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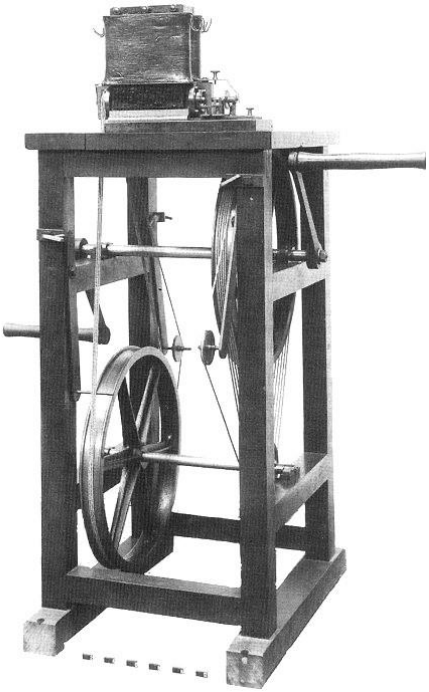
# Newsletter



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Cover picture: Wheatstone's self excited generator

# Contents

# Editorial

**'NEWTONIAN IDEAS OVERTHROWN'**

Thus trumpeted a newspaper headline of 1919 reporting the meeting of the 'Royal and Astronomical Societies' called to discuss the results of Eddington's expedition.

Cosmology, General Relativity as exemplified by the bending of light in a gravitational field, will be one of the leading themes in the 4th International HoP Conference to be held next year (see page 97) and I'd been browsing the headlines to see what was being reported in the press at the time.*

Revisionism as a re-examining of the orthodox view is a fair and reasonable aspect of historiography but if it becomes extreme and ill-founded it may become 'negationism' - coined, I believe, by Henry Rousso - for example the inventing of ingenious but implausible reasons re-interpreting or distrusting accepted accounts or theories.

So, is the headline revisionist or even worse, negationist? No, of course not. It's simply a newspaper journalist trying to be a little provocative to grab the reader's attention.

A more recent example appeared in the 'Daily Mail Online' (yes, really)

## The kilogram has been reinvented!

*'Scientists formally change the measurement based on a lump of metal to a calculation based on the speed of light.'* It goes on:

*'From now it'll be measured by the ratio of energy to frequency of the photon'* Come on now - that's not bad for the popular press.

And there follows a description of the Kibble Balance and how it's used to re-define the kilogram. The article was published on May 20th 2019 - the date of the official acceptance of the redefinition.

Pretty impressive, I thought. Editor

* 'Front Page Physics', IOPP, 1994.

# The Young Lady and the Professor.

***Denis Weaire***
***Trinity College Dublin***


Humphrey Lloyd is one those supporting members of the cast of the history of physics who have retreated into the wings and have hardly been recognised thereafter. Part of the reasoning behind the International Conference on the History of Physics* has been to bring out such characters before the curtain, to receive the applause they deserve.

I talked about Lloyd at the conference in San Sebastian. The context was the discovery of conical refraction [1], in theory by Hamilton and in experiment by Lloyd. I do not propose to tell the story again rather I shall recall some of the tangential thoughts that came to mind in studying his work.

Firstly, I was impressed by the vigour and enthusiasm of the British Association for the Advancement of Science, in which he played a leading role. It was not a British conception: similar societies had been launched on the Continent some years previously, but they cannot have matched it in ambition to advance science, or indeed the determination to have a damned good time while doing so. (I found a reference to finely attired ladies "parading promiscuously" at a *conversazione*, which may be misleading in modern parlance but nevertheless paints a colourful picture. Our dinner speaker at the first (Cambridge) conference had told us of Maxwell and others letting their hair down at the Association meetings. We should not be ashamed to do the same.

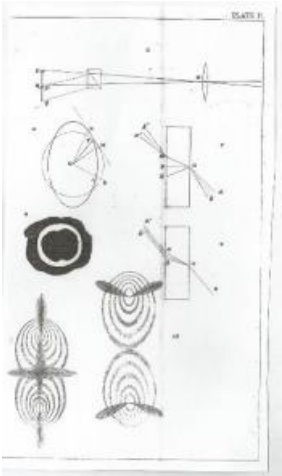
Admittedly, Lloyd was a reverend gentleman and on the face of it, a dry stick. Yet on closer examination he is an appealing personality. My source for this (found accidentally, like so much in historical research) lies the diaries [2] of Caroline Fox. She seems have been the kind of Victorian lady that willingly accepted her subordinate feminine role, yet used every opportunity to interact with the male practitioners of the sciences and arts. She was an inveterate name-dropper, but many of her comments on the friendships that she cultivated are acute. Her father's involvement in the Association gave her the opportunity to mix with the leaders of science. One of her favourites was Lloyd, "the Professor", with whom she conversed whenever she could, particularly when the Association went to Dublin. "He looks on science with the ardour of a lover and the reverence of a child."

Much of her commentary upon Lloyd had to do with his philosophical outlook. Today we have little sympathy with the philosophical debates of our forebears, which underscored their researches. Are we missing something?

Right: Caroline Fox

In trying to unravel Lloyd's work, I was struck by the change of style in publications since that time. His influential report on optics for the Association is a lengthy narrative without the illustrations, tables, graphs, and equations that we deploy today. The prosaic (and occasionally poetic) approach of the Victorian physicists extended to the primary literature as well, including Lloyd's original account of conical refraction. How could he describe it without a sketch of the magical ring of light that he had observed, the "radiant stranger", as a poet and friend called it? I contrasted this with the case of Lord Rosse, sketching his observations while perched on high on his Leviathan in the cold night air (charmingly described by Caroline, when she visited Birr Castle with the Lloyds.)

At the last moment, before leaving for the San Sebastian conference, intending to express this frustration, I thought to look in Lloyd's undergraduate lectures [3], including (doubtless to the bewilderment of the students) the tricky subject of conical refraction. There it was, his sketch of the observation. I am delighted to have brought this particular light to light.



Other thoughts that came to mind, such as were the persistence of topics in research – conical refraction is still studied today on theory and experiment. Also the unpredictability of eventual applications – in this case they have arrived after a century and a half. Style certainly changes, but the essential process of the advancement of science does not, at least yet.

Next year we meet in Dublin for the Fourth International Conference on the History of Physics, As we climb the stairs to the lecture theatre (famously used by another philosophically minded physicist - Erwin Schrödinger), we will pass under the watchful eye of old Humphrey himself.

Remember to have a good time!

References:

[1] Hamilton, W. R.,1837 Third Supplement to an Essay on the Theory of Systems of Rays, trans. Royal Irish Acad. 17, 1-144

[2] Caroline Fox, Memories of Old Friends, Smith, Elder, 1882

[3] **Humphrey Lloyd**, 1800-1881: **Lectures** on the **wave-theory** of light. (Dublin, A. Milliken, 1841

* For more information on the series please see: http://hop.iopconfs.org/events

## ‘Gender diversity and equality in the last century’.

***A personal story by John Dainton.***

By the year 1939 my mother had completed all the requirements for the award of a BA degree in Zoology in Cambridge. She was, however, unable to take her degree because the university statutes stated that only men could graduate. This statute continued in place throughout her PhD work, and she departed with both a titular MA (Cantab) and a titular PhD (Cantab).

My mother was always very proud of her titular degrees, that is of her lack of degrees (!), and she would take pleasure in telling those who asked that she attended Cambridge “but never graduated - twice”! And she continued to say so (wholly truthfully) for decades after the University of Cambridge changed its ways and permitted women who, according to modified statutes, had just achieved the requirements to graduate.

In later life, she was appointed to a Fellowship and Tutorship at St Hilda’s College Oxford, in Zoology. On appointment, and I suspect also because of my mother’s mischievous sense of humour when appointed, and her similarly mischievous silence on the matter when interviewed because she was never asked, she duly took up her appointment. It took some weeks or months before it became known that she was NOT IN FACT a graduate of the University of Cambridge, or indeed of anywhere. The University of Oxford therefore took immediate steps to grant her an MA (Oxon). Given the lack of any degree from any university, it was never made clear to her on what basis this degree was awarded (how could “ad eundem” be possible with no degree?), but a degree was awarded and she was assured that she could call herself Barbara Dainton MA(Oxon). My mother of course continued to take pride in the fact that not only did she fail to graduate from Cambridge twice but only finally graduated from Oxford in her 50s … and that still she didn’t have a PhD!

And the story continues..! She duly retired in the 1980s MA (Oxon). Then in 1998 (I think) the University of Cambridge decided to celebrate the achievement of its women alumni, and organised a degree ceremony to admit all those women who were not permitted to graduate at the time of meeting the university’s requirements when the statute forbidding women was still in place. She duly attended and, along with others of similar age, paraded through the streets of Cambridge and graduated MA (Cantab) PhD

(Cantab). I, at the time, remained confused as to how she could now be awarded MA (Cantab) being already MA (Oxon) and so presumably since then eligible anyway for “ad eundem” in Cambridge?

It was only after she told me why she had relented and decided at this first opportunity to take her Cambridge degrees that I understood why she let go of her pride in NOT having graduated from Cambridge. It was because of incorporation. In the last few years of her life she was able to say that she continued to be unique (with her contemporary colleagues) and that she was a graduate with an MA (Cantab), and an MA (Oxon), when this was impossible BECAUSE of the continuing incongruity and contradiction of one or other or both of incorporation and “ad eundem”!

My mother always chose her words with a twinkle in her eye and denied any knowledge of physics even though she DIDN’T have a degree in it!!!!!) other than to illustrate further the experiences of women in academia in the 20$^{th}$ century, and how with a sense of humour they could deflate any male superiority with mischievous humour when faced with the riddles presented by male domination. For I, of course, knew well that this was her way, and how effective it was throughout her life!

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## Newsletter - Print or Online only?

Your committee has been considering this question - would you prefer to continue receiving print copies of the newsletter or would you be content with online versions only? (At the moment online versions are available as well as print.) A survey is being considered but in the meantime please let me know your views. Thanks.

Malcolm Cooper
Editor

Meeting Report

## Physics at London Universities

Much attention of the History of Physics Group of the Institute of Physics and of other similar organisations is paid to the achievements of physicists at Cambridge University, with perhaps Manchester University running in a good second, and of course this is certainly justified. When attention is paid to London, it is nearly always focused on the Royal Institution, in particular on Michael Faraday, but also on a host of others including Humphry Davy, John Tyndall, James Dewar and the Braggs, father and son. Again this, of course, is quite justified.

Yet it is unfortunate that, for this reason, attention has been diverted from the excellent physics and physicists at the various colleges of London University. Of the colleges, Imperial College was, of course, founded in 1907, though based on existing colleges, specifically for the study of science and technology, and it has maintained a considerable strength in physics. Among its most important researchers have been William Crookes, George Thomson, Patrick Blackett, (the 4$^{th}$) Lord Rayleigh, Dennis Gabor, Abdus Salam and Tom Kibble.

University College and King's College have also been very strong in physics. Among those associated with University College have been Owen Richardson, Frederick Soddy, William Ramsay, W.H. Bragg, Otto Hahn, Francis Crick and Jocelyn Bell Burnell, while those working at King's have included Charles Wheatstone, James Clerk Maxwell, Charles Barkla, Owen Richardson, Edward Appleton, and Maurice Wilkins and Rosalind Franklin. The smaller colleges have also had excellent research workers: John Desmond Bernal and David Bohm worked at Birkbeck, Kathleen Lonsdale at Royal Holloway (as well as University College) and Thomas Young at St. George's Hospital. And Queen Mary (College) was famous for having its own nuclear reactor for a considerable period!

The following articles are based on the talks given in the meeting of the History of Physics Group held in April 2019 on physicists Charles Wheatstone, JD Bernal and RJ Strutt. There was also a talk on Henry Tizard, Rector of Imperial College and an article on the 'scanners', those people (nearly all women) whose job was to look through the piles of pictures produced by high energy particle accelerators in the 1960s and 70s.

We are also pleased to include an article on Kathleen Lonsdale by Jenny Wilson who unfortunately could not attend the meeting.

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Physics at London Universities
24th April 2019
Programme

| | | |
|---|---|---|
| 10.00 | Registration | |

First Session: Chair – Andrew Whitaker

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|---|---|---|
| 10.20 | Charles Wheatstone | Brian Bowers |
| 10.55 | John Desmond Bernal | John Finney |
| 11.30 | break | |

Second Session: Chair – Julian Keeley

| | | |
|---|---|---|
| 11.50 | Tom Kibble | Frank Close |
| 12.25 | scanners | Jim Grozier |
| 1.00 | lunch | |

Third Session: Chair – Vincent Smith

| | | |
|---|---|---|
| 1.50 | Rosy and the Third Man: Rosalind Franklin and Maurice Wilkins | Gareth Williams |
| 2.35 | Robert John Strutt, 4th Baron Rayleigh | Ted Davis |
| 3.10 | David Bohm | Chris Dewdney |
| 3.45 | break | |

Fourth Session: Chair – Jim Grozier

| | | |
|---|---|---|
| 4.05 | Abdus Salam | Michael Duff |
| 4.40 | Henry Tizard | Andrew Whitaker |
| 5.15 | close | |

# Charles Wheatstone 1802-1875

***Brian Bowers***
***Science Museum (retired)***

Fig 1 Charles Wheatstone as a young man

There is no record of how Charles Wheatstone (1802-1875) [Fig 1] came to be appointed Professor of Experimental Philosophy at the new King's College London in 1834, but he later told Sir John Herschel that the appointment 'was quite unsolicited on my part'. When he died, however, he was held in such high esteem that the College physics laboratory was renamed the 'Wheatstone Laboratory'.[Fig 2] Today he is best known - if known at all - for the 'Wheatstone Bridge' circuit. He did not invent the circuit, but he drew attention to it in a paper on electrical measurements which he presented to the Royal Society in 1843.

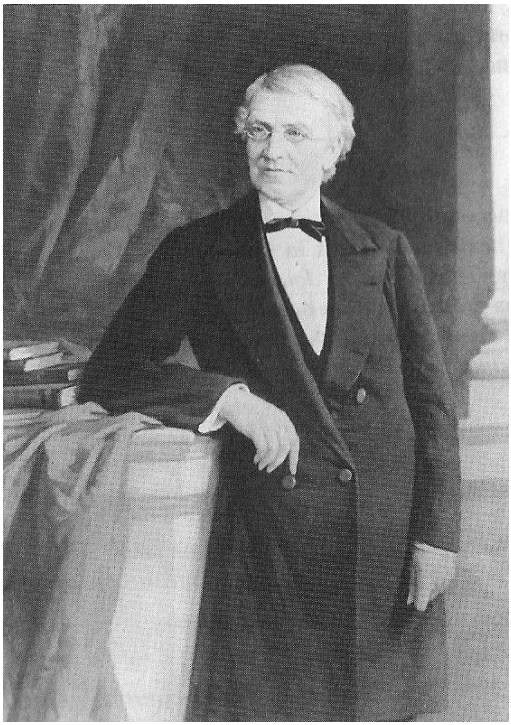


Fig 2 Sir Charles Wheatstone

In that paper he clearly acknowledged the real inventor, S.H. Christie, and also the work of the German scientist G. S. Ohm whose law connected current, electromotive force, and resistance. [Fig 3] Wheatstone's interest in electrical measurements arose from his interest in the electric telegraph, which grew out of his work in the family musical business.

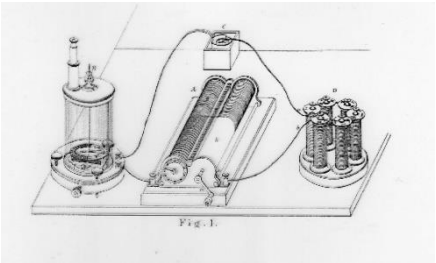
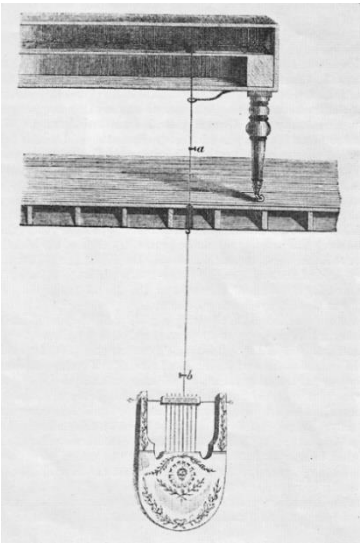



Fig 3 Circuit drawing from Wheatstone's measurements paper showing his rheostat

Charles Wheatstone was born at Barnwood, near Gloucester, into a family who were mostly engaged in the publication and sale of music and the manufacture of musical instruments. When he was four the family moved to London where an uncle, another Charles Wheatstone, was also in business making musical instruments. On leaving school our Charles was apprenticed to his uncle, but business life did not appeal to him. He was, however, fascinated by the science underlying the instruments they made. In particular he was intrigued by the transmission of sound through solids. In a piano the strings are about a centimetre away from the soundboard; in a violin the spacing is greater. In both cases, however, sound is transmitted from the string to the sound board through a solid 'bridge'. Wheatstone wanted to know how far sound could be transmitted through a solid. He tried a number of experiments to find out. First he stretched a string on a steel bow and connected the bow to the sound board of a piano through a glass rod two metres long. The sound was heard just as well as when the string was in direct contact with the soundboard. Later he repeated the experiment with a series of wooden rods twelve metres long.

Wheatstone was very shy and incapable of giving a good lecture himself but he was a showman at heart. In September 1821 he arranged a public exhibition which was reported in several journals. A soundboard in the form of a lyre was suspended on a brass wire 'as thick as a goose quill'. [Fig 4]



The wire passed through a hole in the ceiling and was attached to the soundboard of a piano or other instrument in a room above. Music played on the piano was heard coming from the lyre. Only stringed instruments were used because Wheatstone found that while it was comparatively easy to conduct sound from instruments with a solid soundboard it was almost impossible to couple the lyre to instruments such as the flute where all that vibrated was a column of air.

Fig 4 Wheatstone's drawing of his sound transmission demonstration

People actually paid to come to the exhibition and hear the ‘enchanted lyre’.

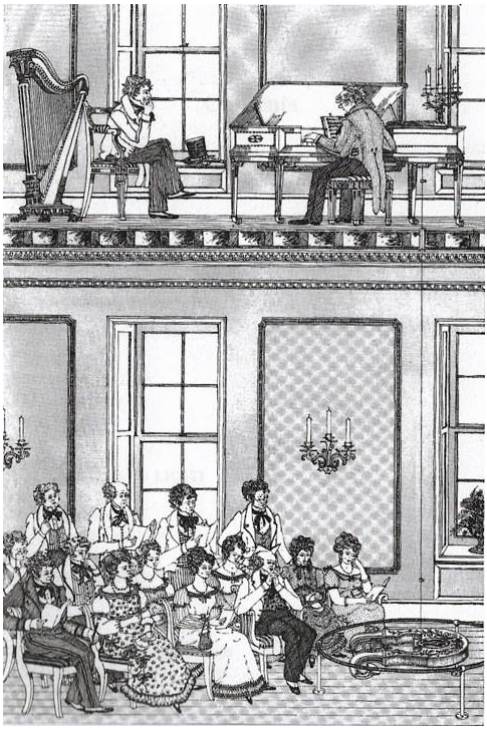


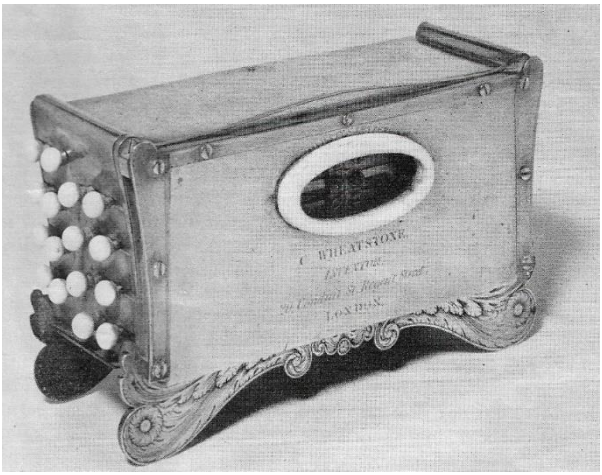
Fig 5 Artist’simpression of the ‘Enchanted Lyre’ demonstration

One visitor to the exhibition was the Danish scientist Hans Christian Oersted who was in London and found that he and Wheatstone had conducted a number of similar experiments. He introduced Wheatstone to the scientific community, with which he was not previously acquainted. This included Michael Faraday, who loved music and subsequently publicised Wheatstone’s work in some of his discourses at the Royal Institution. One lecture included experiments with Javanese musical instruments collected by the naturalist Sir Stamford Raffles who had served in various government posts in the Far East. In another lecture Wheatstone showed how the action of the Jew’s harp depends on the player adjusting his mouth so that the air within it resonates at particular harmonics of the natural frequency of the harp. On another occasion the discourse was illustrated by a Mr Mannin who was said to have the remarkable ability of being able to whistle two notes at the same time and thus whistle a duet!

Wheatstone’s most important work in sound followed Chladni’s discovery that if a horizontal metal plate was made to sound by drawing a violin bow across its edge then a fine layer of sand on the plate would form geometric patterns. Chladni assumed, correctly, that the pattern indicated the mode of vibration and Wheatstone wondered whether an even finer powder would reveal finer patterns, and he found that to be so. (This was one of the experiments that Wheatstone and Oersted had both carried out

independently). Subsequently Wheatstone showed by a semi-mathematical analysis that the more complex patterns could be broken down into simpler patterns superimposed.

His most significant practical work in sound was the development of ‘free-reed’ instruments, especially the symphonium and the concertina.
[Fig 6 below]




The Symphonium

In these instruments the reeds, which are the source of sound, are metal strips clamped at one end. The free ends are set in vibration by a current of air. In the concertina the current of air is produced by bellows on the instrument; the symphonium had the same musical mechanism as the concertina but was blown by mouth. Finger buttons controlled the flow of air so that only the selected reed vibrated, and in 1829 Wheatstone patented a design in which the finger buttons were arranged so that pressing any two adjacent buttons produced a chord. In Wheatstone’s life time the firm produced a large number of concertinas and a smaller number of symphoniums.

He would have liked to transmit sounds over great distances, and suggested a sound conductor between London and Aberdeen. He also suggested a network of sound conductors around London for the broadcasting of Parliamentary debates, but as Wheatstone himself noted, that would require

that the material of 'the conducting-body possesses perfect homogeneity, and is uniform in its structure'. He was also concerned to transmit sounds without distortion. He quoted with apparent approval the observation that sound travels with least distortion through those substances in which it travels fastest. That begged the question what travels faster than sound, and that question led him on to electrical research. He wanted to know how fast an electric current travelled through a conducting wire, and he devised an experiment to measure the speed. [Fig 7]

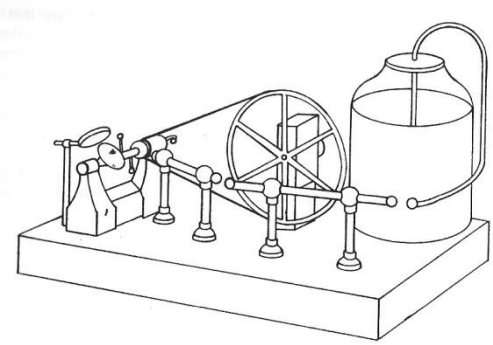


Fig 7 The rotating mirror apparatus

He obtained a grant of £50 from the Royal Society and had several miles of wire suspended in a basement corridor in King's with both ends and the middle brought to the same point. There was a spark gap at each end and at the mid point, and when a current was passed through the wire sparks were seen at each of the three gaps which to the unaided eye appeared simultaneous. Wheatstone's apparatus had the three spark gaps arranged in a line, and when the sparks were viewed in a mirror revolving at high speed it was seen that the sparks at the end gaps were really simultaneous, but the spark in the middle was delayed by a time which could be estimated knowing the speed of the mirror. [Fig 8] He never published an actual figure for the speed of the electric current, but he considered what was 'the greatest velocity of transmission through the wire, that can be detected by means of the instrument which I have described' and concluded that it was about 280,000 miles per second – very much faster than the speed of transmission of sound.

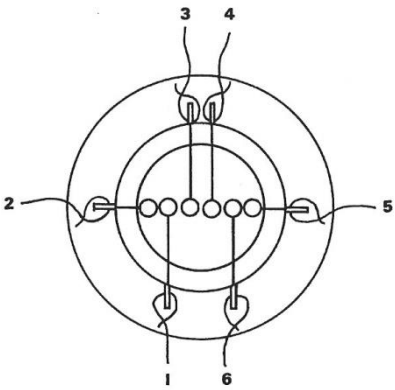



Fig 8 The three spark gaps

Wheatstone then turned his attention to the idea of a practical *electric* telegraph. He had to address two questions: the nature of the receiver, and how the circuit should be arranged to operate through a very long wire. For the receiver he initially used four galvanometers (probably because he had four lengths of wire in the College) arranged so that any two could be deflected, one in each direction. Subsequently he used five galvanometers [Fig 9] connected through five wires and arranged so that deflecting two of them would point out one of twenty letters on a panel. [Fig 10]

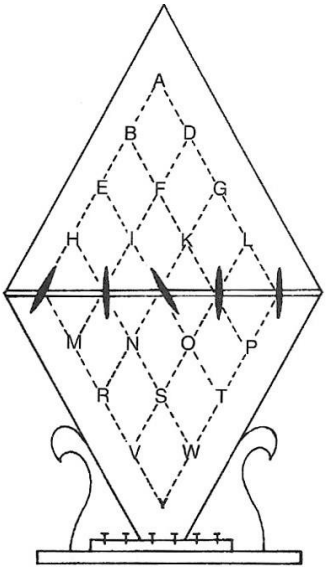



Fig 9 Drawing of the five-needle telegraph instrument

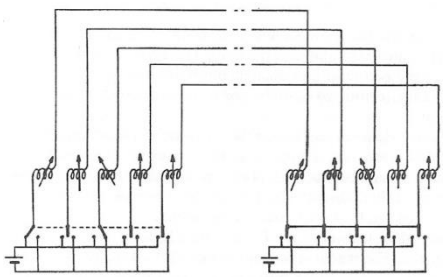


Fig 10 Circuit of two five-needle telegraph instruments

Twenty letters are quite adequate to send a message in intelligible English so the five-needle telegraph could be used by anyone who could read. Later telegraphs used only one (or occasionally two) needles which greatly reduced the cost since the connecting wires were the most expensive part of a telegraph system. Operating over a long distance required an understanding of Ohm's law, which showed that the resistance of the receiver should be a significant part of the total circuit resistance.

Another telegraph inventor was William Fothergill Cooke, who had made a telegraph which operated across a room but would not work through a mile of wire. Seeing that he had a scientific problem Cooke sought scientific advice and was eventually put in touch with Wheatstone. Since the two men had a similar purpose they co-operated and the first practical telegraph in commercial use was installed in 1838 by Cooke using Wheatstone 5-needle instruments on the new Great Western Railway from Paddington to West Drayton. [Fig 11]

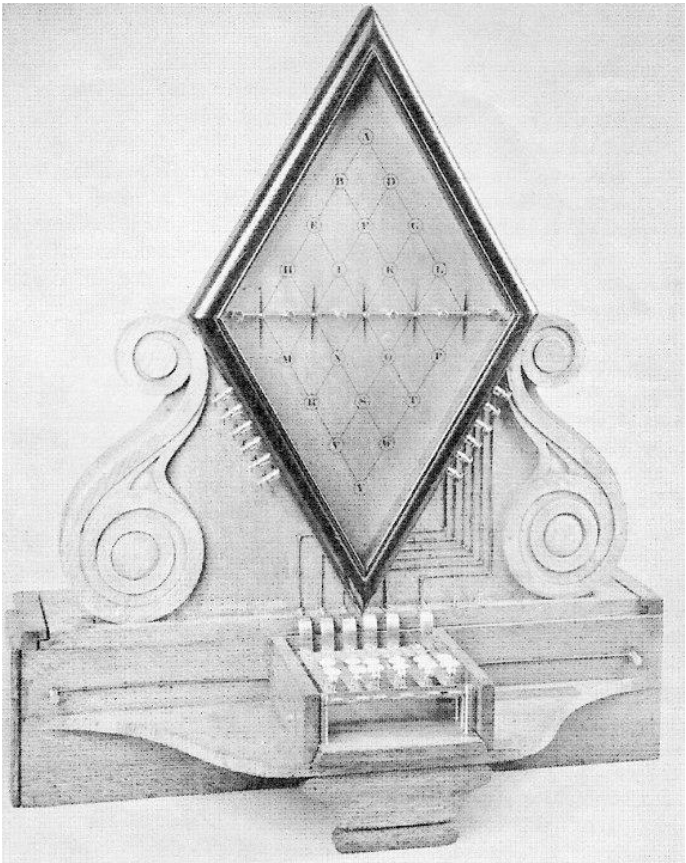


Fig 11 The five-needle instrument used between Paddington and West Drayton

They should have formed an ideal partnership, but sadly Cooke and Wheatstone did not get on together. Cooke bought out Wheatstone's share of their joint enterprise and established the Electric Telegraph Company which arranged many telegraph installations after 1838 until the telegraphs were taken over by the Post Office in 1868.

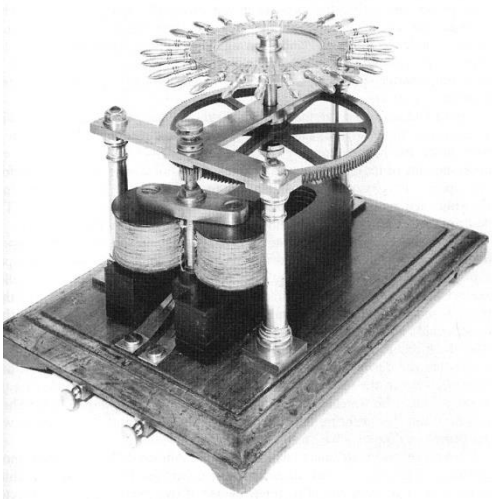
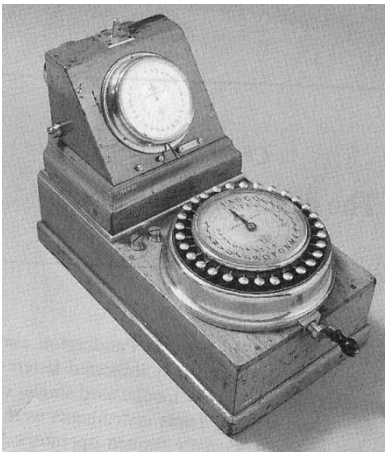


Fig 12 Experimental ABC telegraph transmitter

Wheatstone was always interested in simple to use, direct reading telegraphs, and he went on to develop the 'ABC' telegraph and the automatic telegraph. In the ABC telegraphs the person sending a message turns a pointer always in the same direction over a circle of letters, stopping at the letter to be sent. The person receiving the message sees a pointer moving over a similar circle of letters, and notes which letter is indicated at each pause. His first ABC telegraph, made about 1840, employed a magneto to generate the pulses and sent a pulse as the pointer passed each letter. [Fig 12 above]



Subsequently he made more compact ABC instruments [Fig 13 left] and these were widely used until superseded by the telephone. The simple ABC system required the person sending a message and the person receiving it to be at their instruments at the same time and they had to be in a single circuit. A practical communication system needed receiving instruments which could record the transmitted message and an 'exchange'to connect the sending instrument to the particular

receiver to which it was desired to send the message. Wheatstone made a telegraph receiver which printed out the received message as words, but this would have been very slow in operation. [Fig 14, Fig 15] A practical system required the use of codes. Most telegraph companies adopted Morse Code, and used instruments which printed the received message on paper tape.

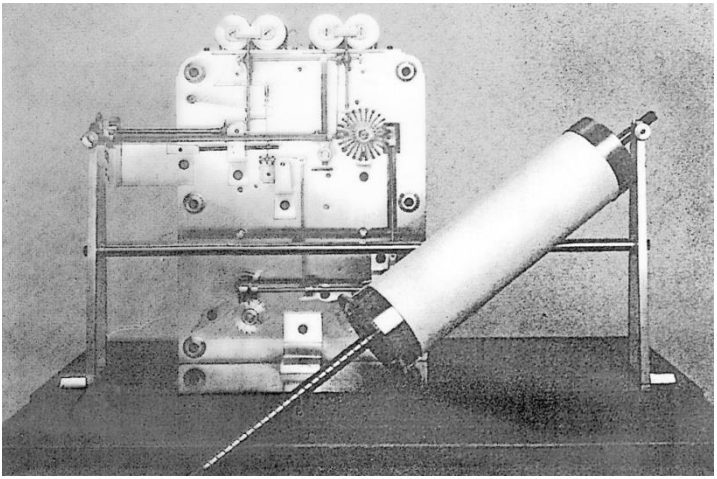


Fig 14 Printing telegraph with drum removed

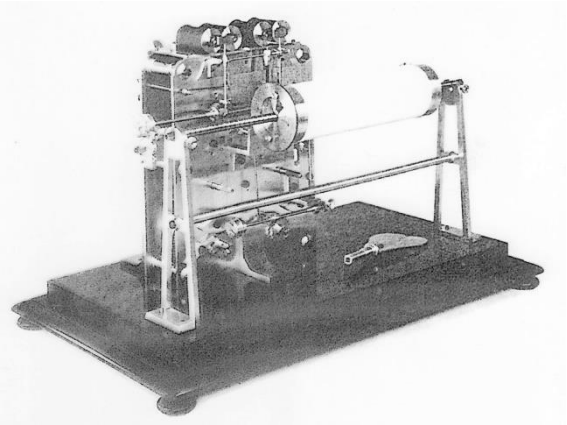
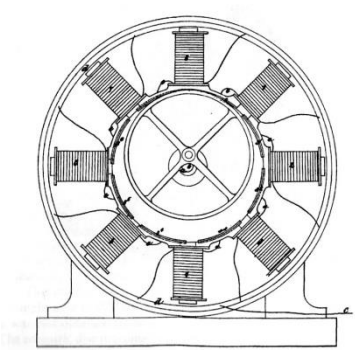


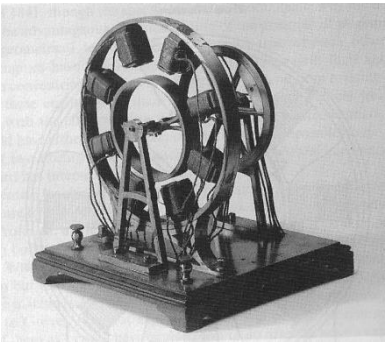
Fig 15 Printing telegraph in operation

The speed of transmission - and hence the revenue of the company - was determined by the speed of the operator sending the message. To speed up the system messages were first punched onto paper tape and the tape then fed into an automatic transmitter which could take tapes from several operators. The first automatic tape readers had sprung contacts pressing through the holes

in the tape, and these could operate at about thirty words per minute. In terms of the number of messages sent Wheatstone's greatest contribution was the automatic telegraph transmitter. This was a tape reader in which sprung rods 'felt' for the presence or absence of a hole at each position, and could operate at almost 200 words per minute. In 1880 Sir William Preece, speaking about 'The Telegraphic Achievements of Wheatstone', said his greatest achievement was the automatic telegraph.

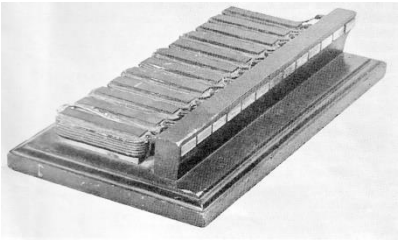
Wheatstone was interested in electricity for other purposes, as well as telegraphy. In the early 1840s he made a number of machines which he called 'eccentric electromagnetic engines'. Other people had tried to make electric motors, but the problem they all found was that although the electromagnets provided a strong force it was effective over such a short distance as to be virtually useless. Wheatstone's machines had the unusual feature that the armatures moved in a line inclined to the direction of the magnetic pull. Several of his motors are preserved in the Science Museum





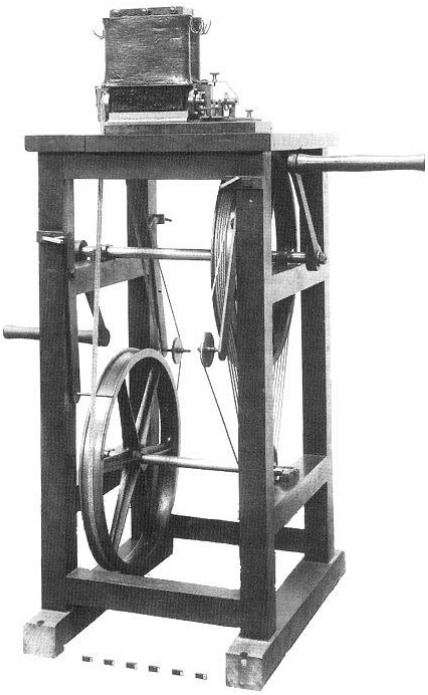
Figs 17 & 18 Drawing and actual device of one design of electro-magnetic engine

Another of Wheatstone's electric motor ideas was a linear motor and the stator of one survives [Fig 19 below].



All of these machines will work, but they require a greater current than the generators available to Wheatstone could have produced. It was probably in attempting to overcome this problem that he made a self-excited generator, though he was not the only person to make such a machine. [Fig 20 below].

The motors just described will work, given an adequate current source, but if Wheatstone had obtained an adequate current the heating effect would probably have burnt out his cotton insulation!



An application of the telegraph which did not require much power was a system of clocks in which one master drove several repeater clocks. This is possibly the point at which to mention Wheatstone's Polar Clock, which was not an electrical device but an arrangement for determining the plane of polarization of light from the sky. It is effectively a sundial which works when the sun cannot be seen - even at night - as the plane of polarization of light from the sky follows the sun around the polar axis.

He also had an interest in codes and ciphers, possibly originating in a desire to keep telegraph messages confidential. He devised the substitution code known as Playfair Code which was used by the British in the Boer War and the First World War. Wheatstone and his friend Lyon Playfair, whose name has always been associated with the code, used to amuse themselves deciphering coded messages in the personal column of *The Times.* He also devised an instrument he called the 'cryptograph', a simple encoding and decoding device which was sometimes sold with the ABC telegraph.

Wheatstone's physical studies included optics as well as acoustics, and he saw analogies between light and sound. Although it had long been appreciated that the two eyes see slightly different views of the world and help us to estimate the relative distances of two objects, he was probably the first person to appreciate that the brain combines the two views of an object to give the impression of solidity. When first made public, in a lecture he gave in 1848, stereoscopy created interest in scientific circles. With the development of photography, however, it became fairly easy to produce stereoscopic pairs of pictures and the stereoscope became one of the most popular scientific toys of the nineteenth century. Wheatstone's stereoscope using mirrors was a bulky instrument. [Fig 21] Sir David Brewster devised a stereoscope using lenses which was more compact and became the most widely used form of stereoscope.

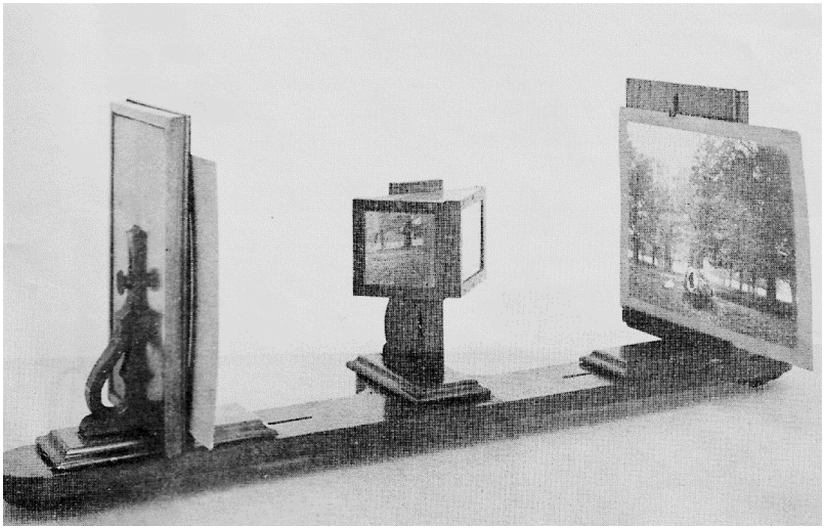


Fig 21 Stereoscope using mirrors

The surviving records of Wheatstone's life are mainly about his scientific work. There is little about the man himself, but the glimpses of his private life suggest a man with a keen sense of fun who could be the life and soul of a private party while painfully shy in public. He married at the age of 45, and had two sons and three daughters.

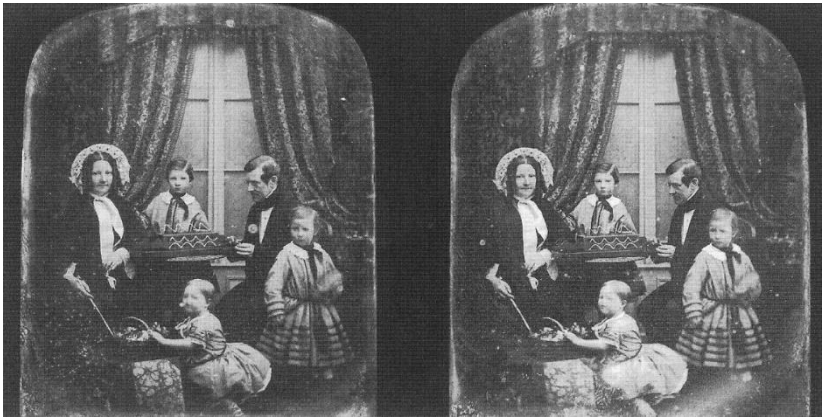


Fig 22 Charles and Emma Wheatstone and three children

He served on committees dealing with such varied topics as the construction of submarine telegraph cables, the design of the bells for the new Palace of Westminster (including 'Big Ben'), the ordnance problems of the navy and army, and the heating and ventilation of houses and schoolrooms. He was honoured by governments, universities and learned societies in many countries, and was knighted by Queen Victoria in 1868. He never retired, but worked until a few days before his death in 1875 while attending meetings of the Academy of Sciences in Paris.

[For further information and detailed references see Brian Bowers, *Sir Charles Wheatstone*, HMSO for the Science Museum, 1975, or, revised edition, Institution of Electrical Engineers, 2001.]

*****

## Disclaimer

# John Desmond Bernal
1901-1971
Birkbeck College 1938-1971

***John Finney***
***University College London***

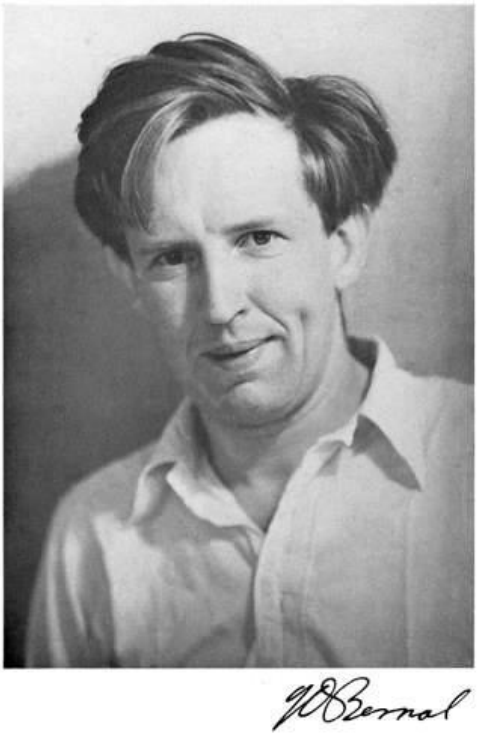



Figure 1. Bernal in his early days.

## Introduction

Bernal (figure 1) was one of the great polymaths of the 20th century. In addition to his science crossing the traditional boundaries, his active interests stretched through the arts and politics. Alan Mackay, who worked with Bernal for twenty years longer than I did, used the word ‘polytropic’ to

characterise Bernal's being active in many dimensions and directions[1]. In fact, Bernal liked to say that his biography should be written in four colours, and on interleaved pages to show how his different activities in science, politics, the arts and his personal life fitted together. An excellent introduction to many of these facets can be found in the proceedings of a 2006 Symposium held in Limerick – the county town of his birth[2]. I shall stick mainly to aspects of his science.

Bernal was born in 1901 to a farming family in Nenagh, County Limerick. His mother was a huge influence on his life, Bernal saying that she made him understand the beauty of the external world. She devoted herself to his education (he was essentially bilingual in French and English from an early age), sending him to a Jesuit school in England when age 12. Recognising his precocious ability in science, he moved on to Bedford School which had a good science programme, and subsequently to Cambridge University in 1919.

As an undergraduate, he studied both the Mathematical and Natural Sciences Triposes. In addition to physics and chemistry, he took courses in geology and mineralogy, which led to his becoming fascinated by the possible different types of arrangements of atoms in space. Though these 230 space groups had been elucidated independently 30 years earlier by Shoenflies and Fedorov, their approaches had been qualitatively geometrical and had taken several years of work. Over a few months, Bernal developed a completely original and quantitative theory such that every crystal structure could be represented by a formula. The work was too long to publish, even in a 60-page cut-down version, but he was awarded a prize for the work.

## Early research

In 1924, he moved to the Royal Institution. In his recommending Bernal to Sir William Bragg, Arthur Hutchinson, the Cambridge Professor of Mineralogy who encouraged Bernal's crystallographic interests, wrote:

> *"I did not however realise (and he never let on) that he had got so keen that he spent the whole of the next vacation in developing a method of dealing with point systems in the hope that it might be useful in X-ray work! When therefore, he*

1 A.L. Mackay. *J.Phys. Conf. Series* **57** (2007), 1.
2 V. Casey (ed.). *J.Phys. Conf. Series* **57** (2007).

> *suddenly appeared and deposited on my table a thick, type-written MS, rather with the air of a dog bringing a poached rabbit to his master's feet, I was quite amazed—of course I make no pretence of being able to appraise its merit or even its usefulness—still it seemed to me a remarkable effort for an undergraduate in his third year..."*

Bernal liked to tell, very shyly, of William Bragg's response when he reminded him of his thesis on space groups:

> *"Good God, man, you don't think I read it".*

The first page was sufficient to show that the young man was worth encouraging in research.

It is worth emphasising that this was in the very early days of X-ray crystallography, just over ten years since the derivation of Bragg's Law, and Bernal was in there developing its potential. While at the Royal Institution he determined the structure of graphite and worked on the crystal structures of bronzes. As was to become characteristic of much of the early work he did on a range of systems, once he had done the foundational work he typically gave away the problems to others – in the case of the bronzes to A.J. Bradley in Lawrence Bragg's Manchester laboratory and Linus Pauling on the US.

He developed much of the early X-ray diffraction instrumentation and methodology of diffraction pattern interpretation. For example, he developed the rotation X-ray camera, the first version of which is illustrated in figure 2 (left). He liked to describe how this was first tested by mounting a crystal at the centre of a kitchen alarm clock, with a piece of brass tube above it. Within the brass tube was placed the X-ray film, held in position with bicycle clips. He also developed the Bernal chart, a transparent film which was overlaid on the exposed film that enabled the various diffraction spots to be indexed – a procedure that was still being used in the 1970s.

In 1927 he moved to Cambridge as the first Lecturer in Structural Crystallography. In addition to developing a flat-plate X-ray camera (the original of which survives in the Science Museum), he initiated many new structural investigations. Particularly fruitful was work on sterols and sex hormones. But perhaps the most dramatic development was his beginning to apply X-ray crystallographic techniques to try to elucidate the structures of biological molecules such as proteins. In collaboration with Dorothy Hodgkin (née Crowfoot), he demonstrated for the first time that a wet protein crystal could diffract X-rays, opening up the possibility of determining the atomic structures of biological macromolecules. He took on Max Perutz who chose to take on the – then – very difficult challenge of the structure of haemoglobin, and was later joined by John Kendrew who was ultimately to solve the structure of myoglobin. Both were to receive Nobel Prizes.

## Birkbeck – and the war interlude

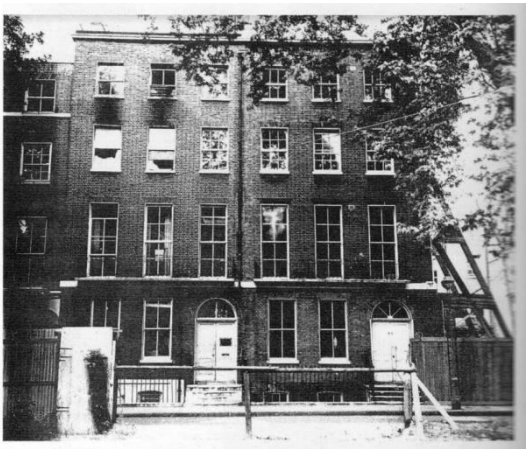
In 1938, Bernal moved to Birkbeck to take up the chair in physics vacated by Patrick Blackett, who had built up the department into "one of the best in the country: a leading centre of cosmic ray research and a haven for émigré scientists from Europe"[3]. Like Blackett, who moved to Manchester to take the chair vacated by Lawrence Bragg, Bernal was attracted to the radical tradition of Birkbeck as a centre focussed on part-time undergraduate students who earned their living during the day.

With the war breaking out soon afterwards in 1939, Bernal became deeply engaged in war work (see reference 3 for a full discussion of this). Together with Blackett, C.H. Waddington and Solly Zuckerman, he was a founding father and leading exponent of operational research. He was scientific adviser to Mountbatten, the Chief of Combined Operations, and contributed a great deal to the war effort, examples being the first proper analyses of the effects of enemy bombing and of explosions on animals and people, and the detailed mapping and tidal conditions of the Normandy landing beaches. In fact, with respect to the latter, Major Logan Scott-Bowden, who was chosen to reconnoitre the proposed positions of the Mulberry harbours (huge concrete caissons with a flexible steel roadway laid over their tops), commented that "Bernal was crucial to the planning of D-Day.

3 A. Brown. *J.D. Bernal. The Sage of Science.* Oxford University Press, Oxford (2005), p. 156.

He was in charge of it in a way."[4]. Not bad for someone who liked to jokingly remember that he "committed the frightful solecism of not knowing which was port and which side was starboard".

In early 1945, Bernal's thoughts began to return to Birkbeck, from where his war work had forced his absence. In February of that year, he drew up a 'Draft Scheme for a Biomolecular Centre', which he envisaged as a multidisciplinary department that would exploit all the available experimental techniques to study the structure and functioning of proteins. Barely a few weeks after setting out his scheme, the Nuffield Foundation came up with an offer to buy the necessary equipment and support the salaries of the senior staff. He set about assembling his research team: Harry Carlisle would head the protein crystallography section; Helen Megaw – his first Ph.D. student in Cambridge – would head a group concerned with the structure of cement and other building materials; Werner Ehrenberg, whom Blackett had brought to Birkbeck, would lead the development of high intensity fine focus X-ray tubes. And finally, recognising the potential of digital computing to crystallography, he appointed Andrew Booth to develop its application. Two Georgian houses in Torrington Square were taken over to house the Birkbeck Biomolecular Research Laboratory, which was opened on 1st July 1948 by Sir Lawrence Bragg (figure 3 below).



**21.** 21–22 Torrington Square just before the houses were demolished in 1966. Bernal's flat was at the very top of No. 21 on the left

4 Ref. 3 p.482.

From these beginnings, the structural research of the department – which gained its independence as a separate Department of Crystallography in 1964 (with Ehrenberg taking over the head of the Physics Department) – began to prosper. Aaron Klug joined the protein team in 1953, while Rosalind Franklin, who had joined the department from King's earlier that year, took up the work on viruses that Bernal had started with Isidore Fankuchen in Cambridge. Following Helen Megaw's move to Cambridge, Jim Jeffrey took on the leadership of the cement work, while Alan Mackay – who in addition to making important contributions related to the structure of materials, predicted quasicrystals in 1981 – focussed on ideas of generalised crystallography that Bernal had begun to develop. The work on liquids, which will be discussed in a little more detail below, he kept to himself.

As stated in the introduction, Bernal was a polymath who had wide interests in the arts as well as science, and the laboratory was not immune to his other interests. For example, the Torrington Square houses that contained the Birkbeck Biomolecular Research Laboratory also housed Bernal's flat. Sometimes this was used to entertain visitors, one of whom was Pablo Picasso, who was in London in 1950 for an Arts Council exhibition of his paintings. During the party Bernal held for Picasso in the flat, he took Picasso round the crystallography laboratory. Bernal later reported that Picasso was "interested in … the form and colour of the crystals. He was struck by the resemblance of his pictures to some of our Fourier diagrams and wondered if he put them [his pictures] through the machine backwards, they would come out as crystals."[5] Returning to the party, Bernal asked Picasso if he would scribble something for him on the wall, which Picasso proceeded to do. After he stepped down from the chair on which he had been standing, someone shouted "what's that to do with peace?" So he added a pair of angel's wings (see figure 4 below).


5 Quoted in ref. 3 pp.333-334.

When some decorators arrived a few days later, they had to be firmly persuaded by Stan Lenton, the department superintendent (who was also to act at times as Bernal's chauffeur, mechanic and occasional butler) not to paint over the only Picasso mural in England. The mural also survived the demolition of the buildings in 1969, going first to the Institute of Contemporary Arts, then, via a sojourn in Birkbeck, to the Wellcome Trust where it now resides.

It has often been remarked that, despite being responsible for the basic work that has led to many Nobel Prizes, he was never awarded a Nobel Prize himself – perhaps because of his habit noted earlier of giving away interesting problems to others who went on to build on the foundations he had laid. As biochemist Martin Caffrey commented in his talk to the 2006 Limerick symposium: "Acknowledging the enormity of Bernal's contributions to the emerging field of structural biology, in 1962 John Kendrew was to send him a note in which he referred to Bernal as having 'fathered' five Nobel Prize winners in that year alone. They included Dorothy Hodgkin, Aaron Klug, Max Perutz, Maurice Wilkins and the note writer (Kendrew) himself"[6].

## Water and Liquids

Though Bernal threw out ideas for others to develop, he kept the development of his approach to liquids for himself. Initially, his interest was sparked by the biological relevance of water, saying "My interest in the subject ...came about...through my biochemical interests, in that all living structures are mostly composed of water"[7]. This interest was to lead to a landmark paper in 1933 published in the first number of the Journal of Chemical Physics[8]. Bearing in mind that this was only 20 years since X-rays were first diffracted from a crystal, and barely a decade since the first methods of working back from a diffraction pattern to a crystal structure were successful, to tackle the much more difficult problem of liquids, where there was inadequate understanding of how to interpret the broad diffraction patterns that liquids gave, was ambitious to say the least.

Arguing that the charge distribution on a water molecule was near-tetrahedral, Bernal proposed that as opposite charges would attract, the local molecular structure should be essentially tetrahedral (see figure 5).

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6 M. Caffrey. *J.Phys. Conf. Series* **57** (2007), 17 (quoted on p.22).
7 J.D. Bernal. *Phil. Trans. Roy. Soc. A* **280** (1964), 299.
8 J.D. Bernal and R.H. Fowler. *J. Chem. Phys.* **1** (1933), 515.

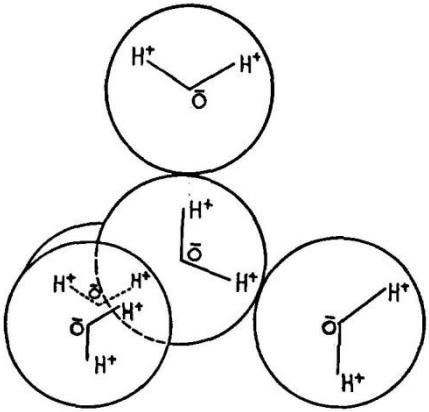



Figure 5. The ideal local tetrahedral arrangement of water molecules as first envisaged by Bernal in 1933. Two of the four molecules surrounding the central one are in the plane of the paper, while one is above and one is below it.

Noting that silica ($SiO_2$) also forms similar local structures, he developed 'disordered' versions of two silica structures (quartz and tridymite) to fit the X-ray data. With this model, he was able to explain a wide range of the properties of water and ionic solutions, as listed in the short summary heading that paper shown in figure 6. below (ref. 7)

SHORT SUMMARY

ON the basis of the model of the water molecule derived from spectral and x-ray data and a proposed internal structure for water, the following properties of water and ionic solutions have been deduced quantitatively in good agreement with experiment.

(1) The crystal structure of ice.
(2) The x-ray diffraction curve for water.
(3) The total energy of water and ice.
(4) The degree of hydration of positive and negative ions in water.
(5) The heats of solutions of ions.
(6) The mobility of hydrogen and hydroxyl ions in water.

And the following inferred in a qualitative way.

(7) The density and density changes of water.
(8) The explanation of the unique position of water among molecular liquids.
(9) The dielectric properties of water and ice.
(10) The viscosities of dilute ionic solutions.
(11) The viscosities of concentrated acids.

It is also worth noting that the not-quite tetrahedral charge distribution he used is essentially the father of the effective pair potentials used today in simulations of water and aqueous systems.

In Birkbeck, he returned to the water problem in the 1950s, when he recognised that his 1933 approach "was, frankly, one of crystal structure, trying to picture water structure as that of a mixture of the analogous four co-ordinated structures of … quartz and tridymite", and that "This was ultimately to prove rather a delusive approach, postulating a greater degree of order … in the liquid than actually exists there."[9]

However, rather than return to the specific problem of water, he recognised that he first needed to understand the structures of simpler liquids. Theoretical approaches to the liquid state at the time treated liquids either as disordered (crystalline) solids (as Bernal had done in the 1933 water paper) or as dense gases. Though the disordered crystal approach was mathematically tractable and could yield correct densities, it assigned too much order to the liquid – the predicted entropy was too low. On the other hand, treating liquids as dense gases required unphysical mathematical approximations; though the entropy could come out OK, the densities that could be handled were too low to be representative of real condensed phase liquids.

Bernal found both these approaches unsatisfactory. So instead he tried to find an approach that:

- was a concrete picture of the structure (Bernal was a crystallographer and so naturally would want to visualise the atomic arrangement);
- was consistent with Ockham's razor;
- was homologous to that of the crystalline solid as well as radically different in kind;
- had a general quality of homogeneity without the assumption of any special groups.

The most general hypothesis he came up with was to treat the liquid "as *homogeneous, coherent* and *essentially irregular* assemblages of molecules containing no crystalline regions."[10]. This concept he realised in the laboratory with assemblies of steel ball bearings, contrasting liquids as irregular *heaps* of molecules as against crystals as regular *piles*. Figure 7 illustrates the differences!

9 Ref. 6 p.300.
10 Ref. 6 p.301.

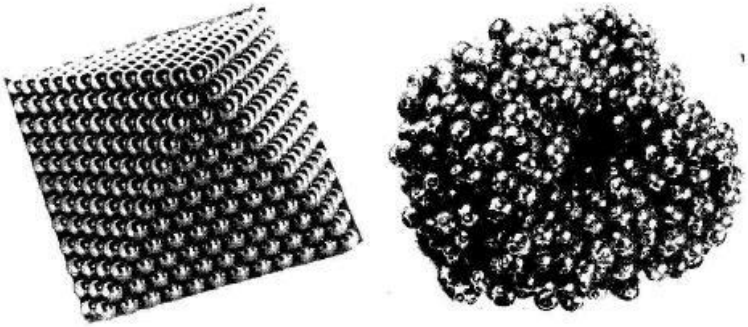


Figure 7. Bernal's concept of the simple liquid as an irregular heap of molecules (right) compared to the regular pile of the crystal.

This structural approach was indeed radically different from that of most other workers on the liquid problem – and indeed Bernal apologised to "the modern theoretical physicist for introducing such a simple way of looking at things, but I believe on the whole that it is better to start with a model that has some resemblance to reality"[11]

And indeed the model was successful for simple liquids such as those of the inert gases. It gave correct densities, explained density changes on melting, had the right degree of disorder, and essentially predicted the observed X-ray scattering. In the late 1960s, it was also successful in explaining structures of amorphous metal alloys. The coordinates of a large laboratory model on which much of the later work was based continues to be requested and used for a variety of theoretical and practical purposes[12]. And John Ziman, a key theoretician of liquids in the second half of the 20th century, commented that "This simple idea…is now seen to be the key to any qualitative or quantitative understanding of the physics of liquids"[13] Similar comments were made in 1970 by John Rowlinson of Imperial College, one of the foremost theoretical chemists who has spent a lifetime working on liquids:

11 J.D. Bernal, *Proc. Roy. Inst.* 37 (1959) p.355.
12 J.L. Finney. *Phil. Trans. Roy. Soc. A* 319 (1970), 479. Data available at https://www.digitalrocksportal.org/projects/47
13 J.M. Ziman. *Models of disorder*. Cambridge University Press, Cambridge (1979), p.78.

> *"It has therefore been hard to admit that the form or even the existence of the attractive forces has little direct effect on the structure of a liquid, as described, for example, by the pair distribution function g(r). The recent realization of this truth has followed the extensive studies … of the properties of assemblies of hard spheres without attractive forces."*[14]

Recent work is also suggesting that Bernal's model can explain the behaviour of liquids above the critical point, where the liquid/vapour coexistence line that vanishes at the critical point continues into the supercritical region with a line of maximum heat capacity[15].

So how did Bernal move from this 'irregular heap' model to the more complex water problem? Simply by recognising that a disordered non-crystalline arrangement could also be built up of molecules interacting in the essentially tetrahedral fashion of figure 5 to produce a random *network* of molecules, as against the random *packing* of the spherical molecules of simple liquids. Figure 8 shows a ball and stick visualisation of a random network compared to the ordered crystalline arrangement of hexagonal ice.

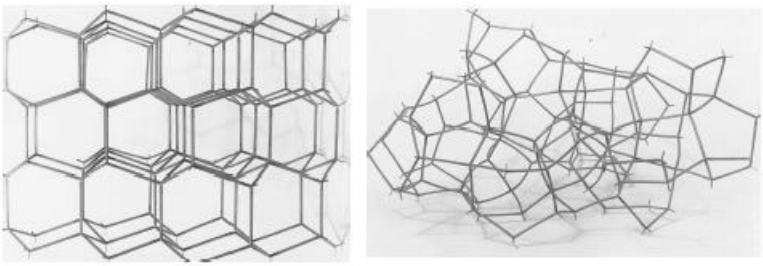


Figure 8. A 'spaghetti model' visualisation of (left) crystalline ice Ih, compared with (right) the 'random network' arrangement of liquid water.

14 J.S. Rowlinson, *Disc. Faraday. Soc.* 49 (1970), p.30.
15 J.L. Finney and L.V. Woodcock. J. Phys.: Condens. Matter 26 (2014), 463102.

So Bernal's final view of water[16] was that:

- Water is essentially a 'random network' of water molecules
- Each molecule interacts with its neighbours in an approximately tetrahedral geometry
- Local coordination is ideally 4-fold, but with some variation

And it compared well with experimental results. It explains the main properties of water such as expansion on freezing, the temperature of maximum density and other so-called anomalies, the mobility of hydrogen, and structural changes with temperature and pressure. It is consistent with current state-of-the-art experimental and computational work, which demonstrates that Bernal's random network concept is essentially correct. And it has indeed helped us to understand water's biological role – the reason that Bernal started working on the problem in the first place.

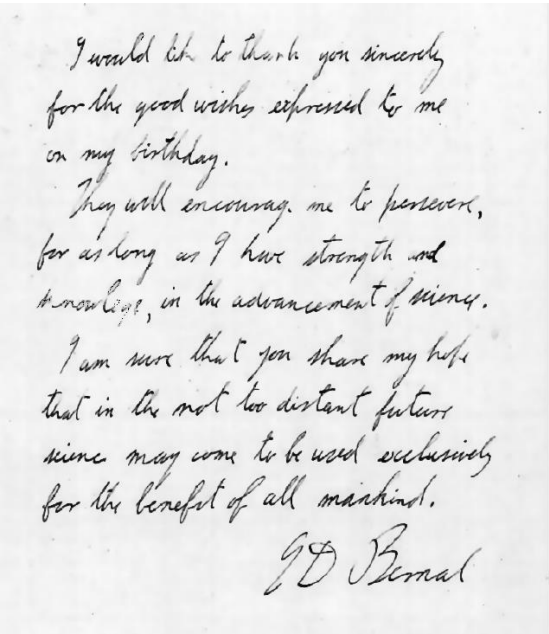
**Some concluding remarks**

It would be remiss to end without commenting on the breadth of his scientific and social interests, which are perhaps illustrated by the following titles of his books:

- *The World, the Flesh & the Devil (1929)*
- *The Social Function of Science (1939)*
- *The Freedom of Necessity (1949)*
- *The Physical Basis of Life (1951)*
- *Science and Industry in the Nineteenth Century (1953)*
- *Science in History (1954)*
- *The Origin of Life (1967)*
- *The Extension of Man. A History of Physics before 1900 (1972)*

It would also be remiss to end without emphasising Bernal's strong commitment to the use of science in the service of humanity. This is rather poignantly illustrated in his thanks for birthday greetings shown in figure 9.

16 For a fuller account of the development of Bernal's ideas on the structure of water, see J.L. Finney. *J.Phys. Conf. Series* **57** (2007), 40.
For an updated account of our understanding of water, see J.L. Finney. *Water. A Very Short Introduction.* Oxford University Press, Oxford (2015).



I would like to thank you sincerely
for the good wishes expressed to me
on my birthday.
They will encourage me to persevere,
for as long as I have strength and
knowledge, in the advancement of science.
I am sure that you share my hope
that in the not too distant future
science may come to be used exclusively
for the benefit of all mankind.

J D Bernal

Figure 9

And we should finally underline his influence on both other eminent scientific pioneers and his fundamental contributions to their work. For example, Aaron Klug, who took up the offer of the Medical Research Council (MRC) of space for the Birkbeck virology group in the new Cambridge Laboratory of Molecular Biology that opened in 1962 under Max Perutz, regarded Bernal as the godfather of the MRC lab. and the man who virtually invented the subject[17]. And Linus Pauling, who won both the Chemistry and Peace Nobel Prizes, regarded him as "the most brilliant scientist I had ever met. … He was astonishingly quick in grasping a new idea, and was often able to contribute an illuminating insight, based on the breadth of his knowledge and his extraordinary ability to see interconnections between apparently rather distant fields of science … Bernal must be considered as one of the greatest intellectuals of the 20th century"[18].

It was both a pleasure and a real honour to have been able to work with him.

17 Ref. 3 p.362.
18 Ref. 3 p.485.

# The Invisible Scanners[19]

***Jim Grozier***

***Department of Physics & Astronomy, University College London.***

## Introduction

A TV programme made for the BBC's *Horizon* series in 1964 features the discovery of the $\Omega^-$ particle in a bubble chamber experiment at Brookhaven National Laboratory in New York. After a lengthy sequence of interviews with theorists Murray Gell-Mann and Yuval Ne'eman, explaining the importance of the particle in relation to theory, the camera switches to a room at the lab where we see Nicholas Samios, one of the physicists involved in the experiment, explaining how it was actually done, and describing the equipment used. The Alternating Gradient Synchrotron (AGS) produced a beam of $K^-$ particles that were fired into an 80 inch bubble chamber, to test the theory that the interaction of a $K^-$ with a proton will occasionally produce an $\Omega^-$, following a distinctive decay pattern from which it can be identified. In the background you can see two women sitting at desks. Samios does not explain who they are, or what they are doing.

In fact the women were part of a large team of *scanners*, and their job involved examining the thousands of photographs produced by the experiment in the hope of finding one that showed evidence of the elusive particle. Any likely candidates – singled out by a tell-tale pattern of tracks which resembled possible expected decay patterns for the particle – would be flagged up for later scrutiny by the physicists.

The $\Omega^-$ was eventually found in photograph no. 97,025. This photograph was seen in the first instance by a scanner, then passed to the physicists for further analysis. But the paper announcing the discovery listed only 33 authors – all physicists. The scanner who discovered it was not acknowledged in person – the only mention of the scanners being in a paragraph at the end, which acknowledged "the excellent co-operation of the staff of the AGS and the untiring efforts of the 80-in. bubble chamber and scanning and programming staffs" [Barnes *et al.* (1964)]

[19] A version of this article was first published in *Viewpoint: magazine of the British Society for the History of Science*, in October 2015 (issue 108)

The rise of "big science" after World War II led to the construction of bigger and bigger machines capable of probing to smaller and smaller scales inside the atom. These machines not only required a huge contingent of staff to run them, but also produced an enormous amount of data – too much for individual physicists to handle. So laboratories all over the world began to engage teams of scanners – people without qualifications in physics, whose job would involve simply looking at bubble chamber tracks and comparing them with expected decay patterns.

This was just one aspect of a development that pushed the individual physicist to the sidelines as far as actual involvement in the experiment was concerned. Physicists had had to defer to engineers in the design, manufacture and operation of particle accelerators and detectors; with the advent of the scanners, they lost control of the raw data – the photographs – as well. Peter Galison describes this phenomenon in *Image and Logic*, his classic study of particle physics in the second half of the 20th century: *"The move from cosmic-ray physics … to accelerator-based experimentation was a shock to the physicists' self-image … as the accelerators grew in size, the experimenter became progressively more distant from the machines, no longer wielding direct control"* [Galison p317].

In the *Horizon* film, it is noticeable that, even during his brief reference to the process of examining the photographs, Samios avoids mentioning the scanners themselves; he says only that the photographs "can be scanned". His use of the passive voice betrays a reluctance to acknowledge the contribution of any other "trades" towards the discovery of the elusive particle; others, as Galison points out, marked out their territory by making artificial distinctions between synonyms, referring to "events *found* by a particular observer, reserving *discoveries* for the physicists themselves" [*ibid*. p200].

The scanners were almost all women, and most of them stayed in the field only a few years before going on to their own careers. They were rarely mentioned in research papers, and almost never by name, their anonymity reinforced by the custom – dominant until fairly recently – of women adopting their husbands' surnames on marriage. They are effectively "invisible" to the historian of science.

**The Invisible Technician**

The absence of "ancillary" staff from scientific literature was highlighted in a classic 1989 paper by Steven Shapin entitled *The Invisible Technician*. Shapin's subject-matter is the work of Robert Boyle and others in late-17th-century work on the air pump; he tells us that Boyle was assisted by a

number of *"laborants, operators, artificers, and servants" who "did different things in making Boyle's science"* but *"had one characteristic very much in common: they were largely invisible .... In the whole of Boyle's published work and correspondence, there are no more than a handful of named references to them and their roles"* [Shapin p556]. And this was a timely observation to make, because "invisibility" was still a common feature of science 300 years later: one example of a 20th-century "invisible technician" was highlighted in a talk given to the History of Physics Group in 2012 in which Geoffrey Constable attempted to reconstruct the lost story of his late father Jack's involvement in James Chadwick's discovery of the neutron in 1932. Jack was a PhD student at the time, and Geoffrey became convinced that his father had actually built the apparatus used by Chadwick; but this fact was not mentioned in the paper published to announce the discovery [see Constable (2012)].

In Bristol in the 1940s, Cecil Powell had used scanners to examine emulsions exposed to cosmic rays in the upper atmosphere, and any discoveries made by his team were credited to the scanners who found them; but the names gradually disappeared from later papers. As Galison puts it, *"with time, the removal of the women from the discovery process became routine"* [Galison p199].

What was life like, then, for a bubble-chamber scanner in the 1960s? A few years ago, this elusive closed chapter in the history of physics was opened up to me by a lucky accident.

## The UCL Scanners Project: Setting the Scene

In 2012 I commenced a short web-based oral history project on the early history of particle physics research at University College London. At the time, apart from a rather dry and sketchy departmental history, the only evidence of a bygone era was a photograph that hung in the High Energy Physics Group's common room. This featured members of the Bubble Chamber Group, and it was clear (from the fashions on display) that it was taken in the early 1970s. The striking thing about it, though, was that it was nearly 50% female – a gender ratio to die for in these supposedly enlightened times. Of course, on enquiry, I discovered that most of the women in the picture were scanners. But nobody knew what had happened to them, and even the names of some of them had been lost, so it was clear that they were not going to be able to feature in the history that their efforts had contributed to.

The physicists, on the other hand, were much easier to track down, and in fact some of them were still visiting the department regularly. Interviews took place both at UCL and, in the case of those who were less mobile, or lived a long way away, at their homes. During one of these visits to the home of a retired physicist, it transpired that his wife had also worked in the department, as a scanner. She was duly interviewed, and incorporated into the study; furthermore, she was able to put me in touch directly with another former scanner, and indirectly with another two. Access to these primary sources enabled me to decouple the scanners from the rest of the story, and use the material obtained to create a separate project, which eventually led to a short article for *Viewpoint* (the magazine of the British Society for the History of Science), to a talk at the History of Physics Group's "Physics at London Universities" meeting on 24 April 2019, and ultimately to this article.

**The Scanners' Story**

In 1961 an advertisement was placed in the London *Evening Standard* announcing that UCL was seeking to appoint a number of bubble chamber scanners. The advert did not spell out the exact duties involved in too much detail, and 20-year-old Jill Flewitt, who spotted it, was intrigued, but also slightly worried, by the reference to "atomic physics", the category to which particle research was assigned at the time. No physics knowledge was required for the job, and indeed, Jill had none; to her, anything "atomic" was scarily redolent of the atomic bomb. Nevertheless, she applied, and was reassured by the interviewer, Russell Stannard, that this was pure research with no "defence" connotations. She had previously studied towards an art degree which involved attending at least two different institutions; she described the course as "a total mess", and gave up after a year and a half. She came from a medical family, which included a physiotherapist, an osteopath, radiologists and dentists, from whom she had absorbed some basic science; the only qualification required of the job was mathematics "O" level, which she had.

Jill joined 19-year-old Pat Procter, who had also done part of a degree course (in her case, physics and maths) but abandoned it after a year. They were the first two scanners; eventually four more arrived. They were all women. Although in those days it would have been considered permissible to advertise specifically for women, this doesn't seem to have been the case; however, given the very low pay on offer, in practice it was only women who applied. Indeed, scanners were often referred to as scanning *girls*.

There may have been other factors involved in their selection though; Galison tells us that *"for decades it had been a common European and American assumption about women's nature that they were specially suited to such "meticulous", "tedious" and "exacting" work"* [Galison p199].

The UCL scanners I spoke to do not appear to have been part of any particular experiment, but they certainly worked on film from a bubble chamber at CERN, looking for evidence of various "strange" particles such as the $\Lambda^0$; being electrically neutral, these particles did not produce tracks, but their decay produced a tell-tale "V" shape made by the decay products, a proton and a pion. The scanner sat at a special table, onto which the bubble chamber film was projected from above. If she found an interesting event, she had to make a print of it, presumably for portability.

Eventually the scanners were also asked to work on the measuring machine. This involved tracing the path of a particle and inputting its trajectory to a computer; it was not popular with the scanners. But on the whole, they had little to complain about in terms of the work environment – if there were no photographs to scan they could play table tennis, squash or badminton; and at weekends there were parties, and sometimes various leisure activities were organised, including a boat trip. Some weekends, Pat got to travel to Darmstadt in Germany to use the computer there to process data – in what probably seems a bizarre inversion to today's 20-year-olds, the data had to be physically transported to a remote computer, rather than sent into the ether as it would be nowadays.

Left: Jill Flewitt at the scanning table. Right: Pat Procter examining a print

What they had more grounds to complain about was the money. Jill in particular was struggling; she and another scanner, Elizabeth Boardman, went to see Russell Stannard and negotiated a small rise; but it was still not really a viable living wage.

In any case, something else was bothering Jill. She felt that she “wasn’t up to” the measuring: “I felt someone should have been watching over me all the time – I was saying things were true that weren’t true. Probably messing things up for the rest of the team. It was very easy to say something was so when it wasn’t. I didn’t like working on it for that reason.” She felt that someone should have been checking her work. Eventually she left, before the others, and somewhat disillusioned. She remembers “a very relaxed attitude towards everything. In a way I liked it but at the same time I thought, 'this can’t be right' ”. Her uneasiness toward the work bred in her a cynicism which persists to this day, as she wonders whether public money invested in scientific research is really being spent wisely.

The tragedy here is that Jill’s concerns were largely based on a misunderstanding. When I mentioned her comments to one of the physicists who was working at UCL at the same time, he replied that the measurements didn’t have to be super-accurate, and pointed out that the computer produced a “best fit” trajectory – “but she wouldn’t have known that”.

Why wouldn’t she have known? Well, it’s possible that the scanners were being deliberately kept in the dark. Physicist Walter Barkas is quoted by Galison as saying that “it is important that the scanner be unaware of the result that is expected in a measurement or observation … it is very human to try to obtain the answer that pleases” [Galison p200]. Clearly they had to be told roughly what the “answer” would look like, in the form of a typical track pattern, but it’s possible that they were given only what was considered a minimum of background information. There was a “work-in-progress” feel to the scanning room, with various kinds of machines being tried out, and bits stuck on with Sellotape; Jill even mused that maybe the scanners themselves were “just their guinea pigs …” And it’s also possible that they, as women, were viewed with a certain amount of prejudice by the male physicists, who may have subscribed to the oft-quoted view that women are “less objective” than men, and hence might see no strict dividing line between what they actually saw and what they felt they were expected to see, and thus were more prone than men to the danger of providing “the answer that pleases” [see *e.g.* Fox Keller (1978)].

**Gargamelle**

If those early scanners were guinea pigs, one must ask whether the experiment was successful: were lessons learnt and corrected for in future experiments, on the basis of their comments? To find out, we fast-forward 10 years to another era in UCL particle physics: the Gargamelle experiment at CERN. This was an international collaboration, made up of groups based at LAL (Orsay), Ecole Polytechnique (Paris), UCL, Aachen, Brussels, Milan, and CERN itself. The experiment featured a large bubble chamber into which a neutrino beam was directed. The object was to look for weak neutral currents, which had been predicted by theory. It was a high-profile experiment, which, if successful, might have merited a Nobel Prize. (In the event, the untimely death of the principal investigator sadly ruled this out).

The Gargamelle bubble chamber was cylindrical in shape, 4.8 metres long and 1.9 metres in diameter. It had 8 cameras arranged in pairs, with each pair giving a stereoscopic view of a section of the cylinder, and a 2 tesla magnetic field. A neutrino beam was directed along the axis. It was a *heavy liquid* chamber, containing 18 tons (12 $m^3$) of freon, $CF_3Br$. This offered much better gamma ray and muon detection than, say, a liquid hydrogen chamber, and its greater target mass meant more neutrino events and hence more chance to spot the elusive neutral currents – and of course that meant more work for the scanners too. By now, all the scanners from the early 1960s had moved on; a new team was put together, working round the clock in shifts, and using new scanning machines. As well as preparing punch cards for the computer, the scanner had to trace events onto paper. Each photograph was scanned at least twice by different people, and there was a supervisor to check the work, and look for inconsistencies and evidence of "trying to obtain the answer that pleases". It does appear, then, that lessons were indeed learned, and the experiences of the 1960s "guinea pigs" were taken into account.

In January 1973, some evidence of neutral currents was seen by the collaboration. Later that year, a competing team in the USA, the Harvard-Pennsylvania-Wisconsin-Fermilab (HPWF) collaboration, announced that it had seen neutral currents, and so the Gargamelle group published its own results; then in November, HPWF withdrew its claim, leaving the field clear for Gargamelle. But the on-off HPWF claim (dubbed the "alternating neutral current") caused confusion in the community, and Gargamelle's own results were not immediately accepted by all. By the time a consensus was achieved, the initiator of the project, André Lagarrigue, had died of a heart attack, and so the Nobel prize that he and the collaboration surely

deserved for this work was never awarded, but nevertheless the result was recognised as a milestone in experimental particle physics.

**Invisibility revisited**

Did the scanners, then, get due recognition for the part they played in this? The crucial "single-electron" event had, after all, been first spotted by a scanner in Aachen. But the paper announcing the discovery listed only 55 names – all physicists. No scanners were mentioned, and there was not even a blanket acknowledgement like the one included in the $\Omega^-$ paper [see Hasert et al. (1973)].

This is not, on the face of it, an example of sexism. After all, there were *some* male scanners; and besides, the engineers, technicians and programmers (probably mostly male) were similarly sidelined by the Brookhaven paper, and appear to have been completely ignored by the Gargamelle announcement: although the author list does not specify professional status, the numbers involved do not seem compatible with full visibility of the "massive teamwork" referred to by Galison [Galison p 318].

Nevertheless, overall it does seem that women have been ignored more than men. One thinks of examples from history such as Caroline Herschel, Fanny Mendelssohn, and of course the "Harvard Computers" – a group of women who were not allowed to be astronomers but worked with Edward Pickering at the Harvard Observatory, laboriously analysing photographs – work that resulted in regularities such as the period-luminosity law for Cepheid variables (Henrietta Leavitt) and the Harvard stellar classification system (Annie Jump Cannon). These and others have been granted retrospective acknowledgement; but there is still much work to do in identifying others who have so far escaped attention. A good example of such work was revealed to me at the meeting where I delivered my talk on the scanners, by a member of the audience, Dr Jessica Wade. She referred me to a paper by a team of students from two US universities who had carried out research on female programmers who contributed to genetics papers but were not recognised as authors, being instead "buried in footnotes" [Dung et al. (2019)]

**The Fading of the Image**

Peter Galison's title sums up the main subject of his book – the existence of two separate experimental "traditions" in 20$^{th}$ century particle physics, based around "image" (nuclear emulsions, and cloud and bubble chamber photographs) and "logic" (counter technology, featuring the direct detection of particles by electronic devices). These two existed side-by-side for many

years, but ultimately the "logic" community triumphed, and the scanners were no longer needed, as all the necessary information could be obtained automatically by machines. Nowadays electronic particle detectors dominate; they still can, and do, produce pictures, but these are a by-product, rather than being an essential part of the chain, as was the case with cloud and bubble chambers.

Particle physics has entered the age of the giant collaboration, whose papers list hundreds of names. These are not just the names of physicists; technicians, programmers, and many other "trades" also get a mention. If the scanners were still with us, it is clear that they would not be left out – but for Jill, Pat and their colleagues, it is too late for official recognition. Hopefully this article will help to redress the balance.

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# Henry Tizard 1885-1959

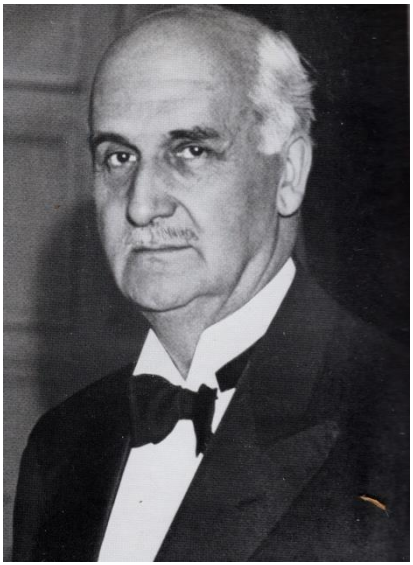
***Andrew Whitaker, Queens University Belfast***

Henry Tizard

Henry Tizard was not a great physicist though he was an extremely good one. However his other abilities and achievements make a study of his life and work interesting and exceptionally worthwhile. Today he is mostly remembered for two things. The first is his intense feud with Frederick Lindemann – if we were to ask why we should be concerned with an argument of 80 years ago, an answer might be that it could just possibly have affected the outcome of the Second World War. The second is the Tizard mission to the USA in 1940.

Tizard came from a moderately well-off family but needed scholarships to study at Westminster School and then Magdalen College Oxford, where he studied under the young Neville Sidgwick whose career was to flourish and who will make rather a surprise appearance much later in this story. His performance was of such a high standard that Sidgwick recommended him to Walter Nernst and Tizard spent the year 1908-9 working under him in Berlin.

Nernst was exceptionally well-known for his work on thermodynamics, in particular the Nernst Heat Theorem, which led to the Third Law of Thermodynamics, and for which Nernst was to be awarded the Nobel Prize for Chemistry in 1920. However, he put Tizard on to the task of condensing acetylene to benzene, which Tizard considered 'hopeless'. He did feel that he learned a lot in this year, but his only real success (highly surprising as it may seem in light of the opening paragraph) was the close friendship he built up at the time with Lindemann (below).



In contrast to Tizard, Lindemannn's family was exceptionally well-off, his father's annual income being around perhaps £2 million in today's terms. The German name was from a generation back, and perhaps to compensate, the family were all extremely strongly British. Lindemann was an excellent physicist and worked with Nernst in Berlin for a number of years, obtaining his PhD in 1910. Much of this work was important, checking Nernst's ideas as the temperature moved towards absolute zero. It also included study of early quantum ideas, in particular Einstein's theory on the decrease in specific heats of solids at low temperatures.

During Tizard's year in Berlin, he and Lindemann became great friends. They addressed each other as H.T. and Lindy, they played tennis together, and they corresponded when they were apart. It was much more than just taking advantage of each other's company – as late as 1916 Lindemann was godfather to Tizard's first son; in 1919 when Lindemann applied for and gained the Chair of Experimental Physics [strictly Experimental Philosophy] at Oxford, Tizard was back in Oxford and gave him great internal support; and when Tizard was in London during this period he dined with the Lindemann family.

When Tizard returned to England, he worked at Oxford and also at the Davy Faraday Laboratory at the Royal Institution, producing a number of excellent papers on colour charge indicators. His abilities were recognised by his being elected to a Fellowship at Oriel College Oxford, where he

demonstrated in the Electrical Laboratory. However it seems that even at this time he felt that his life was 'too cosy' and decided that fairly soon he would move on.

In the summer of 1914, together with a number of other scientists, Tizard travelled to Australia for a meeting of the British Association, and during the journey he became particularly close to Ernest Rutherford, who for the rest of his life was his standard of a great physicist. On the declaration of war, of course, all had to rush home, and, after a brief period training soldiers in the army, Tizard joined the Royal Flying Corps at the Central Flying School at Upavon Wiltshire to undertake experimental work.

However he soon became unconvinced by many of the beliefs and myths about aeronautics put about by actual flyers, and he determined to learn to fly himself, though he was only given permission to take to the air in weather that was 'too rough for cadets'. He then qualified to fly after 3½ hours dual instruction and two hours solo, and after that acted as his own test pilot, determining the performance of aircraft and their use in reconnaissance and fighting. He analysed the use of aerial photography and radio, and also studied the development of cloud flying. He stimulated the development of better equipment to measure speed, rate of climb, height and fuel consumption, but he was frustrated by the failure of the War Office to understand not only science but even engineering.

In 1915, Tizard met Bertram Hopkinson, formerly Professor of Mechanical Sciences at Cambridge, now with the Department of Military Aeronautics at the War Office, with responsibility for the design of bombs, guns and ammunition. Tizard was hugely impressed with his way of command – good sense, stimulation of research and sensible handling of staff, and throughout his time in administration, he used Hopkinson's example as a model. In October 1917, Tizard moved to London as Hopkinson's deputy, but in August 1918 Hopkinson, who had also learned to fly, was descending through clouds, lost control and allowed the aircraft to go into a spin, from which he could not recover. He was killed. Tizard took over Hopkinson's position, but was demobilised in spring 1919 as a lieutenant-colonel, a rank which he certainly did not feel measured up to his abilities and duties.

Even during the war, he had become concerned about submarine attacks on tankers bringing in the all-important petroleum fuels. At this time it seemed that Pennsylvania oil, which consisted solely of aliphatic hydrocarbons, was particularly suitable for aircraft engines. To see if a more convenient alternative was possible, Tizard arranged for tests to be made on a mixture of aircraft petrol and benzol, which was essentially a waste product from the

gasworks consisting mostly of benzene and toluene, aromatic hydrocarbons. At low enough heights it worked even better than the standard aircraft petrol, but unfortunately the benzene froze at low temperatures. However there was a fair supply of toluene from Burma, and although most of this was dedicated to the manufacture of explosives, sufficient could be spared for aircraft engines.

Tizard returned to Oxford, to his Fellowship at Oriel, to his lectures on physical chemistry, which were judged successful, and to his research on chemical indicators. Overall his standing improved so much that he was made Reader in Chemical Thermodynamics in February 1920. However he had never lost his interest in the composition of engine fuels and became involved in a major piece of research, which had actually been planned even during the war, on the influence of the chemical composition of the liquid fuel on the thermodynamics of the spark-injection engine.

This research was carried out using the financial support of Sir Robert Waley-Cohen of Shell, and under the general leadership of Harry Ricardo, who was one of the foremost engine designers and researchers in the early years of the development of the internal combustion engine. He had followed up studies in engineering at Cambridge with some research into engine performance with Hopkinson, and in 1915 he started up his company 'Engine Patents Limited'. Following essential design work by Ricardo this company was to produce many thousand tanks during the war, and his company, now known as Ricardo plc, is still in existence.

Also working with Tizard was David Randall Pye, before the war a Lecturer in Engineering at Oxford University, and later in succession Lecturer at Cambridge, Director of Scientific Research at the War Office during the Second World War and then Provost of University College London. Incidentally all three of Ricardo, Tizard and Pye would be knighted.

The terms that Ricardo had obtained from Shell were that they should investigate all the properties of a fuel that could influence the performance of a spark-ignition engine, detonation being only one, and to investigate their relative importance. It was Tizard who insisted that the results should be published, and Shell agreed, with the stipulation that no publication was made for 18 months after Shell received their report.
At the outset of the project, Ricardo said that it had long been his ambition to have a really versatile research engine in which, for example, the rate of compression could be varied while the engine was running and so without disturbing the temperature or other physical conditions. It was Tizard who suggested also designing and building another unit with the same variable-

compression cylinder and mechanism, but in which the piston made only one stroke and was then locked dead-centre. In this way and by finding the critical rate of compression for each fuel, they could study the unstable conditions when the engine was on the brink of spontaneous emission.

At the start of the project, Tizard and Pye, working at Oxford, produced an enormous volume on the physical characteristics of all the light hydrocarbon fuels and also other possible volatile liquid fuels, including alcohol, acetone, ether and carbon disulphide. They used all the known data and also performed their own research to produce values for: the specific heats and modes of dissociation of gases before and after combustion at temperatures up to $2500^0$C; and also the total energy per cubic inch of combustible mixture, latent heat of evaporation, change in specific volume before and after combustion, temperature coefficients and so on, and an analysis of how they might affect the performance of the engine.

Ricardo and Shell built the necessary laboratory at Shoreham in Sussex, where Ricardo plc is still based, and also the engines, while Shell provided the fuels, and then an enormous amount of research work was carried out, mainly by Tizard and Pye. Their most important finding was that the most significant single factor limiting the performance of a petrol engine, as regards pinking or knocking, was the incidence of detonation.

Tizard came up with the idea of the 'toluene number'. With toluene the fuel least likely to detonate and eta-heptane the most prone, the toluene number of a fuel is the proportion of toluene that needed to be added to eta-heptane to match the fuel under test. Under American influence this has been renamed 'octane number', and it may be said that this is perhaps a third thing for which the name of Tizard is quite well-known today.

Tizard's general summary may be stated as follows:

1. At a given compression ratio, the nature of the liquid fuel did not materially influence the maximum power developed. The best fuel was the one that showed the least tendency to knock.
2. The efficiency of the engine, measured by the specific fuel consumption, was highest with a weak mixture.
3. The maximum power developed, at a fixed compression ratio, was constant over a wide range of mixtures. This was explained by the dissociation of carbon dioxide at high temperatures.

Tizard added that, but for the dissociation of carbon dioxide, it would have been very difficult, if not impossible, to design a practical multi-cylinder petrol engine.

This was obviously extremely important work, both scientifically and technologically, and Tizard was to be elected a Fellow of the Royal Society in 1926. (Ricardo and Pye were also to become FRS.) Tizard was also to be elected an Honorary Fellow of the Royal Aeronautical Society, and became CB (Commander of the Bath) in 1927. (He was later promoted to KCB and then GCB – Knight Commander and Grand Cross.)

We may now briefly catch up with Lindemann. He too had been involved in flying during the war, and had gained a certain notoriety for first working out theoretically how to extricate a plane from a spin, and then learning how to fly in order that he could demonstrate that the method worked. Then, as said, in 1919 he was elected Professor of Experimental Philosophy at Oxford with the support of Tizard. Tizard would never deny Lindemann's ability – 'He was one of the cleverest men I ever met,' he said 'as clever as Rutherford.'

Physics at Oxford was in an appallingly bad state. The first Professor had been appointed in 1860, and Robert Bellamy Clifton was in post from 1865 to 1915. The Clarendon Laboratory had been built in 1872, but, under Clifton, even such facilities as gas and electricity were practically non-existent, and unsurprisingly Clifton had no record of research at all. In the years coming up to the First World War it had been assumed that Henry Moseley, whose work establishing the significance of atomic number had been hugely admired, would become the next Professor, just as it was assumed that he would be awarded the Nobel Prize for Physics within a year or so, but, as is well-known, Moseley was killed in 1915 at the Battle of Gallipoli and this led to Lindemann's opportunity.

Lindemann's task was clearly to bring Oxford physics up to date and make it comparable with that of Cambridge. It must be said that he achieved this very well. He attracted a number of extremely good physicists to Oxford. Thomas Merton, an excellent spectroscopist, became Treasurer of the Royal Society and was knighted. Gordon Dobson, an expert in meteorology, also became an FRS. Alfred Egerton, who worked in combustion, became Physical Secretary of the Royal Society and was also knighted, while Derek Jackson, who succeeded Merton in spectroscopy, was yet another FRS.

During the 1930s, ICI enabled him to bring a number of excellent Jewish physicists out of Germany and to Oxford. The most important was Franz (later Sir Francis) Simon, who took Nernst's work on the Third Law of Thermodynamics further and himself came close to a Nobel Prize. Also involved in low temperature physics were Nicholas Kurti and Kurt

Mendelssohn, while Heinrich Kuhn worked in optics; all three were to become Fellows of the Royal Society.

Unfortunately Lindemann himself did little actual physics. It seemed that if he could not compete at the highest level – and, for him, that meant being as well respected as, say, Heisenberg, he was not willing to enter the race. Instead he spent a lot of time in 'high society' and became regarded as something of a 'society pet'. In particular, for better or worse, probably some of each, he became very close to Winston Churchill.

Meanwhile in June 1920 Tizard had left Oxford and joined the Department of Scientific and Industrial Research (DSIR – a relatively new Department) as Assistant Secretary. There were a number of reasons for this move. He had never liked Oxford's damp climate, and he would earn considerably more money in his new job – for much of his life he was concerned about his health and worried about money. But also, not unlike Lindemann, he felt that he would never be outstanding as a pure scientist. In his new job he would be successful – he would be promoted to Principal Assistant Secretary in 1922 and to Permanent Secretary in 1927.

He did, of course, have distinct views on how science should interact with government. During the war a range of decisions had been made but, not surprisingly, little had been achieved, and then in 1920 four civilian boards had been set up in Chemistry, Physics, Engineering and Radio. Tizard thought it ridiculous that committees in civilian departments should organize work for the fighting services. At that time, in fact, only one service department – the Admiralty – had a Director of Scientific Research, and that was very recent. Tizard believed that each service should have a Director of Scientific Research, who should exert an influence over the whole service, making sure that Research and Experimental Establishments were aware of the actual needs of the service, and that their efforts were directed to things that actually mattered. His general message was that the country was failing to make use of the knowledge that its own research was providing. He argued that we had men of science with power and insight but they lacked the appropriate influence.
During this period Tizard kept a close eye on the Radio Research Station at Slough, whose Director was Robert Watson-Watt. We shall meet much more about his work later! Particular successes were the establishment of the Chemical Research Laboratory in Teddington and the appointment of Harry Wimperis as Director of Scientific Research to the Air Force.

Before leaving Oxford, Tizard had been invited by the Secretary of State for Air to join the Aeronautical Research Committee. He would be a member

practically continuously for 22 years, and from 1933 he was Chairman. This would be one of his most important services to the country and to the war effort.

Then in 1929, Tizard made another fairly dramatic change of career. He resigned from the DSIR and became Rector of the Imperial College of Science and Technology. He was to stay until 1942 but from about 1935 he was also doing an enormous amount of war work. Tizard was convinced that Imperial College should be the first of a range of technological universities and that the future prosperity of the country would depend very largely on them. Thus he was constantly looking ahead and so was highly influential in shaping the College for future decades. Not surprisingly he gave particular attention to the Departments of Aeronautics and of Chemical Engineering and Applied Chemistry. He recognised the need for expansion and began negotiations to take over the Exhibition Road premises of the Royal School of Needlework, and he also purchased land at Harlington for a sports ground and helped to secure Silwood Park to act as the College's field station. Most importantly when the government was attempting to use the region around Imperial for its own purposes, Tizard retained the College's hold on its central area, and it is this that has allowed the creation of today's South Kensington campus.

Even in his busiest years, after a full day or even more of war work, he would arrive at the College at midnight, rouse the College Secretary from his bed and work together for several hours. Far from objecting to this, the Secretary expressed his opinion that Tizard was absolutely wonderful and that his decisions were 'almost without exception proved absolutely right.' He added that Tizard was 'probably the first Rector who combined with intellectual ability and great personal charm such a high degree of organizing and administrative capacity, with a knack for quick understanding and a willingness to enter into detail.'

A slightly different perspective on these years is that 'Outspoken and warm with a caustic wit, he is remembered for spending nights at the College during the war, playing snooker and billiards and chatting to members of the staff.'

We now turn to preparations for air defence in the 1930s, and it is certainly massively to their credit that Lindemann and Churchill applied systematic pressure demanding that action should be taken. The standard view was probably that of Stanley Baldwin in 1933 that 'the bomber will always get through'.

In fact two committees were set up. The first, conceived by Wimperis from the Air Ministry and chaired by Tizard, was the Committee for the Scientific Survey of Air Defence, always called the Tizard Committee. Other members included A.V. Hill and Patrick Blackett, both highly eminent scientists, Hill having already been awarded a Nobel Prize and Blackett being similarly honoured after the Second World War. This committee met for the first time on January 28th 1935.

The second committee was the Air Defence Research Sub-Committee of the Committee of Imperial Defence, which met for the first time on 10th April 1935. This committee had been formed under the pressure exerted by Churchill and Lindemann, and Tizard was a member. As it was to turn out, this committee was to make political and military decisions to implement the results of research and development, while the Tizard committee became concerned with the research itself.

Lindemann had to be persuaded to become a member of the Tizard Committee, as he felt it was stuck at departmental level and would become a talking shop, moving exceptionally slowly and without the right to create political input. The other members felt that his ideas were often wild, in particular his plan of dropping bombs hanging on wires suspended from parachutes in the paths of attacking aircraft. They also found him arrogant, rude and objectionable. His bouts with Tizard became legendary. There was at least one occasion when the secretaries had to be removed from the room as proceedings became so exceptionally heated. Also he felt he had the right to report any lack of progress to Churchill, who would raise the matter at the Air Defence Research Sub-Committee, of which he and Tizard were both members. One such incident led to a vituperative exchange of mail between Tizard and Lindemann.

This really could not go on. In July, Hill and Blackett resigned from the Tizard Committee. This led to the Air Ministry closing the Committee, which they then re-formed without Lindemann. For the moment it seemed that Tizard had won, though obviously the deeper significance of these events was not personal.

Meanwhile in January 1935, Robert Watson-Watt, Superintendent of the Radio Division of the National Physical Laboratory, suggested that if a beam were reflected by an aircraft, enough energy might be reflected to be detected by a radio receiver on the ground. Such was the priority this idea was given that there was a practical demonstration as early as 26th February.

One might ask why the development of radar (as, of course, it was to become known) was far more successful in the UK than in other countries. An obvious answer is that the UK realised that they needed it more and more quickly than other countries. But there was clearly a massive amount to be done. There had to be a build-up of faith in the actual employment of radar, the intent to develop the entire system, the skill in getting the government to pay the very large sums that would be needed, the encouragement of the RAF to embrace the concept and the ability to work with them on the actual use of it.

During 1938 and 1939, British scientists based on the Suffolk coast at Bawdsey working under Watson-Watt (pictured right) designed and built a line of radar stations along the south and east coasts of Britain. Each station consisted of a set of giant towers that sent radar pulses out to sea. At the base of the towers was a receiver hut where signals from incoming German planes were analysed. Radio communications between stations were essential in order that information could be correlated. While Watson-Watt and his colleagues deserve the greatest credit for all this preparation, just as much credit should go to Tizard and his own colleagues for first convincing the pilots of the RAF to work with the information provided by the system and then to train them in the use of it.

Tizard was later to say that: ‘When I went to Washington in 1940 I found that radar had been invented independently in America about the same time as it had been invented in England [and indeed as it had been in Germany]. We were, however, a very long way ahead in its practical application to war. The reason was that scientists and serving officers had combined before the war to study its tactical uses.’

One might ask what would have happened if Lindemann had been in charge. It would certainly be unfair to suggest that he was ‘against’ radar, but it was certainly not as high on his agenda as other schemes, while, as has been stressed, it was essential that it should be absolutely top priority. Also it is definite that he would never have had the personal skills to work with all the necessary groups to ensure that the scheme would be operational when it was urgently needed.

Tizard was also centrally involved in other crucial developments made in this period. Operations research grew from the partnership with the RAF over radar and spread to all forms of military activity. In a different field, Tizard persuaded the intelligence community to accept a scientist working with it and this was the beginning of scientific intelligence.

It may also be mentioned that Tizard and Lindemann were broadly at one over the possibility of building a nuclear weapon. Both were interested and, of course, thought that the idea should be followed up, but actually rather suspicious of the possibility of developing such a weapon.

Churchill became First Lord of the Admiralty in 1939 and brought in Lindemann as his adviser on scientific and economic matters. This was a personal blow to Tizard, who accepted an appointment as Scientific Adviser to the Chief of Air Staff, but his position became even more difficult in May 1940 when Churchill became Prime Minister. He thought of resigning but almost immediately an enormous opportunity was presented, the so-called Tizard mission. One might say it showed that Lindemann might be vindictive but Churchill was not.

The British Ambassador in Washington had suggested an exchange between the UK and USA of scientific information and service experience. A.V. Hill went out as an attaché in May 1940 but he found he had insufficient mandate to release information. As a result Tizard was chosen to head a mission, the terms of which were: ‘To tell them what they want to know, to give all assistance I can on behalf of the British Government to enable the armed forces of the USA to reach the highest level of technical efficiency.’ These terms were chosen by Tizard, and only after a struggle agreed by Churchill. It must be remembered that Pearl Harbour was well over a year away and many Americans were deeply antagonistic towards joining the war. The Tizard mission was a statement that Britain was staying in the war and had much to offer scientifically, technologically and in practical experience. It was an act of faith and courage, particularly on Tizard’s part.

Other members of the mission included the well-known physicists, John Cockcroft and R.H. Fowler, and ‘Taffy’ Bowen, the radar expert. Among the secrets was the cavity magnetron, much later described by James Baxter, an official American historian, as ‘the most valuable cargo ever brought to our shores’. Others included the proximity fuse, details of the jet engine, the Frisch-Peierls memorandum on the atomic bomb and designs for rockets, superchargers, gyroscopic gunsights, submarine detectors and plastic explosives.

Initially there was caution, particularly on the American side. They described their own microwave research and it was the British who realised that Bell Labs and General Electric could contribute a lot. However the Americans were bowled over by the cavity magnetron. This was a device that would allow the production of radar devices small enough to be incorporated into night fighters, allowing aircraft to locate surfaced U-boats and providing enormous navigational assistance to bombers. After that the meeting proceeded highly positively, the Americans showing great interest in many of the British devices.

The delegation also visited Enrico Fermi in Columbia and, in a trip to Canada, George Laurence in Ottawa. Both were working on production of nuclear energy and were surprised by the contents of the Frisch-Peierls memorandum. The encounter with Fermi was to play a part in the very much later US-UK collaboration on the making of the atomic bomb.

As a result of the mission, Bell Labs were given the task of making cavity magnetrons and by the end of the war had made over a million. The MIT Radiation Laboratory was set up for research and development in this general area and at its peak was employing over 4000 people. The proximity fuse enabled precise anti-aircraft fire, dramatically reducing the threat posed by Japanese planes to allied ships in the Pacific. Also, although Tizard was coy about the details of the jet engine, the Americans soon realised that it was far in advance of their own work and General Electric were to produce it in great numbers.

All this was positive as it enabled the two nations to work together highly fruitfully even before the USA were in the war and then through the allied campaign. However it is only fair to recognise the negatives. Certainly the UK was in a desperate position and the Tizard mission was essential, but the mission had to release technology that would have had enormous commercial impact after the war and despite the collaboration over the atomic bomb during the war, in 1945 the UK was frozen out of nuclear development and had to build their own bomb.

In October 1940 Tizard returned to England but, with Lindemann ever closer to Churchill, he played a gradually decreasing role in operational matters, though he was generous enough to remark that it was good that *any* scientist had an important position in deciding policy. Tizard moved to the Ministry of Aircraft Production, first being responsible for research and development, and later being given a roving commission, and he was a member of the Air Council.

In 1942 there arose one more occasion of conflict between Tizard and Lindemann. Churchill and Lindemann were in support of the policy of Arthur 'Bomber' Harris, Officer Commanding Bomber Command, for saturation bombing of German cities, which he claimed would lower morale and win the war. Tizard broadly disagreed, not fundamentally for moral reasons, but because he felt that the policy could not be carried out with the resources available to the RAF, and he was concerned that there would be insufficient aircraft for use against enemy ships, and as a result the UK might lose control the seas. Harris had his way, though the policy was not as successful as he had predicted, and since the war it has come under greater scrutiny, more of it now on moral arguments.

In 1942 Tizard, presumably disenchanted with his role in the war, gave up his MAP job to become President of Magdalen Oxford. The College was delighted to have obtained his services. They announced that they were making 'a timely and important break with tradition, for he is the first man of science to become head of a College here'. They recognised that his time would be limited during the war but looked forward to long service when peace came. Unfortunately this was not to be. During his time at the College, he carried out a few reforms – reorganisation of the bursary, revision of the relations between the College and its schools at Brackley and Oxford, and an increase in the number of undergraduates. However, after having considerable authority in many of his important jobs, he presumably found it difficult to work with a democratic body of Fellows, and he resigned in 1946.

Tizard was Foreign Secretary of the Royal Society from 1940 to 1945, and in this period, despite all his war work, he put considerable effort into the post-war organisation of international scientific collaboration. However he also became involved in a highly embarrassing election for President of the Society in 1945. At this time, Edward Andrade was a member of the Council of the Society and was concerned about two aspects of its governance and attitudes. The first was that essentially one Council selected the member of the next one, with no input from the other Fellows. The second was that he felt that the Society was too inward-looking and concerned merely with creating good science, and he wished it to become the spokesman for British science with government.

Andrade rated the requirements for President of the Royal Society (PRS) as being a good organiser, a good speaker, being able to speak for science in public life, very experienced in working with government, and being an excellent scientist. If the Fellows were asked what 'excellent scientist'

meant, the expected answer would be that the PRS should preferably have a Nobel Prize, but failing that at least a Copley Medal of the Society.

The two chief candidates for PRS at the time were G.I Taylor, an excellent mathematician and physicist with a Copley Medal, but by no means an extravert character, and Robert Robinson, who would be awarded the Nobel Prize for Chemistry in 1947, but who was described by one Member of Council as 'cantankerous'. Andrade's choice would undoubtedly have been Tizard, who was ideal for the first four criteria. It seems that some members of Council felt that his admittedly falling some way short on the fifth, scientific excellence, should rule him out, others not.

Yet here Andrade made a disastrous tactical error. Tizard would have been happy to 'emerge' as a front-runner. He was happy to end up as a unanimous or practically unanimous selection, but was determined not to become involved in a contested election. He begged Andrade not to use his name in his politicking but Andrade persisted in doing so, as a result of which Tizard stated he would not take the Presidency, and as a result of other arguments, Robinson did the same.

It looked like a shoe-in for Taylor, but practically as members of Council were on the way to vote (and it must be remembered that all those so far mentioned were members, making the situation somewhat incestuous), it was decided by some that it was bad to have only one candidate. Neville Sidgwick, Tizard's tutor from forty years before, was proposed, and at this Robinson took umbrage. While Sidgwick was certainly an excellent chemist, he declared that he himself was much better, and he reinstated his candidacy. Thus there were now three candidates, Taylor, Sidgwick and Robinson. The election was man against man: A v B, A v C and B v C, Robinson won both his bouts narrowly and he was elected PRS. There was then a final 'vote' for show which Robinson 'won' unanimously. From Andrade's point of view, reform of the Royal Society was put off for another five years.

While the series of events was not without its amusing side, the point should not be missed that Tizard was considered by many to be an excellent choice for PRS.

In any case it was now 1945. Lindemann, now Lord Cherwell, had returned to Oxford and Churchill had lost office. The way was clear for Tizard, and indeed he became Chief Science Adviser to the government, and also Chairman of two new committees – the Defence Research Policy Committee and the Advisory Committee on Scientific Policy. However one must suspect that this period gave him comparatively little satisfaction.

Compared to the practically single-issue days of the decade from 1935, there was a considerable rise in awkward bureaucracy; for example his committee on defence was not allowed to concern itself with nuclear weapons.

Tizard gave these organisations a good start, insisting that science should take its rightful place in government departments, and that its advice should be valued, but he was perhaps glad to retire in 1952. As well as his British knighthood he must have been proud to receive the American Medal for Merit in 1947.

Yet before his retirement there was one more interesting and amusing development. There had been a spate of UFO sightings and he was clear that they should not be dismissed without scrutiny, so the Flying Saucer Working Party or FSWP was set up. It contained representatives from numerous departments, and it was felt that it might also be necessary to consult the Meteorological Department and Fighter Command. Perhaps sadly it seems that no clear evidence for UFOs was presented.

Tizard was to die in 1959.

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# David Bohm

***Chris Dewdney***

***University of Portsmouth***

There is a unifying thread that weaves throughout David Bohm's life and that binds his intellectual output from physics to philosophy to global politics. That thread is his perception of the unbroken wholeness of all being, from the fundamental level in physics to the nature of existence itself. Throughout his life Bohm was deeply concerned with our structures of thought and our perception of the world. He saw as essential the dialectical relationship between the individual parts that arise from analysis through thought and the whole in which the parts participate. He argued that the modern tendency to analyse systems into parts, or fragments, themselves taken as independent wholes, that together completely determine a system's behaviour as a great limitation, not just within science but in all fields of human enquiry and understanding.

David Joseph Bohm (1917 - 1992), physicist and philosopher, was born on 20 December 1917 in Wilkes-Barre, Pennsylvania, USA, the son of Samuel Bohm, proprietor of a furniture store, and his wife, Freda. Both parents were immigrants from Europe. Bohm graduated High School in 1935 and then studied locally for his Bachelor's degree at Penn State University. In 1939 he moved to Caltech where he met J. R. Oppenheimer around 1941. But Bohm felt limited at Caltech, and Oppenheimer, who appeared more interested in general ideas than he had found possible at Caltech, invited Bohm to Berkeley where he worked from 1941 until 1947. Initially, with Oppenheimer as supervisor, Bohm worked on scattering problems (neutron-deutron and proton-proton) and completed his PhD thesis in 1943. His thesis had a bearing on the work at Los Alamos and it was immediately classified. General Groves refused to clear him for work on The Manhattan Project and in some ways this freed Bohm from extending the detailed calculations for which he had laid the ground work in his PhD thesis. Bohm comments in interview [1]:

“In the long run, I think it turned out for the best that I didn't go. I don't think it would have been very interesting to me, now that I know what they were doing there. I wouldn't have been very interested in it, anyway. I probably found it more interesting to stay where I was in Berkeley, although I had to go through about or year or two of nobody really to talk with. But

eventually [Harrie] Massey and [Eric] Burhop came from England and began to do plasma work, which probably was more interesting than anything I could have found to do out at Los Alamos."

At the Radiation Laboratory, the group including Massey and Burhop, worked on the separation of ionized uranium hexaflouride isotopes using a mass spectrometer. During this period Bohm also started work on the behaviour of ions produced in electric arcs when subjected to electromagnetic fields. Together with his first research student, Eugene Gross [2], he developed a theoretical ex-planation of the mechanism whereby coherent plasma oscillations arose and were sustained in ion beams. The formula for what has become known as the “Bohm diffusion" coefficient was derived in this work. The key conceptual advance in this work was the recognition that within plasmas, local average velocities and densities were not important, locally the plasma behaved almost completely like a perfect gas, rather it was the long range organisation that was significant for understanding the plasma. The apparently independent individual motions of the plasma's charges gave rise to, and was in turn directed by, the plasma oscillations as a whole. This type of interrelationship between the parts and the whole became a focus of Bohm's thought throughout his life.

In 1947, at the invitation of John Wheeler, Bohm moved to Princeton as an assistant professor. The big thing in physics at that time was renormalisation. Bohm attended the conferences at Fire Island and the Pocono Mountains when these ideas first emerged. J.R. Oppenheimer, who had become Head of the Advanced Study Institute, felt that Bohm should work on orthodox lines but Bohm was not motivated in that direction, he was “always trying to approach something more fundamental", and so the two met only occasionally at Princeton.

At this time, with his graduate student David Pines, Bohm extended his treatment of plasmas to the behaviour of conduction electrons in metals. They attempted to show that both plasma oscillations and electron screening also played a role in metals. A series of four papers with the collective title “A Collective Description of Electron Interactions" was published in Physical Review between 1950 and 1953 [4]. They developed a method for rewriting the Hamiltonian of the system in terms of “collective coordinates" thus focussing on collective motions rather than the usual individual particle description. Bohm and Pines initiated a new collective coordinate approach to many-body systems that nonetheless retained the success of the

independent particle theory. They showed that the density fluctuations of the electron gas could be split into two approximately independent components “associated, respectively, with the collective and individual particle aspects of the assembly". The collective component is present only for wavelengths above a certain threshold and represents organised oscillations brought about by the long range part of the Coulomb interaction. Each particle experiences a perturbation arising from the combined potential of all other particles, the perturbation is in phase with the potential producing it, is independent of position, and results in a small organised motion superimposed on the larger thermal motion of the particles. There is also an out of phase response which does depend on the position of the particle, but because of the random positions of the particles this averages out to zero. In this collective approximation the random phase approximation was an important factor. Feynman, in his Lectures on Physics states [3]: “It was first observed experimentally in 1936 that electrons with energies of a few hundred to a few thousand electron volts lost energy in jumps when scattered from or going through a thin metal foil. This effect was not understood until 1953 when Bohm and Pines showed that the observations could be explained in terms of the quantum excitations of the plasma oscillations in the metal."

As argued by Hughes [5], “The random phase approximation, for example, is arguably the most valuable contribution to the deductive practices of solid-state physics directly traceable to Bohm and Pines. .... The work of Bohm and Pines made an essential contribution to the theory of superconductivity published by Bardeen, Cooper and Schrieffer in 1957 and in 1958 Anderson published a paper which drew on the work, both of Bohm and Pines and also of Bardeen, Cooper and Schrieffer. The title was “Random-Phase Approximation in the Theory of Superconductivity."

Whilst teaching at Princeton Bohm, wrote up the notes from a series of graduate lectures he was giving, in the form of a textbook on Quantum Theory [6]. At this time Bohm was receptive to Bohr's approach to the interpretation of quantum theory and in the book he set out to give an account of Bohr's approach that was as clear as possible. His approach deliberately eschewed the more abstract postulational approach to quantum theory, preferring to make the physical meaning of the theory clearer. The book emphasises conceptual under-standing, and in fact the Schrödinger equation itself does appear until chapter nine. Bohm set out to show that quantum theory was based upon “comparatively qualitative and imaginative concepts, which are, however, of a totally different nature from those

appearing in the classical theory." He contrasts the classical notion of a mechanistic universe with what he sees as the quantum conception that “the world acts more like a single indivisible unit, in which even the intrinsic nature of each part depends on its relation to its surroundings." It is only at the microscopic level that such wholeness is significant. Bohm argues, at first sight somewhat surprisingly given what followed, that: “We cannot visualise simultaneously a particle as having a definite momentum and position. Quantum theory has shown that it is unnecessary to try, because such particles do not exist." In fact, “the concept of a continuous trajectory does not apply to the motion of real particles." (p148). In the book Bohr's notion of wholeness is emphasized: “At the quantum level of accuracy, an object does not have any “intrinsic" properties belonging to itself alone instead it shares all its properties mutually and indivisibly with the systems with which it interacts."

There is a prescient chapter (22) devoted to the quantum theory of the measurement process. Bohm argues that the indivisibility that characterises the quantum level would lead to paradoxes if applied to measurement processes. The paradox is avoided by emphasising Bohr's observation that “all real observations are, in their last stages, classically describable." Furthermore, the classical level is taken as given, not derivable from quantum mechanics. In section eight of the chapter, Bohm introduces what today has become known as decoherence theory. He shows that the physical processes involved in measurement-like interactions destroys, for all practical purposes, the possibility of any interference between the eigenfunctions of the operator representing the observable undergoing measurement. The collapse of the wave packet is simply a change in information that an observer acquires once an individual result is apprehended. The sudden replacement of the statistical ensemble of wave functions by a single wave function represents absolutely no change in the physical state. The change of the wave function has no physical significance; it is simply a matter of convenience for calculation simplicity. Bohm develops a completely objective description of the process of measurement, which does not involve human observers in any way at all. The book also has a discussion and reformulation of the argument for the incompleteness of quantum theory that was initially proposed in the form of a thought experiment by Einstein, Rosen and Podolsky (EPR). EPR presented the experiment in terms of position and momentum measurements, but Bohm reformulated it in terms of spin component measurements carried out on two spin one-half particles in a total spin zero (entangled) state. By mapping the spin to polarisation, Bohm's reformulation lead to real

laboratory experiments to test quantum predictions of entanglement. In his text book, Bohm also pro-duces arguments against the possibility that hidden variables may exist that could explain the quantum predictions. He emphasises that no experiment has shown the slightest trace of hidden variables. Furthermore, he suggests that assuming a well-defined position and momentum for a particle actually exist but cannot be measured together, leads to deductions for measurements that contradict the uncertainty principle and so undermine the whole notion of wave-particle duality and consequently quantum theory itself. [1] Bohm concludes that no theory of mechanically determined hidden variables can lead to all of the results of quantum theory.

Bohm sent his Quantum Theory book to the prominent quantum protagonists. Pauli approved, but disappointingly, Bohm heard nothing directly from Bohr. He was, however, invited by Einstein to discuss the limitations of Bohr's views on interpretation of quantum mechanics.

During the latter period at Princeton Bohm was also developing alternative approaches to quantum theory, one of which was a hidden variable theory - but significantly the hidden variables he proposed were not mechanically determined as he had assumed in his argument against hidden variables in his text book. Bohm found that the Schrödinger equation could be rewritten as two real equations, one could be interpreted as an extension of the Hamilton-Jacobi equation of classical mechanics and the other as a continuity equation for the probability density. This simply reformulation of quantum mechanics allowed one to retain the notion of well-defined trajectories in the quantum domain, trajectories that could explain in a clear and intuitive way all those effects that appeared paradoxical in Bohr's approach. Just as in classical physics, Bohm's equations allowed the definition of precisely defined particle trajectories, but the form of the trajectories is determined, not by classical forces alone but by the evolution of the Schrödinger wave which modified them according to the action of a “quantum potential". The quantum potential is not some arbitrarily cooked up, ad hoc, mechanism cunningly designed to give the same statistical results as quantum theory, it just emerged naturally from the mathematical transformation of Schrödinger's equation. The quantum potential is not a preassigned function of coordinates, instead it depends on the form of the system's wave function (and not simply its amplitude). In this theory a characteristic of the whole system (its wave function) influences the motion of its constituent parts, but not in a mechanistic manner.

[1] See chapters 6.11 and 22.19.

Bohr had argued that quantum phenomena were unanalysable, whereas Bohm had explicitly shown that Bohr was wrong: quantum phenomena could indeed be analysed in detail but the analysis showed such systems to be none-the-less “undivided". Clearly, both Bohm and Bohr held that wholeness was an essential characteristic of quantum phenomena, but in Bohm's approach such wholeness was not opaque; there was a story to tell about the origin of quantum phenomena that had more in common with our traditional conceptualisation of physics systems than did the approach of Bohr. Bohr demanded a new epistemology limiting what it is possible to know, whereas Bohm's approach was based on a new ontology.

Bohm's hidden variables papers were not published until 1952 [?] when Bohm was in Brazil. He arrived in Brazil in 1951 following a very unfortunate episode involving Senator McCarthy's House Un-American Activities Committee. Bohm recalls that he was a member of the Communist Party on campus for a short time (around 1942 or 1943). He explains his sympathies as arising from the collapse of Europe in the face of the Nazis. It appeared that a lot of people preferred the Nazis to the Russians (in America too) but to Bohm it appeared that the Russians were the only ones fighting the Nazis. [1] Shamefully, it was the authorities at Princeton who refused to renew his contract. This episode is treated in detail by David Peat in his biography of David Bohm. [22]. Bohm taught in Brazil for four and half years. Contrary to other accounts, Friere [?] argues that Bohm developed a large and intense scientific activity in Brazil. He had many visitors including Feynman and J-P Vigier, the latter visited for three months to work with Bohm on his new interpretation of quantum mechanics. Ralph Schiller stayed in Brazil for two years as Bohm's assistant. Whilst in Brazil, Bohm wrote `Causality and Chance in Modern Physics" (published 1957) [9] and this, together with his work with Tiomno and Schiller (extending the hidden variable theory to include spin angular momentum) and with Vigier (introducing a stochastic sub quantum medium to explain the origin of the quantum statistics), represent his main achievements during his Brazilian period.

In 1952 Bohm also applied his hidden variables ideas to provide a deterministic description of the behaviour of well defined quantum fields. He later returned in the 1980's to give his theory in much greater detail in papers with B J Hiley and P Kaloyerou [10]. But this work on quantum fields is even less well known. In Bohm's approach, quantum fields have a well defined form in space and time, as do classical fields, but their evolution is determined by their wave functional which exists not in

ordinary space and time but in a field configuration space. Contrary to what is often assumed, bosons, photons for example, in Bohm's theory, do not have trajectories, they are not particles at all, they are simply field excitations (as in the standard theory) but the fields are always well defined. The interaction between quantum fields (bosons) and quantum matter (fermions) is determined by the evolution of a single many dimensional wave function defined in a configuration space spanned by both particle and field coordinates. One has well de ned extended fields interacting with localised particles, in a manner determined at the level of configuration space, that appears grossly nonlocal when projected onto our commonplace three dimension space. The motion is entirely local in the multidimensional configuration space of the whole system. Once again there is a subtle connection between the whole system and its component parts. The continuous and well defined motions of both particle and field involved during a quantised exchange of energy between field and matter has been modelled in detail by Lam and Dewdney [11]. The motion of the whole system of field and particle is determined at the level of the evolution of the configuration space wave function. The field evolution and particle motions that Bohm's theory provides in real space are grossly nonlocal. Perhaps the defining characteristic of quantum mechanics is that there is no way to formulate it entirely in real space, a characteristic that manifests in Bohm's theory connecting individual parts in space non-locally.

Bohm never believed his hidden variable theories would constitute a nal description of ultimate reality, instead he saw his theories as forming a basis for further development, allowing analysis where Bohr had denied it, he argued that some of the assumptions he had made could be relaxed possibly leading to new results - perhaps in domains (10 13 cm or less) where quantum mechanics ran into difficulties. After publication, both Einstein and Pauli informed Bohm that Louis de Broglie had proposed a similar approach in 1927, only to abandon it under criticism from Pauli (and others). de Broglie's theory had in fact been widely discussed, for example at the Solvay conference, but contrary to some accounts the matter was never conclusively settled negatively, at least not on scientific grounds [13]. In 1952 Bohm's development of the theory, including a complete theory of measurement and a treatment of the Frank- Hertz effect, showed that Pauli's criticisms were unfounded. Eventually Pauli conceded that Bohm's model was logically consistent. Pauli, as reported by Olival Freire [8], stated “I do not see any longer the possibility of any logical contradiction as long as your results agree completely with those of the usual wave mechanics and as long as no means is given to measure the values of your hidden

parameters both in the measuring apparatus and in the observed system." Pauli ended with a challenge: “as far as the whole matter stands now, your `extra wave-mechanical predictions are still a cheque, which cannot be cashed." There has never been any demonstration that Bohm's theory is inconsistent or inadequate, it reproduces all of the results of non-relativistic quantum mechanics. Nonetheless, such was the influence of Niels Bohr, Heisenberg, Pauli and the “Copenhagen School", that Bohm's theory was never taken seriously.

Bohm's initial enthusiasm for Brazil quickly waned, and in a letter to Einstein he stated “I am afraid that Brazil and I can never agree." It was as a Brazilian citizen that in 1955 he left for the Technion at Haifa. Thereafter, in 1957, Bohm moved to Bristol, along with two young Israeli research students Yakir Aharonov and Gideon Carmi. In Bristol, together with Yakir Aharonov, he discovered what is today known as the Aharanov-Bohm effect. The effect had in fact already been predicted in 1949 by Ehrenberg and Siday. If one measures a scientist's impact by citations to their works, then this is the most signi cant of Bohm's work, published in 1959 in Physical Review, jointly with Yakir Aharonov, with the title “Significance of electromagnetic potentials in the quantum theory."[12]. The abstract of the paper is as follows

“In this paper, we discuss some interesting properties of the electromagnetic potentials in the quantum domain. We shall show that, contrary to the conclusions of classical mechanics, there exist effects of potentials on charged particles, even in the region where all the fields (and therefore the forces on the particles) vanish. We shall then discuss possible experiments to test these conclusions; and, finally, we shall suggest further possible developments in the interpretation of the potentials."

Initially, the Aharonov-Bohm paper caused controversy in the physics community, but the effect has been widely experimentally verified and today it is routinely recognised and exploited. There is also an aspect of wholeness in the AB effect, once again Bohm had shown how a global property of a system determined the evolution of its parts in a way that transcends local description in space and time: electrons could be influenced by electromagnetic fields in distant regions with which they had never interacted.

Bohm's work on plasmas and electromagnetic potentials continues to be deeply influential and cited widely, but his work on the foundations of quantum theory, and his philosophical ideas, were initially largely ignored

by the established academic community of scientists and philosophers. When I first produced (in the late 70's and 80's) the detailed calculations and associated visualisations showing how Bohm's 1952 hidden variable quantum theory worked and how it accounted for all quantum mechanical `paradoxical' behaviour (see for example [14], [15], [16], [17] ) I believe that I was the only person working in that direction on foundations of quantum theory. Gradually, for a number of reasons, that has changed and today there is a small industry based upon calculations of Bohm trajectories. In the 1980's, at conferences, when I projected my 16mm computer generated motion pictures illustrating the '52 theory's description of tunnelling, interference, spin measurement and superposition and nonlocal correlation, most participants were very surprised that such a thing could be done at all and many had not even heard of Bohm's 1952 theory.[2] There have been many objections to Bohm's hidden variable theory, but in the end critics have had to admit that the theory is consistent and their objections would then simply boil down to the fact that they simply didn't like some particular aspect of the theory or that it had nothing to add to the development of quantum mechanics. I would argue that, even if one doesn't like the theory, it can be used as a 'touchstone' to establish whether a particular claim really does follow from quantum mechanics or is just a prejudice based on a predilection for a particular philosophical approach. Of course, beyond the Schrödinger theory, there are significant problems, particularly with relativistic extensions and creation and annihilation in field theory, but it is certainly not hopeless and there has been progress on those lines.

Even though, in 2019, some 67 years later, Bohm's '52 theory is much better known, there has been a tendency to miss the very points that Bohm would have considered its essence. The common nomenclature of `Bohmian Mechanics' illustrates the point. To reuse a footnote in Bohm's Quantum Theory textbook, that referred to quantum mechanics itself, it could be argued that it would be better to refer to the Bohm's hidden variable theory as Bohmian non-mechanics.

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[2] For example, in 1989 Michael Redhead published a book: `Incompleteness, Nonlocality, and Realism' in which there was no mention of Bohm's theory. Also, at a conference, with Bohm sitting in the front row, the philosopher Hilary Putnam gave a review of the interpretations of quantum mechanics without mentioning Bohm's theory, even though Bohm had presented his ideas at the conference.

The theory is not a conservative reinstatement of a modified classical mechanics, it is an indication of the need for a new order in physics which requires a radical revolution in our basic conceptions of the nature of fields and matter. It was this radical revision that occupied Bohm's thoughts for the rest of his life.

Bohm took up the Chair of Theoretical Physics at Birkbeck College in London in 1961, the environment suited Bohm well and he remained there until his death in 1983. Bohm felt that the problems that beset physics could not be resolved in a satisfactory manner within the bound of current theories. He believed a thoroughgoing analysis and revision of our most fundamental notions was required if real progress was to be made. Bohm felt that the spacetime arena was not the fundamental ground on which to construct physics. His idea was to reconsider what we mean by order and structure in our physical conceptions. Bohm's new theory would exist in a kind of pre-space, the real determining relations would not be in the space-time arena, but the space-time description (involving both quantum theory and relativity theory) would arise naturally as an expression of the behaviour of something beyond space and time, in some pre-space-time. It was only by such a radical reformulation that Bohm felt that the uneasy relationship between quantum theory and relativity could be resolved. One can see how these ideas developed for Bohm. His hidden variable theory of quantum mechanics emphasised the fact quantum theory could not be formulated solely in terms of processes taking place locally in space and time: a system as a whole had a single wave function that determines the motions of its parts, but this wave function was defined in configuration space and the whole was irreducible to a description in solely in everyday space and time. Systems that were widely separated in real space, but described by entangled wave functions, acted jointly with no interaction: for Bohm they were contiguous in the deeper pre-space.

Bohm argued that an algebraic formulation of quantum mechanics could provide the basis of a new way forward that gave mathematical expression to his ideas of pre-space. Such a reformulation was a radical and ambitious programme requiring a complete rebuilding of our concepts. He described the thoroughgoing revision needed at the conceptual level in a book coauthored with David Peat entitled "Science, Order and Creativity. He also provided a novel analysis of our concepts of space and time in his text book entitled “Special Relativity". Ultimately Bohm's ambitious programme could not be completed during his lifetime, but the work is carried on today by Basil Hiley, his long term collaborator at Birkbeck.

Although Bohm's 1952 Hidden Variable Theory was largely ignored by the physics community and by Bohm himself in the years following its publication, it had a major impact on John Stewart Bell and important consequences followed. Bell worked in the theory group at CERN, but was also interested in foundations of quantum mechanics. He felt there was something wrong with von Neuman's proof of the impossibility of hidden variables and when he read Bohm's papers he “saw the impossible done". Bell was struck by the explicit non-locality in Bohm's theory and he wondered whether non-locality must be an essential feature of any hidden variable theory that reproduced the results of quantum mechanics. As a result of this interest, he went on to develop a statistical inequality amongst sets of measurement results on entangled particles, that any local hidden variable theory of quantum mechanics must satisfy, but that is violated by quantum mechanics. Subsequent experimental tests, following Bohm's reformulation of the EPR experiment (mentioned earlier) showed that Bell's inequality was violated in practice, proving that no local hidden variable completion of quantum mechanics was possible. Such highly verified test results showed that if a hidden variable theory exists that is consistent with quantum theory then it must be nonlocal, as is manifestly the case in Bohm's theory. Non locality was often cited as a reason not to accept Bohm's approach, but today, nearly all physicists take the experimental results as a clear demonstration that at the fundamental level the world behaves non-locally.

Bohm's Hidden Variable Theory has grown in acceptance in recent times and it is certainly no longer ignored. Bohm himself returned to its consideration in the 1980's and Bohm's later ideas on his hidden variable theory were published in 1992 (with Basil Hiley) in his book entitled “The Undivided Universe".

Today, Bohm's version of quantum mechanics has received a further stimulus and one that appears to allow Bohm's “cheque to be cashed". It turns out that “weak measurements" of the quantum mechanical momentum (followed by strong measurements of position) allow the deduction of a statistical estimate of the particle momentum [18], [19], [20] and [21].Theoretical analysis shows that the estimate produced in this way is actually an estimate of the momentum as defined in the de Broglie-Bohm theory. A set of such weak measurements, carried out on a grid of points, allows the numerical reconstruction of de Broglie-Bohm particle trajectories. To date experiments have been carried out using photons and the trajectories inferred are very similar to those first calculated, for example,

for the two slit experiment. However, the trajectories produced in this way are controversial. Firstly because, according to Bohm's specific approach, there are no photon trajectories and secondly because of the limiting and statistical nature of the measurement techniques. Beyond physics, Bohm was concerned with philosophy, he understood that the relationship between theory and reality was subtle, he certainly did not subscribe to the “natural attitude" in which theory is simply a reflection of a xed reality that our theories can approach ever more closely. He believed that what we perceive depends on our conceptual scheme and that our current tendency is to conceive, and hence perceive, reality as an independent, fragmented and well-defined thing-in-itself that may be completely understood by an analysis which focusses on the parts and ignores the whole; the whole is nothing more than the parts in interaction. Our world view, and our theories, arise from this fragmentation of being and they become self-justifying as we simply ignore whatever is not working. Bohm argued that in a fragmented world view, whether within physics or at the individual, institutional, social, national or global level, problems will be insoluble unless we come to recognise the constitutive nature of wholeness and engage in true, selfless dialogue to create a common perception of the wholeness of all being.

Bohm's notion of the relation between parts and whole is subtle, transcends the western philosophical tradition and remains alien to most people, so he often proceeded using analogies to develop his meaning. Possibly his favourite analogy was the hologram. In lens imaging each point in the image contains information about one point in the object, whereas in lensless, holographic imaging each point in the hologram lm contains information pertaining to the whole object. Each small area of the hologram can reproduce a limited image of the complete object. The analogy is helpful in understanding his concept of enfoldment - the whole is enfolded in the parts and the whole may, to some extent, be unfolded from any of the parts. Individuals stand in relation to the whole of which they are a part as does a small area of the hologram in relation to the object. This Bohm believed true at the level of individual human being and at the fundamental within physics.

In Bohm's epistemology there is no sharp distinction between observer and observed, thinker and thought, theory and reality. All that exists is an unbroken, owing wholeness. Bohm referred to all that exits as “actuality", unknowable and infinite in its possibilities, qualities and levels. That we refer to as reality is a particular human abstraction, of limited validity, that may be considered correct (rather than “true") within certain constraints and

in particular con-texts. Individuals (objects or people) can appear at one level as independent and autonomous, but ultimately everything is connected at the deeper level. Bohm refers to these levels as the implicate and the explicate orders. In the late seventies, whilst I was a PhD student in Bohm's group at Birkbeck College in London, and struggling to understand these ideas, I remember that Bohm directed me to read Lenin's “Materialism and Empirio-criticism". I was also reading Bohm's “Causality and Chance in Modern Physics" and I was struck by the strong parallels. In particular the notion, common to both, of the “qualitative infinity of nature" and the dialectical relationship between theory and reality. Certainly in his younger years, Bohm had sympathy with the idea that a rational society must be based on a materialist understanding of history for which it was also necessary to have a rational, causal and materialist science.

Bohm also published across a broad spectrum concerning the relationship between language and thought (see for example his book “Fragmentation and Wholeness"). In his textbook on “The Special Theory of Relativity" he has a chapter on our natural language concepts of space and time which he argues, drawing on the work of Piaget, have more in common with special relativity than with Newton's concepts of absolute space and time. Bohm was also interested in the nature of consciousness which he discusses in his book entitled “Wholeness and the Implicate Order". In later years he also published a series of dialogues with Jiddu Krishnamurthi. His lifelong quest was to understand wholeness in all aspects of existence.

David Bohm was elected Fellow of The Royal Society in 1990 and died on October 27 1992 after returning home from a day's work at Birkbeck College, London, convinced he was “on to something".

It is a tribute to the influence of David Bohm in many fields of human under-standing that his work continues to be widely cited. A complete bibliography of Bohm's publications and citations can be found at Google Scholar.

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# The Fourth Baron Rayleigh

***Edward A Davis, University of Cambridge***

Robert John Strutt (1875-1947) was the son of the more famous William John Strutt (1842-1919), the Third Baron Rayleigh, who received the Nobel Prize in Physics for the discovery of argon, and with whom he is sometimes confused.

Figure 1. Robert John Strutt (1875-1947)

## 1. Early years

R J Strutt was educated at Eton College and Trinity College Cambridge where he initially read mathematics but switched to the Natural Science Tripos after two terms. Following graduation, he became a research student under the supervision of J.J. Thomson in the Cavendish Laboratory, soon after Thomson had discovered the electron. His research there included measuring the conductivity of gases under the influence of radiation from radioactive sources, showing, for example, that the conductivity arose from ionised species and depended on the type of radiation and the nature and pressure of the gas.

His first paper, published in 1900, was ‘On the behaviour of Bequerel and Röngten rays in a magnetic field’. (Note at this time these rays were named after their discoverers, namely radiation from radioactive materials by Becquerel in 1896 and X-rays by Röntgen in 1895.) He studied the deflection in a magnetic field of the various rays emanating from radioactive sources, distinguishing those rays that were easily bent (the beta rays) from those that were not (the alpha rays). His suggestion that the latter were positively charged atomic particles was ahead of its time in 1900. In other experiments he demonstrated conclusively that beta rays were different from X-rays.

His authoritative work at the forefront of what were new and exciting fields at the time is reflected in his authorship of one of the earliest books on radioactivity, namely ‘*The Bequerel rays and the properties of radium*’.

Strutt devised an interesting and informative demonstration of Madam Curie’s discovery that beta particles emitted from radioactive substances leave them positively charged. It was named the radium clock. A vial containing a radium salt is suspended inside an evacuated glass container as illustrated in Figure 2. To the base of the vial are attached a pair of electroscope-type gold leaves, which separate with time as the radium decays. When the separation is such that the leaves touch an earthed metal foil lining the inside of the vessel, they immediately collapse to their uncharged state, where after the cycle repeats. In the original apparatus the period was about a day.

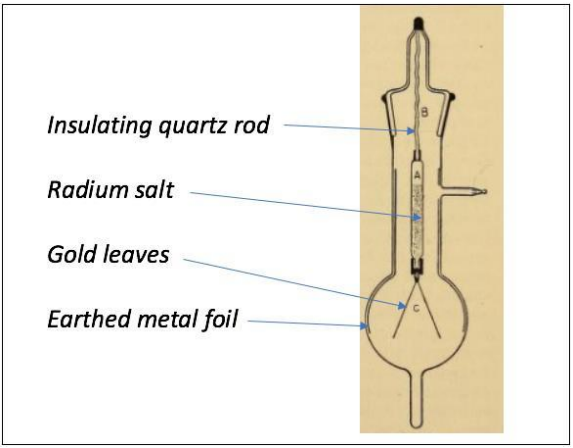


Figure 2. Strutt’s radium clock

On the occasion of a visit to Strutt's ancestral home in Terling Essex by Lord Kelvin and Lord Rutherford in 1904, the radium source was replaced by a larger 10 mg sample that Rutherford happened to have with him. This reduced the period to about 30 seconds. Kelvin called it a perpetual motion machine and considered it violated the second law of thermodynamics that he himself had proposed. The reason for this stemmed from Kelvin's belief at the time that the source of energy for radioactivity was not inherent in the material itself but in the surrounding atmosphere.

## 2. Age of minerals and the Earth

Strutt's interest the radioactivity of naturally occurring rocks was stimulated by his father drawing his attention to radioactivity in the crater of the King's Spring at Bath. During a visit there he detected both radium and helium and estimated that the spring delivered water containing as much as 0.3 grams of radium each year. These investigations led him to study the radium content of rocks, following Bertram Boltwood, a Harvard radiochemist, in establishing that the radium content of uranium-bearing minerals bore a fixed ratio to their uranium content. This finding confirmed that radium was a direct descendant of uranium in the decay chain.

Armed with a knowledge of the heat evolved in the radioactive decay of radium itself and an estimate of the average amount of radium in rocks, Strutt estimated that the Earth's crust could not be more than about 45 miles thick, otherwise the Earth would not be in temperature equilibrium and would be getting hotter with the passage of time.

Strutt then went on to use the measured ratio of helium to uranium in radioactive rocks to determine their geological age. The helium of course arises from alpha decay. These ages, of the order of 600 million years, were considerably older than the estimate of the Earth's age of the order of 20 million years made by Kelvin in 1894, who had calculated the time it would have taken for the Earth to have cooled to its present level from an initially molten sphere. Furthermore, Strutt recognized that his estimates were minimal values because they assumed 100 % helium retention by the rocks during their lifetime. (The Earth is now believed to be around 4.5 billion years old from measurements on meteorites and rocks recovered from the moon, as no known terrestrial rocks have remained unaltered since the Earth's formation.)

## 3. Active nitrogen

An area of study that pre-occupied Strutt throughout his life was that of 'active nitrogen'. This was the name he gave to the whirling cloud of brilliant yellow light remaining for a considerable time after cessation of an induction-coil-induced discharge in nitrogen gas (Figure 3).

Figure 3. The afterglow in nitrogen.

He wrote a large number of papers on this phenomenon in an attempt to understand its nature. These described the results of numerous studies investigating the effects of temperature, the introduction of impurity gases, coatings on the walls of the containing vessel, etc. His belief, now currently accepted, is that the light arises from recombination of monatomic nitrogen created by the discharge, the energy thereby released causing excitation of vibrational levels of molecules. The apparatus he used to investigate the effect is still in existence in the family home (see Figure 4). He continued to work on it after he inherited the title of Lord Rayleigh in 1919 right up to his death in 1947 (see Figure 5)..

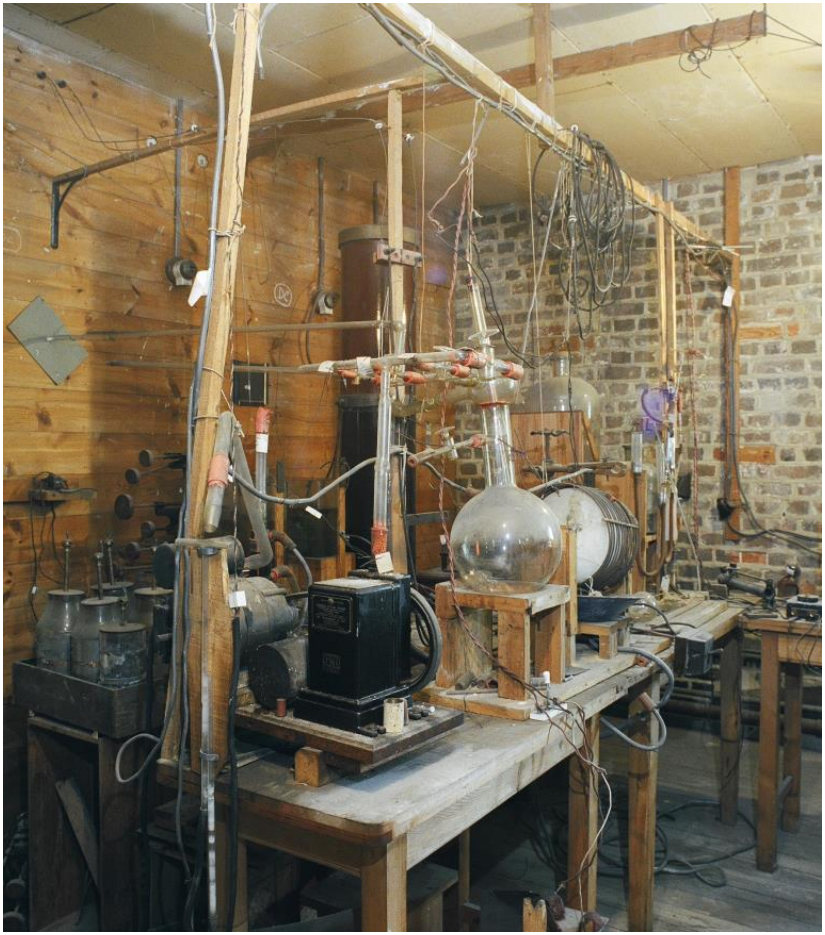

Figure 4. Still extant apparatus used to study the afterglow in nitrogen.

Figure 5. The Fourth Baron Rayleigh in the laboratory at Terling Place

## 4. Light of the night sky

Following his appointment as Professor of Physics at Imperial College in 1908, Strutt worked on several topics, including that of the faint light that prevents the night sky from ever being completely dark. He was the first to differentiate between the aurora or northern light and in his experimental investigations used filters to cut out that light. He found that the 'airglow', as he termed it, was not polarised, unlike that of scattered sunlight, which of course his father had studied both experimentally and theoretically.

The topic was of some military importance and led the US Airforce Cambridge Research Laboratories to acquire Rayleigh's notebooks, some containing unpublished work, in 1963 after his death. They are now housed in the library of the United States Air Force Academy in Colorado Springs. Professor Roach (University of Boulder in Colorado) and others proposed that the airglow be measured in units of 'rayleighs' with one rayleigh being defined as a flux of $10^6$ photons per $cm^2$ per second. The airglow lies between one and a few hundred rayleighs, whereas the aurora has a

brightness 10 -100 times greater. It is of interest that the Third Baron Rayleigh also had a unit named after him, namely the 'rayl', which is a unit of acoustic impedance.

The origin of the airglow is not singular but predominantly arises from the recombination of ions (e.g. between oxygen atoms and between nitrogen and oxygen atoms) generated by the Sun's ultraviolet rays during the day.

What Strutt could never have ever imagined is that the light is now routinely observed by astronauts as a greenish band surrounding the Earth at a height of about 100 km above its surface (Figure 6).

Figure 6. The airglow as seen from the International Space Station. It is seen as a green band towards the top of the atmosphere.

## 5. Atmospheric ozone

This research was undertaken in collaboration with Alfred Fowler at Imperial College, who brought Strutt's attention to a photograph of the spectrum of the star Sirius, which showed regularly spaced bands in the neighbourhood of 3,000 angstroms. These bands, about 28 angstroms apart, had first been observed by Huggins in 1890. Subsequently Fowler had found the same bands in the spectrum of the setting sun.

It was thought that absorption in the atmosphere might be responsible. In order to test this, Strutt filled a long tube fitted with quartz end-plates with ozone. Using a burning magnesium ribbon as a source of ultraviolet light and the same spectrograph that Fowler had used on the sun, the spectrum revealed the same bands as seen from the stars. In Strutt's own words "It was a dramatic moment". This was the first definite proof that ozone was present in our atmosphere.

Further experiments on the solar spectrum revealed that the nearer the sun was to the horizon, the greater the intensity of the absorption bands owing to the longer the column of air traversed. Likewise, a longer tube in the laboratory enhanced the bands. However, chemical experiments on the lower atmosphere had failed to detect ozone. So Strutt devised another optical experiment. This is best described in his own words:

> *"During a vacation at Terling, I applied myself to this question. A small prismatic camera was constructed with a quartz prism and lens. The source was a mercury vapour lamp. I was able to place it at Whitelands, Hatfield Peverel, a house then in the occupation of my uncle, E. G. Strutt. A "black out" was prevailing directed against the Zeppelins (rigid air ships), for aeroplanes were not yet regarded as a serious menace.*
> *It was necessary to get permission to show the light, across the valley of the Chelmer, and after some preliminary trouble in trying to make the local military understand why I wanted it (they could not understand the idea of a research not directed to military ends) I got the required permission through my uncle A. J. Balfour, who was then a member of the Government.*
> *The prismatic camera was set up in a field near "The General's Arms", a public house at Little Baddow, whence*

*a view could be obtained of the lamp 4 miles distant. Photographs of the spectrum were obtained, extending as far as the mercury line at A2536, which is in the part of the spectrum most opaque to ozone. This proved for the first time that there was little ozone in the lower atmosphere and that the ozone seen in the spectrum of the low sun must be located in the higher regions. Later research has confirmed and amplified this conclusion.*
*This discovery has become of great importance in the advance of meteorological science, particularly owing to the connexion found between barometric pressure and ozone distribution in the upper atmosphere by G. M. B. Dobson, F.R.S. Measurements of ozone distribution are now made in many meteorological stations throughout the world."* R J Strutt 1916

## 6. Other topics

The full breadth of the Fourth Baron Rayleigh's research is perhaps best illustrated by a random selection from his 301 publications.

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- (with H. B. Baker of IC) *Uber die aktive modifikation des stickstoffs* Ber. dtsch. Chem. Ges. **47** 1049 (1914)
- *The age of the earth* Nature **108** 335 (1921)
- *Sir William Crookes and spiritualism* Nature **118** 843 (1926)
- T*he iridescent colours of birds and insects* Proc. Roy. Soc. B **106** 618 (1930)
- *The bending of marble* Proc. Roy. Soc. A **144** 266 (1934)
- *The green flash at sunset* Nature **135** 760 (1935)
- *The problem of physical phenomena in connexion with psychical resea*rch Proc. Soc. Psych. Res. Lond. **45** 1 (1938)
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## 7. Envoi

If it were not for the outstanding ability and numerous contributions to science of his father, the Fourth Baron Rayleigh would be much better known. He was a very distinguished scientist who was elected to the

Fellowship of the Royal Society in 1905, received the Rumford Medal from that Society in 1920, and served as its Foreign Secretary. He was President of the Physical Society 1934-1936 and President of the Royal Institution 1945-1947. Honorary degrees were awarded to him from the Universities of Durham, Edinburgh and Trinity College Dublin.

Figure 7. The Fourth Baron Rayleigh at his desk at Terling Place

Acknowledgements

The principal sources of information for this article were:

1. A.C. Egerton, *Lord Rayleigh 1875-1947*, Biographical Memoirs of the Royal Society, November 1949, which contains a full list of the Fourth Baron Rayleigh's publications.

2. Applied Optics, volume 3, number 10, October 1964, which contains several papers relating to the work of the Third and Fourth Baron Rayleighs.

# Dame Kathleen Lonsdale FRS (1903-1971)

***Jenny Wilson***
***Dept. of Science and Technology Studies, University College London***

Kathleen Lonsdale in her laboratory at UCL, undated.
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## Introduction

Dame Kathleen Lonsdale (née Yardley) FRS (1903-1971) was not only one of the most prominent female scientists of the twentieth century but also a leading campaigner for peace, penal reform and the position of women in science. She started her career in X-ray crystallography, a technique which had been developed ten years before her birth following the discovery of X-rays in the late nineteenth century. She married and had three children but managed to continue with her career, a feat relatively rare at that period in time. Although born a Baptist, she joined the Religious Society of Friends (Quakers) and endured a prison sentence for her beliefs. During her later career, her scientific activities became more closely entwined with her religion and social concerns.

## Early days

Kathleen Yardley was born on the 28 January 1903 in Newbridge, County Kildare. She was the youngest of ten children in a family which had four girls and six boys, four of whom died in infancy. Her father was Harry

Frederick Yardley who was the post-master at Newbridge Post Office following a career in the British Army. Her mother was Jesse Cameron who was of Scottish descent. Unfortunately, Yardley's father had a drink problem, home life was not happy with the result that her parents separated in 1908. Her mother concerned about the stormy state of Ireland decided to bring her children (four girls and two boys) to England and they settled in Seven Kings, Essex. Her father, who had moved to South Africa, visited them occasionally but died when she was twenty.

Yardley's formal education started at Downshall Elementary School (now Downshall Primary School) in Seven Kings from 1908 to 1914. As a result of her academic ability she won a scholarship to Ilford County High School for Girls. In order to study science she had to attend in physics, mathematics, chemistry and higher mathematics at the Boy's school as these were not available at the Girl's school. In 1918 she applied to Bedford College for Women, a constituent college of the University of London for a place on the BSc Honours Mathematics course. She was academically extremely able having obtained first-class honours in the Senior Cambridge Examination with six distinctions but was only sixteen. Her age appeared to have caused concern with the Principal of Bedford College but after being interviewed she was accepted as a non-resident student from October 1919.

During her first year Yardley' reports indicated excellent progress in all subjects which resulted in her being awarded a University Exhibition. At the end of her first year, she made the decision to change to the Physics Honours course as she felt that careers in mathematics would be teaching or lecturing which she was unwilling to undertake. In 1922 Kathleen Yardley was awarded a first-class honours degree in physics, being one of eight students to achieve this award. The examiner for internal students for the final examination was Professor Sir William Henry Bragg (1862-1942) K.B.E, Quain Professor of Physics at University College London (UCL). As a result of her success in the final examination she was offered a research position by Bragg at UCL where she received a Department of Scientific and Industrial Research Grant of £180 a year.

**Joining the team of Sir William Bragg at University College London**

In 1922 she started her career in X-ray crystallography working in the physics department at UCL. She was one of a small team which Bragg had assembled which included five Research Assistants and two students in training, one of whom was Yardley. She first learnt to construct her apparatus, which included an ionisation spectrometer designed by Bragg,

with which the position and intensity of X-rays reflected from crystal planes could be accurately measured. She then used her apparatus to determine the structures of simple organic compounds using X-rays of known wavelength.[20]

In 1923 Bragg moved to the Royal Institution (RI) as Director of the Davy Faraday Research Laboratory (DFRL) and took his research team, including Yardley with him.

**Transferring to the Royal Institution**

She became very skilled in the use of the ionisation spectrometer and started publishing her own papers. In 1924 she was awarded a MSc from the University of London. In the same year Yardley carried out her first major contribution to X-ray crystallography in conjunction with William Astbury (1896-1961) [21] on the relationship between X-ray diffraction patterns and the space groups from which they arose. A space group is the mathematical description of the symmetry elements in a crystal structure. The problem had been to present the space groups in a form useful to X-ray crystallographers. Astbury and Yardley's paper contained a set of tables which would enable crystallographers to determine the space groups of crystals from their diffraction patterns.[22] Symmetry and space groups were of particular interest to her and would be an important feature of her future career.

Whilst at UCL, Yardley had met another student Thomas Lonsdale who had moved to Leeds to work at the Silk Research Association, located in the Textile Department of the University of Leeds. They were engaged for four years and married in 1927. Although she planned to retire and become a wife and mother Thomas Lonsdale would not consider her giving up her scientific career. Now Kathleen Lonsdale, she left the RI and moved to the physics department at the University of Leeds.

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[20] The technique of X-ray crystallography had been devised by Bragg and his son Lawrence Bragg (1890-1971) for which they had been jointly awarded the Nobel prize for physics in 1915.

[21] Astbury became a life-long friend and professionally supported Lonsdale's nomination to the Royal Society.

[22] Astbury, W.T. and Yardley, Kathleen, 'Tabulated data for the Examination of the 230 Space-Groups by Homogeneous X-rays', *Philosophical Transactions of the Royal Society of London A*, **224** (1924), 221-257.

**Working at the University of Leeds**.

Lonsdale found that her working environment was very different from that of the DFRL. She had to set up her own research laboratory using the equipment grant she had been awarded by the Royal Society. She had also been awarded an Amy Lady Tate Scholarship by Bedford College which she supplemented by a part-time demonstrator's post.

One advantage was that the physics department was next to the chemistry department headed by Professor Christopher Kelk Ingold (1893-1970) who gave her a sample of solid hexamethybenzene for her X-ray analysis work. She used this to confirm the structure of benzene which was her second major contribution to crystallography. From her experimental work she deduced that the benzene molecule existed in the crystal as a separate entity, the benzene carbon atoms are arranged in a ring formation and the ring is hexagonal or pseudo- hexagonal in shape. She concluded that this supplied a definite proof, from an X-ray point of view that the chemist's conception of the benzene ring is a true representation of the facts.[23]

Throughout her time at Leeds, Lonsdale had been in contact with Bragg who had given her ongoing support and advice. When she started work on her next analysis, hexachlorobenzene, she was expecting her first child. While at home before and after the birth, she worked on complex calculations resulting from her analysis. She had used Fourier analysis – a mathematical method in which general functions are represented in terms of sums of trigonometric functions. Unfortunately, her results did not match those obtained for hexamethylbenzene and she was unable to make the same conclusion. However, she was the first to use the Fourier method in the structural analysis of an organic compound.

Her daughter Jane was born in October 1929 and it was Bragg who persuaded the Managers of the RI to give her a grant of £50 for one year with which to hire a daily domestic helper. Thomas Lonsdale's position came to an end in 1930 but he obtained a new post at the Road Research Laboratory, Harmondsworth. She was now expecting her second child and the family moved back to London. Her second daughter Nancy was born in July 1931.

[23]Lonsdale, Kathleen, 'The Structure of the Benzene Ring in $C_6(CH_3)_6$', *Proceedings of the Royal Society of London A*, **223** (1929), 494-515.

**Returning to the Royal Institution**

From 1930 to 1932 she stayed at home with her young children but used her time productively by continuing with her work on the preparation of tables of mathematical formulae needed in practical work on crystal structures. For this work, no laboratory was needed only the use of reference books and old X-ray photographs which she used to undertake the demanding calculations by hand. This work would provide a significant contribution to crystallography and would be the basis for her future involvement with international developments within the field.

Lonsdale was keen to resume her research and in 1931 Bragg managed to secure sufficient funding from Sir Robert Mond (1867-1938), Honorary Secretary of the DFRL which enabled her to get assistance at home and to return to the DFRL to continue her work on the X-ray analysis of organic compounds. In 1934 she was expecting her third child and left the RI temporarily. Her son Stephen was born in June 1934 and in 1935 she returned to the RI, having obtained a Leverhulme Scholarship which was supplemented by funding from the RI. In 1936 Lonsdale was awarded a DSc by the University of London.

On her return to the RI, she undertook new areas of work starting with optical and magnetic anisotropy as an aid to structural analysis. Her work on diffuse reflections gave rise to two further research areas, thermal vibrations in crystals and the behaviour of diamond under different conditions both of which became lifelong interests. She also became involved in the new technique of divergent beam X-ray diffraction, in which a strongly divergent source was used instead of the usual highly collimated beam. She used this technique for the examination of crystal texture and for her ongoing research on diamonds.

She also became involved in the early development of what were to become the International Tables. She was one of the authors of the first International Tables which were entitled *Internationale Tabellen zur Bestimmung von Kristallstruckturen (International Tables for the Classification of Crystal Structures)* published in 1935 by Gebrüder Borntrager in Berlin. The Tables soon became the standard reference book for those engaged in crystal structure analysis. However, she was keen to publish her own tables as she had been working on these since returning to London. Her book, funded by the RI was a copy of her handwritten structure factor tables and had been photo-litho printed from the original to avoid error.[24]

[24] Lonsdale, Kathleen, *Crystals and X-Rays* (London: G. Bell & Sons, 1948)

In 1942 Sir William Henry Bragg died. For Lonsdale this was the end of a working relationship which had spanned twenty years, fifteen of which she had worked under Bragg's directorship at the RI. For the next four years she worked under the new Director, Sir Henry Hallett Dale (1875-1968) and became a Dewar Fellow from 1944 to 1946. It was Dale, as President of the Royal Society who played a pivotal role in the election of women to the Society. Lonsdale, together with the biochemist Marjorie Stephenson were nominated in 1944 and in 1945 these two were the first women to be elected as Fellows of the Royal Society.

**Moving back to University College London**

In 1946, she moved to a position in a university department when she accepted the post of Reader in the chemistry department at UCL. She established a very successful course in crystallography for chemistry undergraduates and an inter-collegiate MSc course in collaboration with John Desmond Bernal (1901-1971) at Birkbeck College. Like Lonsdale, Bernal had also been a member of Bragg's team at the RI. In 1949 she was promoted to Professor of Chemistry, the first woman professor at UCL. Lonsdale set up her own research school in crystallography within the department which attracted many international students. She continued with her own research interests but added problems of medical and biological interest particularly the pharmacological activity and structure of n-methonium compounds and studies on urinary calculi, a project funded by the Medical Council.

She became very involved in the establishment of the International Union of Crystallography and contributed to the 'general good' of crystallography in editing the second edition of the International Tables. This work absorbed considerable amounts of time which probably detracted from her own research interests. She maintained that crystallography was a scientific discipline in its own right and not just a technique. She campaigned for the proper training of crystallographers. Unfortunately, she was unable to set up training courses or first-degree courses in crystallography at UCL despite making a viable case to her Head of Department. She did, however initiate an International Commission of Crystallographic Teaching which examined the current status of crystallography and crystallographic teaching, provided examples of good practice and gave recommendations for the future development of crystallography as a scientific discipline.

From the mid-1950s, Lonsdale was honoured for her scientific work and particularly her contribution to crystallography. In 1956 she was created a

Dame Commander of the British Empire and a year later received the Davy Medal from the Royal Society whom she had served as Council Member and Vice-President from 1960-1961. She was also very active in the British Association for the Advancement of Science being General Secretary from 1959-1964, President of the Physics Section in 1967 and then in 1968 became its first female President. She was Vice-President of the International Union of Crystallography from 1960-1965 and then in 1966 became President, the first woman to hold this office. During the period 1960-1969 nine British universities awarded Lonsdale honorary degrees. She was also honoured for her contribution to the knowledge of diamond when a rare form of hexagonal diamond found in meteorites was named Lonsdaleite.[25]

**Campaigning for peace, penal reform and other social concerns.**

Whilst at Leeds, she had come into contact with the Quakers and on returning to London, both she and her husband joined the Religious Society of Friends (Quakers). Becoming a Quaker affected her personal life following the outbreak of World War Two. Although she was undertaking fire-watching duties, she refused to register for civil defence duties as there was no clause allowing for conscientious objectors. As a result of refusing to pay the fine for this offence, she was committed to Holloway gaol for a month. Her husband who had supported his wife's actions felt it was a 'life changing experience'. This could explain why Lonsdale's life, which up to this point had been very private working in a laboratory with like-minded individuals, became more focussed on people and for the rest of her life her public image increased as she was often quoted in newspapers and journals and addressed many public meetings in this country and throughout the world. Going to gaol was an experience that gave her a lifelong interest in penal reform. She later became involved in prison visiting serving as a member of the Board of Visitors at Aylesbury Prison for Women and later as Deputy Chairman of the Boards of Visitors, Bullwood Hall Borstal Institution for Girls.

She was a very active campaigner for peace both on an individual basis and as a member of several peace organisations, the Atomic Scientists' Association (ASA) where she held the post of Vice President and the Pugwash Conferences on Science and World Affairs. The ASA was established following the development of the atomic bomb which contributed to East-West tensions leading to the Cold War. It focussed on

[25] Frondel,C. and Marvin, U.B., 'Lonsdaleite, a Hexgonal Polymorph of Diamond', *Nature* **214** (19670, 587 -589.

the broader aspects of atomic energy. Pugwash continued the work of the ASA but broadened and addressed a range of international issues particularly involving conflicts which were a feature of the Cold War.

In her Swarthmore lecture *Removing the Causes of War* she attempted to analyse the causes of war and the ways in which they may be removed undertaken within the framework of the Friends' ways of thinking and acting.[26] Lonsdale wrote *Is Peace Possible?* [27] in which she discussed problems of peace, freedom and justice in an era of expanding world population and technical development. She was also President of the British Section of the Women's International League for Peace and Freedom and a member of the East-West Committee of the Society of Friends. Eight members of this Committee including Lonsdale visited Russia in 1951. The aim of the visit was to improve relations between East and West during a period of great distrust by making contact with Soviet peace organisations.

Later in her career she was concerned about the lack of women in science particularly in senior positions and as members of the various scientific societies. She undertook extensive research in order to identify the problems facing women seeking scientific careers which she used in many of her lectures and speeches. These included comments from her own experiences providing reasons for her success in having found the 'right husband'.[28]
She travelled widely and achieved an international reputation as an effective public speaker. She managed to bring together the themes which were important to her, science, religion, peace, penal reform and women in science. She also included the ethical responsibilities of scientists which was a topic promoted by many at this time. She managed to combine in a remarkable way, her scientific profession and her Quaker faith and was held in high esteem by both the scientific and Quaker communities.

Thomas retired in 1961 and they moved to Bexhill in Sussex with Lonsdale undertaking the long journey to London. She retired in 1968 and became an Emeritus Professor. She continued her research interests and wrote on a range of topics until the end of her life.

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[26] Lonsdale, Kathleen, *Removing the Causes of War Swarthmore Lecture* (London: George Allen & Unwin Ltd, 1953).

[27] Lonsdale, Kathleen, *Is Peace Possible*? (Harmondsworth: Penguin Books Ltd, 1957).

[28] Lonsdale, Kathleen, 'Women in Science- why so few?', *Equipment Laboratory Digest*, **86** (1971), 85-88.

**Conclusion**

Kathleen Lonsdale's contribution to science and technology was by any standards immense and which increased with every stage of her career. Her contribution involved the results of her experimental work, her work for the crystallographic community and her involvement with numerous scientific societies and organisations. Her early work on the structure of the benzene ring showed her expertise as an experimental physicist and throughout her career she placed greater emphasis on the importance of practical work rather than relying on complex theory.

Her career was not typical of her generation in having a full scientific career alongside marriage and a family. However, there are several reasons as to why she built a successful scientific career. The first was the support from her husband, who assumed many domestic responsibilities from the early days of the marriage and which increased as Lonsdale developed both a national and an international profile. Secondly, she developed a network of supportive colleagues. In her early career she had the support and guidance of Bragg who obtained financial means to pay for domestic help so that she could care for her young family and continue with her work. Astbury, her former colleague at the RI and co-author of the paper on space-groups supported her in her election as a potential Fellow of the Royal Society. Ingold was another important colleague whom she had first met at Leeds where he had been very impressed by her work on benzene and other aromatic derivatives. He later supported her application for the post of Reader within his department at UCL. Thirdly, whilst at home she was able to continue with her career by creating a productive role suited to her situation which did not require laboratory facilities. During this time, she kept in contact with scientific colleagues particularly Bragg regarding developments at the RI. It was with Bragg's assistance that she was able to resume her experimental work at the DFRL.

Considering what she achieved during her lifetime it is surprising that she is not considered such an important figure in the history of science. She appears to have been overlooked compared with female scientists working in the same field such as Dorothy Hodgkin (1901-1994) and Rosalind Franklin (1920-1958). Kathleen Lonsdale had a very successful career despite incurring obstacles but overcame these using a variety of strategies. She spent much of her professional life in a male-dominated environment but this did not seem to bother her. Later in her life she did refer to the difficulties encountered by a married woman with children who wished to become a first-class scientist which suggests this was a reflection on what it had taken her to achieve success.

# Meeting

## ‘The Scientific Legacy of the Third Baron Rayleigh’

## Thursday 19th September 2019 at

## Department of History and Philosophy of Science, University of Cambridge

This is a one-day meeting sponsored by the History of Physics Group of the Institute of Physics.

Contact: Professor EA Davis.

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## Newsletter - Print or Online only?

Your committee has been considering this question - would you prefer to continue receiving print copies of the newsletter or would you be content with online versions only? (At the moment online versions are available as well as print.) A survey is being considered but in the meantime please let me know your views. Thanks.

Malcolm Cooper
Editor

# JOINT MEETING OF THE ROYAL SOCIETY OF CHEMISTRY HISTORICAL GROUP, THE SOCIETY FOR THE HISTORY OF ALCHEMY AND CHEMISTRY AND THE ROYAL INSTITUTION

## William Crookes (1832-1919)

*Saturday 19 October 2019, Royal Institution, 21 Albemarle Street, London, W1S 4BS*

This year marks the centenary of the death of William Crookes. Journalist, chemist, photographer, spiritualist, businessman, sometime Secretary of the Royal Institution and President of the Royal Society of London, Crookes was a key figure in the science of the second half of the nineteenth century and beginning of the twentieth. This meeting, which is part of the ChemFest celebrations of the sesquicentenary of the periodic table, will examine various aspects of Crookes's extraordinary career and his place in science.

**Programme**

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| 13.45 | Registration |
| 13:55 | Welcome and Introduction: Frank James, (Royal Institution and Chair of SHAC) |
| | **First Session** Chair: Anna Simmons (UCL) |
| 14.00 | Richard Noakes (Exeter University) |
| | *'Two Parallel Lines'? The Trajectories of Physical and Psychical Research in the Work of William Crookes* |
| 14:20 | Kelley Wilder (De Montfort University, Leicester) |
| | *William Crookes, a life in Photo-Chemistry* |
| 15.00 | Refreshment Break |
| | **Second Session** Chair: Peter Morris (Chair of RSCHG) |
| 15.30 | Frank James (Royal Institution and UCL) |
| | *William Crookes and Michael Faraday* |
| 16.10 | Paul Ranford (UCL) |
| | *Crookes's "Invisible Helper" – George Gabriel Stokes (1819-1903)* |
| 16.50 | William Brock (University of Leicester) |
| | *The key to the deepest mystery of nature: Crookes, periodicity and the genesis and evolution of the elements* |
| 17.30 | Close of meeting |

There is no charge for this meeting, but **prior registration is essential**. Please email Robert Johnstone (robert.johnstone.14@ucl.ac.uk

## 4th International Conference on the History of Physics

We are pleased to announce that the next in this biennial series will take place at

**Trinity College Dublin**

from

**Wednesday 17th to Friday 19th of June 2020**

The first three conferences in the series were held at Trinity College, Cambridge UK in 2014, Pöllau, Austria in 2016 and San Sebastian, Spain in 2018. Their aim was to bring together physicists interested in the history of their subject and professional historians of science in the belief that proponents of the two disciplines, with their different perceptions and methodologies, can benefit from interaction and discourse.

Inspired by the recent centenary of two major landmarks in modern physics - nomination of the proton as a fundamental nuclear particle and discovery of the bending of light in a gravitational field - the leading theme of the present conference will be:

**'On the road to modern physics'**

Presentations on the history of particle physics, general relativity, cosmology and astrophysics will be particularly welcome. However, *invited and contributed papers on any topic related to physics history will be considered for inclusion.*

We are very pleased to announce that Professor Dame Susan Joyceln Bell Burnell will give the keynote speech to the conference.

The conference is organised by a local committee chaired by Professor Denis Weaire (TCD) and a programme committee chaired by Dr Cormac O'Raifeartaigh (Waterford Institute of Technology).

Further details such as programme, speakers, registration etc. will be posted on the website: http://hop2020.iopconfs.org/Home

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