pursued and published in isolation during the early Thirties, included radically new designs for telescopes and microscopes (the scanning near-field microscope) and LIDAR; they are gaining belated recognition, as the earliest intimations of possibilities for major modern technologies, including elements of nanotechnology. Synge’s withdrawal into a private world may well have been a classic case of Asperger’s syndrome. He spent his later years in even greater obscurity, in a mental institution.

There is to be a one-day symposium in his honour at TCD on April 19, 2012.

Organiser : J Donegan, School of Physics, TCD, Dublin jdonegan@tcd.ie

Registration is essential, but there will be no fee.

Denis Weaire dweaire@tcd.ie
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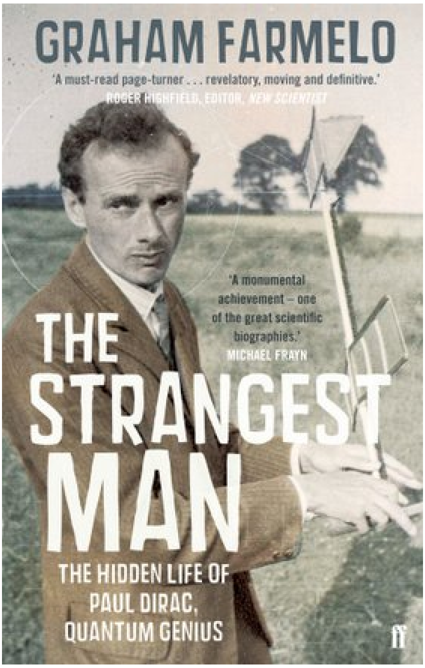
Book Reviews



The Strangest Man

The hidden life of Paul Dirac, Quantum Genius

Graham Farmelo

Faber and Faber	*2009*
ISBN	*978-0571222780*
560pp	*Hardback £18*

Reviewed by Professor E A Davis
Department of Materials Science and Metallurgy
University of Cambridge

This compelling biography does much to raise the profile of a 20th century theoretical genius whose name, although commemorated by a plaque in Westminster Abbey next to that of Sir Isaac Newton, is virtually unknown to the general public. One reason for this is that the contributions to physics of this theoretician, while immense, are difficult to describe to non-scientists.

Graham Farmelo not only presents us with a vivid account of Paul Dirac as a person and throws considerable light on the driving force behind his insights, but also succeeds in the difficult task of making his scientific discoveries accessible. Take for example Farmelo's description of the delta function - a concept introduced by Dirac in 1936 early in his working life: "The object that he (Dirac) called the delta function, resembles the outer edge of the finest of needles, pointing vertically upwards from its base. Away from the base, the numerical value of the delta function is zero, but its height is such that the area enclosed between the perimeter and the base is exactly one unit." This lucidity is evident in Farmelo's description of Dirac's other accomplishments.

The book reads like a novel (and a gripping one at that), with the characters having names that are more familiar to most scientists than the principal one. We read about Dirac's 'interactions' with, amongst others, Einstein, Heisenberg, Schrödinger, Bohr, Born, Pauli, Wigner, von Neumann, Larmor, Landau, Rutherford, Oppenheimer, Peierls, Gamov, Kapitza and Feynmann. I apostrophize 'interactions' because we are told that these were often minimal in terms of the extent of his conversation or the exchange of ideas. Indeed Dirac could be described as unresponsive in the extreme, speaking it would appear reluctantly only when pressed to answer a direct question. In his early days at Cambridge, his colleagues invented a new unit, 'a Dirac', for the smallest imaginable number of words that someone with the power of speech could utter in company in an hour, namely one! In a final chapter of the book, Farmelo speculates as to whether or not Dirac was autistic.

Dirac's motivation was the search for fundamental equations that describe the natural world. He was to tell students they should not worry about the meaning of equations, only about their beauty. It was the merger of two early twentieth century revolutionary ideas in physics – namely quantum mechanics and relativity – into a single equation that was Dirac's most notable achievement - the Dirac Equation.

In Farmelo's account, Dirac's achievements, and indeed the progress of science itself, are set against the unfolding history of world events. It is this chronological background that drives the story onwards and makes the book so compelling. During the First World War, Dirac was in secondary education at the Merchant Venturers' School in Bristol. Some of the older pupils served and lost their lives in the conflict, which, by a twist of fate, served to deplete the number of pupils in the classes and enabled those who escaped national service to progress faster. Even at this young age, Dirac was beginning to explore the connection between space and time and suggested that they ought to be considered from a general four-dimensional point of view. A few years later in 1919, when taking a degree in electrical engineering at Bristol University, he read in *The Times* that an obscure scientist, Albert Einstein, had proposed a revolutionary theory of relativity, a discovery that was to have a lasting impact on Dirac. Indeed when he won a place at Cambridge a few years later, he asked if he could make relativity his subject of study for a PhD. It did not take long for his supervisor, Ralph Fowler, to recognize his talent.

He achieved world-wide recognition in 1925, whilst still a postgraduate student, after publication of his first paper '*The Fundamental Equations of Quantum Mechanics*', which was inspired by Heisenberg's revolutionary ideas that electrons should no longer be visualized as point objects circulating in well-defined orbits around a nucleus but rather should be described in mathematical terms in the form of a matrix. In this, his first paper, Dirac introduced into the subject the concept of non-commuting variables and the Poisson bracket.

An alternative description of the behaviour of small particles to the one used by Heisenberg and Dirac appeared on the scene just as Dirac was writing his PhD thesis. It was Erwin Schrödinger's wave mechanics. Little did Dirac realize when he first read this work that eight years later he would be sharing the Nobel Prize with its author! During a short period of intense activity, Dirac was to generalize the Schrödinger equation to include situations in which systems varied with time (for example, an atom in a fluctuating magnetic field) and, soon thereafter, to draw a clear distinction between two types of subatomic particle according to whether they behaved according to the Pauli exclusion principle (fermions) or whether they did not (bosons). The terms in parentheses are Dirac's own and have become accepted names for particles with half-integral or integral spin.

There was no let up in Dirac's insights into nature during a short stay at Neil Bohr's Institute for Theoretical Physics in Copenhagen. Rarely interacting with any of the other scientists in that famous laboratory he discovered a connection between the Heisenberg and Schrödinger versions of quantum theory, shedding, as Farmelo puts it, 'light on both'. He also began to develop a quantum version of Maxwell's field theory of electricity and magnetism, which reconciled wave and particle descriptions of light leading him to exclaim that the two pictures were in complete harmony. By this time Dirac' talent was becoming well known as were also his eccentricities. Albert Einstein, 23 years his senior, remarked 'I have trouble with Dirac. This balancing of the dizzying path between genius and madness is awful'.

It was during a short stay in Göttingen in 1927 at the invitation of Max Born, where he met Robert Oppenheimer who was to become a lifelong friend, that Dirac began to consider how Einstein's theory of special relativity could be combined with the new ideas of quantum physics. His solution was expressed in an equation of such power and beauty that a simplified version of it was chosen to adorn his memorial in Westminster Abbey.

Ironically, it was what Dirac thought was an unacceptable feature of this equation – namely that its solution included what appeared to be unrealistic negative energy values – which led to the prediction of antimatter, particles with the same mass as the corresponding 'normal' particle but with opposite charge. It was several years after publication of the Dirac equation that the first experimental evidence for one of these particles – the anti-electron or positron – was obtained. Considering that it is now accepted that half the material generated at the creation of the Universe was antimatter, it is not surprising that the prediction is considered as one of the greatest triumphs of theoretical physics.

There is a general conception that Dirac was so utterly focused on his work to the exclusion of social niceties that he had no time or inclination to pursue other interests. The book reveals otherwise. One of his lifelong friends, Igor Tamm, a Russian theoretician whom he met in Leiden but later visited several times in his native country, introduced him to mountain climbing and also, it appears, influenced his political thinking. Another of his close friends was Kapitza, whose detention in Russia on one of his return visits home from an extended stay at the Cambridge Cavendish Laboratory, caused Dirac great anguish. Many of his contemporaries assumed he would never marry but he did – to Eugene Wigner's sister, Margit, known as Manci, who bore him a son and daughter.

There are some delightful passages in Farmelo's book, some of which have little to do with Dirac but simply pertain to the events of the time. One concerns the splitting of the atom by Cockcroft and Walton in 1932. When this sensational discovery was announced, we are told that Einstein happened to be in Cambridge and dropped by for a demonstration, no doubt 'gratified to see that the results were consistent with his most famous equation'.

It was not of surprise when, in 1932, Dirac was appointed Lucasian Professor of Mathematics - even though at 29 he was just a few months older than Newton when he was appointed to the Chair in 1669. Dirac's salary rose to £1,200 per annum, which was supplemented by a college income of £300. That this sum is equivalent today to £256,000 provides a telling story as to how academic salaries have eroded since that time!

It was in the same year of 1932 that the first experimental evidence for the existence of positrons was found by Carl Anderson at Caltech in the USA during studies of cosmic rays using a cloud chamber. Surprisingly, Anderson, along with other eminent scientists, for example Oppenheimer, did not relate this discovery to Dirac's 'hole in a negative sea of electrons'. It was left to a talented but not-well-known mathematician, Rudolph Langer, to make the connection, but his paper on the subject contained so many speculative ideas that it made little impact. Even when Patrick Blackett in the UK observed the same tracks and gave a seminar on the subject at Cambridge with Dirac in the audience, the full significance of the discovery did not seem to dawn on those present. Twelve months later, Ernest Rutherford was uncomfortable with the idea that abstract theory could predict a new particle! Nevertheless the Nobel Prize committee awarded Dirac the physics prize for physics in the same year when he was just 31.

It might come as a surprise to learn that Dirac tried his hand at experimental research. Working with Peter Kapitza in the new Mond laboratory in Cambridge, he showed how isotopes of an element could be separated by forcing a jet of gas to follow a spiral path, whereupon the heavier atoms were forced to the outside of the stream. The technique was to be considered seriously at a later time during the efforts to build an atomic bomb. It was during this period that Kapitza came under the watchful eyes of MI5 with the whole of Europe now feeling political pressures from either Hitler or Stalin. Kapitza was later to be detained by the Soviets during one of his return visits to Russia, an event that was to appall Dirac who worked tirelessly but unsuccessfully for his release.

The political events in Europe were to create tensions and suspicions amongst Dirac's friends and colleagues who had contributed with him to the development of quantum mechanics. At first Schrödinger appeared to side with Hitler but later left Germany for Dublin claiming he had been forced to make public his approval of the Nazi regime. Heisenberg and Jordan remained in Germany. Others took flight from the country. The discovery of nuclear fission in 1938 by the German chemists, Hahn and Strassman, meant that all scientists were drawn into warfare and secrecy. Leo Szilard in America believed that if a nuclear chain reaction could be sustained in uranium, then the results should be kept from their former colleagues in Germany. Others, including Bohr, Blackett, Fermi, Joliot-Curie, Teller and Wigner, were divided in their views as to the necessity of secrecy, leading to acrimonious exchanges between them. The war would change Dirac's life – like all scientists he could not avoid the consequences of war or of undertaking some work in its cause - but he refused invitations to work at Bletchley Park or Los Alamos.

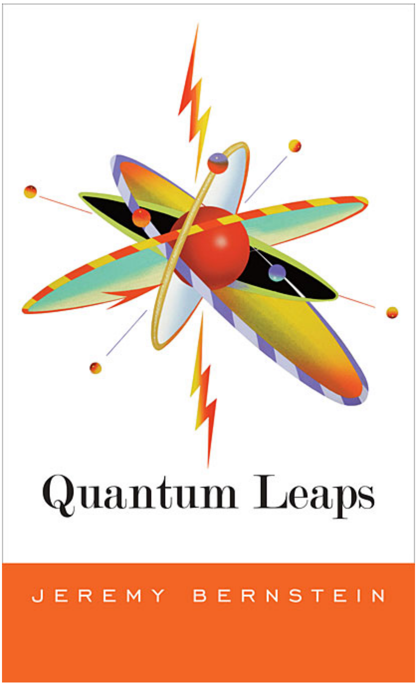
Just before the outbreak of war in 1939 he gave a lecture in Edinburgh in which he expounded his views on the principle of striving for mathematical beauty in theoretical physics, illustrating relativity and quantum mechanics as examples of its success. He also spoke on his new interest in cosmology, enthusing about the recent discovery of an expanding universe. With remarkable insight he suggested that classical mechanics will never be able to explain the present state of the universe and that unpredictable quantum jumps at its birth are required to explain its complexity.

In the 1940s, Dirac showed that he was more than capable of producing innovative ideas by developing a notation that is widely used by quantum theorists and known by his name, namely the *bra* and *ket* symbolism for quantum states. He introduced the concept in the third edition of his classic text *The Principles of Quantum Mechanics*. After the war he escaped the austerities of post-war Britain by accepting an invitation to work at the Institute of Advanced Studies in Princeton, where Einstein was still absorbed in finding a unified field theory. Dirac's work there included theories of monopoles, quantum electrodynamics and a demonstration of how the theory of relativity could be combined with Hamilton's description of motion. At the age of 50 there were signs of his drifting away from mainstream physics, which was beginning to fall into the hands of particle physicists. In 1953 he refused a knighthood, mainly because he disliked the thought of people addressing him as Sir Paul.

Dirac's contributions to fundamental physics were however not yet over. At a seminar he gave in Cambridge in 1955 he suggested that the universe might consist not of particles, but of one-dimensional 'strings' – tiny fictitious one-dimensional curves in place of physical entities. He had found a way of reformulating electrodynamics in terms of gauge-invariant quantities so that an electron, for example, became inseparable from its field. Once again he was ahead of his time.

In the end Dirac was marginalized in Cambridge, at odds with the new director of his Department. Encouraged by his wife, who felt that he was not afforded the respect she felt he deserved, the couple spent more and more of their time visiting America, eventually settling for what would be the last ten years of Dirac's life at Florida State University in Tallahassee, Florida, where his office is maintained as a shrine for those who wish to pay homage to a theoretician who was regarded as a towering genius by his contemporaries and who is still revered by physicists appreciative of his immense contributions to their subject.

Dirac's story has elements of tragedy about it. Farmelo's book provides insights into his personal life that at times sit uncomfortably with the magnitude of his discoveries and his creative genius. But by revealing all the passions and tribulations experienced by Dirac, and providing a glimpse into his what at times must have been a tortured soul, his accomplishments seem to be the more remarkable, soaring to levels that could not be conveyed by a purely scientific discourse.



Quantum Leaps

Jeremy Bernstein

Harvard University Press, Oct. 2009
ISBN 9780674060142
240 pp Hardback £15

Reviewed by Peter Rowlands
University of Liverpool

Quantum mechanics, the most fundamental theory created by twentieth century physics, is the basis of all advanced physical thinking and also of much modern technology, but is still little understood. It entered popular culture, however, at a very early date and has maintained a significant place there ever since. Jeremy Bernstein's book investigates some of the very diverse routes by which this happened, at the same time as discussing some of the fundamental issues which quantum theory has raised. Among his varied cast of characters contributing to this cultural absorption, Bernstein lists bishops (E. W. Barnes, second wrangler and Bishop of Birmingham), poets (W. H. Auden), historians, playwrights and novelists (Stoppard, Goldstein and Houellebecq), film-makers, Buddhist monks (the Dalai Llama), and Communist idealogues (Rosenfeld – the square root of Trotsky × Bohr, according to Pauli – and Fock), and many of their contributions are described in fascinating detail. While Bernstein is respectful of the serious scientific attempts at understanding quantum mechanics by religious leaders, such as the Dalai Llama and Bishop Barnes, and the use of quantum metaphors in plays and novels, he has no time for the 'New Age mysticism' represented by such books as Zukav's *The Dancing Wu Li Masters* or Capra's *The Tao of Physics*.

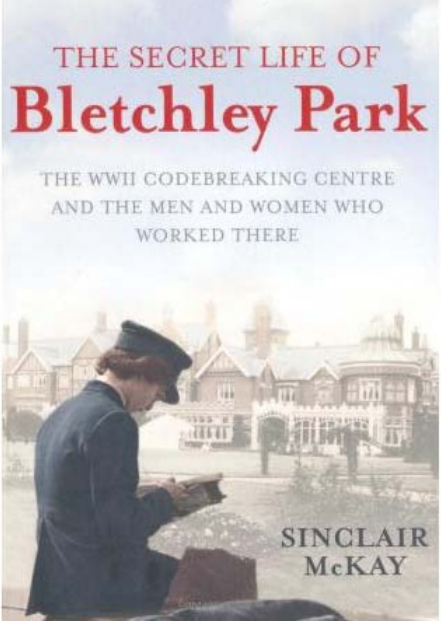
Against this background, Bernstein describes the contemporaneous development of the understanding of the subject among the physicists – Heisenberg, Born, Schrödinger, Bohr, Einstein, Dirac, Pauli, von Neumann, and others, with an extensive account of the later interpretation of David Bohm. On the way are interesting discussions of such things as the Copenhagen interpretation, entanglement, collapse of the wavefunction, the two-slit experiment, nonlocality, the many-worlds view, hidden variables and decoherence While much of this is familiar, Bernstein puts the issues into a stimulating context and relates it to the more general cultural response which is the main subject of the book.

There are some interesting personal details relating to Bernstein's own career, including a fascinating account of how, without the least intention of becoming a scientist, let alone a theoretical physicist, he gradually became one through the initial stimulus at Harvard of the great science historian I. Bernard Cohen. Bernstein also describes a memorably awkward lunch-time meeting at Princeton with Oppenheimer, Auden, the philosopher Reinhold Niebuhr and the British historian Llewelyn Woodward, where he imagines Niebuhr thinking 'And this too shall pass'.

If the book has a hero it is John Bell, who according to Bernstein, almost single-handedly revived interest in foundational questions after a long period of neglect and indifference. He illustrates the generally hostile atmosphere in the 1950s with an anecdote in which Oppenheimer, at the Institute of Advanced Study, cut dead a seminar speaker on the quantum theory of measurement after only five sentences, saying that Bohr had answered all the relevant questions in the 1930s. Bell, by contrast, was generating packed audiences for his discussions by the 1980s. Bell's contribution, of course, which is discussed in considerable detail, led to the first direct tests of the validity of quantum theory by such experimenters as Clauser, Shimony and Aspect, and has itself entered into aspects of general culture.

Despite the general acceptance of and interest in this work, however, Bernstein is, I believe, too sanguine about the reality of a foundational revival. The general attitude in the whole of physics is still very much one of 'shut up and calculate'. Foundational questions are routinely ignored and the subject still commands no great respect. Attitudes like Oppenheimer's are still found among journal editors and referees; there is no major journal or institute which is genuinely devoted to such questions. Bell's work is, I believe, only respected because it led to more or less immediate experimental testing. In addition, a whole industry has developed which propounds the 'strangeness' of quantum mechanics to popular audiences, almost demanding that its spell should never be broken by rational explanation.

Bernstein's book is, by its very nature, unsystematic. He ends by saying that he had no idea when he started it where his journey would end, but thinks that we are nearer the beginning of our understanding of quantum mechanics than the end. He quotes Richard Feynman saying 'I cannot define the real problem, therefore I suspect there's not a real problem, but I am not sure there's no real problem.' (p. 201)



The Secret Life of Bletchley Park - the WW 11 codebreaking centre

Sinclair McKay

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Reviewed by Emeritus Prof Derry W Jones Chemical and Forensic Sciences, University of Bradford

In the 1930s, the British Government's Code and Cypher School (GC & CS), nominally part of the Foreign Office, numbered about 180 of whom 30 were codebreakers, and was based in St James's, central London. Early in 1938, MI 6 decided that the wartime location of GC &CS would be Bletchley Park (BP) with its ornate late Victorian (now Grade 2 Listed) house and spacious grounds on the outskirts of Bletch1ey, Bucks, which had ready access by train to Oxford, Cambridge, London and the north. The red-brick mixed mock Tudor/Gothic mansion was thus saved from demolition intended to make way for new housing. Bletchley was a backwater, unpopular with London staff, before the time of Milton Keynes New Town, but it was far enough from the expected bombing of London. The move was rehearsed during the Munich crisis of 1938 and, with cables, mains and roads laid on the estate, effected in August, 1939. Clever academic mathematicians, including Gordon Welchman, Alan Turing (in the USA in the late thirties) and Peter Twinn, all from Cambridge, had been sounded out in 1937-39 by Alistair Denniston (1881-1941), linguist head of GC & CS; they attended GC & CS courses and were ready to be summoned, together with maths undergraduates, to BP in September, 1939. By January, 1945, over 9000 people in three shifts - officers from all three Services, former academic mathematicians, historians, and linguists, clerks (civilian, ATS, WAAF and WRNS), and administrative staff - worked in the house and, predominantly, in the 'temporary' prefabricated single-storey wooden, brick and concrete huts which replaced the gardens of the Park. Despite the need for lateral thinkers to try to break the German Enigma cyphers, the contributions of physicists as such don't seem to have been recorded, presumably because they were so much in demand for other scientific aspects of the war effort. However, as will be seen, only the immense efforts of the electronics group under Dr Tom Flowers at the Post Office Research Station, Dollis Hill, made possible the construction for BP of the electronic valve machine, Colossus, designed by Flowers, Turing and, from Manchester, Professor Max Newman. Also, physicist RV Jones (scientific intelligence) received from BP much valuable information which, for example, helped in devising weapon countermeasures.

Despite the numbers, BP security, said to be almost pathological, was such that for 30 years nothing was publicized about this secret establishment, initially called Station X (ie No 10) after the SIS listening post. If the content of German, Italian or Japanese coded messages became readable to the Allies through the laborious activities of cryptographers, translators and numerous clerks at BP, it was obviously essential that the enemy should remain unaware of this. To help preserve this state, there was no unauthorized interaction between those working in different Park huts, but also, when ultra-secret knowledge of high-level enemy communications was passed to senior Allied commanders, the requirement for destruction of written records of information had to be emphasized. The publication of the recollections of secret service officer Group Captain Frederick Winterbotham in *The Ultra Secret* (Weidenfeld and Nicolson and Pumell Books; index but no notes) in 1974 (Ultra was the code-name for BP's ultra-secret intelligence), regarded by some as premature or worse, made wartime BP public knowledge; even so, many participants revealed nothing in their lifetimes to their families. Although Winterbotham was criticized for breaking the silence, he disclosed rather little about BP activities. He was not a cryptographer but his book concentrates on the processing and distribution of the resulting ultra intelligence and its decisive role in the strategy - and sometimes tactics - of the land and air war. Surprisingly, a memoir by the Third Man spy, Kim Philby, *My Silent War,* published in the UK (MacGibbon and Kee) and USA in 1968, five years after his defection to the USSR and before the existence of BP was officially acknowledged, apparently mentioned BP and (unkindly) Denniston (head 1919-1942 of the secret cipher-breaking bureau and later GC & CS which metamorphosed into BP). The only belated official recognition was the presentation in 2009 by the then Foreign Secretary of commemorative badges (not medals) to known surviving BP veterans. Unfortunate personal and national consequences of the enforced long-term silence were that neither the brilliant achievements of Flowers nor the effective invention of the large-scale electronic computer at BP in 1943 could be declared, so that it was widely believed that the electronic computer was invented in the USA.

Since 1974, and especially since the availability of more archives in 1995, 50 years after the War's end, scores of books have been published about BP. A few of these will be recommended or mentioned in this review. The publications include biographies about and first-hand accounts by several of the main cryptographers as well as assemblies of reminiscences by some of the very many more junior participants. Obviously, the senior people have died while survivors even of those recruited at age 20 or less - and many were - will be aged around 90 now, so that it is high time to record any remaining individual recollections. Because of the security requirement of 'need to know' and the ban on discussion between people working in different segregated sections within the Park, anyone person will only be aware of a fragment of BP's codebreaking activities. In their 1993 book *Codebreakers: the inside story of Bletchley Park* (OUP), codebreaker Alan Stripp and codebreaker historian Sir Harry Hinsley edited the accounts of 27 of BP's former staff, mostly codebreakers but some less celebrated. More recently, a local resident interviewed lOOs of people to assemble *Bletchley Park People* (The History Press, 2004) which, though subjected to criticism, gave a more anecdotal impression of everyday life for the supporting staff. Though not purely about BP personnel because it fits BP into the context of code development and application, *Voices of the Codebreakers: personal accounts of secret heroes of World War* 11 (David and Charles, Cincinnati, 2007), by the military historian Michael Paterson, is largely based on direct quotations from the people involved. Also, *Station X: the Codebreakers of Bletchley Park* (Channel 4 Books, 1998), by the military intelligence writer Michael Smith, despite a rather brief index, deftly links memories of BP participants and BP-derived intelligence with the progress of the War.

It is thus surprising that *The Secret Life of Bletchley Park* (SLBP) is proclaimed as the first history of BP, albeit from the viewpoint of the "ordinary" men and women who worked there. This is not to detract from the value of the book, which is broadly chronological, interspersed with several chapters devoted to such topics as billeting, secrecy (about which Winterbotham is quoted), social class, and culture. What is distinctly evident from SLBP is that there were many variations of the ingenious Enigma cypher system used by different branches of the German hierarchy and forces and that, apart from the daily changes to settings, the Enigma machines were subject to continuous long-term developments and improvements to wrong-foot suspected or potential codebreaking. Codes previously broken could suddenly be read no longer. Even as late in the War as February, 1945, for example, the Luftwaffe implemented a new system of daily changes of encryptions of call-signs (denoting individual squadrons or formations) so that codebreakers had to fall back on catalogued idiosyncrasies of individual German signal operators, available through tedious cross-referencing of card indexes in shoe boxes by the well-heeled 'debs' delight" group in C block (with a back-up store in the Bodleian). By the same token, t justifiably eulogized valuable seizures at sea of tables of Enigma settings from weather or other vessels off the Lofotens or Iceland or from U-boats, or the inspiration of "cribs" (likely phrases in messages about the weather or locations of known Allied attacks, for example), generally served only to enable one of the cyphers to be read for a few weeks. All the cipher cracking of Enigma and other codes needed not only the trial and error, inspired leaps and psychological insights of a few brilliant, logical, nimble minds but also the patience, concentration and dedication of many intelligent untrained amateurs. As Paterson puts it, the sequence for treating decrypts was emendation and translation by the "watch" (who covered 24 hours in three shifts), and then evaluation, commenting and signal drafting by the Service advisers before information was sent to Whitehall and hence, if prudent, to operational headquarters. McKay also senses the shift through the 1940s from the rapidly expanding group of distinguished, if chaotically managed, amateurs to a large more professional organization that could still accommodate eccentric near geniuses at its heart. The advent of specific machines like the bombes (the setting of which absorbed many shift workers) and the expansion in signal traffic accessed required more rather than fewer people at BP.

The early history of GC&CS and its origins in the Admiralty's Room 40 in World War I is covered in the first-hand accounts by Denniston as published in his son Robin's *Thirty Secret Years* (Polperro Heritage Press, 2005). This memoir, which also describes early US/UK intelligence co-operation in World War II, includes comments on primary and secondary sources but has no index and is not chronological. It does give Denniston's notes on the remarkable and significant meeting in late July, 1939, in the Kabackie Woods near Pyry, Poland, at which he, Old Etonian cypher expert A Dillwyn Knox and the French code specialists met their Polish opposite numbers. Helped by a German spy, the Polish mathematician Marian Rejewski had worked for several years on the wheel settings for Enigma and deduced the instructions; Denniston and Knox were able to collect and bring to the UK an Enigma encypherer. The compact Enigma cipher generating machine, of which perhaps 100,000 were made, looked like a large typewriter with lights and had been in use by the German forces from the late 1920s. It defeated attempts to break it through letter frequency or contact (frequency of adjacent letter pairings, like TH in English) by moving the wheels every 26 strokes; a deficiency was that no letter could be represented by itself, but the system was regarded as unbreakable. McKay lists the rotating wheels, rings, contact studs, plugboard and lampboard components of the Enigma machine, which needed a similarly set up machine at the recipient's end to decipher messages from the four- or five-letter groups transmitted. Both Paterson and Smith describe the mechanism in rather more detail. In addition to diplomatic eavesdropping, some attempts to break Enigma had been made by Knox at GC&CS in the late 1930s ; Denniston considers that this contributed to BP's ability to read Luftwaffe codes within a few months of the outbreak of war. At different times later, apparently, Rommel and Montgomery in North Africa each had his codes penetrated by the enemy. Brilliant eccentrics Hugh Foss and Col John H Tiltman had been reading Japanese naval and diplomatic codes (and those of other countries) through the 1930s. The work of these two on Japanese codes at BP after 7 December 1941 is mentioned by McKay but recounted more fully, together with the substantial Australian effort in Ceylon and Australia, by Michael Smith in *The Emperor's Codes: Bletchley Park and the breaking of Japan's secret cyphers* (Bantam Press, 2000). Interestingly, breaking the code used by an eminent Japanese visitor to occupied France in 1944 revealed valuable information about the German defences.

Mckay draws attention to the recruiting for BP from the intellectual and social elite, more old University gown than old school tie. There were five senior code breakers from Trinity, Cambridge, alone, while young Peter Hilton and Donald Michie, later to be distinguished professors of mathematics and artificial intelligence, respectively, were among those extracted from Oxford. Patriotic daughters from good families were sought (including a governess-educated goddaughter of Louis Mountbatten and another who drove a Bentley) for meticulous secret filing and, as mobilization of women extended, advantage was evidently taken of family and social contacts to secure what were supposed to be congenial but vital appointments. Academics and aristocrats apart, the sources of senior staff included crossword experts, linguists, antiquarian booksellers, barristers, and gifted chess players. Young women graduates were extracted from Newnham, Cambridge, and Scottish Universities. All but the most senior staff were first billeted or housed in hostels in villages within 20 miles of the Park, including some of the WRNS at Woburn Abbey, with daily transport by bus on shifts. Some of the newcomers remained as civilians but many Service officers were in post, while the influx of ATS, WAAF (on teleprinters) and, especially, WRNS (totalling l000s) caused BP staff to be overwhelmingly female. Despite the deference of the time, it appears that all, civilian and Service, honourables and untitled, graduates and undergraduates, young and old, collaborated with little concern over rank or status: more University common-room than officers' mess. What was, however, a predominantly young, well-educated, middle-class society enjoyed, off-duty, relatively high-brow artistic and recreational activity, including recitals of lieder, madrigals and quartets, play-reading, amateur dramatics, languages, cinema club, chess, badminton, Scottish dancing, and Footlights-standard (including future West End professionals) Christmas revues. Among notable subsequent appointments of former BP people, McKay mentions Chancellor of the Exchequer, Ambassador to Moscow, Director of GCHQ, Head of MI 5, National Gallery Curator, Director of Sadlers Wells, with several chairs of mathematics and history.

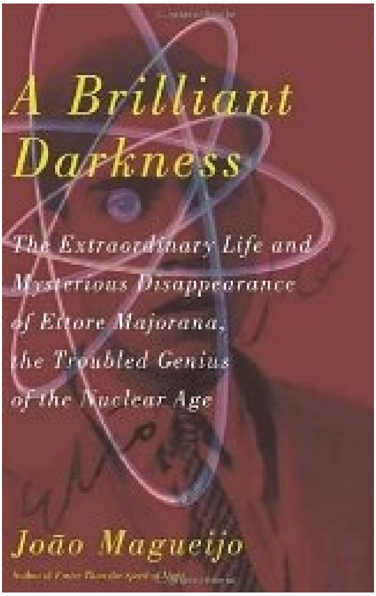
Physicists may be particularly interested in the re-invention by Turing of the 'bombes' (machines, called after - though different from - the Polish bombas, which could rapidly check hundreds of combinations) and of progress towards a machine, Colossus, that could cope with more complex encoding than Enigma. In principle, Turing felt that if wiring, rotors and a steckerboard could encrypt, then another mechanical system should aid decryption. He and Welchman designed an electro-mechanical machine to check rapidly endless combinations; it evolved and was constructed under Harold Keen at BTM, Letchworth. The first bombe, Victory, was a noisy fast-running electrical machine in a large bronze cabinet, weighing a ton, with 30 rotating drums, corresponding to the wheels of ten Enigmas. It ran through wheel choice, order, ring position, and machine setting at high speed to check on and so exploit cribs or Cillis (easily remembered letter key combinations used by a German operator ignoring the rules). During 1940, mathematician Joan Murray/Clarke, Turing's sometime fiancee, utilized Victory on naval Enigma, but a breakthrough on the U boat home-waters Enigma did not come until the capture of key tables for February, 1941, off the Lofoten

Islands. This was critical because at this time the German B-Dienst had broken the current Royal and Merchant Navy convoy codes (as they had done for a time previously and did in early 1943), giving U-boat wolf packs access to convoy routes. In May, 1941, the highly organized Royal Navy attack on the weather ship Munchen and the opportunistic action on U-110 enabled June and July Enigmas to be read and led to breaking the naval Enigma, with dramatic reduction in shipping losses. Meanwhile Welchman's realization that a reciprocity property could be exploited led to the incorporation of a plugboard in the bombes, of which over 200 were manufactured, distributed over several different sites. Large arrays of noisy Hollerith punched-card machines were also used.

Messages between Hitler and his generals were enciphered in more complicated teleprinters, codenamed Fish by BP, who concentrated on the one, codenamed Tunny, which turned out to be the multi-rotor Lorenz SZ40, of which several hundred were built. This had two drive wheels and two sets of five enciphering wheels and employed the Baudot-Murray system of five-hole paper tape. Although no-one at BP had seen a Lorenz, Tiltman, capitalizing on a German operator's abbreviations, managed to recover a text. Equally remarkably, a modest young chemistry graduate, Bill T Tutte, sought patterns among the sequences of characters and in two months managed to work out the complete internal structure. In 1942, Tunny material still had to be worked through by hand, but Peter Hilton, chief Tunny cryptologist, was adept at visualizing two teleprinter streams and deducing wheel patterns, aided by slips by less punctilious German operators. Recalling their notions from the 1930s of a computing machine, Newman and Turing got Charles Wynn Williams at TRE to design and build from telephone components what became the Robinson, delivered in May, 1943, but its spiked sprocket wheels for two synchronized paper tapes kept ripping the tapes. Some of the codebreakers were sceptical of the suggestion by Flowers (who had first met Turing at BP in 1939) of a thermionic valve machine with photo-electric reading, obviating the need for synchronizing two tapes, to search rapidly for the encypherer settings. So Flowers's group at PORS, Dollis Hill, independently worked frantically for ten months and presented Colossus to BP in December. The first Mark 2, built in early 1944, contained 2500 valves, and was in time to yield valuable high-level intelligence just before and after D-day, June, 1944 (4500 encrypts per day). To deduce codewheel positions at the start of a message, Colossus tried all combinations, processing at 5000 teleprinter characters per sec. A cryptanalyst and ten or twenty WRNS were needed each shift for setting up Colossus and processing. Not shown by McKay but, after Colossus had in effect stripped the input of the effect of the first set of wheels, cryptanalysts in the Testery (named after Major Ralph Tester) worked out the settings for the second set of wheels before the tapes were fed to a third set of machines. These large Tunny racks, built from Post Office components at Dollis Hill [and illustrated only in Ted Enver' s much reprinted little book *Britain's Best Kept Secret* (Bletchley Trust, 3rd edition, 1999, reprinted 2010)], simulated the Lorenz machines to yield, it was hoped, plain German text.

Of the ten Colossuses built, mostly installed in late 1944 in a new block H, eight were destroyed in 1945 to preserve secrecy (mainly from the Eastern bloc, presumably, but some countries still used such encryptions). Two went eventually, with about 50 bombes - which continued to be used after 1945; the rest were dismantled - to GCHQ and later were also destroyed. Models have since been painstakingly built; the Colossus was switched on in 1996. It later transpired that John Cairncross, believed to be the fifth Cambridge spy but never prosecuted, who was in Hut 3 of BP 1942-43, regularly passed decrypts from BP (Krurost to the Russians) to a USSR handler. Although programmed essentially for one task, Colossus was the first semi-programmable electronic computer, despite earlier claims of priority, unchallenged for decades, from ENIAC at Harvard. The ingenuity and persistence of Flowers, who was tragically required to destroy all plans and drawings for Colossus, in achieving the logical concepts of Newman and Turing has been celebrated publicly only in recent years. After VE-day, BP dealt with a mass of technical documents from Germany but Barbara Abemathy, who had joined GC&CS in 1937, closed the gates of BP in 1945. War-shortening estimates are conjectural but BP as the hub of strategic intelligence certainly had a profound influence on progress to the Allied victory in World War H, as Eisenhower appreciated. Winterbotham even wondered if the War in the West could have been won without ultra intelligence.

Mckay's book provides a valuable survey of the Wartime activities in BP, covering both codebreaking and the informal recruitment and social story of its people. It has a fair index, has notes, mainly books and interview attributions, and ends with a summary of the rescue and restoration by the BP Trust. This rescue was prompted by a letter in 1991 from senior codebreakers to the Prime Minister, John Major, timed for the anniversary of an even more important appeal by the codebreakers to the then Prime minister, Winston Churchill, on 19 October, 1941. The signatories of the 1941 letter calling for more resources for BP included Turing, Welchman (author of *The Hut* 6 *Story* (AlIen Lane, 1982), two chess masters Hugh Alexander (Cambridge mathematician successor in 1941 to Turing as Hut 8 head) and Stuart Milner- Barrie (Cambridge classicist stockbroker, Welchman' s successor as head of Hut 6). Milner-Barrie was also one of the signatories of the 1991 letter calling for listing to preserve the site from destruction and for support towards the formation of a charitable trust. Enver's 106-page book explains the achievement of the BP Trust, illustrates the 1991 letter and, unusually, gives drawings of the tall Tunny machines. Since it details the existing and wartime mansion, huts and other buildings, Enver is recommended for anyone considering visiting the historic site, which also houses several other displays, not least a history of computing exhibition. Incidentally, Enver raises an eyebrow at Channel 4's 'never before published" claim for its *Station X* programmes in the 1990s. Paterson's book fits BP into the broader intelligence story and is particularly good with personal accounts of the naval exploits to seize current Enigma settings from U-33, U-11 0, and U-559. Finally, for those with little time, Lindsey Butters's little booklet *Bletchley Park: Home of Station X* (Pitkin Guide, 2000, reprinted 2007) illustrates the BP story succinctly and remarkably well.



A Brilliant Darkness: The extraordinary life and mysterious disappearance of Ettore Majorana, the troubled genius

Joao Magueijo

Basic Books, New York, 2009
ISBN 978-0-465-00903-9
280pp Hardback £15.99

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In common, perhaps, with many scientists who are neither Italians nor particle physicists, I first encountered the name of Ettore Majorana (E M) at a conference/workshop in Erice, a small medieval, traffic-free hill town above the port of Trapani on the NW coast of Sicily. In 1962, Blackett, Dirac and three other eminent signatories established the E M Foundation Centre for Scientific Culture there. In practice, the Centre's most widely known activity has been to host relatively small (under 100 participants) highly international meetings and summer schools, generally including eminent scientists, in which students of whatever seniority or age can truly mingle and interact informally. With accommodation in former monasteries, long lunch breaks at different small restaurants in Erice, and late-evening wine-stimulated sing-songs, Erice meetings make American Gordon Conferences seem stressful by comparison. But who was E M and why should he be so revered and commemorated?

In his short career, E M (1906-1938) made such memorable contributions to particle, condensed matter and mathematical physics that he would almost certainly have been a future Nobel prizewinner. His 1937 paper in *Nuovo*

Cimento on a symmetric theory of electrons and positrons introduced the Majorana neutrino hypothesis that the antimatter partner of a matter particle could be the same particle, even if of spin $^1/_2$. This was in contrast to Dirac's theory that involved states of negative energy. The far-sighted influence of his genius was further compounded by the mystery of his disappearance in March, 1938, apparently from a ferry between Naples and Palermo. Conjectures about his fate range from the obvious ones of accident or suicide from the ferry (but without recovery of the body), through intentional disappearance (either to an unknown monastery or convent or to Buenos Aires, of which he had some knowledge about scientific institutes), to kidnap (at a time when the release of atomic energy was in prospect) by a foreign power or even by extra-terrestrial forces. These are a few out of many more fantastic hypotheses.

The combination of a short-lived far-sighted scientific genius and a surprising unexplained disappearance has prompted many reports and books, some painstakingly detailed and others more popular, stimulated by the centenary in 2008 of E M's disappearance. Most accounts of E M's life as distinct from his physics have been in Italian. The story has for long captured the serious interest of the Portugese-born theoretical physicist Joao Magueijo, now a professor at Imperial College. In his book *A Brilliant Darkness* he recounts interviews with E M descendants and places where E M lived and worked. He has thus compiled an unusual combination of physics explanations with an account of the life, disappearance and lasting influence of E M. The fairly gentle reader should be warned that Magueijo occasionally resorts to crude, normally unacceptable, words that might offend some without aiding understanding. Interviewing E M's aged sister-in-law and Fabio, son of E M's brother, Magueijo indicates his own uncultured Italian by writing in fractured English his side of a conversation conducted in Italian. Also, while paying appropriate respect to most individuals encountered, his setting of the 1930s background refers to Fascist attitudes to race (there is even a picture of the German and Italian leaders) and makes more than oblique references to Italianate notions of elections and open competitions. The thirty headings for chapters and end pieces are either ironic clichés or are artistically enigmatic, such as Don't cry for him Argentina, Pagliacci, or Pirandellian Intermezzo (E M was knowledgeable about the Sicilian playwright.) Magueijo's earlier book, *Faster than the speed of light* [1], ostensibly expounding his theory of varying speeds of light in the solar system but also trying to explain how some science is done, has been criticized for its attacks on academic opponents and on the University system.

One of the most diligent of recent researchers into E M's documents, unexplained departure, and inheritance to science is the Italian theoretical physicist at Naples, Salvatore Esposito, author of several books on E M (none of them included in Magueijo's "what I like best [on E M]' book list). In an analytical examination of plausible theories about E M's disappearance, Esposito's recent article [2] briefly summarizes E M's home and scientific life before presenting a careful chronological reconstruction of his last 75 days in Naples, paying due regard to the reliability and corroboration of alleged events. Neither Magueijo nor Esposito refers to the other's publication which, fortuitously, appeared at around the same time. Both authors acknowledge the biographical studies in the 1960s by E M's friend and colleague Edoardo Amaldi (1908-1989) and later by Erasmo Recarm. Surprisingly, a recent biographical article on Amaldi by his son Ugo [3] makes no mention of E M. Recami has been critical [4] of Magueijo's conjectures about psychological differences between E M and Enrico Fermi (head of the Rome Institute of Physics and a Nobel prizewinner in 1938) and is evidently offended by unsubtle punning in the title of Magueijo's chapter 'bread and sperm'.

Ettore Majorana was born in Catania on 5 August, 1906, the fourth of five children to somewhat domineering parents, Fabio Majorana, a senior telephone engineer, and Salvatrice (Donna) Corso. The extended family displayed many talents and exerted influence in Italian political and academic life. E M experienced an affectionate but strict home education with little time for play up to age eight; he was then sent to, and scored high marks at, a prestigious Jesuit school in Rome. His mother and family moved to Rome in 1921, joined subsequently by Fabio M. Already an arithmetic prodigy, E M graduated *licenza liceale* in 1921, aged just 17. He then did well in engineering at the University of Rome but, encouraged by his friends Amaldi and the slightly older Emilio Segre (later a Nobel laureate), he transferred after four years to physics. Well before his Master's graduation in 1929, E M was collaborating with the Via Panisperna Boys, as the team of bright but mischievous nuclear physicists was known (from its address) in the 1930s. Fermi was to emigrate to the USA early in 1939 and, of the Boys, only Amaldi remained in Rome. Despite much theoretical activity for the research group, recorded in personal notes 1925-1933, E M published surprisingly few articles, not for lack of encouragement by Fermi and the Boys. Much of it has now been published, thanks largely to Esposito. E M achieved the degree of *libera docenza* in 1932 but made little effort at academic advancement. Magueijo draws attention to differences between E M, the theorist, and Fermi and the Boys, who combined theory and experiment; they relaxed through energetic sport while E M enjoyed the arts.

In 1933 (the time of Hitler's assumption of power), in possession of a substantial grant, E M went to the Physics Institute of Heisenberg (depicted as a cultured playboy genius) in Leipzig, where, away from his mother, he blossomed socially and scientifically. In March, he made a side trip to Niels Bohr's Institute in Copenhagen and spent Easter in Rome. But back in Leipzig in May, his mood declined sharply. Magueijo keeps referring to a burned baby incident in a family linked with the Majoranas. This mysterious bedroom cot death also affected E M's brother and both mourned the death of their father in 1934. But E M was greatly disturbed by the discovery of the positron and from August 1933 to 1937 he became a recluse, rarely leaving his bedroom/office in his mother's large house in Rome. Of the Via Panisperna Boys, only Amaldi and E M's contemporary and best friend the Sicilian Giovanni Gentile, Jr, kept in touch.

In 1937, an Italian competition was announced for a professorship in theoretical physics. E M was persuaded to enter and included in his submission a paper based on his earlier relativistic theory of elementary particles with arbitrary spins, what Magueijo calls his masterpiece. The commission of which the members were all, like most successful scientists, members of the Fascist party, appointed E M 'for high and well-deserved fame' to a new professorial chair of theoretical physics in the Institute of Physics, directed by Antonio Carelli, at Naples. Taking this up in January, 1938, E M arranged to give a course of two or three lectures per week, barring national holidays, and from late February lived in the Hotel Bologna. However, Sciascia [5], listed by Magueijo, thinks that E M felt trapped by the prestigious chair into the academic normality of publishing, teaching, and maintaining a reputation, which he had always tried to avoid. He believes that E M pondered at length over a means of disappearing and doesn't dismiss entirely the monastery explanation.

Magueijo devotes only part of his book to the disappearance and possible interpretations although the premature curtailment of E M's public scientific life hangs over much of it. Magueijo refers to facts and factoids while Esposito lists evidence, uncertain data and remarkable coincidences. Some time before apparently boarding the overnight ferry *Citta di Palermo* from Naples to Palermo on Friday night, 25 March, E M had withdrawn a large sum (perhaps one-third of an annual salary) from his bank and he took his passport (unnecessary for an internal ferry). He had handed notes on scheduled lectures to his student, the ultimately long-lived Gilda Senatore (Magueijo interviewed her), left what could be regarded as suicide notes to his family (announcing an intention to 'disappear') and to Carelli, and was not unequivocally identified again. He apparently booked a room at a Palermo hotel, apparently wrote another letter to Carelli (saying he was giving up teaching) and sent a telegram, and apparently boarded the return ferry at 11pm on Saturday night, 25 March. Certainly, he did not appear at the Hotel Bologna or the Institute on Monday, 28t" March. The family was convinced that he had not committed suicide and even petitioned the Pope and attempted through Fermi to contact Mussolini. It happens that the *Oceania* would depart from Naples on 26 March, on voyage from Trieste to Buenos Aires (E M was well aware of this shipping line, which would have only rather cursory checks on its passenger list; he had met Guiseppe Occianili (collaborator of Blackett) when he called briefly at Naples on 18 January as the *Oceania* returned from South America to Trieste). Both Magueijo and Esposito devote some attention to this South American trail, prompted by the reports of Rivera in Buenos Aires (B A) in 1950 and 1961 suggesting the presence of an E M-like character. E M knew about the Faculty of Exact, Physical and Natural Sciences in the University of B A from several visits by friends and colleagues (including Fermi) 1928-1937. Fermi pointed out that E M was such a thoughtful and intelligent person that he could have planned his disappearance carefully so as to appear like suicide and be so timed that attempts to check up on events would be delayed a day or two. Enquiries by the Italian police and the political police ceased after Spring, 1939.

Magueijo lists a selection of the many books and documentaries on E M and includes a fair index. One feature of the book, which includes many small illustrations of people and places, is the inclusion of mildly anthropomorphic (smiley faces) diagrams to illustrate concepts such as quantum spin, parity invariance, and electron-positron annihilation. For those mainly interested in a careful evaluation of the facts about E M's disappearance, together with a summary of E M's upbringing and scientific appointments, the article by Esposito is recommended, with its clarity about which data are uncertain and its laconic reporting of the coincidence of independent observations. (This issue of *Contemp Phys* also contains two of my essay reviews on James Lovelock and Hungarian physicists.) Both Esposito and Magueijo conclude that there is still no satisfactory explanation of E M's disappearance but his lasting influence is evident in laboratories and institutes worldwide. Magueijo is more expansive than Esposito (in his recent article) on E M's wider interests, on his idiosyncrasies and uneasy relationship with the Panisperna. Boys, and on the causes of his depression. He ends with brief updates on the distinguished later careers of Fermi, Heisenberg, and the Panisperna Boys, including Bruno Pontecorvo (the only one that Magueijo met) who spent most of his time in the USSR. Magueijo may not have illuminated the darkness hiding his aloof but theatrical hero's disappearance much but his original and provocative, if sometimes coarse, approach does help to explain what troubled the genius.

References

1. Joao Magueijo, *Faster than the speed of light* (Basic Books, 2003)
2. S Esposito, *Contemp Phys* ***51*** [3] 193-209 (2010).
3. Ugo Amaldi, *Phys World 21* [9] 32-36 (2008)
4. Erasmo Recami, *Phys World 23* [4] 20 (2010)
5. Leonardo Sciascia, *(The Moro affair and) the Majorana Mystery* [Two books of the 1970s translated from the Italian by Sacha Rabinovitch] (Carcanet Press,1987)

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Local Hero

One of the most appropriate functions of our History Group is to help in the bringing to light of significant physicists of the second rank, too often overlooked in the accounting of the progress of science. One such is John A. McClelland (1870-1920), a student of JJ Thomson who founded an enduring school of research in University College Dublin. Tom O'Connor has recently provided a comprehensive biographical article* on McClelland . Not only does he recount this career in detail but he provides a valuable sketch of its background in the changing Irish university scene of the early 1900s.

McClelland was a Protestant Ulsterman, as was his predecessor at UCD, Thomas Preston, a remarkable fact given that UCD was a remnant of the unsuccessful Catholic University, under the supervision of the Catholic hierarchy. Unlike Preston (also the wretched Gerard Manley Hopkins, another Professor of that period), he lived to see the tiny Jesuit institution emerge from unhappiness and obscurity. Both Professors of Natural Philosophy founded enduring traditions in the Department: Preston in atomic spectroscopy, McClelland in atmospheric aerosols. Half a century after McClelland, it was instrumental in the invention of the kind of smoke detector that most of us use at home. A wonderful example of pure science leading to applied, if on rather too long a time scale for our political masters!

O'Connor's current project is a guide to the physicist visiting Dublin. No doubt it will take in the building in Stephens Green, where McClelland lectured. Among those that heard him there must have been a young man by the name of James Joyce.

*T O'Connor, Phys. Perspect. **12** (2010) 266-306

Denis Weaire
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Rutherford

University of Manchester March 31st 2012

There will be three lectures in the afternoon. Pear Lecture Theatre.

'Rutherford and the 1912 Extension to the Physical Laboratories of the University of Manchester.'

Neil Todd

On the evening of 1st March 1912 a conversazione directed by Arthur Schuster was held in the Physical Laboratories of the University of Manchester to mark the opening of the new extensions. It seems appropriate then to mark the centenary of the 1912 extension with a discussion on its significance for Rutherford's science at Manchester, especially since our meeting will be held in March 2012 the old 1912 Physical Lecture Theatre. In addition to the fact that there was considerable overcrowding in the old 1900 Building, in part due to the massive expansion in the number of researchers in the Rutherford school, an important argument for the new extension was that it would create an environment free from the radioactive contamination which was by then widespread throughout the old Laboratory. The contamination free physics rooms enabled Rutherford to develop his interest in gamma and beta-ray spectroscopy, which required long exposure times, and which became a major thrust of his work at Manchester before the outbreak of war in 1914.

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'Rutherford's resonance: responses to the discoveries of 1911 and 1932'

Brian Cathcart

Abstract expected shortly

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‘The apparatus used for the discovery of the neutron’

Geoffrey Constable

Following a series of famous experiments, Dr Chadwick announced the discovery of the neutron in February 1932 via a letter to Nature. The scientific arguments supporting this discovery were described here and, more fully, in the subsequent Royal Society paper, and are well known.

These documents provided an outline of Chadwick's apparatus sufficient for scientific purposes. However, the details of this apparatus are sketchy and there are no detailed drawings or records in the Cavendish archives or elsewhere in the public domain. Hence there are gaps in our knowledge, particularly as to how and where this apparatus – which was innovative – was constructed, proven and refined. The object of this paper is to fill in some of these gaps.

Dr Jack Constable – the father of the present writer – was a research student at the Cavendish Laboratory from 1928 to 1931. Under the supervision of Chadwick, he contributed to the design of a novel 'valve counter' plus associated devices that collectively formed a system for detecting and automatically recording radiation. He then undertook the construction of this system, plus its proving and subsequent development. His PhD studies concluded with a series of experiments that involved bombarding various elements with alpha particles from a polonium source in order to measure the energy levels of the emerging radiation. Such experiments were similar in function to those undertaken a few months later by Chadwick (but had a different objective) and it is clear from contemporary papers that the apparatus used in both cases was one and the same.

A recent examination of family papers and other records, some photographic, has yielded fresh information concerning the apparatus used by Chadwick. In particular, light is shed on a (then) new experimental technique, an unusual approach to constructing scientific apparatus, a raft of detailed refinement that led to the remarkable sensitivity and resolution that was achieved, and a little known experimental result that could have led (but didn't) to the earlier discovery of the neutron.

There will be a tour in the morning of the Rutherford building, possibly also a visit to Rutherford’s house followed by lunch.

For further information visit:
www.iop.org/activity/branches/north_west/manchester/calendar/index.html

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