

Cover picture: An early demonstration version of a light modulator, rather grandly called an Acetylene Photophone c1920.
Taken from *'The Moon Element'* by E.E. Fournier D'Albe. TF Unwin, 1924.

Contents

Editorial	2
Meetings Reports	
Physics at the Clarendon Laboratory	3
Lectures:	
Theoretical Physics in Oxford <i>by Roger Elliot</i>	4
Frederick Lindemann <i>by Adrian Fort</i>	12
Features:	
Why Physics Needs Oral History <i>by Jim Grozier</i>	22
Sir Joseph Rotblat and the Forgotten Physicists	
<i>by Neil Brown</i>	30
Physics in Heaven and on Earth <i>by Peter Schuster</i>	34
Hibbert's Magnetic Balance <i>by Stuart Leadstone</i>	38
Book reviews	
Thomas Young – The last Man Who Knew Everything	51
From Clockwork to Crapshoot – A History of Physics	58
Patrick Blackett – Sailor, Scientist and Socialist	61
Book notices	67
Letters	68
Web news	70
Forthcoming meetings	71
Committee and contacts	72

Editorial

The history of physics (and of chemistry and mathematics) gets little serious exposure in the popular media and *comic*, almost never. But do you remember the 60's American comedian, pianist (and mathematician), Tom Lehrer, who sang a song, the words of which consisted of nothing (well, almost) but the chemical elements? He also sang about the Russian mathematician, Nikolai Ivanovich Lobachevsky, (famous for his introducing non-Euclidian geometry), and told a story of a remote tribe in the Andes who furtively practised gargling, passing the secret down from father to son as part of their *oral* tradition. As he would have said, that last was just a sneaky way of commenting that our oral tradition is none too strong but on page 22, Jim Grozier presents a thoughtful case for pursuing this aspect with all vigour, in his article on a history of the Neutron Electric Dipole Moment. Neil Brown recalls another piece of oral history - the talk given to the group in March 1999 by Sir Joseph Rotblat – and some unexpected problems in its transcription, and Colin Hempstead is searching for aged physicists who worked in industry to record some oral history before it's too late!

It was most pleasing to see a name, very well known among physicists, mentioned in a newspaper, and in a comic sense too. A few weeks ago, a story about David Hilbert, (who, coincidentally, followed Lobachevsky in the field of geometry) was featured in Sandi Toksvig's column in the Sunday Telegraph and so to finish on a note of humour and another piece of oral history, I give an abbreviated version:

*He was invited to give a talk on any subject he liked. This was in the early days of commercial aviation and the venue required that he catch a plane to the lecture. Prof Hilbert duly advised that the title of his talk would be: 'The Proof of Fermat's Last Theorem.' This caused a sensation, as the theorem was then one of the great unsolved mathematical mysteries. On the day, Hilbert arrived and spoke brilliantly, but failed even to mention Fermat. After the talk, he was asked why he had chosen a title that had nothing to do with his lecture. 'Oh' he replied. 'that was just in case the plane went down.'**

Malcolm Cooper

* Reproduced by kind permission of the Sunday Telegraph

Meeting Report

Physics at the Clarendon Laboratory

On Saturday, June 9th, some thirty people attended a very interesting afternoon meeting on the above subject. Many of those attending met up at the nearby Café Rouge for lunch. The first speaker was Robert Fox, who until recently was the Professor of the History of Science at Oxford, and is the outgoing President of the European Society for the History of Science. He had literally just arrived back from Geneva and gave a most interesting lecture on the early days of Physics at the Clarendon and explained why at that time the Clarendon was not as successful in physics research as the Cavendish Laboratory at Cambridge. This was followed by an excellent talk by Adrian Fort on the extraordinary Frederick Lindemann. It was Lindemann who did much to transform the Clarendon into a formidable place for physics, which it still remains to this day, as well as exerting enormous influence in the corridors of power and in No 10, especially during the Second World War. Many of the audience took the opportunity to buy a copy of Adrian Fort's book about Lindemann – details of which are on page 67.

After tea, which provided an excellent opportunity for people to meet each other, the final talk was given by Prof. Sir Roger Elliott on the development of theoretical physics, as an independent department, at Oxford. This was only developed after the Second World War but rapidly built up a good reputation. However, this was greatly increased in 1963, by the arrival of Rudolph Peierls from the University of Birmingham.

Articles, based on the lectures by Fort and Elliott, appear in this volume.* The meeting was enhanced by a collection of photographs from the archive of the Clarendon Laboratory. Thanks must go to Dr. Jim Williamson for assembling these and to Dr. John Roche of Oxford, who also did much to help with the local organisation of the meeting.

Peter Ford

*It is hoped to include an article by Robert Fox in the next newsletter.- Ed

The Development of Theoretical Physics in Oxford - an Informal History

Prof. Sir Roger Elliott

Theoretical Physics in a recognisable prototype form of what we have today only began in Oxford after the Second World War. Before that there was no such tradition within Physics although the subject was represented in the Mathematics Faculty by the Rouse Ball Professor, Edward Milne and later Charles Coulson. In the interwar years Lindemann had been rebuilding Oxford Physics from its moribund state but his priorities were clearly in the experimental area. Here he used his extensive contacts with German physicists to help many scientists who were being persecuted by the Nazis and he succeeded in bringing some to Oxford to create a world class low temperature group with Francis Simon, Kurt Mendelssohn, and Nicholas Kurti and in strengthening the spectroscopy group with Heinrich Kuhn. There were opportunities on the theoretical side but for various reasons these did not mature. Einstein himself spent some time in Christ Church before being tempted away to Princeton although his interaction with the physicists appears to have been modest. Schrödinger was for a time a Fellow of Magdalen although his erratic behaviour and unconventional lifestyle alienated many of his supporters. He returned to Vienna, only to be rescued again by Eamon DeValera to his Institute in Dublin. Max Born was also approached but he preferred to go to Edinburgh apparently because of his dislike of Lindemann.

Part of the problem was the lack of a permanent post to lead Theoretical Physics and one possibility was a dedication of the Wykeham Chair to this subject. The idea was apparently first suggested by Bragg when he became an elector in 1938 but there was an insuperable obstacle in the person of John Townsend. He had held the Chair since its inception in 1900 and although initially he had done important work on ionised gases and had built up the Electrical Laboratory in the form we now call the Townsend Building, he had long since ceased to be active in research and showed little interest in teaching. He was already over 70 but in those days there was no retirement age. Eventually pressure from the University forced him

to retire but by then the war had started and everything was on hold. (There is a story, probably apocryphal, that when asked why he did not resign he replied that he was waiting for his knighthood to recognise his lifelong contributions to science. Lindemann supposedly then arranged this and he was indeed knighted just before his resignation.) Lady Townsend remained active in local politics into the post-war years.

After the war it was agreed that the Chair should be allocated to Theoretical Physics and attached to the Clarendon Laboratory which had now absorbed Townsend's Electrical Lab. It was advertised with a salary of £1200 p.a. (with children's allowances).



Maurice Pryce

The elector's choice fell on Maurice Pryce, (left). Others known to show interest included Heitler, London and Wheeler but the electors took the bold option of electing someone only 32 years old who showed exceptional promise. In fact he looked so much younger than his years that he was once challenged by the Proctors in the Kings Arms who thought he was an undergraduate. At Trinity College, Cambridge, in the early 1930's, Pryce had been regarded as one of the brightest mathematicians of his generation. Fred Hoyle, in his autobiography, says that Pryce was much the cleverest of his contemporaries; he calls him a 'wizard'.

He did important work with Born on early versions of field theory and made contributions to neutrino physics, demolishing in a classic paper the briefly fashionable view that photons might consist of pairs of neutrinos. He worked in Princeton with Pauli and von Neumann before returning to Cambridge and moving to Chadwick's department at Liverpool just before the war.

Pryce in his reminiscences mentions two highlights from his scientific career. At Princeton he had an introduction to Einstein from Born and went along with some trepidation to see the great man. He found Einstein deep in conversation with Rosen and immediately included Pryce into that discussion, showing considerable interest in his views and insights even though he was a strong supporter of the Bohr interpretation of Quantum Mechanics. On a second occasion when he presented one of his papers in Cambridge, Dirac spontaneously offered to communicate the results to the Royal Society. Indeed Dirac was a strong supporter in his application for the Chair.

During the war Pryce led a team at the Admiralty Research Station and made an important contribution to the theory and practice of the propagation of radio waves and radar, and subsequently with the Anglo Canadian team working on the design of reactors. During his time at Liverpool he had worked on fission but later declined to be involved in the atomic bomb project. He was therefore a man with very extensive interests and knowledge of physics.

Once in Oxford he rapidly built up a large group of research students. At that time there were many people returning from war service, some from the military and some from scientific occupations and they were all anxious to get on with their studies. These included Anatole Abragam, who went on to have an important influence on the rejuvenation of French physics, John Ward, whose contributions to field theory are well known, Ken Stevens in magnetism, Roger Blin-Stoyle a former president of the IOP, David Brink in nuclear structure, John Ziman who worked in solid state and later in the sociology of physics, and several others. Pryce's technique was to offer only broad advice and let people find their own specific problems. While this was conspicuously successful in some cases there were some casualties who ended up with what were effectively insoluble problems for the time. There was only one other permanent appointment in Theoretical Physics, Stanley Rushbrooke whose interests and knowledge in thermodynamics and statistical mechanics attracted Simons attention. Cyril Domb who had worked with Pryce at the Admiralty held a short term position though he eventually went back to Cambridge and then to Kings College London. Nevertheless there was a core to make an active theoretical physics group and Pryce instituted regular seminars, and a

journal club which met in the evenings in Balliol, and some graduate lectures. I still remember how totally incomprehensible were those given by John Ward, but then he was famously taciturn and brief. His thesis for example was only 26 pages long which might serve as a model for some of our graduates but did contain the proof of “Ward’s Identity”. Pryce’s own research was in magnetism and the nuclear shell model and he developed a close collaboration with the groups doing paramagnetic resonance under Brebis Bleaney and others.

Pryce however was frustrated by his position within the Clarendon Laboratory where every administrative and financial decision had to be referred to Lord Cherwell as he had then become. Accommodation in the Townsend Building was very cramped – indeed part of it was an incompletely converted gentlemen’s lavatory.

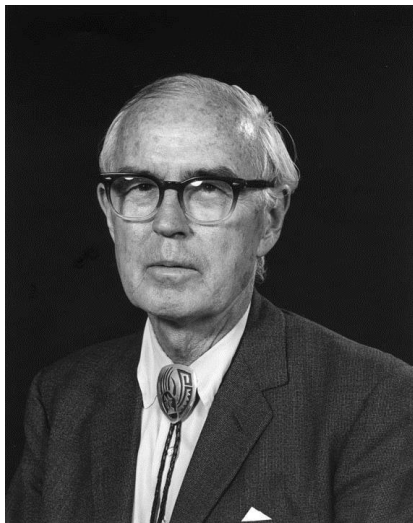
On one occasion John Ziman and I asked Pryce if we could have an electric calculating machine, a Marchant, to replace the hand Facit machines which were then in use. Pryce said he had no money and we had to ask Cherwell ourselves which we did with some trepidation. He received us courteously, showed us how to use a slide rule (with which he claimed to have won the war) and sent us away. But we did eventually get the machine.

For this reason, and possibly for personal ones, Pryce accepted an invitation to replace Neville Mott as the Head of Department in Bristol when the latter moved to the Cavendish Chair in Cambridge.

The file relating to the subsequent election to the Wykeham Chair is still closed but it appears that the electors faced a dilemma since there were no applicants. Several feelers were put out and Weiskopf headed the group for a time as a Visiting Professor but in the end the electors made, what seemed to many, a surprising choice. Willis Lamb was an American with little previous contact with Oxford but he had some obvious attractions. He was already a Nobel Laureate and his work, both theoretical and experimental, exploited spectroscopy and microwaves, both techniques which were widely in use in the Clarendon. Lamb was also a highly versatile physicist with a wide knowledge of the subject. He was therefore able to nurture the

broad interests of the large group which Pryce had left behind, although he took no research students of his own. He also interacted strongly with the low temperature group under Kurti.

His personality and American background also left him at a disadvantage



when faced with the complexities of the Oxford system. Although technically Theoretical Physics had now become a separate department as part of the package to attract him, together with other inducements such as a designated parking place, not all the promised new appointments materialised, and it was essentially fully parasitic on the Clarendon Laboratory.

In the meantime great changes were taking place in Oxford Physics with
Willis Lamb
the creation of the Nuclear Physics

Department under Denys Wilkinson. It had been decided that there should be a large research institution based in a university to carry forward research in nuclear physics outside the Atomic Energy Establishments where for historical reasons this was then concentrated. Oxford was an obvious choice because of its proximity to Harwell and Denys Wilkinson was an inspired leader. At a stroke the size of Oxford Physics was doubled with a large number of new appointments and Wilkinson not surprisingly demanded a separate department so that the generous financing could be separately administered. This put considerable pressure on the Theoretical Physics group since those like Roger Blin-Stoyle, Brian Buck and David Brink who were working in the nuclear area were naturally attracted to work closely with this new group while those of us with interests in condensed matter felt more closely allied with the experimental groups in the Clarendon Laboratory. It looked for a while as if the department would dissolve and the pattern, not unknown elsewhere, where the theorists mixed with experimentalists in the same discipline, rather than with each other, was the norm.

It was at this point that Lamb decided that he would return to the US and the electors made what seemed the obvious choice by appointing Rudi Peierls. By then it was universally recognised that he had created in Birmingham the largest and most successful group of theoretical physicists in the country.

On accepting the Chair, Peierls made it clear that he wanted to recreate an effective Department of Theoretical Physics in a similar form, though of course adapted to the Oxford system. For this to happen he recognised that such a department needed its own premises and he was promised that new accommodation would be provided in the continued expansion plans for Oxford Physics. In the meantime the department was allocated 12 and 13 Parks Road which, with some outstations as the department grew, were to be the Department throughout his tenure.

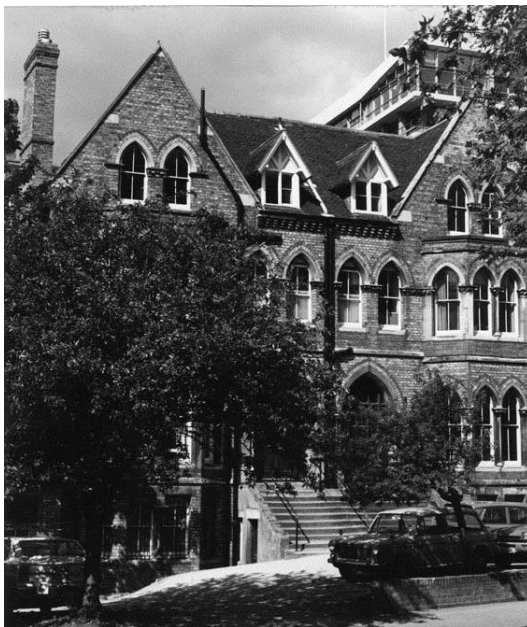
Because they were supposed to be temporary, conditions were relatively Spartan but this did not affect the spirit of the group.



Rudolf Peierls

The transition to the new premises was made by Ter Haar who had been appointed as Reader by Lamb, again with Simon's influence, to provide some expertise in statistical mechanics, although his interests in astrophysics were an added bonus. Once Rudi had arrived he set about creating the atmosphere for a department which still lingers today. Coffee at 11.00 and a weekly picnic lunch (supplied by Mrs Palm from the Market) increased the interaction between all branches of the Department. A more formalised programme of graduate lectures was instituted to ensure that graduate students got a wide grounding in all aspects of theoretical physics.

His concept of theoretical physics as a unified subject using common techniques though in different experimental areas was the philosophy which led the Department and still does so today. He also brought to Oxford a number of important new appointments.



12/13 Parks Road

Dick Dalitz came with his Royal Society Research Professorship; John Taylor, Ian Aitchison, Robin Stinchcombe and others all joined us in this period. Although the diffuse nature of Oxford with its conflicting loyalties between College and Department meant it was not possible to recreate the family atmosphere which had prevailed at Birmingham, Peierls created a strong sense of community.

One of the things that never happened was the promised new building as the unfinished walkways in the Keble Road Triangle testify. Not only did the money run out but a change in fashion meant that it was now impossible to knock down the row of Victorian houses in Keble Road to create that building on the corner of Parks Road where it had been planned.

As I know personally Peierls never relinquished the promise he had been given believing that it would strengthen the hand of his successor even if he could not use it himself. And indeed that proved to be the case since when I succeeded him I was able to bargain with the University for the reasonably acceptable and comfortable accommodation which we still occupy in Keble Road, (below).



It is not the new building which Peierls wanted but it is at least a functioning home for a Department which has served Theoretical Physics for 30 years. During this period I and my successor David Sherrington have attempted to maintain and develop the tradition of a unified department of Theoretical Physics emphasising common techniques and approaches. The group has normally included about one hundred physicists with a distinguished staff (including more FRS's than the rest of Oxford Physics all together) many postdocs and visitors. Several hundred graduate students have passed through and it is this training and the moulding of our approach which is the most important contribution. There are too many to mention in detail but a number have gone on to achieve great things in science and outside. They include one Nobel Prize winner, Ter Haar's student Tony Leggett.

The traditions have continued into a reunified Physics Department where as a Sub-Dept, Theoretical Physics still inhabits the separate premises in Keble Road and is ably headed by Dame Carole Jordan.

Frederick Lindemann

Adrian Fort

The Clarendon Laboratory at Oxford, which had a distinctly somnolent early life, was born anew in state-of-the-art glory just as the Second World War broke out. The prime mover and driving spirit behind its reincarnation was Lord Cherwell, formerly Professor Frederick Lindemann, and usually known as ‘Prof’.

At that time the Prof was a mere commoner, although he was soon to rise to dizzy heights, as satirised in the celebrated verses which did the Oxford rounds at the time, *Lord Cherwell, when the war began, Was plain Professor Lindemann...* Yet he was already a celebrated Oxford don, and, during the twenty years since the Armistice promised an end to all war he had almost single-handedly radically transformed the fortunes of the Clarendon.

When he first turned a rather baleful stare upon the Laboratory it was open for only three days a week, and even then it played host to a mere six undergraduates, two researchers, one part-time demonstrator and an assistant with a foot-lathe, none of whose endeavours were much helped by there being no electricity in the building. As Lindemann himself remarked, possibly with a touch of the self-serving, by the time he took it over the Clarendon’s reputation had been reduced ‘almost to zero.’

The laboratory’s present condition is a far cry from that, of course, and by the time that the new building opened Lindemann could rightfully claim that Oxford was in the world’s first rank for the study of physics. That had been his intention from the start, and long before he had achieved his aim the atmosphere of the building had become infused with his brooding presence, which lingers there to this day. By 1939, both he and Oxford physics had travelled a great distance, but the Prof himself was about to move to even greater heights, from which he would eventually leave a mark both on the scientific and the political history of England.

At the Clarendon, as elsewhere, he was rather a distant figure, and although encouraging and reasonably friendly when he spoke to those at work in the laboratory, he seldom shed an aura of aloofness; whether in the laboratory, or walking across an Oxford quad, people felt, in Lord Blake’s celebrated description, ‘that here was a man not to be trifled with.’ To some extent, no

doubt, that was because of his appearance: he was a tall man, by the standards of the time, comfortably over six foot, and of strong and upright stature. He invariably dressed formally, his clothes immaculately maintained by his faithful valet, Harvey, and was usually clad in dark suit and white shirt, with a bowler hat and often also with a black overcoat made of Melton cloth, almost the heaviest material known to tailors. When the weather was cold – Oxford in his day not being troubled by global warming – he would sport an even weightier construction, with an Astrakhan collar. Very occasionally, however, in the balmy days of high summer, he might be glimpsed in a grey suit and a Panama hat, rather crumpled.

The formal impression was accentuated by the fact that in Oxford he was usually accompanied – perhaps ‘attended’ is the more appropriate word – by one or more servants, or by other dons or science personnel eager for an audience. More distant excursions from his magnificent set of rooms in Christ Church were themselves a minor pageant: the first sign of impending movement would be when the arrival, on the drive outside Lindemann’s staircase, of a large limousine, usually a Rolls-Royce, although at one time he was conveyed about the place in an enormous Packard; the chauffeur, Topp, taking his place by the car’s door. The imminent appearance of the great man himself would be heralded by a Cairn Terrier, Dinah, a present from the famous lawyer and politician F.E.Smith; a little way behind Dinah would come Lindemann; behind him, at a discreet distance, would be Harvey, usually carrying baggage of some sort; and finally a footman, Rosborough, carrying newspapers or sandwiches for the professor’s journey. To this day Rosborough recalls, with wry amusement, how he was always ‘fourth in rank – behind the dog.’ Characteristically, Lindemann never took this animal for a walk, or stroked it, or in any way vouchsafed acknowledgement of its presence, although it shared his rooms, and it was noted that when she and the professor passed each other in the passageway there would not be the slightest reaction from either party.

Yet for all the outward and visible signs of correctness, there was always something very faintly foreign about Lindemann’s appearance. This seemed strange because he had early in life formed a great love of England and he became dedicated to striving for the country’s good – with results of the utmost importance as he drew near the pinnacles of power during the Second World War. Indeed, he could properly be described as fiercely

patriotic, nourishing an equally severe dislike for England's old foe, Germany. Many found this odd, as they believed Lindemann to be of German origins himself, and people were not shy of casting aspersions – behind his back. On one such occasion, during the dark days of the war, the culprit was a Member of Parliament, whom the Prime Minister – always staunchly loyal to the Prof - threatened to sack and expose in disgrace, being prevailed upon to let the matter drop only because of reasons of security.

In fact Lindemann's family did hail from Germany, but on its very border with Alsace, which is perhaps why Lindemann himself was so strong for England, his father's adopted country, becoming, in the old expression, 'more royalist than the King' – a trait he shared with some other famous men who rose to fame in an adopted country – Stalin was not Russian, for example, nor Napoleon French, and Hitler, of course, was not German.

Details of Lindemann's immediate forbears are scant, partly because enquiries on the subject would meet with a dusty response, a disposition he perhaps inherited from his mother, who firmly discouraged discussion of her origins. Word in the family, however, had it that she was the result of an encounter between a Russian noblewoman and her father, a Scotsman who worked for many years in Russia before emigrating to America. Lindemann was a child of her second marriage – the first had been to a partner of Rothschild's Bank, which was partly the origin of the *canard* – untrue – that Lindemann was Jewish. His father was a very able scientist, particularly in the field of astronomy, and he built an observatory in his garden in Devonshire, where Lindemann spent his very happy early years. Adolf Lindemann was also a successful entrepreneur, one result of which was that all his life the Prof was a very rich man, which no doubt contributed to the independence of his outlook on life, and to his unshakable confidence in the rightness of his opinions; also perhaps to his willingness, when he felt like it, to treat many people as if they hardly deserved to exist.

His introduction to science came from his schooling in Germany, where he was educated from the age of fourteen, latterly in Berlin, where he soon became the favourite pupil of Professor Walther Nernst. Those years, running up to the Great War, were, as all physicists know, a time of great excitement and remarkable development in the understanding of science.

Lindemann excelled himself in low temperature physics, both theoretical and practical – he was a very adept glassblower and designer of instruments, and invented with his brother a ground-breaking form of X-ray glass. At the age of only twenty-five he was invited to be one of two secretaries at the first Solvay Conference, where, it was later said, ‘it is doubtful whether in the history of science so much genius had been contained within the four walls of a single room’ – the genius being that of, inter alia, Planck, Einstein, Rubens, Madame Curie, Rutherford and Poincaré. Lindemann was encouraged to take part in the discussions, and his own paper on Melting Point theory was the subject of detailed analysis. By the great men at the Conference he was accepted as being a physicist of the first order, and with some of those taking part he became firm friends, particularly with Einstein, whom he was later to entice to Oxford.

Lindemann had meanwhile developed another strand of his character – his love of High Society – doing so through the introductions he received in the field of tennis. He was an outstanding player, winning many trophies in pre-WWI Europe, and played with both the Czar and the Kaiser; a few years later he was to feature in both the singles and the doubles championships at Wimbledon, not usually an arena for Oxford dons. Tennis was later to be instrumental in his meeting Winston Churchill, to whom in due course he became counsellor, confidant and closest companion.

Before that happened, however, the Germans had started the First World War, but Lindemann, because of his name, and suspicions about his origins, was not permitted to join up. This was perhaps fortunate, both for him – trench life would not have suited him – and for the country, because instead he found a berth at the Royal Aircraft Factory at Farnborough; there, working with a group of scientists many of whom were later to achieve worldwide fame as physicists, Lindemann did invaluable work, much of fit on the design of aeronautical instruments. More famously, he was the first man to discover the scientific basis of aircraft spin, a fearful problem which was causing great loss of life and aircraft in the battlezones. Having worked out what he thought was causing aircraft to spin, Lindemann had himself taught to fly, and within a very short time of his first solo flight he deliberately took an aeroplane up to 3000 feet and put it into a spin. It was an act of ice-cold, almost suicidal, courage, and not only did his theory work, but before he recovered control his aeroplane – converting it into a

dive – he had entrusted to memory seven or eight vital measurements which explained what happened while an aircraft was gripped by spin.

This brave and invaluable feat soon came to the notice of the Minister of Munitions, at that time Winston Churchill, who stored it away in his mind so that when, a few years later, his wife became a partner of Lindemann on the tennis lawns of upper class England, and spoke in glowing terms of this apparently brilliantly clever and demonstrably able sportsman, Churchill determined to meet him. The ensuing friendship flowered at a time when the country became threatened once more with devastating war: ‘He and Churchill’, wrote Jan Bronowski, ‘were matched socially, in tastes and in temperament; but they were also matched in intellect, and from the time of Hitler’s rise they formed the most powerful combination of minds that dared look over the political horizon.’

Churchill’s enquiring intellect was inspired by Lindemann’s scientific knowledge and by his capacity for associating ideas. His ability, unarguably unrivalled, of distilling into simple terms the most complex scientific ideas, enabled Churchill to develop his political attack on the drift and neglect into which Baldwin and Chamberlain allowed Britain’s defences to sink, as international relations deteriorated through the 1930s.

During the latter part of that decade Lindeman was by some margin the most frequent guest at Chartwell, where, weekend after weekend, Churchill prepared his speeches for Parliament and articles for the Press, for which Lindemann’s scientific input into how Britain’s defences – particularly air defence – could be rebuilt, proved an invaluable prop. Churchill’s valiant work was helped also by the Prof’s ability swiftly to analyse the secret dossiers, smuggled out of the Air Ministry, which portrayed the state of Britain’s defences – or the lack of them – as one country after another fell to the Dictators. Fighting his corner, Lindemann himself stood for Parliament, for an Oxford seat, in 1937, and with Churchill’s help he was placed on committees working on vital defence matters, most notably the Tizard Committee – until Lindemann’s blistering rudeness and antipathy to Tizard and his colleagues, A.V.Hill and Patrick Blackett, forced the government to close the committee down. Meanwhile Lindemann greatly encouraged Watson-Watt as he developed the one bright spot in those dark years, the invention of radar.

Meanwhile the Prof had been extremely active at Oxford. As the university was flat broke after the Great War, and as the Clarendon was moribund and direly in need of funds, Lindemann turned his energies to raising outside money for the Laboratory's development: he inveigled equipment from military sources, and used his burgeoning connections among the rich and well-born to prise money for the hiring of researchers and equipment. His contacts with the rich enabled him to lure men of independent means to come to the Clarendon, for nominal pay or none, notably Thomas Merton, Jack Egerton, Gordon Dobson and Derek Jackson, whose research was of the highest quality. The work of these men began to attract wide interest in the Clarendon, and demand for its courses began to grow. In his early years there Lindemann himself contributed much original thought, pioneering work on the ozone layer, for example, with Gordon Dobson, and before too long his work and persistence began to melt the disdain for science affected by what the Prof called the 'arts men' who dominated the university hierarchy.

However, Lindemann's contempt for those who tried to thwart him did not accelerate this process. Being very well read in literature, classics and history himself, the Prof was able to joust on equal terms with the arts men – who seemed to him to corner most of the available university funds; it was not something which they welcomed. 'Nasty, brutish and long-winded' is how, paraphrasing Hobbes, he described one very eminent Oxford philosopher, and of another leading don, he announced for all of Christ Church's Common Room to hear, that he 'would like to castrate him – not that it would make any difference.'

Yet despite the jealous dislike that he aroused in 'North Oxford', particularly among the university wives, who felt slighted that he would not condescend to enter their lowly parlours, money began to flow to the Clarendon coffers, and his development of Oxford physics powered forward. He himself continued, as contracted, to give one lecture a week, of which the content was of the very highest quality but was largely inaudible; and his exceptional mastery of detail meant that the Laboratory was very well-run, within the confines of the money available.

A quantum leap in this process occurred after Hitler's rise to power in Germany. Lindemann had always maintained his connections with the Fatherland, and he had kept in good repair his friendships - in particular

with Nernst; as soon as he heard of the passing of the Nazis' draconian anti-Jewish laws the Prof embarked on a tour of Germany, with chauffeur and Rolls-Royce, and, underwritten by the promise of funding from his friends the Monds, whose chemical interests were by then known as I.C.I., he persuaded some of Germany's leading physicists to come to England, thereby both revitalising Oxford physics and helping the lucky scientists escape the Gestapo's torture-chambers. Franz Simon, Ernst Schrödinger, Kurt Mendelssohn, Nicholas Kurti and Heinz London, all of whom found a berth in Oxford thanks to Lindemann, placed Oxford almost instantaneously on the world physics map, and, to the Prof's pleasure, made it a credible rival to Rutherford's Cavendish Laboratory, in what Lindemann was in later years to refer to as 'the eastern zone'.

When the Second World War began, Lindemann, seemingly assuming *droit de seigneur*, handed the Clarendon over to the Admiralty, to which its work on infra-red research proved of particular value. So did the Clarendon's growing expertise in nuclear physics, led by Lindemann's German imports: being aliens they were considered too insecure to be allowed near the infra-red programme, but it was thought quite all right to let them work on the atom bomb. Lindemann himself had pioneered research into the properties of uranium, and the development of the Tube Alloys programme led to a debate in the highest circles on whether to remove from public access Lindemann's paper on uranium isotopes, which he had written as long ago as 1919.

When Churchill became premier in May 1940 he installed Lindemann at his right hand, to establish a Statistical Department, to analyse and question all manner of ideas and figures across the spectrum of officialdom and Government ministries. The Prof became the filter for material entering the War Cabinet's lair, and his ability, almost as a human computer, to distil the most complex and wordy documents was invaluable to a Prime Minister conducting, for a time almost single-handedly, the myriad affairs of an embattled Empire.

Soon Churchill realised that the Prof's enormous influence, particularly as the scientific war developed, meant that he needed status and support to help him in his vital work – and to insulate him from the machinations of jealous rivals. So he was made a peer, and therefore a voice in Parliament, and given a seat, as Paymaster-General, in the War Cabinet.

Ruefully, the leaders of Britain's scientific establishments now realised that if they wanted to be heard on high they would have to go through Lindemann; and so it was, for example, that the famous interview between Frisch and the Prof led almost at once to the green light being given for Britain's development of the atom bomb, merged in due course with the Manhattan Project.

Through these years Lindemann encouraged scientific developments and ensured that they gained the support of high authority: Gee, H₂S, Oboe, the cavity magnetron, Window, anthrax bombs, the inventions of MD1, and numerous other initiatives were carried forward in good time partly as a result of Lindemann's smoothing their way. In doing so he worked himself to the bone: after long hours at his desk, poring over documents with a magnifying glass – he considered spectacles to be effeminate – he would be called into Churchill's presence, and the two men would talk alone late into the night, until even Churchill felt the need for repose.

With relief Lindemann would return to Oxford at weekends, and relax a little by writing learned articles for the *Quarterly Journal of Mathematics*, or in joining Christ Church dons in the Common Room after dinner, as candles would illuminate portraits of Viceroys and Prime Ministers, and Oxford prejudices would thaw a little in the warmth of Barsac, snuff, nuts, gold-plated fruit bowls, pipes and ancient panelling.

He rarely ate dinner in Hall, as he lived off an extraordinary diet, largely restricted to olive oil, half-grown peas, truffles, scrambled eggs – white-of-egg only, yokes being considered 'too exciting' – and Port Salut cheese. On Saturday mornings he would be driven to the Clarendon, where his chauffeur would collect eggs laid for his lordship's particular pleasure, while the Prof would run over the affairs of the Clarendon with his factotum, the ever-reliable Tom Keeley.

However, he did occasionally dine at High Table, sometimes when there was no one present except a few aged and unworldly dons, so that, with his immense intellectual power and surprisingly deep sense of humour, he could take his mind off the war and relish the harmless idiocies of others. For example, he recalled with pleasure sitting next to a rather ancient don and his guest, an equally elderly clergyman, who were discussing the imminent running of the Derby: "I've never actually been to the Derby",

said the don, turning to his guest, “have you?” “No”, replied the clergyman, “once, though, I very nearly did: I happened to be in Derby that very day.”

However, on other occasions the guests would be senior military men or high civilian officials, or others engaged in vital war work; at one such dinner the Prof sat next to the young Solly Zuckerman, and from their conversation stemmed the whole saga of Britain’s saturation bombing of Germany, which to an extent, and for a long time after the war, wrongly blackened Lindemann’s name. This was in part because of the revelation of his famous minute assessing the effects of bombing the fifty-eight towns inhabited by over 100,000 Germans, the benefits of which included the fact that the RAF might at least manage to find their targets, which – as a result of the Prof’s persistence, it had been discovered that they were clearly been unable to do when precision bombing was required - and also that if the German workers were bombed out of house and home their morale would collapse.

Lindemann’s reputation also suffered from his perceived scepticism about the Germans’ ability to build rocket bombs, so that when the first V2s landed on London Churchill sadly asked him “Why did you stick out your neck so far?” The answer was perhaps from jealousy of Duncan Sandys, Churchill’s son-in-law, who had been placed in charge of the programme for defence against V1s and V2s; it was a rare lapse from Lindemann’s disciplined objectivity.

When the war ended, with the release of forces that he did not believe that a Creator could have intended to be harnessed, the Prof returned with weary relief to Oxford. He was allowed little respite, however, as Churchill insisted that he become Opposition spokesman on economics in the House of Lords. That he was qualified to do so was partly because his ‘S’ Branch during the war had been staffed by very able young economists, including Donald MacDougall, David Bensusan Butt and Roy Harrod; and the Prof had himself led the Treasury’s team in America, negotiating Lend-Lease aid, until Keynes arrived to take over.

When Churchill returned to power in 1951 Lindemann joined the Cabinet once more, again as Paymaster-General, and was given the task of taking forward Britain’s independent atomic development. From his Cabinet seat he single-handedly opposed the Prime Minister, the Chancellor of the Exchequer and most of the Treasury, preventing them from putting into

effect ‘Operation Robot’, a plan simultaneously to float sterling and to block sterling balances held in London, which would almost certainly have paralysed the British economy. His battles to wrench atomic development from the dead hand of the Civil Service, housed in the Ministry of Supply under Duncan Sandys, at length resulted in the creation of the Atomic Energy Authority, the success of which may be taken as Lindemann’s political monument, just as the Clarendon Laboratory is his scientific monument.

On July 2nd, 1957 he fell ill, and his biographer describes his last hours, in his rooms overlooking Christ Church Meadow,

‘he would have heard the distant chiming of the bells and the slight evening breeze rustling the leaves of the tall elms, the quiet sounds gentle on the air. As he lay in bed his mind might have traced the path of a life of the most unusual breadth, passed in extraordinary times. Back through the recent battles in Government, and his success at last with Britain’s nuclear future; to his years of comparative rest after the war; and then to the war itself, six years of unrelenting work by the side of the country’s greatest leader, as England fought off the threat to her survival and saw her enemies utterly vanquished, one of them by an elemental power that he had not wished to contemplate.

Then back through his years in the wilderness with Churchill, seeing so plainly the threat that few would acknowledge, yet living in a world of ease that had now vanished. He might have felt himself once more spinning towards the earth in his flimsy, noisy aeroplane – entirely confident that his solution was correct – and landing unharmed, with the answer to the airmen’s fears safe in his pocket notebook. Before that, the trumpets and the laughter of Berlin, and dear Professor Nernst, and wondering at the discoveries of the age and his own part in bringing them about. At last he would find again the sun and happiness of his childhood, the pleasure of his father’s company and the joy of being with him in his garden laboratory. The thoughts of his life’s journey would have given him pleasure, richly deserved.

He died shortly after midnight.

This article is based on the biography of Lindemann by Adrian Fort - for details see page 67

Why Physics Needs Oral History – A Case Study

Jim Grozier
University of Sussex

The great thing about the history of physics is that there's so much of it – and more is being created all the time, as experiments are performed, theories emerge, get disproved, are replaced by new ones, requiring new experiments, and so on.

Of course, we often tend to think of the word “history” as applying only to events that happened before a certain date, perhaps related to our own lives; we think of the heroes of the 18th, 19th and early 20th centuries, but perhaps not of things that have happened in our own lifetimes. Like many modern buildings, ignored in their time and only appreciated by later generations – a recognition which sometimes comes too late – recent events are often not thought to be so interesting or worthy of study. In 1991, the World War II codebreaking centre at Bletchley Park was about to be demolished to make way for housing, and was only saved following a “farewell party” at which many of those who worked there met up for the first time since the war. By sharing their own personal stories – until then forbidden by the Official Secrets Act – the participants realised what a treasure house they were sitting on, and formed the Bletchley Park Trust, which saved the site for the nation. Newhaven Fort, in East Sussex, was less fortunate; although dating back to the 1860s, and on a site which had seen guns placed in defence against the Spanish Armada, it had been used as recently as the Second World War and therefore did not count as “history”. By the time it was declared an “ancient monument” in 1979, large sections of it had already been demolished by developers, making the subsequent restoration much more difficult and expensive.

As with buildings, so it often is with physics experiments and research groups; and again, recognition often comes too late. Physicists grow old, retire, and eventually die. Much later, the true significance of their work may be discovered for the first time, but by then their only legacy will be their published scientific works; everything else – the lives behind the science, and the contexts in which it took place – is lost, unless they have written it all down, or someone has done that for them.

I have spent the last four years doing a PhD at Sussex University as part of a collaboration involved in the search for an electric dipole moment (EDM) of the neutron; I was aware that the experiment had been going on for decades in one form or another, but it was not until well into my final year that I discovered that our experiment had such an interesting history. Following the publication of a paper on the previous generation of the experiment by the group in early 2006, an e-mail arrived from Norman Ramsey, who had pioneered the experiment and invented the magnetic resonance method it uses, for which he was awarded the Nobel Prize in 1989. He described the publication as “a great paper with which to end 56 years of searching for an EDM with room-temperature neutrons”. (No experiment has ever found a non-zero value for this quantity, but over the last few decades, increasingly sophisticated techniques have driven down the experimental error by several orders of magnitude, disproving several theories on the way; the next generation of the experiment, which I am working on, will employ cryogenic techniques to increase the number of neutrons available at the required energies, and thus improve the statistics).

This made me wonder how it all started, and why people should have thought there might be a neutron EDM back in 1950, before there was any coherent theory of the internal structure of the particle. (We now think of it as being composed of three charged quarks, so that although it is electrically neutral, it is quite possible for it to have an EDM if the charge distribution is not uniform). It is difficult to find this kind of history written down anywhere; there are the occasional “review” articles in scientific journals, which sometimes attempt to give historical contexts, and in the case of very high-profile work there may be articles in magazines such as *New Scientist* or *Physics World*, or in books. But the richest source of such material is in the form of *oral history* – memories that reside in the heads of the people involved, which can be coaxed out by the appropriate interview technique and written down – or, of course they can be written down by the person him/herself in the form of an autobiography.

I talked to our professor, Mike Pendlebury – himself officially retired, although still very much a part of the group – and to his former supervisor, Ken Smith. Suddenly – before I’d even thought of interviewing anyone else in the collaboration – I had a wealth of material, not just about our experiment, but much else besides, as you will see. I found out that

Professor Smith had been one of the first physicists at Sussex, arriving in 1962 with his student, Mike, when the university first started; before that they had been at the Cavendish Laboratory in Cambridge, working on molecular beam experiments with Otto Frisch. Frisch is best known for having conceived of the process of nuclear fission, in conversation with his aunt, Lise Meitner, in 1938, and for coining the term “nuclear fission” itself, but his time at the Cavendish was devoted to molecular beam experiments.

Nowadays, we work with *ultra-cold* neutrons (UCN) – neutrons which have been slowed down to speeds of a few metres per second – because at such energies it is possible to store them in a container for several minutes, increasing the observation time for each neutron, and hence the sensitivity of the experiment. However, before UCN techniques were improved in the late 1970s, all neutron EDM experiments were done with beams of neutrons, and the experiment was basically a variant on a standard magnetic-resonance beam experiment which had already been used to measure the magnetic moments, and hence spins, of many atoms and molecules, including those of radioactive isotopes. Much of this work was carried out by Frisch and Smith at the Cavendish, and continued by Smith and his group at Sussex, and the results made major contributions to the emerging theories of nuclear and atomic structure. Frisch had first learnt the molecular beam technique in Hamburg, where he had worked with another Otto – Otto Stern – between 1930 and 1933.

Stern, of course, is famous for the classic experiment he carried out in 1922 with Walter Gerlach. This experiment, as every physics student knows, consisted of a beam of silver atoms which passed through a non-uniform transverse magnetic field on its way to a detector, and the resulting angular distribution at the detector showed that the atoms were deflected by discrete, not continuous, amounts – this was the first experimental evidence of the quantisation of angular momentum in neutral atoms, and made a major contribution to early quantum theory.

Ken referred to Frisch’s autobiography, *What Little I Remember*, during our conversation. I had not come across this book before, but discovered a copy in the university library (where, according to the flysheet, it had lain, scandalously unread, for the previous 19 years!) and found it to be a delightful little volume packed with anecdotes and portraits (in both the

literary and the artistic senses – Frisch was a gifted artist as well as a musician) of people with whom the author had worked – in Berlin, Hamburg, London, Copenhagen, Birmingham, Liverpool, Los Alamos and finally Cambridge. In the book, Frisch describes Stern as “the great-grandfather of molecular beams”, to whom nearly everybody who works in this field can trace their ancestry. Of course, Ramsey – who had been one of the pioneers of the neutron EDM experiment in 1950 – had learned the craft, not from Stern, but from Isidor Isaac Rabi in the USA; but a brief piece of internet research revealed that Rabi, too, had worked with Stern in Hamburg, having left a year before Frisch arrived.

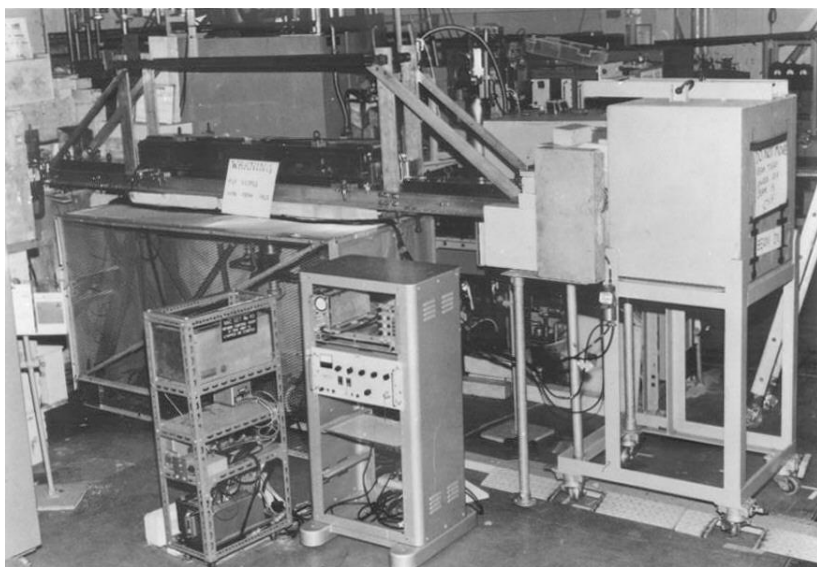
Frisch’s title is significant, as is his dedication “For my daughter Monica, who made me write this”. I couldn’t help wondering about all the things he had forgotten about by the time he wrote the book, when he was already over 70. And I thought about all the other physicists who did not have persuasive daughters, and who might perhaps have taken their memories to the grave instead of writing them down.

In the course of my researches, I discovered a fascinating interplay of political and personal forces at work which helped to shape the future direction of the experiment. The Mecca of neutron experiments is the Institut Laue-Langevin (ILL) in Grenoble, France. It consists of a small (about 50 megawatt) nuclear reactor which produces neutrons at all sorts of energies for all sorts of research, and is the world’s premier neutron source; although there are other sources (such as ISIS in the UK, where neutrons are produced by accelerating protons into a metal target), none can produce as many neutrons per square metre per second, or rival the great variety of energy spectra available.

Much of the work done at the ILL concerns the structure of materials; neutrons are particularly useful as probes of matter at the molecular and atomic scales because, being neutral, they are not repelled by positively-charged nuclei, and are able to “see” small nuclei that are invisible to X-rays. But the institute also plays host to a small number of *physique fondamentale* endeavours; in fact, all three of the current neutron EDM experiments in the world have connections with the ILL. But it was not always so.

Before the ILL was commissioned, jointly by France and Germany, in 1971,

there were various small reactors dedicated to researching nuclear energy, and some whose sole purpose was to produce plutonium for weapons. Mike Pendlebury told me about their first EDM experiment, which took place in the late 1960s; they had planned to use the facilities at the UKAEA site at Harwell, but a reorganisation of government departments, coupled with the unmasking of the spy Klaus Fuchs, resulted in a move to the Atomic Weapons Research Establishment at Aldermaston (albeit “outside the fence”) where there was a neutron beam which emanated from the HERALD reactor. Ken had had a long association with Harwell, which had started during the war; in more recent times he had visited the site in connection with the radioactive-beam experiments, which, although normally done in the university laboratories, sometimes had to take place right next to the reactor, because the isotopes in question were so short-lived that they had to be created in the reactor and then immediately blown into the beam oven using compressed helium.



The Aldermaston Experiment

Interest in EDMs had been boosted by the result of an experiment in 1964 which had shocked the world of physics. Originally, the laws of physics

were thought to obey certain symmetries, including parity conservation – the symmetry between what we think of as “left” and “right”. In other words, it was thought that nature had no built-in preference for left- or right-handedness, and one consequence of such a symmetry is that the EDM must be zero; in fact the result of the very first neutron EDM experiment, carried out by Ramsey and others in 1950, was not published for several years because it was considered to be consistent with this theory, and publishing numerical limits on something “known” to be zero would have been regarded as somewhat absurd. But in 1956, Chien-Shiung Wu and her team had shown that the β -decay of cobalt nuclei exhibited parity violation, and parity conservation was replaced by a new symmetry known as “CP” symmetry – which amounted to the idea that nature cannot distinguish between left-handed particles and right-handed antiparticles. If this symmetry holds, the EDM must still be zero; but in 1964, Christenson, Cronin, Fitch and Turlay showed that some particle decays violated even CP symmetry, and three years later André Sakharov pointed out that this asymmetry could explain why there is an excess of matter over antimatter in the Universe, despite a strongly-held belief that equal amounts of each were created in the Big Bang. The asymmetry only needed to be small, since it is believed that our present Universe consists of only a billionth of the original matter, the rest having been annihilated along with all the antimatter. The 1964 experiment did not imply that the neutron EDM was *necessarily* non-zero, however, and it alone could not provide a complete explanation of the matter-antimatter asymmetry; but if the EDM could be *shown* to have a finite value, this would provide a suitable mechanism.

The CP violation result sparked off a whole new generation of EDM experiments. At Sussex, the Professor of Theoretical Physics, Roger Blin-Stoyle,* persuaded Ken to get involved, but because Ken was busy setting up the experimental physics group, the responsibility for this fell largely on Mike, then a young postdoc. They faced stiff competition in the form of Ramsey’s group in the USA, now using a special cold neutron source at the Oak Ridge National Laboratory. Although the British experiment was an order of magnitude behind the Americans in terms of sensitivity, it stood to benefit from a new UCN source being planned at Harwell, and eventually from a possible UK-based research reactor similar to the one then being built at the ILL; however, these plans were thwarted by a change in government policy. Prime Minister Ted Heath’s pro-European policies

meant that collaborations with other European countries were favoured over the “go-it-alone” approach, and in 1972 the British source and reactor were cancelled and the UK became a partner in the ILL instead. Meanwhile, across the Atlantic, a change of management at Oak Ridge meant that certain experiments were no longer favoured, and the American group also had to find a new home. Ramsey’s group started a beam experiment at the ILL, later joined by Pendlebury, and in 1981 a collaboration consisting of Ramsey, Smith, Pendlebury and others started work on a new experiment using stored ultra-cold neutrons. The Russians had been the first to store neutrons back in 1970, and the fact that, soon afterwards, Pendlebury, accompanied by another collaboration member, Bob Golub, had been able to visit the Soviet group at Dubna, near Moscow, at the height of the Cold War, shows that science – even a branch of science that was dangerously close to that of nuclear weapons – was able to transcend ideological boundaries.

The rest, as they say, is history; Ramsey, and later Smith, eventually retired but the group continued, strengthened by the addition of younger recruits, and went on to refine the experiment again and again, culminating in the final result – the world best to date – announced in that paper in early 2006 (which, by the way, gave the maximum possible value for the EDM as $2.9 \times 10^{-28} e$ metres, where e is the charge on the electron).

As Ramsey had said in his e-mail, this was the end of the line for the room-temperature version of the experiment; the new experiment, currently being set up at the ILL, will be cryogenic. It is the creation of a new, larger collaboration in which the Sussex core has been augmented by groups from Oxford University, the Rutherford Appleton Laboratory, the ILL and Kure University in Japan. The cryogenic experiment will use superfluid liquid helium to slow down the neutrons, and the all-important magnetic field stability will be improved by the use of a superconducting solenoid and a superconducting magnetic shield. The combination of these and other improvements will increase the resolution by a factor of 100, and present a major challenge to one of the most popular candidates for physics beyond the Standard Model, namely supersymmetry.

One of the things one finds it necessary to cope with when researching oral history is an occasional discrepancy between sources, and this can be traumatic for those of us who have been used to the concept of scientific

truth – even of the probabilistic quantum-mechanical kind. One sometimes has to accept that there is no objective truth, or that if there is, it is inaccessible. Ken and Mike's accounts had marked differences of emphasis, and there was a slight contradiction between Ken's account of an incident at a conference in 1952 and that given by Frisch in his book. This incident was particularly interesting because it involved the same Chien-Shiung Wu who, four years later, would discover parity violation. She was apparently so delighted by a measurement of the nuclear spin of bismuth-210, announced by the Sussex group, that the conference had to be adjourned for a few minutes to allow her to recover! It would have been nice to have had a full account of what happened; but Frisch died in 1979, and so I was resigned to not knowing whose memory was the more accurate.

Oral history is not as easy as it might first seem; as well as discrepancies or gaps in the available information, there are other problem areas such as evidence of major disagreements or disputes between collaborators, and such matters are probably best not aired while any of the participants are still alive! But even if there are no skeletons in the cupboard, the simple act of transcribing an oral account into one's own words can introduce inaccuracies or change the original emphasis, and it is imperative to check your account with your source.

In order to learn how to deal with all the potential pitfalls, it is advisable for anyone planning to undertake such work to take a short course in the subject first; these are available at several universities and colleges in the UK. But do get involved, before it is too late; even if your experiment is not as long-lived as ours, your colleagues' memories – if not your own – will be just as long.

When I pressed Ken on the subject of what had happened at that conference in 1952, he did volunteer the name of another physicist who had been there. "But it's no good asking him, as he is very ill", Ken said. Later, I learnt that the physicist in question has Alzheimer's disease. So – unless he too has written it all down – we will never know the truth. All the more reason for taking oral histories when it is still possible to do so! And don't worry if there is no obvious outlet for your writing, or if the subject-matter is not thought to be of interest outside a very small group of people; it will be valuable one day, if not now. In fact, the article you are reading started off

as an attempt to put some historical context into my thesis, but ended up having its own momentum, albeit without any obvious purpose. It was a chance meeting with Peter Ford that made me think the History of Physics Group might be interested.

Oh, and in case you're wondering whether I ever found out what had started it all off, it turns out that, back in the late 1940s, Ramsey simply had a hunch that parity symmetry might not hold. Richard Feynman disagreed, and the two made a bet. Although Ramsey's first attempt to find evidence of parity violation was inconclusive, six years later, thanks to Wu's experiment, he collected his winnings.

* Readers may know that Roger Blin-Stoyle died earlier this year –Ed.

Sir Joseph Rotblat and the Forgotten Physicists

Neil Brown

The report of the Sir Joseph Rotblat conference printed in the January 2007 newsletter reminded me that Sir Joseph twice addressed meetings of the History of Physics group. In the late 1990s the group tried holding evening lectures in which eminent physicists gave informal personal accounts of their life and work, particularly in the early parts of their careers. Sir Joseph gave one of these talks, on his early years as a physicist in Poland, on 8th March 1999. He addressed the group again on 1st December 2001 as one of the speakers in a half-day meeting entitled "The Nobel Century". Both talks were reported in the subsequent newsletters (2000 and 2002), and both were excellent and fluently delivered talks, notwithstanding Sir Joseph's advanced age.

It is the first of these meetings that sticks in my mind. The institutions and people about whom Sir Joseph spoke with so much feeling had been an integral part of the international scientific community, working in one of the most exciting fields of research at the time, radioactivity. Not only were

these institutions destroyed completely in the upheavals of the Second World War, there is almost no mention of them or of the people who worked in them in the modern historical record – it is almost as if they had never existed. This was brought home to me more forcibly shortly after the talk. The text printed in the newsletter was produced by the editor, Lucy Gibson, recording the talk and laboriously transcribing the tape – with the speaker's permission and co-operation in providing copies of his photographs. This process creates a particular problem in a talk littered with Polish names, and because I worked at the Science Museum and had easy access to a suitable library I was asked to check some of the spellings. A straightforward request, I thought, but it was not so.

The people concerned had published scientific papers so I assumed they would be cited in the standard biographical dictionaries, and the obvious place to start was *World Who's Who in Science* (edited by A. G. Debus, Chicago, 1968). I did not find any of them. Only one of them is listed there, one who survived the Second World War (probably the only one) and did significant work afterwards, and I did not find that one at the time because I did not have a good enough approximation to the name. The people I was looking for were not among the first rank of scientists, though some of them would doubtless have achieved greater distinction had they lived longer, but they were definitely among the large supporting cast who merit, and in America or Western Europe would receive, at least a mention in the core historical record. I had a little more success looking in contemporary references. The appropriate volume of J. C. Poggendorff's *Biographisch-Literarisches Handwörterbuch* has a full entry for the most eminent of those mentioned, Ludwik Wertenstein. He was the Director of the Kernbaum Radiological Laboratory in Warsaw and Dean of the Faculty of Science in the Free University of Poland, and he had published papers in respected journals in France, Britain and Germany, but he had still been overlooked by the compilers of later collective biographies. I did eventually find two of the other names, but no details about them. The rest had to be checked, I presume, with Sir Joseph himself.



Staff of the Kernbaum Laboratory, 1938 - Ludwik Wertenstein is front row, centre and Joseph Rotblat is back row, left

Sir Joseph showed three photographs and remarked about one of them, a group picture of the staff of the Kernbaum Laboratory where he had worked taken in 1938, that he was the only person alive from all those shown, and not because it was sixty years since the photograph was taken. Most of them died shortly afterwards, during the Second World War in individual executions or in the gas chambers. As he acknowledged, he would have been among them had he not come to England on a research fellowship shortly before war broke out, a fellowship arranged for him by Ludwik Wertenstein, who had become his counsellor and friend, and who was killed just before the end of the war.

Careers cut short in their prime provides one of the reasons why these men and women and their work have not found their way into the historical record, but by no means the only reason because many other scientists on both sides in the world wars died prematurely and are still remembered.

Both the Free University of Poland, where Sir Joseph was educated and where he was employed, and the Kernbaum Radiological Laboratory, in which he actually worked, existed effectively only from the end of the First

World War to the start of the Second World War. Neither had government support or recognition, though they had high standards and support in other ways – Marie Curie was Honorary Director of the laboratory. Some of the reasons for this were petty personal jealousies, but one of the main reasons was anti-semitism, which was rampant in Poland between the wars, and neither the Free University nor the Kernbaum Laboratory paid any regard to class or race. The endowment provided just before the start of the First World War to set up the privately funded Kernbaum Laboratory was completely devalued by the end of the war, so the staff received no salaries from the laboratory, they made their livings by working elsewhere as well. How the Free University was financed Sir Joseph did not explain, but it was clearly not well funded. Because of the official disregard, the discrimination and the lack of resources it would have been hard for these institutions and the scientists working in them to obtain the recognition they deserved, even without Hitler's depredations.

Sir Joseph confessed to a sense of unreality about the Kernbaum Laboratory because he had no documents, memorabilia or photographs apart from a very few sent to him later by friends. He had photographs and notebooks with him when he came to England, but they were (in his words) "stolen by the CIA" when he left the United States at a time when he was suspected of being a Soviet spy. So, in the supposedly free western world, excessive suspicion and the heavy-handed security measures that followed from it removed what little tangible material he had managed to retain.

Historians can only work with the material available to them, and we need always to remember that all recorded history is partial. Historians are aware of the pitfalls created by lack of access to foreign publications and lack of facility with other languages, and can make some allowance for them, but it is impossible to compensate in areas where almost all information has disappeared. We need to treasure the reminiscences of people like Sir Joseph because they are the only corrective we have to the imperfect formal record. They also make more apparent the painful nature of so much of the missing history in ways the formal record cannot do.

~~~~~

## Physics in Heaven and on Earth

*Dr. Peter Maria Schuster  
Pöllauberg. Austria*



Alexander S. Popov

The text of the first radio telegram, sent over a distance of 250 m by Alexander S Popov in 1896, read “Heinrich Hertz”. That was 2 years after the death of Hertz. He, Hertz, had, throughout his life, persistently denied the possibility of transmitting via radio. Why then was he granted this honour by Popov? And, further, why was the unit of frequency, a Hertz, being one cycle per second, named after him?

Five years later, on December 12, 1901, the engineer Guglielmo Marconi (1874–1937) waited in St. John, Newfoundland, Canada for the arranged Morse signal from Poldhu in Cornwall, England. The scientific world thought it completely mad. As far as the physics was concerned, transmitting from Europe to America was not a technical, but a theoretical impossibility: that was in direct conflict with the laws of propagation of electromagnetic waves that Hertz had discovered. The amateur, Marconi, in 1899, had been quite successful in transmitting radio waves across the English Channel, at its narrowest point, of course, of just 32 km. Radio waves, however, are, as Hertz had shown, electromagnetic waves, like light, only differentiated from light by their range of wavelengths. Guglielmo Marconi



Though they are propagating in a deflected and not in a straight path through the atmosphere, nevertheless their direction remains parallel to the earth's tangent. Their dispersion would not be able to compensate for long for the curvature of the earth, Hertz believed, and the radio waves would be lost in space.

That was also expounded by the great mathematician and physicist, Henri Poincaré (1854–1912). He showed that the effective range of radio waves could not exceed 300 km. His calculation was correct. At that time, not one in a thousand physicists around the world would have given the Atlantic transmission a chance. What then was Marconi waiting for with his headset in St. John? Hertz was, at the end of the 19<sup>th</sup> century, the scientist most competent to assess the possibility of radio transmission, and he had categorically denied it.



The young Heinrich Rudolph Hertz, born in Hamburg on February 22, 1857, exactly 150 years ago, wanted to become a structural engineer, because, “Mathematics is such an abstract science, in which one must totally immerse oneself, and I love to live among people.” In 1877 then he changes to physics and becomes the favourite pupil of Hermann von Helmholtz (1821–1894). When alone, he liked to recite, aloud and from memory, verses from Homer and Dante.

Heinrich Hertz

In 1885, whilst Professor in the technical high school in Karlsruhe, Hertz works on electrical oscillations, in the course of which, as Max Planck will say: “He was led to observations that no one had noticed before. Hertz was able to ‘produce’ waves as short as a few centimetres length, as he had discovered that the principle of resonance is also applicable to electrical oscillations.” Max Planck, who has been considered to be the most

dispassionate of men, falls into great agitation, all of him in resonance, so to speak, as if it robbed him of breath, when he describes the process of this Hertz discovery in his commemoration speech of February 16, 1894.

In 1888, Hertz will show that light is an electromagnetic phenomenon of very short wavelength. He is successful in constructing an oscillator to generate waves of substantially shorter wavelengths than his earlier attempts produced. In his famous experiments he shows that the fundamental laws of optics, like reflection, refraction and polarization, apply also to electromagnetic waves. With that Maxwell's Theory conclusively obtained its key breakthrough. With classic concision and lucidity, which even surpasses the work of Maxwell, he gives electrodynamics that wonderful architectural form of which the famous Boltzmann, astonished, will say: *"Was it a god who wrote these symbols?"* Ever since, every physicist on being asked about Maxwell's Theory answers with Hertz's words: *"This theory is the system of Maxwell's equations."*

This genius Hertz, who, wherever there was a problem in physics, grasped it and could solve it in such an original way, he who all physicists had the greatest admiration for, could he be wrong? Unconcerned by this, Marconi prepares his special antennae for the reception in St. John.

*"If they could obtain concave mirrors the size of a continent they would be in a position to arrive at the results which they have in mind: with normal mirrors, on the other hand, they could scarcely begin: they would not have the slightest effect,"* writes Hertz.

Marconi utilizes no gigantic reflecting mirrors; he has only a small glass tube with two electrodes and a small amount of powdered metal, which has to capture the electrical waves in the tube. And the unbelievable happens: Marconi defies the laws of electromagnetism; the signal from Poldhu is clearly received in St. John. No one has any idea why. There must be in "Heaven" a kind of mirror, by means of which radio waves become reflected back to earth and arrive at a point which, because of the spherical form of our planet, lies outside our range of sight.

In 1924, when Sir Edward Appleton (1892–1965) starts his research into the propagation of electromagnetic waves, he will soon find the explanation. He shows that layers of ions that are produced in the atmosphere through

solar irradiation from what is called the ionosphere, which is impenetrable by electromagnetic waves of certain lengths.

The laws of physics were no longer infringed by the radio transmission, and given the absence of an ionosphere the operational range of Marconi's signals would actually have been no greater than 300 km. Hertz had, as Boltzmann wrote, investigated the only route which had, for a long time, been indicated. Also, radio astronomy is founded on Hertz's work, and the enormous radio telescopes, which can span a distance of a billion light years, are constructed after the example of the parabolic mirror, which Hertz used for his experiments. And now, here below, on earth, the analogy between the behaviour of radio emitters and light sources had put blinkers on the researchers.

For Marconi, whose work had been prompted by that of Hertz, but who was not that familiar with the theory of electromagnetism, the analogy was of no consequence and had not the slightest influence on his scheme. He will, in 1909, receive, jointly with Ferdinand Braun (1850–1918), the Nobel Prize. An evaluation by experts—in a preliminary way as is common practice today—of his project 'Poldhu – St. John' would have given its realization no chance.

Hertz experiences these successful issues no more. On December 9, 1893, he writes to his parents:

*"If anything should really befall me, you are not to mourn; rather you must be proud a little and consider that I am among the especially elect destined to live for only a short while and yet to live enough. I did not desire or choose this fate, but since it has overtaken me, I must be content; and if the choice had been left to me, perhaps I should have chosen it myself."*

On January 1, 1894, nearly 37 years of age, Hertz succumbed to septicaemia.

Article translated from the original German by Diane Cooper and Dr. Lily Wilmes

~~~~~

Hibbert's Magnetic Balance

*Stuart Leadstone
Banchory, Kincardineshire*

Introduction

Some years ago an article appeared in *Physics Education* with the intriguing title "Please sir, what else did he do?" ⁽¹⁾ The subject of the article was Edwin Barton of "Barton's Pendulums" fame. Recently I had a similar impulse prompted by the clearing out of a store in the Physics Department at Aberdeen University. Amongst other things a set of magnetic balances came to light which I recognised immediately as being associated with the name of Walter Hibbert. [Fig 1.]



Fig 1 Walter Hibbert (1852 – 1935)

Reproduced with permission of The University of Westminster Archive Services

References to "Hibbert's Magnetic Balance" [Fig 2 below] are common in text-books of a certain age ^(2,3,4,5). Indeed the apparatus figured in my own A Level physics course in the 1950's and I have the evidence both in my

A Level notes and in my Lab Book, of which more later. I remember the apparatus with affection because it was conceptually simple and easy to use in practice. Moreover it was simple to recall for examination purposes and enabled one to give a good answer to the question: How would you attempt to verify the inverse-square law for the force between magnetic poles?

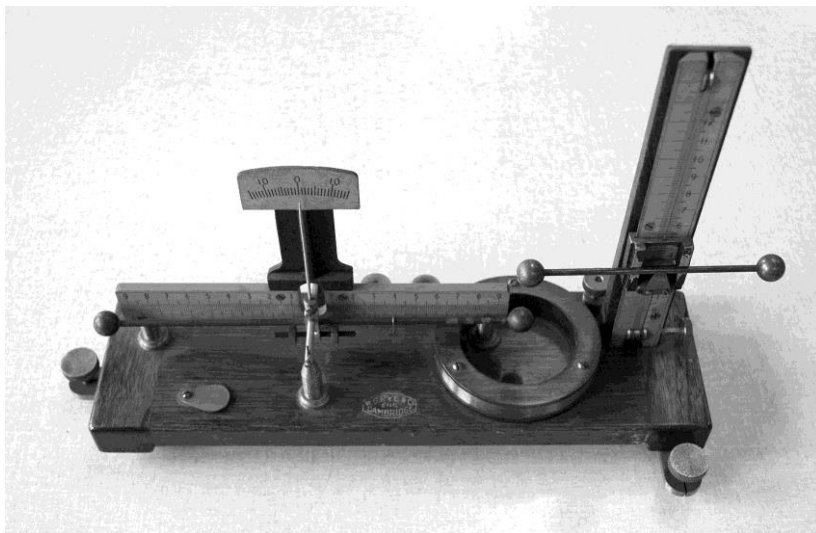


Fig 2 Hibbert's Magnetic Balance

Courtesy of the Physics Department, Aberdeen University.

Nowadays, of course, we live at a time when the magnetic pole concept is denied, and, so the philosophers of science tell us, one cannot "verify" anything, one can only fail to falsify, or, better still, falsify! The modern experimenter is also more remote from the phenomenon of interest than his or her predecessors because measurements are made using sensors, and analysis is done by computer-processing. The basis of my affection for pieces of apparatus such as Hibbert's Balance is that they address themselves directly to the underlying theory; and they have working parts which are clearly visible and accessible, enabling faults to be easily identified and rectified. They also work during a power cut!

What, then, of the man and his apparatus?

The Man

Walter Hibbert was born in 1852 at Droylesden, a small village four miles from Manchester. He worked from an early age at the National Telegraph Company, showing great aptitude for the work so that before the age of 20 he was in charge of the Central Telegraph Office in Manchester. He took up evening classes at the Mechanics Institute, and after two years he gained first class awards in Organic and Inorganic Chemistry, and also the Gold medal given by the Science and Art Department for work in Organic Chemistry.

Hibbert left the Telegraph Company when it was taken over by the Post Office as he had a religious objection to Sunday duty. He came to London and took up a position as assistant to John Hall Gladstone, who had succeeded William Odling as Fullerian Professor at the Royal Institution in 1874. Later Hibbert became chief assistant in Gladstone's private research laboratory. The relationship between Hibbert and Gladstone was clearly more than that of professor and assistant, since they were joint authors of twenty papers.⁽⁶⁾

It was through Gladstone that Hibbert became connected with the Polytechnic⁽⁷⁾ which had moved into its new premises at 309 Regent Street in 1882 and was rapidly expanding its technical and trade classes. Gladstone was a member of the Governing Body of the Polytechnic and brought Hibbert to the attention of Robert Mitchell, later Director of Education at the Polytechnic. Mitchell was seeking someone to undertake the formation of an Evening Department in Electrical Engineering. Hibbert's name first appeared on the Polytechnic timetable for the 1884-1885 session beginning in September 1884. He was listed as the Instructor for the Electrical Engineering, Telegraphy, Electric Lighting and the Instrument-Making class which took place on Tuesday evenings from 8 to 10 pm, costing 5 shillings for members of the Polytechnic Institute, and 9 shillings for non-members. He also taught a class on Practical Electrical Work on Friday evenings, 7-10 pm, which cost 5 s /10 s.

On 1st October 1884 Hibbert gave an introductory lecture on electrical engineering to start the new session. An abstract was published in the Polytechnic Magazine and mention was also made of evening classes

which he was teaching for the Science and Art Department at South Kensington, in addition to his responsibilities at the Polytechnic. The earliest surviving prospectus of the evening classes is that of 1888, which details the Electrical Classes as including such subjects as: "Characteristic Curves and Efficiency of Dynamos, Electromotors, Electric Railways, Secondary Batteries or Accumulators, Transformers or Secondary Generators, Arc and Incandescent Lighting". Hibbert was also President of the Polytechnic Electrical Engineering Society, which met regularly to "interchange ideas and diffuse information upon matters connected with electrical engineering".

It is known that Hibbert also gave practical demonstrations in the theatre adjoining the main Polytechnic Building in Regent Street, echoing the activities of the earlier Royal Polytechnic Institution. The Times newspaper of 30 January 1897 reported that each Saturday afternoon throughout the winter, Mr Hibbert would be demonstrating the newly discovered "Röntgen Rays" when "an opportunity will be afforded to spectators of seeing their own bones".

Hibbert was a man of deep religious conviction and found in Polytechnic life the opportunity to promote spiritual growth and to develop missionary spirit. He delivered a number of addresses, always filling the Large Hall to capacity. He also regularly spoke at the "Family Gathering", a religious and social occasion which took place on a Sunday at the beginning and end of each term. His last address before his retirement on 28th March 1920 emphasised the importance of "family" and what it meant to the Polytechnic. Hibbert retired at the end of March as Head of the School of Electrical Engineering, and was presented with "a wall barometer in an oak case". His farewell dinner was reported in the *Polytechnic Magazine* ⁽⁸⁾ and tributes include mention of his skill in "teaching science from a Christian standpoint". Perhaps the best insight into his character is given by the telling phrase, made with respect to science and faith: "With Mr Hibbert there was no incongruity between (these) two great handmaids of human progress." He had impressed his students from early in his career, as instanced by the presentation in May 1885 of a "very handsome timepiece as a mark of esteem and gratitude for the great interest he has taken."

In January 1935, when Hibbert was 83, it was reported that he was seriously ill following an operation.⁽⁹⁾ No further details appeared until November of that year, when his death was sadly acknowledged with an obituary and photograph.⁽¹⁰⁾ The obituary also mentions some of his writings: a substantial article on accumulators in the tenth edition of the *Encyclopaedia Britannica* (reprinted in the eleventh edition); volumes entitled *Popular Electricity* [Fig 3] and *Magnetic and Electric Ignition for Petrol Motors*; and a philosophical treatise *on Life and Energy*.⁽¹¹⁾

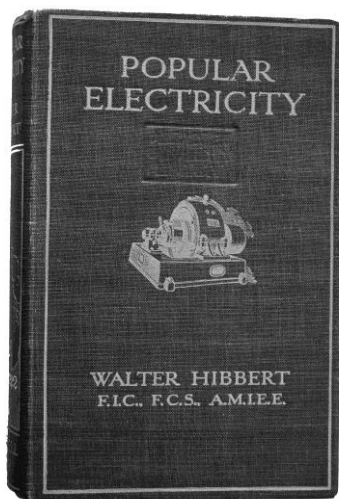


Fig 3

The book *Popular Electricity*⁽¹²⁾ still reads well, and there is a chapter on: *How Electricity Does Work - Meaning of the Word "Volt"*, which has much to commend it, though a reference to "positive electrons" in a work of 1909 did surprise me.

The entry on Hibbert in *Who Was Who*⁽¹³⁾ also mentions another work: *Magnetism and its Elementary Measurement*.⁽¹⁴⁾ However, despite its obvious relevance to this article, I have been unable to trace a copy of this book.*

The obituary also notes that, at the time of his death, Hibbert was engaged on a book dealing with "Sleep".

Hibbert was a fellow of both the Institute of Chemistry and the Chemical Society. The proposal certificate for his election to the Chemical Society in 1876 is reproduced in Fig 4. He was also an associate member of the Institute of Electrical Engineers, and the range of topics in his published papers reflects the breadth of his professional affiliations. (To date, I have located 12 papers in full and summaries of 5 others.)

* The author has since been advised of the whereabouts of a copy.

CHEMICAL SOCIETY.

Certificate of a Candidate for Election.

Name Walter Hibbert

Qualifications or Occupation { Assistant to Prof^o
Gladstone

Usual Place of Residence Royal Institution

being desirous of Admission into THE CHEMICAL SOCIETY, We, the undersigned, Fellows of the Society, propose and recommend him as a proper person to become a Fellow thereof.

Dated this day of 18.....

<p style="text-align: center;">FROM PERSONAL KNOWLEDGE.</p> <p><u>J.H. Gladstone</u> <u>Matt Williams</u> <u>Alfred Tribe</u> <u>Charles R. Wright</u></p>	<p style="text-align: center;">FROM GENERAL KNOWLEDGE.</p> <p><u>B. Meldola</u></p> <p style="text-align: right;"><u>W. A. R. R. R.</u> <u>1876</u></p>
--	---

Proposed to the Society on May 4th 1876

To be balloted for on June 1st 1876

N.B.—Directions for filling up this Certificate are given on the other side.

1431

Fig 4

Hibbert's Magnetic Balance

In his obituary it is stated that:

Mr Hibbert was the inventor of a standard permanent magnet, a standard one-volt primary cell, a magnetic balance and other instruments.

The impetus for the first of these, as Hibbert recounts⁽¹⁵⁾, was the fact that:

In the electrical laboratory of the Polytechnical Institute, Regents Street, the earth's magnetic field varies so much that it cannot be assumed as a basis for reasonably accurate measurement.

He therefore devised and had made a reliable secondary standard of magnetic field strength which was not subject to significant ageing. The essential structure consisted of a permanent magnet with cylindrical symmetry having an annular air gap in its upper surface. [Fig 5].

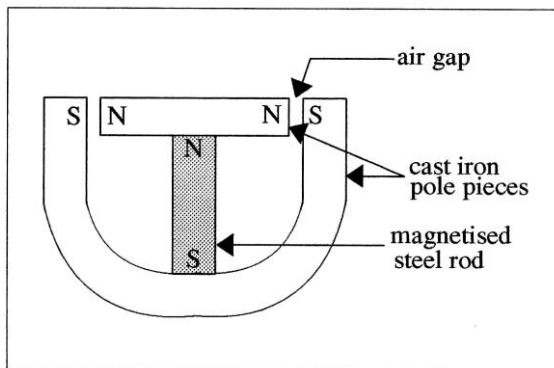


Fig 5 Geometry of Hibbert's Magnetic Standard

The field was sampled by dropping a coil of fine wire through the gap, and measuring the induced charge. In a series of tests on one prototype version of his apparatus, taken over the period April to November 1891, Hibbert gave values for the "number of lines in the inductor field" as ranging from 21,680 to 22,030, indicating a variation overall of less than 2%.

Hibbert's magnetic standard eventually made its way into some physics text-books^(16,17,18)

Regarding the genesis of the "Magnetic Balance" I have been unable to locate a seminal paper. However it is possible that it was first presented to the scientific world at a Royal Society conversazione held in the Royal Society's Rooms at Burlington House. In June 1904 Chemical News gave an account of this event, hosted by Sir William and Lady Huggins⁽¹⁹⁾. It was reported there that:

"Mr. W. Hibbert showed a new Magnetic Balance, constructed as follows:- The beam of a balance is made of a magnetised steel rod 27 cm long. The 'centres' of the poles are 25 cm apart. The repellent pole of a second magnet being placed over one end of the beam causes this to descend, and the force of repulsion is balanced by a weight sliding on the other half of the beam. The absolute value of a pole in C.G.S. units can be ascertained in a very few minutes, without reference to terrestrial or other field. The instrument can also be used for finding the approximate value of an electric current."

At some stage the balance was manufactured for use in physics practical classes. The version which is in the Aberdeen University archives is that shown in Figs 2 and 6. It is listed in the 1911 Catalogue of Scientific Apparatus published by W.G.Pye & Co. of Cambridge⁽²⁰⁾. The version illustrated in Figs 2 and 6 is more sophisticated than some in having a horizontal coil incorporated. This is for use in place of the non-pivoted magnet which slides up and down the vertical scale. That the coil is not simply a means of compensating for the vertical component of the earth's magnetic field can be deduced from the fact that (i) compensating nuts mounted on a screw thread beneath the fulcrum are provided for this purpose; (ii) the catalogue entry specifically states that the apparatus can be used for "the measurement, in C.G.S. units, of currents and ratios of current strengths". Furthermore, the coil has three terminals, clearly visible in Fig 6, by means of which different numbers of turns may be selected. Assuming uniformity of wire used throughout, simple resistance measurements indicate turns ratios of 1 : 4 : 6.

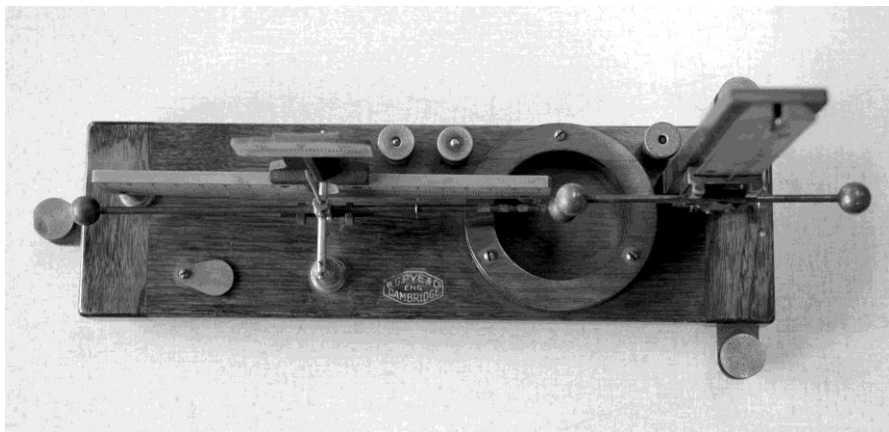


Fig 6 Hibbert's Magnetic Balance, showing the current carrying coils and terminals

Courtesy of the Physics Department, Aberdeen University.

The illustrations of Figs 2 and 6 clearly show the long ball-ended magnets used. This design feature seeks to achieve:

- (i) a good approximation to the concept of an idealized bar magnet, namely two point poles separated by a definite distance;
- (ii) a situation in which only the interaction F_1 between nearest poles (N_1N_2) is important (see Fig 7), enabling the law of force between these poles to be investigated.

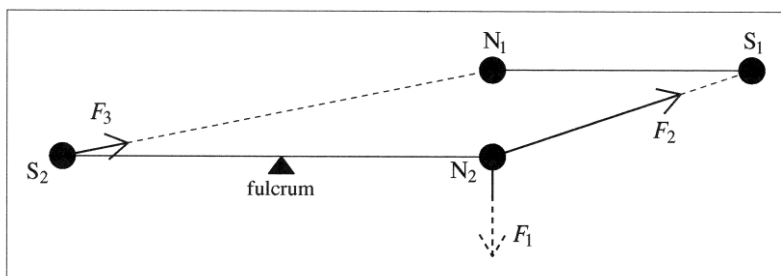


Fig 7 Geometry of interactions between magnetic poles.

The balance is supplied with a set of interchangeable deflecting magnets. In Figs 2 and 6 the deflecting magnet is shorter than the pivoted magnet. This introduces an asymmetry into the geometry, as shown in Fig 7, which has consequences for the remote-pole interactions F_2 (S_1N_2 interaction) and F_3 (N_1S_2 interaction). Thus $F_2 > F_3$ and is closer to the vertical. Hence, F_2 and F_3 exert a small counter-clockwise torque on the pivoted magnet. If the two magnets have equal lengths, however, then the resultant torque due to F_2 and F_3 is zero. This leaves only the even smaller S_1S_2 interaction (not shown in Fig 7), which adds a tiny counter-clockwise torque to the system. A later version of the magnetic balance due to Bateman^(21,22) specifically eliminates all the secondary torques.

Before leaving this matter of geometrical configuration, I would like to refer to an enigmatic entry in the notes accompanying the illustration of Hibbert's Balance which appears on the website given in reference 20. It is intimated there that the 1911 Pye Catalogue Notes claim that the balance can be used, amongst other things, for "*The measurement of the intensity of the equatorial or axial field of a magnet*". This is clearly not possible

without significant modification of the balance, since the configurations which would be required are as shown in Figs 8 and 9. Whether this error resides in the catalogue itself, or in the website résumé, I have been unable to discover. Enlightenment on this point from readers of this article will be welcome.

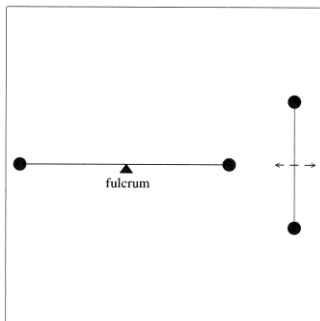


Fig 8 Equatorial field configuration

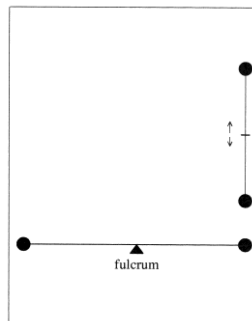


Fig 9 Axial field configuration

How well does the magnetic balance perform? I will leave the interested reader to investigate this by presenting one historic set of measurements. These were taken by myself on 22 June 1954, as a student in the first year of an 'A' Level Physics course. Fig 10 shows the experimental set-up and identifies the relevant variables.

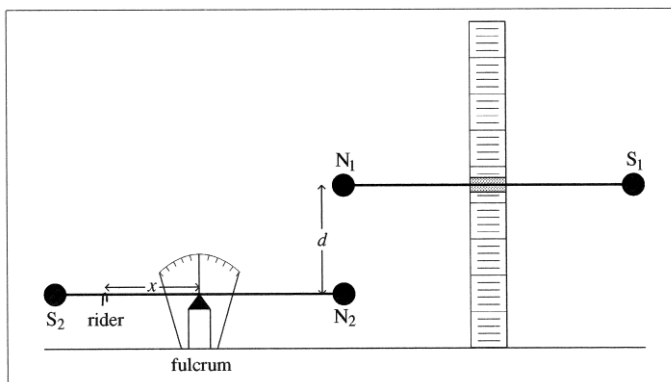


Fig 10 Diagram of apparatus used to obtain the data for the graph of Fig 11

By equating mechanical and magnetic torques, and assuming an inverse-square law of magnetic force between poles, the outcome of the measurements should be consistent with the prediction $1/d^2 \propto x$. Fig 11 displays the results graphically, together with a 'best-fit straight line'.

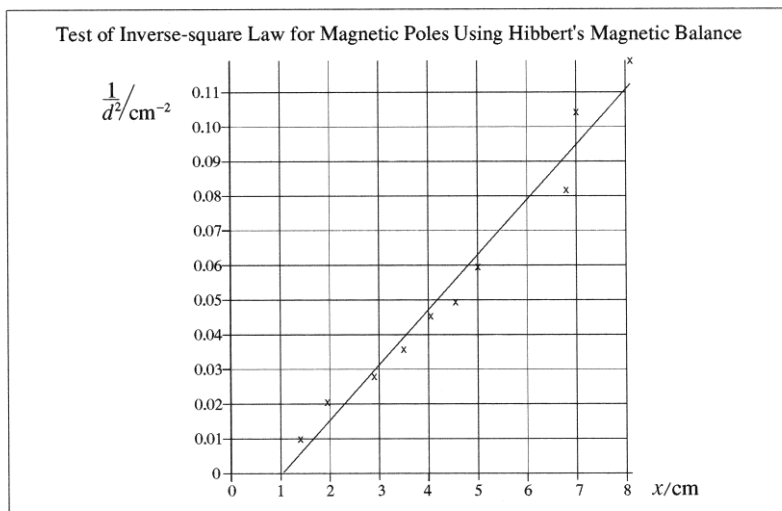


Fig 11 Data obtained with Hibbert's magnetic balance (on 22 June 1954)

As with most experimental investigations the outcome is far from clear-cut. With the advantage of more experience and wisdom than I had when the experiment was performed, I note that:

- (i) there is evidence of a "zero-error";
- (ii) there is distinct non-linearity, especially at small values of d .

I will leave the reader to ponder possible causes of these departures from a pure Coulomb interaction between the magnetic poles. Personally, not having the confidence that I have falsified Coulomb, I might just set up one of the Aberdeen magnetic balances and do a repeat!

Notes and References

1. Cooper M L "Please sir, what else did he do?" *Physics Education* 27 (6) (Nov 1992) 334-335.
2. Allen H S and Moore H A Text-Book of Practical Physics (3rd Ed) Macmillan & Co Ltd (1948) 346.
3. Barton E H and Black T P An Introduction to Practical Physics for Colleges and Schools (2nd Ed) Edward Arnold & Co (1932) 120-123.
4. Noakes G R A Text-Book of Electricity and Magnetism (3rd Ed) Macmillan & Co Ltd (1956) 87-88.
5. White H E Laboratory Exercises for Physics D Van Nostrand Co Inc (1959) 135-136.
6. Tilden W A Obituary of John Hall Gladstone in *Journal of the Chemical Society* 87 (1905) 591-597.
7. The polytechnic institution referred to grew out of the Youth Christian Institute (later called the Young Men's Christian Institute) founded by Quintin Hogg in 1873. In 1878 it moved from premises in York Place, Charing Cross to Long Acre, Covent Garden. A further move into new premises at 309 Regent Street took place in 1882. This location had formerly housed The Royal Polytechnic Institution, and because of this association it gradually took on the name "Polytechnic Institute". In 1891, as a result of the Charity Commission Scheme, it was decided to set up a set of similar institutions across London. It therefore became necessary to distinguish itself from all the others, hence adopting the more specific title The Regent Street Polytechnic. It became The Central London Polytechnic in 1970, and finally The University of Westminster in 1992. The premises at 309 Regent Street are still a part of the latter.
8. Account of the retirement of Walter Hibbert in *Polytechnic Magazine* April 1920 pages 57-58.
9. *Polytechnic Magazine* January 1935.
10. Obituary of Walter Hibbert in *Polytechnic Magazine* November 1935 page 345. (See acknowledgements.)
11. Hibbert W *Life and energy: An attempt at a new definition of life with applications to morals and religion* Longmans, Green & Co 1904
12. Hibbert W *Popular Electricity* Cassell & Co Ltd 1909
13. *Who Was Who* Vol III (1929-1940) (2nd Ed) Adam and Charles Black 1967
14. Hibbert W *Magnetism and it s Elementary Measurement* Longmans, Green & Co 1904.
15. Hibbert W "On a Permanent Magnetic Field" *Philosophical Magazine* **33** (1892) 307-314.
16. Harnwell G P 28th June *Electricity and Electromagnetism* (2nd Ed) McGraw-Hill (1949) 362.

17. Nightingale E *Higher Physics Part IV: Magnetism and Electricity* G Bell & Sons Ltd 1961.
18. Starling S G *Electricity and Magnetism* (5th Ed) Longmans, Green & Co (1932) 263.
19. *Chemical News* **89** (1904) 308.
20. [http://physics.kenyon.edu/EarlyApparatus/Electricity/Magnetic Balance](http://physics.kenyon.edu/EarlyApparatus/Electricity/Magnetic%20Balance).
21. Bateman P "Two Magnetic Balances" *School Science Review* **15** (58) (Dec 1933) 185-188.
22. Smith C J *Intermediate Physics* (4th Ed) Edward Arnold Ltd (1957) 946-947.

Acknowledgements

This article would have been impossible without unstinting help from the following people and institutions:

For access to, use and photographing of Hibbert's Balance:

Dr John Reid, Senior Lecturer and Curator, Department of Physics, University of Aberdeen

Bob Mowat, Physics Technician, University of Aberdeen

Steve Black, Technician in Charge of Photography and Reprographics, University of Aberdeen

For archive information concerning Walter Hibbert:

Elaine Penn, University Archivist, University of Westminster

David Allen, Library Collections Coordinator, Royal Society of Chemistry

For access to early copies of *School Science Review*:

Geoff Auty, Editor, *School Science Review* and Rita Harris, Association for Science Education

For access to material in issues of *The Polytechnic Magazine*:

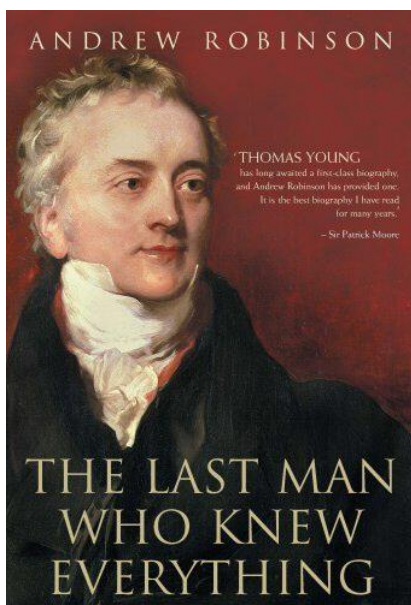
City of Westminster Archive Centre, Westminster, London

British Library Newspapers, Colindale, London

For much help in getting started on the 'Hibbert trail':

The Science Librarians, Queen Mother Library, University of Aberdeen

Book Reviews



Thomas Young

‘The Last Man Who Knew Everything’

Andrew Robinson

Oneworld Publications 2006

ISBN 1-85166-494-8

304pp Hardback £17.99

*Essay reviewed by:
Dr. Peter Rowlands
University of Liverpool*

It is good to have a new book on Thomas Young, even though Andrew Robinson describes his book as ‘an introduction to Thomas Young for a new audience’ rather than ‘a full biography’ (p. ix), and it is particularly good to have one as well written and informative as this one, for Young has a special importance in the history of science that has never been fully recognized. Polymaths like Young are important not just because they make contributions in widely diverging fields, but also because their particular way of thinking – by massively parallel rather than serial

processing – has had a special importance in breaking through the deadlocks that periodically limit the development of subjects when too narrowly defined. Unfortunately, the thrust of most human thinking and nearly all human education is strongly ‘serial’ and it is all too easy to write off the unique importance of the polymath’s contribution as though it were an ‘inevitable’ component in a smoothly serial progression.

However, though scientific history can be written to make scientific progress appear to be ‘inevitable’, a series of facts just waiting for a discoverer to come along, this does not reflect the real nature of scientific development, where discoveries can be held up for decades or even centuries by prejudice or the prevailing climate of opinion, and where the actual sequence of discovery often determines the direction in which a science evolves. As the author points out, Young’s special importance at the beginning of the nineteenth century has been totally misunderstood by scholars who have regarded him as a ‘dilettante’ or ‘amateur’, as lacking in discipline and even in originality (because he was an avid reader of the literature). These claims are made by those who have gone to considerable lengths to deny his particular significance in relation to the successors who extended his breakthroughs in the fields of optics and Egyptology, seemingly with a considerable degree of independence. If it can be shown that Young’s ideas, however prescient, had no real influence on the people who carried through the work to a ‘professional’ conclusion, with the thoroughness and discipline required, then the status of science as depending mostly on linear logic is preserved.

Andrew Robinson quotes Young’s critics at some length before setting out to refute them, and he puts forward some very strong arguments in his subject’s favour. However, there are further arguments which one could add which, in my view, make the case unanswerable. The strongest case against Young has been in the field of optics where Augustin Fresnel succeeded in producing a beautiful and fully worked-out mathematical wave theory, explaining the effects of interference, diffraction and polarization with a precision and clarity seldom equalled in theoretical physics. The argument is that, although Fresnel conceded priority to Young in relation to the principle of interference and transverse waves, his theory was worked out independently and would have happened even if Young had never existed.

We can never know, of course, what Fresnel would have done if there had been no Young, but we can establish pretty convincingly that neither Fresnel's research programme nor the particular direction it took were independent of Young's work. It is not difficult to establish an exact sequence for the relevant events. Young's work on interference was published between 1800 and 1803; at the same time Young informed his friend William Wollaston that his work on Iceland spar supported the Huygens explanation of double refraction developed from a wave theory; Wollaston published this in 1802 and it was translated into French in 1803. Laplace saw the problem for his own emission theory of light and, in 1807, set up a prize for a particle explanation of double refraction; Étienne Malus and Pierre Laplace found solutions based on Huygens' construction, but using physical forces between particles (1808). Dominique Arago made some objections to Malus's theory, and specifically referred to Young's explanation of Newton's rings (1811). Soon after, he began to encourage the young Fresnel to take up the wave theory (1814). Then, Fresnel met Arago in Paris (July 1815), with Arago seemingly suggesting the problem of diffraction as the main one to be solved. Immediately after this, Arago suggested that Fresnel read the works of Young, Newton, Grimaldi and Brougham. Though Fresnel did not read these works, it is inconceivable that Arago could have had a long discussion on wave theory and diffraction with Fresnel without mentioning Young's work on interference, especially as Young had become a close friend, corresponding with him regularly on the subject.

It is a classic case of the 'principle of hidden diffusion' – once in the literature, an idea diffuses so rapidly that people lose sight of the point where it originated. Generally, it enters the subconscious of readers or auditors to resurrect itself seemingly as an independent idea. Thus, Einstein was convinced he hadn't been influenced by Michelson-Morley, Charles Darwin was convinced that he hadn't been influenced by his grandfather Erasmus's *Zoonomia*, even though he opened his first notebook with this word as heading; the founders of kinetic theory were convinced that they had originated the subject, even though the abstract of Waterston's rejected paper had almost certainly entered into their subconscious.

It takes nothing away from Fresnel's unique mathematical development of the subject to say that his realization of the principle of interference was almost certainly not at all independent of Young, nor was his decision to take up the subject of the wave theory of light.

As for the idea of transverse waves, which enabled him to make his own great breakthrough with regard to polarization, Fresnel conceded that the initial trigger for this work was Young's letter to Arago written in January 1817, suggesting that the supposedly longitudinal light waves might have a small transverse component. This is well described by Robinson, but we can also add that Fresnel further conceded that Young went on to hint in a later letter, now lost, that the waves might be *entirely transverse*: 'A remark in a letter of Dr Young, dated on the 29th April 1818, which M. Arago communicated to me, helped to raise in my mind a doubt of the existence of longitudinal vibrations. Dr Young inferred from the optical properties of crystals of two axes, discovered by Sir David Brewster, that the vibrations of the ether might resemble those of a stretched cord of indefinite length, and be propagated in the same manner.' Even Robinson, so thorough in the rest of his analysis, has missed this point, but it is crucial in establishing how dependent Fresnel was on Young's lateral thinking for the insights which would be the basis for his own great optical theory.

In the case of hieroglyphics, it is quite clear, as Robinson shows at length, that Young had made the decisive breakthrough and published it in an article available to all, years before his 'rival' Jean-François Champollion had made any significant headway. Champollion used Young's work, and that of William Bankes (whose identification of the cartouche of Cleopatra totally depended on Young's prior identification of those of Ptolemy and Berenice) without acknowledgement, partly because it was published in an anonymous encyclopaedia article and so had become 'general knowledge', like so much currently on the Internet. Champollion's subsequent work was outstanding – a classic contribution by a specialist in the field – but there is no evidence that he was anywhere near making the breakthrough before he read Young.

In fact, there is documentary evidence that he was still working on entirely the wrong track as much as two years after Young's work had been published. There is no excuse whatsoever for scholars to try to write Young

out of the story – his work was seminal and decisive, and the true foundation for the decipherment of hieroglyphics, though it was not the largest contribution in bulk.

To those who say that Young never went on to ‘complete’ anything, we could argue that no piece of science is ever complete; every scientific achievement can always be made to look incomplete with regard to what follows on from it. Young’s forte was in making the key breakthroughs by lateral thinking that could then be followed up and exploited by others trained to use more conventional approaches. It is most unlikely, in my view, that Young could have duplicated either Fresnel’s or Champollion’s contributions. Fresnel had been trained in the special mathematically rigorous methods unique to the French school of the early nineteenth century, while Champollion was an expert on Coptic, who had been immersed in Egyptology from an early age, but Young’s contributions were not those of a ‘dilettante’ or gentleman amateur. Young was not only a physician but a *professional lecturer* in the sciences, who read deeply into the literature of everything he studied. From Robinson’s account of the genesis of his ideas, it seems that Young’s decisive contributions to optics and Egyptology came from applying lateral thinking and parallel processing of a kind which does not seem to have occurred independently to his successors. His contributions to both fields provided a decisive twist which significantly affected their subsequent development. If it could be argued that the decipherment of hieroglyphics was inevitable given a long enough time period, this is not true of the development of optics, which could have taken a path entirely independent of the Huygenian method, revived by Young and employed by Fresnel. (It could, for example, have been based on the characteristic function of Hamilton which did not distinguish between wave and particle theories.)

In addition, the historiography of optics was to a large extent decided by Young. Even though the Huygens theory cannot be used to explain interference, diffraction or polarization, Young’s use of ‘Newton’ and ‘Huygens’ as ‘counters’ to represent the respective particle and wave theories has somehow endured in historical accounts of the subject. He also emphasized the different velocity ratios that would result for the two theories at the boundaries between two media, although, in the strict terms provided by the de Broglie relation, this does not actually decide the issue.

In fact, as Young recognized with his usual acuteness, the final solution might require a dualistic theory: ‘Whether, therefore, light may consist in the projection of detached particles with a certain velocity, as some of the most celebrated philosophers of modern times assert, or whether in the undulations of a certain ethereal medium as Hooke and Huygens maintained, or whether, as Sir Isaac Newton believed, both of these causes are concerned in the phenomena ...’ This seems to have been the earliest suggestion that Newton’s theory, which unlike Huygens’ involved periodicity, might be considered dualistic.

The special nature of Young’s contribution is encapsulated in the ‘two slit’ experiment, which has now acquired an almost iconic status in discussions of the nature of quantum mechanics. For all the importance of mathematical equations in physics, the most significant physical ideas are, ultimately, the qualitative ones which result from them and relate them to the ‘real’ world of observation, and it is a mistake to think that qualitative ideas are not the basis of the most important thinking even of mathematical physicists like Newton and Einstein. If the wave theory had emerged directly from Fresnel’s mathematical analysis of diffraction, there is no reason to suppose that this particularly striking illustration of the theory would have come forward to direct people’s thinking on some of the most profound problems at the heart of physics two hundred years later. A significant point here, in view of the strong case Robinson makes elsewhere for Young’s complete integrity, is that it is inconceivable that he would have referred so confidently in his lectures to the outcome of an experiment he had never tried. He wouldn’t, of course, *need* to say that he had done it himself if he was discussing it as part of a general public lecture, rather than in an account of his own work in a scientific paper.

As Robinson shows in his fascinating account (and as we can also supplement from a few extra sources), Young’s work extended far beyond optics and hieroglyphics – to mechanical engineering (through Young’s modulus and his work on bridges and the ‘sandblast effect’), to the first use of the modern concept of ‘energy’, to physiology (through his discovery of the mechanism of accommodation of the eye and the three-colour theory of vision), to linguistics (in his definition of the Indo-European family of languages), to the tides (he first used cotidal lines), to medicine (Young’s rule), to life assurance, to music (Young’s temperament) and the

propagation of sound, to further archaeological work (in his almost single-handed decipherment of the demotic script and language), to capillary action and the first realistic estimate of molecular sizes, to the definition of an electromagnetic spectrum (which included the first demonstration of diffraction in ultraviolet waves). He also anticipated later work by assuming that the particles of substances have their own natural frequencies of vibration, and supposing that the action of light waves on the particles of a substance was accompanied by a reaction of the particles on the wave. His discussions of the aether theory led directly to later developments in this area, including its final disappearance as a material medium. In practical terms, he invented the kymograph and an eriometer, as well as introducing the ripple tank. Young's successes were not isolated speculations which had no significant effect on later developments. His *Course of Lectures* (1807) profoundly influenced many later physicists.

Robinson's book gives us some idea of the personality behind this remarkable 'phenomenon' and the events of his life beyond the scientific field. The discussions of scientific concepts and developments in terms that are understandable by a lay reader are lucid and informative – the author has clearly mastered this very difficult art. He makes Young's extraordinary achievements believable in the context of his life and his intellectual development, and discusses well the tension between his wide-ranging creativity and the need to maintain his social respectability as a career physician. In view of the fact that so many important scientists have stressed the value and quality of Young's work and discussed how they have been influenced by it, it is surprising that he has so frequently been the subject of what appears to be ill-informed criticism, but, rather than meeting the critics head on, Robinson has decided to adopt the strategy of letting Young's own achievements counter the accusations. It is, in my view, important that he succeeds in his enterprise, for it is not only Young's reputation which is at stake, but also the idea that the polymath has a special and unique contribution to make and that people with such skills should be encouraged to develop them for the ultimate good of science.



From Clockwork to Crapshoot

A History of Physics

Roger G. Newton

Belknap Press

2000

ISBN

978-0674023376

350pp

Hardback £20

***Reviewed by:
Dr. Peter Ford, Bath***

This book attempts in one volume to give a history of physics, from the dawn of mankind to the present day. It is a formidable task but one which I believe has been largely successful. Roger G. Newton is Distinguished Professor Emeritus of Physics at Indiana University.

The book is divided into thirteen chapters. After a brief Prologue, the first short chapter, "Beginnings", deals with what little science we have gleaned from the Ancient Egyptians, Babylonians, Hittites, Assyrians and others. The second chapter called "The Greek Miracle" is much more substantial and discusses the work of known individuals whose thoughts are well documented. Throughout the book the author devotes quite a lot of coverage to developments in mathematics, which underpins physics as well as to astronomy. It is amazing what the Greeks managed to find out about the Earth and the Solar System as well as basic physics, especially with their lack of experimental equipment.

The third chapter was on "Science in the Middle Ages" an area about which the average physicist probably knows very little. The chapter highlights the important contribution to science and mathematics made by Europeans but also discusses the contributions made by the Chinese and the Arabs. I was interested to read that the greatest of the Muslim natural philosophers around that time was Alhazen, who was born in Basra at about 965 AD.

The fourth chapter, "The First Revolution" should be much more familiar to people. In it the work of well known scientists like Copernicus, Tycho Brahe, Kepler, Galileo and Newton is discussed. Most readers interested in the history of science should be familiar with the content of much of this chapter but the author presents his material well. The following chapter deals with "Newton's Legacy", over the two centuries following his death. The author highlights the fact that by now great emphasis was being placed on experimental observation and theoretical explanation using mathematical analyses. During this period there were important developments in astronomy. Notable among them was the work of William Herschel, who, among his many achievements, made the discovery (here in the city of Bath) of the planet Uranus, the first new planet to be observed since antiquity. Throughout the book, the author gives brief but interesting biographies of many of the leading scientists with which he is dealing. This gives a good human touch to science, something which is probably much needed. In this chapter he highlights the work of prominent mathematicians whose work underpinned developments in physics. These included Daniel Bernoulli, a member of a prodigiously talented family of Swiss mathematicians, and Leonhard Euler, who was also born in Switzerland three hundred years ago (see *Physics World* for April 2007). Other mathematicians discussed are Gauss, d' Alembert, Lagrange, Laplace, Legendre, Fourier and Cauchy as well as William Rowan Hamilton, from Trinity College, Dublin whose seminal work appeared slightly later in the first half of the nineteenth century.

Chapter 6 is called "New Physics" and is a nice account of much of the physics that I learnt at my grammar school in North London in the 1950s. Included are Dalton's work on atomic theory and its developments, heat, light and sound as well electricity and magnetism. The three following chapters are on "Relativity", "Statistical Physics" and "Probability". The material discussed in the first two of these chapters should be familiar to

most physicists while the third is less so. The chapter on probability discusses the work of the French mathematician and philosopher Blaise Pascal as well as that of Jacques Bernoulli, Thomas Bayes, Jules Poincare and several others. Their work has not only been influential in physics but has impacted in such diverse areas as geology, actuarial work, economics, sociology, biology, evolution and much else.

Not surprisingly, the longest chapter in the book is Chapter 10, "The Quantum Revolution", which mainly took place in the first thirty years of the twentieth century. This was a truly remarkable period in the history of physics, the consequences of which have transformed the world. There are good accounts of the work of Planck, Rutherford, Bohr, Einstein, Heisenberg, Pauli, Schrödinger, Dirac, Born and others. The brief history of the lives of many of the scientists was particularly interesting. I found it poignant to read that the last thirty or so years of Planck's life, (a man who comes across as a very decent and upright person), must have been blighted by the death of all four of his children. His son was killed in the First World War; shortly afterwards both his twin daughters died in child birth; and his remaining son was executed in 1944 for his alleged participation in a plot to assassinate Hitler. The chapter also briefly discusses some of the philosophical aspects of quantum theory such as the Einstein-Podolsky-Rosen paradox and the ideas of David Bohm and John Bell.

The last three chapters are on "Fields, Nuclei and Stars", "The Properties of Matter" and "The Constituents of the Universe". These chapters very much bring the subject up to date. As a condensed matter physicist for much of my working life, I was impressed in Chapter 12 by the authors' account of the main features of this vast subject. The book concludes with an Epilogue and a good selection of sources for further reading.

On the back of the book, the eminent historian of science, Stephen G. Brush, has written: *"Although there are several books on the history of physics, none is as up-to-date, comprehensive and well written as Newton's. Most other books either provide a very superficial explanation of the concepts and theories or are too technical for most non-scientists to understand. Newton says just enough about the difficult issues to get the reader interested but not overwhelmed"*

It is a viewpoint with which I would concur.



Patrick Blackett:

Sailor, Scientist and Socialist

Peter Hore (Editor)

Frank Cass Publishers 2003

ISBN 0-7146-5317-9

330 pp *Hardback* *£75*

Reviewed by:

Emeritus Prof. Derry W Jones

Chemical and Forensic Sciences, University of Bradford

Patrick M.S. Blackett (1897-1974) was not only a Nobel Prize winner (1948) in one field and a gifted experimentalist in two other fields of physics but during World War II he made notable operational-research contributions to the effectiveness of all three Services. Morally he showed courage in standing out against indiscriminate bombing of German cities and post-War he argued that nuclear weapons should be treated along with other weapons of mass destruction (a phrase used in his widely read 1948 book on atomic energy) and controlled internationally. His concern for developing countries was especially manifest in a long association with India as a scientific adviser.

Politically, Blackett was a Fabian, active on the left in the 1930s, and from the 1950s he helped evolve a peacetime Labour policy for science and technology. He was largely responsible for the form of the Ministry of Technology set up by Harold Wilson in 1964. His Presidency of the Royal Society, 1965-70, laid a new emphasis on international scientific collaboration and interaction with technology and engineering.

Unusually, this hugely successful science career was founded on an entirely naval education under the so-called Selborne Scheme, with two years each at Osborne Naval College and Dartmouth College, then both fairly new. These residential institutions combined a rigorous general education with about one-third engineering while the many hours in workshops built on Blackett's mechanical aptitude.

Blackett was precipitated into active service in the Royal Navy in August 1914 when not yet 17. He thus took part as a cadet in the Battle of the Falkland Islands in December 1914 and then controlled a battleship gun turret at the Battle of Jutland before seeing action in destroyers. Some would say that in his later academic career Blackett's scientific management style had something of a ship's captain (though he had objected to class distinction aboard ship), delegating but retaining ultimate responsibility.

Blackett's sudden transition in early 1919 from naval lieutenant to science undergraduate arose after the young officers had been sent to Cambridge for a short civilising experience as compensation for premature curtailment of the Dartmouth course. Graduating at 21, he was able to begin under Rutherford a research career which went successively from Cambridge (including a spell in Germany) to Birkbeck College, Manchester University, and Imperial College.

The physics of Blackett's career was superbly covered in Sir Bernard Lovell's affectionate 150-page Royal Society obituary published in 1975, unusually soon after the subject's death in July 1974. Lovell recounts how Blackett first took over and greatly modified the Schimatzu version of the Wilson cloud chamber to study the impact of α -particles on a nitrogen nucleus. With the arrival in Cambridge of Occialini, Rossi's Geiger-counter coincidence counting was linked to the cloud chamber.

The recording of tracks of “penetrating corpuscular radiation” or cosmic rays in high strength magnetic fields led to the observation of the positron and Blackett’s concentration on cosmic rays at Birkbeck 1933-37 and then at Manchester. Here, before and after the War, greater resources than at Birkbeck enabled Blackett to expand the cosmic ray team. However concurrent active interests in geomagnetism and palaeomagnetism led to the development of a magnetometer sensitive to fields as small as 10^{-8} - 10^{-9} G. His post-war Manchester staff included many who later went to prestigious chairs including C.C. Butler (Imperial), L.Janossy (Budapest), A.C.B. Lovell (Jodrell Bank), G.D. Rochester (Durham), S.K. Runcorn (Newcastle), S. Tolansky (Holloway), and J.G. Wilson (Leeds). He lectured to undergraduates but, one recalls him telling the first year in 1945, only on those parts of the subject (properties of matter) that interested him.

Lovell’s Royal Society biographical memoir devotes just two pages to Blackett’s education and naval career. Almost the only mention of naval experience is to Blackett retrieving scrap naval gun-turret racks to elevate Lovell’s massive radio-telescope. Peter Hore, a retired naval Captain, on the other hand, has edited a volume, largely based on papers read to a 1998 centenary conference, that highlights the naval background and influence through 16 essays.

Most of Hore’s authors are either career or latter day historians of science or war and at least half have a naval connection by service or teaching at Dartmouth or both. About half also have a link with Cambridge where, in fourteen years, Blackett went from beginning an undergraduate course to F.R.S. Inevitably there is overlap so that, for example, particular parts of Blackett’s naval and scientific career are each referred to in several places.

The first five chapters, including an autobiographical reminiscence, deal with Blackett’s education and the scheme under which he studied, followed by his World War I naval career, a total of ten years in uniform. Hore also includes contributions by three authors without historian claims or links with either Cambridge or the Navy: Bernard Lovell, Harold Wilson and Richard Ormerod. Lovell’s Royal Society memoir is especially strong on Cambridge, Manchester and defence science. After the Tizard Committee in the 1930s, Blackett served successively at the Royal Aircraft Establishment in 1939 (designing with HJJ Braddick the Mk XIV vector bomb sight), at the army Anti-Aircraft Command, at the RAF Coastal

Command (with severe comments about the deployment of Bomber Command), and then at the Admiralty, dominated by the anti-submarine battle. In only ten pages in Hore's book, Lovell puts the scientific achievement in context with just one paragraph on defence science and with only brief reference to Blackett at the Ministry of Technology. This latter topic is enlarged on by Wilson who, as President of the Board of Trade, had appointed Blackett in 1949 to the new National Research (with no "and") Development Corporation, precursor of the British Technology Group. Andrew Brown, the biographer of J.D.Bernal, also summarises Blackett's time at Cambridge, setting the physics in its artistic and political background.

Despite running operational research groups in all three Services, and being acknowledged as one of the founders of O.R. – he set up the Operational Research Society and initiated and wrote the first paper in its *Journal* in 1950 – Blackett was not subsequently involved directly with the discipline. In a different style from the other essays in Hore, Ormerod concentrates on the subsequent development from Blackett's view of O.R. as the application of the scientific method to help managers of organisations, avoiding unjustified predictions from data of spurious precision, towards a greater emphasis on overall policy decision-making aided by algorithms and contemporary rapid data manipulation.

In addition to Ormerod, at least four authors make brief reference to Blackett's WW II OR., but four chapters are entirely devoted to aspects of it. David Zimmerman describes Blackett's early participation under Tizard (greatly admired by Blackett) in the Aeronautical Research Committee (1934) and the Committee for the Scientific Survey of Air Defence (1935). On the CSSAD's Radio Research Sub Committee (1939), Blackett helped confirm the extension of operational research, a term probably used first by Watson Watt, as he tried to optimise processing of information from radar stations at a filter centre, to air defence generally. Jock Gardener, a submariner with experience in both Intelligence and OR., maintains that there was a link between these two communities late in the War in Blackett's Dept. of Naval OR. Blackett is often credited with making the case for larger convoys in May 1943 (the perimeter to be defended is proportional to a linear dimension while the area occupied by ships depends on its square), though Crowther and Whiddington (1947) attributed the detail to H.R. Hulme and J.H.C. Whitehead. In one of the longest articles

in the book (99 references), Malcolm Llewellyn-Jones concludes that convoys became larger perforce because of the needs for enhanced imports to prepare for the invasion with limited availability of escorts but Blackett's detailed analysis engendered confidence in best use of resources.

Paul Crook gives a fresh appraisal of the 1942 case against area bombing of Germany, much discussed in the 1960s by Blackett, C.P.Snow (for whom Blackett was probably the model for Francis Getliffe in the novel *Corridors of Power* (1964)), and others. In the Tizard-Cherwell debate, for which Cherwell misused data collected by J.D.Bernal and S.Zuckermann on the German bombing of Hull and Birmingham, Blackett opposed the case for ruthless saturation bombing of German towns on both operational and moral grounds. His regret at failing to be sufficiently persuasive over a more intelligent policy than indiscriminate bombing influenced his writing, unpopular in 1948, on nuclear strategy discussed by Philip Towle.

The longest article in the collection is by Robert Anderson about Blackett's affection for India, starting in 1947 with his advice to Nehru on military affairs and subsequently on scientific research institutions. Blackett made many visits and Lady Blackett has said how passionately he cared about his work there. From the 1930s Blackett frequently wrote and spoke in public of the disparity between rich and poor in Britain but interaction with India turned him towards world-wide disparities so that he argued in his 1957 BA address (Blackett, 1966) that differences in power, wealth and health among the nations were sources of discontent that needed to be relieved. He urged the West to sacrifice immediate prosperity to give massive aid to the have-not countries.

Taking the title of her reflective and well-referenced article from C.P.Snow's novel *Corridors of Power*, Mary Jo Nye (who has also studied the life of Polanyi, Blackett's contemporary at Manchester) summarises Blackett's views as those emanating from one brought up as an English gentleman and as a naval Officer trained in the Edwardian customs of war. From his early Cambridge days he was in touch with Fabians, socialists and writers such as Kingsley Martin, as well as the A.Sc.W. scientists. Partly through dining with the Tots and Quots (convened by Bernal, Zuckermann and others), Blackett had access to an influential elite, so that his views on war and politics would still be heard even if controversial. He recognised that his naval education provided a strong intellectual and mechanical

foundation for a wide-ranging life in physics. Also the bulk of his time during World War II, whether as Director of Naval O.R. or at RAF Coastal Command, was devoted to the Battle of the Atlantic. Until then, he could hardly be said to have looked back after resigning from the Navy in 1919. However Tam Dalyell writes in the Forward to Hore's book that in the 1960s he still recognised Blackett's commanding presence and "lifelong naval officerhood".

Lovell's fine 1975 biographical memoir remains the best account of Blackett's life. However, Hore's authors provide complementary coverage at some length of his naval education, active service, and subsequent significant advisory activities outside mainstream physics research. The several essays and parts of essays on O.R. activities in World War II throw an interesting light on the novelty then of the use of science in the Services and how a scientific approach had to compete with distinct Service cultures, sceptical about slide-rule strategy, when a very small group was reaching decisions with far-reaching consequences. Hore's book is not the equivalent of a full biography in that there is little reference to family and friends. But, taken with Lovell's memoir, it provides an amplification of the educational, Senior Service, and political milieu under which physics of the highest distinction was carried out.

Bibliography

P.M.S.Blackett, *The scientist and underdeveloped countries* in *The Science of Science*, eds. M.Goldsmith and A.Mackay (London, Souvenir Press, 1964).

J.G.Crowther & R.Whiddington, *Science at War*, (London, HMSO, 1947).

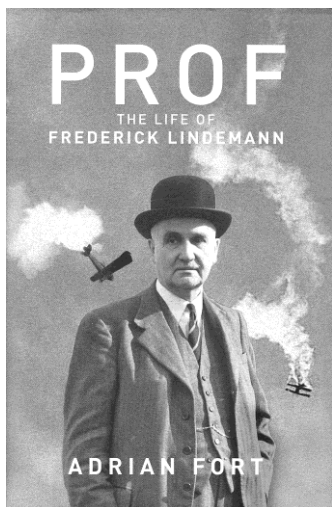
Sir Bernard Lovell, *Patrick Maynard Stuart Blackett, Baron Blackett of Chelsea*, Biog. Mem. F.R.S. **21**, 1-154 (1975).

J.A.Ratcliffe, *Robert Alexander Watson-Watt*, Biog. Mem. F.R.S. **21**, 549-568 (1975).

C.P.Snow *Corridors of Power* (London, Macmillan, 1964).

Significant aspects of Blackett's career are also covered by Mary Jo Nye in *Blackett: Physics, War & Politics in the Twentieth Century* (Harvard University Press, 2004).

Book Notices



'Frederick Lindemann's private Life was a closed book. His arrogant wit and supreme confidence in his own judgement brought him many enemies. But no other scientist in history has achieved more political power. His remarkable contribution in both spheres remains unparalleled.'

The award-winning biography of Frederick Lindemann entitled 'Prof' by Adrian Fort, upon which the article is based, is available at a discounted price of £10 plus P & P. Please send orders to me (Newsletter editor) – contact details on page 72.

ISBN 0-224-06317-0

Zero to infinity: the foundations of physics

by Peter Rowlands.

This is a physics book of a totally new kind. It starts from the simplest possible foundation, the concept of zero, and develops what is described as a universal computational rewrite system as the most fundamental information processing system in nature. One immediate result is a version of relativistic quantum mechanics which is not only simpler, and more fundamental, but also seemingly more powerful than any other quantum mechanics formalism available, with immediate applications in particle physics, theoretical physics and theoretical computing. Relativistic quantum mechanics turns out to be easier than the non-relativistic version, and much of the conventional apparatus of quantum mechanics and quantum field theory becomes redundant. However, the applications of the method extend far beyond this. The aim in all cases is to develop a profound qualitative understanding, often using symmetry, before mathematical formalisms are applied. No other work on physics has used so foundational a viewpoint and so minimalistic a technical apparatus.

This, according to the author, is the way to find a route through the current impasse in our search for the ultimate foundations of physics.

World Scientific Publishing, Winter 2007, ISBN-13: 978-981-270-914-1

Letters

Dear Editor,

March 2007

I am a member of the IoP and a retired Reader in the History of Science and Technology (Teesside). Recently I have begun a project to study the place and work of physicists in industry, a somewhat large task, but I intend to start with now elderly men and women who began their industrial employment in the 10 to 15 years immediately after the second world war and I wonder whether any interested readers would get in touch with me about this.

By the way I am an elderly physicist who began his work with de Havilland Propellers in 1955. Thus I am conscious of two factors making my task easier: the number of people who form my target 'audience' are somewhat small, for they are old(ish) and the cohorts will have been reduced by time, and originally there were not many around anyway. I calculate that there might some 2500 - 3000 men and women still around so if I could contact 100 of these I would consider myself lucky.

Dr Colin A Hempstead
2 Uplands Road
Darlington
DL3 7SZ
+44(0)1325 483439
colin.hempstead1@ntlworld.com

~~~~~

Dear Editor,

May I offer two comments on Derry Jones's substantial notice of Andrew Brown's even more substantial biography of J.D.Bernal (Newsletter No.21, p.54)? Eartha Kitt's entertaining and extended list of infidelities includes the phrase "faithful in [my] fashion" (here the "my" surely means "her", not "his"). Although surely anyone might have coined these words (especially if caught in flagrante delicto), authorship is usually ascribed to the poet Ernest Dowson (1867-1900) in his poem Non Sum Qualis eram Bonae Suo Regno Cynarae, where the line "I have been faithful to thee, Cynara! in my fashion" rounds off each verse.

My second comment is on a quite different matter. Derry Jones writes (p.55) that Bernal, though brought up a Roman Catholic in Ireland, "attended an English Protestant boarding school". According to Chambers Biographical Dictionary he was in fact educated by the Jesuits at Stonyhurst in Lancashire, which might well explain his lurch at Cambridge from one extreme to the other of lifelong commitment to the Communist Party.

Richard Crossley  
York

~~~~~

Derry Jones replies:

The Bernals were originally Sephardic Jews who fled from Spain in the 17th century; the Irish branch adopted Catholicism early in the 19th century. J.D.Bernal spent one Christmas term 1912 at the Jesuit public school Stonyhurst, preceded by a year at the English prep school Hodder. However most of his education was at Protestant schools. From 1906 to 1911 he was at Nenagh, Northern Ireland, and then from January 1914 to 1919 (apart from two short interruptions) he was at Bedford School, chosen by his mother because it was strong on science.

Apparently he hated Stonyhurst and, although he may not have liked military drill and repetitious games at Bedford, he enjoyed rowing and the cross-country paper chase. He displayed a precocious talent for science there, taking up astronomy, microscopy and mathematics and also studying mineralogy and crystallography. Bernal remained a devout Catholic and an Irish nationalist at Bedford. He records his conversion to socialism on 7th November, 1919, at Cambridge but continued to attend Mass regularly for over a year until, he said "religion gradually slipped from one like a worn-out cloak".

Apart from Andrew Brown's biography, Dorothy Hodgkin's Biographical Memoir of FRS, **26**, 17 (1980) includes a summary of Bernal's early life and background.

During a BBC radio interview around the year turn 2006/7, Eartha Kitt, at 80, rendered again her well-known song, but still in English, not Latin.

Web News

The History of Physics Group web pages have been updated with most of the information I sent to the IOP Webmaster in May. Please will members look at it and let me have your comments, particularly if you have an old home computer without the latest Web browser. I think there may still be some missing links and I have trouble reading some of the pages.

The July copy of the IOP publication *'Interactions'* had news of another English Heritage Blue plaque, this one marking the home of Hertha Ayrton, (1854 - 1923), at 41 Norfolk Square, London. She is one of the women who has a room named after her in the Institute's Conference centre. I am looking for a brief summary of the careers of the other physicists after whom rooms were named. I understand there was a leaflet produced when the Conference centre was opened, if anyone has a copy please send it to me to mount on our Web pages.

News of blue plaques in Ireland: one to Fitzgerald in Dublin, and another to J.D. Bernal, unveiled in July 2005, in the Heritage Centre of his Birthplace, Nenagh, Co.Tipperary. I will be adding these to our web pages shortly. Meanwhile you may like to know that there is also a blue plaque to Bernal in London, at 44 Albert Street where he lived for many years.

Bernal was also in the news in 'The Times' of London on 2 April, 2007. He had many friends in many spheres of life, one of whom was the artist Picasso, who drew a mural on the wall of his flat during an evening party. This was saved when the flat was demolished and has now been bought by the Wellcome Trust who plan to have it permanently displayed and freely accessible from June 2007.

The conference 'John Desmond Bernal: Science and Society' held in Limerick University on June 1st 2006, has proceedings published by the IOP in their Journal of Physics: Conference Series, Volume 57, 2007 at www.iop.org/EJ/toc/1742-6596/57/1 where the complete text of the papers is available for downloading on-line. Please note I have a new email address, please note that from 1st July 2007 my email is

kmcrennell@iop.org

Forthcoming Meetings

Early Days of Particle Physics, Bristol, October

On Wednesday 3rd October the H.H. Wills Physics Laboratory of the University of Bristol is hosting a meeting to mark the discovery of the pi-meson and V particles. The title of the meeting is "Early Days of Particle Physics". In addition to the University of Bristol, the meeting is being supported by the History of Physics Group and the South Western Branch of the Institute of Physics as well as by PPARC. The meeting will begin at 1.30pm and will be held in the Powell Lecture theatre of the H.H. Wills Physics Laboratory, University of Bristol, Tyndall Avenue, Bristol BS8 1TL. The meeting is free and all are welcome to attend. Further information will be available from Dr Vincent Smith

(e-mail: vincent.smith@bristol.ac.uk), at the above address. It would be useful if the secretary of the History of Physics Group or Dr Smith are informed if you plan to attend the meeting.

History of Physics Group half day meeting and AGM, Glasgow, November

The Annual General Meeting and lecture programme will take place on Thursday, 15th November, at the University of Glasgow, starting at 2pm. The theme of the lecture programme is "**Kelvin in Context**" and it will dwell on various aspects of Kelvin's life and work. It marks the centenary of his death in December, 1907. It is envisaged that the speakers will include Professor Crosbie Smith from the University of Kent, who, together with M Norton Wise, has written the classic book "*Energy and empire: a biographical study of Lord Kelvin*". The meeting takes place the day after another meeting devoted to Kelvin's legacies, which is being organised by the University of Glasgow and the Institute of Physics (see the Institute of Physics conference website for further information on this meeting or www.kelvin2007.org).

The **Annual General Meeting** of the group will be held at around 5pm, immediately after the lecture programme. Our secretary and Treasurer, Peter Ford will be standing down after 5 years of sterling service, so nominations for that office or for the new committee would be most welcome. Please send any nominations/suggestions to Peter or myself.- Ed

History of Physics Group Committee

Chairman	Professor Denis Weaire Department of Physics Trinity College Dublin Ireland denis.weaire@tcd.ie
Hon. Secretary & Treasurer	Dr. Peter Ford 13 Lansdowne Crescent Bath BA1 5EX * P.J.Ford@bath.ac.uk
Newsletter Editor	Mr MJ Cooper Ivy Cottage, Fleetway North Cotes, Grimsby Lincs DN36 5UT mjcooper@physics.org 01472 389467
Web Pages Editor	Ms Kate Crennell kmcrennell@physics.org 01235 834357
Also:	Dr. P. Borchers Dr. C. Green Dr. J. Hughes Mr. A. Jackson Mr. S. Richardson Dr. P. Rowlands

* Peter Ford, our current Secretary/Treasurer of the Group has recently retired from the Physics Department of the University of Bath. Please note the new address; his email will remain as given above for the time being.

-Ed