Institute *of* **Physics**

History of Physics Group

Newsletter

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Contents

Editorial

I'd like to open Newsletter Number 14 by welcoming our new chairman, Dr. Ian Butterworth. He takes over from John Roche, who is enjoying an extremely well deserved rest after 14 years running the group. See page X for a personal appreciation.

You are amongst friends! The Group's membership has risen from 400 to 500 over the last year. This is a fantastic increase, so thank-you to everyone who recommended the Group to a friend; and to those 100 new members: you are very welcome to join us. Our website is also the most popular of all the Group sites – congratulations are due to Mike Thurlow, who writes about the website on page 9.

We have two feature articles, based on two evening lectures given to the group, one in the past year, and one from a few years ago. On page 10, Howard Watson gives us a taster of his research into Physics Pubs of Britain in his investigation of <u>What's in a Name?</u> After that, you can read John Mallard's personal account of the development of MRI at Aberdeen University, starting on page 19.

We held two longer meetings over the year, on <u>Time</u> (starting on p. 28) and on <u>Quantum Concepts</u> (starting on p. 37). I hope you enjoy the wide scope of the varied contributions.

And finally, a word of advice for computer users – backup your documents now!. You may have noticed that this newsletter is slightly later than usual. Due to a complicated chain of events, I managed to remove all the spaces between the words in this Newsletter. I do not recommend re-inserting word spaces as a pastime. You have been warned …

Lucy Gibson

Dates for your diary:
- Saturday 12th May
- one Saturday in October
- an evening meeting (date TBC)

History of Physics Group Committee:

Chairman	**Professor Ian Butterworth** The Blackett Laboratory, Imperial College Prince Consort Road, London SW7 2BW i.butterworth@ic.ac.uk
Hon. Secretary & Treasurer	**Ms. Sophie Duncan** The Science Museum, LONDON, SW7 2DD s.duncan@nmsi.ac.uk
Newsletter Editor	**Mrs. Lucy Gibson** 56 Priory Avenue, LONDON, E17 7QP lucy.gibson@bbc.co.uk
Web Pages Editor	**Dr. Mike Thurlow** Dept. Physics & Astronomy University of Manchester, Oxford Road MANCHESTER M13 9PL mike.thurlow@man.ac.uk
Also:	**Mr. C. N. Brown** **Prof. R. Chivers** **Ms. O. Davies** **Dr. C. Green** **Mr. S. Leadstone** **Mr. H. Montgomery** **Dr. C. Ray** **Dr. P. Rowlands**

Chairman's Report

Given to the Group at the AGM, October 21st 2000

Introduction

As incoming chairman, my report is, of course, based on information given to me by the committee members.

Membership

This has now risen to 505 members (from around 400 last year), which is quite respectable for a group which is not one of the big battalions.

Administrative Matters

John Roche, together with other acting and retiring chairmen of groups, on 7th September had a meeting with Dr Alun Jones, Chief Executive of the Institute of Physics, and with various other officers of the IOP. Two matters of interest to our group emerged at this meeting:

(a) They pleaded with us to run a meeting of our group during the Annual Congress of the IOP. We have co-operated with the congress from time to time in the past, but not on a regular basis.

(b) We now have the freedom to use our funds as we please – provided it "serves the interest of the Group". This means that we can run one day meetings if we wish without having to charge heavy registration fees to our members and prohibitive fees to non-members. This is good news, indeed, for the Group. With an annual budget of £4000 (£1500 + an additional £5 for each member) we ought easily to be able to run two one-day meetings.

The IOP has received a gift of four 78 rpm records of lectures by Sir William Bragg. We have been given a tape recording and asked if we have any suggestions concerning what can best be done with the records.

Public Meetings

On the corresponding day last year, on 23rd October 1999, Stuart Leadstone, with the co-operation of Willem Hackmann and the History of Science Museum, Oxford, ran a meeting in Oxford to celebrate Volta's discovery of the electrochemical battery. Since the museum there was closed for refurbishment the meeting was held in the History Faculty Building. Appropriately, two Italian historians of physics gave interesting lectures about the work and travels of Volta. Neil Brown has given a very full account of the meeting in the latest (Spring 2000) issue of our Newsletter.

On Saturday 26th February 2000 our Honorary Secretary Sophie Duncan, with the help of Neil Brown and colleagues at the Science Museum organised a meeting on Time and its measurement - to celebrate the end of a millennium. The meeting became very lively indeed when Einstein's theory of time was raised - in particular the interpretation of the twins paradox.

On June 12th 2000, Howard Watson gave the group the benefits of his extensive research into pubs with connections to Physics. This was followed by a very enjoyable meal in a local pub.

Planning Meeting (Committee)

On Saturday 4th December 1999 the Committee had a very productive planning meeting hosted by Sophie Duncan in the Science Museum. Nine committee members were present. The convenors of various meetings planned for 2000 reported the progress in organising the meetings - in particular the meeting on Time in February 2000 and the concept meeting on Quantum Theory in October 2000.

Newsletter and Website

Lucy Gibson brought out her second issue of the Newsletter (Spring 2000, No.13) and Mike Thurlow again put it on the website. This was even better than the first Newsletter, absolutely packed with interesting information, and with a high standard of editing and production. Lucy and Mike are to be particularly congratulated on creating a website and contents which have received the highest number of "hits" this year.

Elections

John Roche and Neil Brown retired as chairman and honorary secretary at the AGM in 1999. I - although I was unable to be present - was elected chairman (without opposition, I understand!) and Sophie Duncan, Honorary Secretary. I am delighted that our link with the Science Museum is maintained. Indeed, it should be strengthened since Sophie and I are now near neighbours.

I would like to take this opportunity to thank John Roche and Neil Brown for their services as Chairman and Honorary Secretary to this group.

Ian Butterworth, Chairman

Dr John J Roche: a personal appreciation

Stuart Leadstone
History of Physics Group Committee

In the late 1970s, just before a mid-career teaching contract overseas, I was encouraged to join the Institute of Physics - a organisation which, until then, I had perceived as being for "professional physicists" only. Thus fortified by association with this prestigious institution I set off to ply my pedagogic trade in South-East Asia for four years. Having always hankered after clarification of the concepts of Physics by a return to their origins, I wrote to the Institute of Physics to ask for information regarding its activities in the History of Physics. To my dismay I was told that no formalised activities of this nature existed. No doubt my sense of let-down was exacerbated by my remoteness from my European roots, but my disappointment was real enough. Was there no one "out there" in the community of physicists who cared about the origins of our discipline?

We move to 1984. I am back in the United Kingdom teaching physics in a sixth-form college in South Wales. Out of the blue there appeared in *Physics Bulletin* an invitation to interested parties to attend an exploratory meeting at the Royal Institution to discuss the formation of a History of Physics Group. The date of the meeting was 1st February 1984, and the convener was John Roche. By July of that year the new Group was formally recognised by the Institute, and it was nurtured from its inception by John Roche. He not only shouldered the double burden of Secretary and Treasurer in the early years, but was inspirational in generating ideas for meetings and in encouraging others, myself included, to organise and contribute to meetings. More recently John has also served a term as Chairman of the Group.

My personal debt to John is beyond quantification. He brought a unique combination of scholarship, enthusiasm and grace to bear on all he did, and this very much forged the character of the group. A clinical listing of our meetings and activities would not do justice to the Group's significance.

In an age when learning, rigour ad culture have become *outré*, John has provided an invaluable restorative influence through the Group which he has seen through birth, adolescence and towards maturity.

As he withdraws from “official” involvement in the running of the Group we thank him most warmly for his inspiration and faithfulness over the 17 years of our existence. We look forward to the keeping up of his unequalled record of attendance at meetings and we wish him well in his continuing intellectual and spiritual quests.

Notes

1. John’s own summary of the first decade of the History of Physics Group can be found in the Group’s *Newsletter No. 8* (Spring 1994).
2. Two articles which very much give the flavour of John's *Weltanschauung* are:

J. Roche	History as Surgery	*Phys. Bull.*	**35**	414-415
J. Roche	Some Useful Fictions	*Phys. Educ.*	**7**	506-508

Disclaimer

The History of Physics Group Newsletter expresses the views of the Editor or the named contributors, and not necessarily those of the Group nor of the Institute of Physics as a whole. Whilst every effort is made to ensure accuracy, information must be checked before use is made of it which could involve financial or other loss. The Editor would like to be told of any errors as soon as they are noted, please.

Our Website

Dr. Mike Thurlow
University of Manchester

We are a group that, by definition, looks to the past but for the past couple of years I have been developing a website which to some extent would reflect the activities of our group. This has been fairly successful but the limit of what could be achieved had, I believe, been reached. At about the same time things began to change. . . !

The software used for maintaining the web pages was upgraded. It now allows us to develop the site in ways that are tuned to the needs of our group. We will, for the first time, be allowed to include images (subject to copyright) and have the means to create pages for whatever purpose we deem fit.

Over the coming months I am planning to introduce new features, many of which have been suggested by group members. For example, the agenda for the AGM and minutes of the previous meeting will be made available on line so that members who wish to contribute but cannot physically attend will have their opinions heard. Please let me know what features you would like to see.

I would also like to improve the physics content of the site with improved links to sites of interest. I occasionally trawl through dozens of web pages to update these but I would welcome any suggestions. Accessible to anyone with a connection to the World Wide Web, the site is the public face of the group. Our pages are the most visited of any group in the Institute and are linked to by sites of related interest throughout the world. We have high standards to maintain but with active support we can develop a site which shows the dynamism of our group.

The Group's Website:
www.iop.org/IOP/Groups/HP/

What's in the name?

Howard Watson
Chair, South Central Branch, IoP

How did this search for 125 pubs start? Several of us were talking about various ways of celebrating the 125th Anniversary of the Institute. Someone wondered whether there were 125 pubs in this country, which have connections with physicists. And even if there were not the direct link, would an accidental one be acceptable? It appealed to me especially because of the possible historical connections. As a teacher of physics I was always interested in the man or woman behind the discovery and conveyed the details to my students, even though it was not necessarily in the syllabus. Sometimes they found the history of the physicist more interesting than the law he propounded. We had no trouble in finding 125 pubs. The names were of three kinds:

Names of objects with scientific connections

I did a lot of my research in the early part of the total eclipse year, so I was interested to discover there are several *Eclipses* around. One is in The Square at Winchester and there *was* another one at Egham in Surrey. Imagine my disappointment in the total eclipse year to discover that it had been changed to *Café Uno*! There is also one at Tunbridge Wells, but here it refers to the name of a stage coach. There is also an *Electric Arms* at Portsmouth. The name probably originated from those pubs that were the first to have electric light in the mid 19th century. There is an *Air Balloon* at Horley and Portsmouth and a *Rainbow* at Cowplain in Hants. *The Surveyor* at Hersham portrays not an object, but a reference to a profession that makes use of principles of physics.

Accidentally named

I particularly enjoyed researching those with a name that accidentally linked with a physicist. There is the *Brewster* in New Malden, Surrey. Now, of course it has nothing to do with the physicist of that name. It was originally a woman who brewed beer. I immediately thought of Brewster's fringes, or perhaps better known, Brewster's Law. There was one in the south east actually dedicated to the physicist, but despite vigorous efforts by a fellow member of the History of Physics Group to locate it, so far it has eluded him.

David Brewster was born at Jedburgh, in the Scottish borders in 1781. He studied for the ministry of the Church at Edinburgh University, but after completing the course, left for science. He published almost 300 papers, mainly to do with optical measurements. He was most famous for his work on the polarisation of light, discovering his law in 1813, and making important discoveries about double refraction. What is not so generally known is that he also invented the kaleidoscope (1816). He was professor of Physics at St Andrew's University and became Principal of Edinburgh University in 1859. David Brewster received a knighthood at the age of 51. He died in 1868 at the age of 87.

There is another one of these accidental connections in the south. It is the *Hamilton Arms* at Stedham in West Sussex. The Hamilton I have in mind is **Sir William Hamilton** who was born in Dublin 1805. He was undoubtedly a child prodigy in Mathematics, but his ability was also shown in the learning of a large number of languages. In 1823 he entered Trinity College, Dublin and four years later at the age of 22 was appointed professor of astronomy and Astronomer Royal for Ireland, so that he could do research unhampered by teaching commitments. The very same year (1827) he produced his first original work on the theory of optics – *A Theory of Systems of Rays*. In 1832 he did further theoretical work on rays and predicted conical refraction under certain conditions in biaxial crystals.

In dynamics he introduced what have come to be known as *Hamilton's equations* – a set of equations describing the positions and moment of a collection of particles. The equations involve the *Hamiltonian function*, which is used in quantum mechanics.

Towards the end of his life he drank incessantly, dying of gout in 1865 at the age of 60.

One of my favourites is the group of pubs with the name *Cavendish*. There is the *Cavendish Hotel* in Bakewell, Derbyshire; there are the *Cavendish Arms* in Grange-over-Sands, Cumbria, and again in Brindle, Lancashire. I like them because of their accidental reference to the Cavendish Laboratory at Cambridge. I say "accidental", but probably many of the pubs with the name "Cavendish" included do derive from the original Cavendish estate, which was responsible for endowing the Cavendish Laboratory.

Henry Cavendish was born in 1731 in Nice, his father, himself a Fellow of the Royal Society, was administrator of the British Museum. Henry went to Cambridge University, but left without a degree after four years. However, he devoted the rest of his life to science. He inherited from his uncle a vast fortune with which he built up a large library and financed his scientific work. He was an eccentric recluse, appearing only rarely in public. He communicated with his housekeeper by a system of notes and ordered all his female domestics to keep out of his sight. Much of his work was unpublished during his lifetime, but he is now known to have come very close to or anticipated major discoveries. For example, the clear distinction between electrical quantity and potential, the measurement of capacitance and the anticipation of Ohm's Law (1781). Ohm himself produced his work in 1827.

He died in 1810 and left over a million pounds to his relatives. From this came the endowment of the Cavendish Laboratory at Cambridge in 1871.

In Islington in North London, there is the *Compton Arms*. Someone, like me a keen follower of cricket, suggested that it was named after the famous England Test Cricketer, Denis Compton, who came from that area, but unfortunately that is not so. Neither is it named after **Arthur Compton,** the American physicist. His father was a professor of philosophy, while his brother, Karl, was a physicist, and became president of the Massachusetts Institute of Technology. The main part of Arthur's career was spent at the University of Chicago where he was professor of Physics from 1922 to 1945. He is best remembered for the discovery of the effect named after him, for which with C T R Wilson he shared the Nobel Prize in 1927. He was investigating the scattering of X-rays by light elements such as carbon and found that some of the scattered radiation had an increased wavelength, an increase that varied with the angle of scattering. According to classical physics this was impossible. Compton then assumed that X-rays behave like particles, losing some of their energy on collision. The photon would thus have less energy, the frequency would be lower and hence the wavelength greater.

My last mention of the pubs with an accidental connection with physicists is *Young's* in the Lower Richmond Road in Southwest London. It has absolutely no connection with **Thomas Young** who is one of my favourite characters in the physics pantheon. We all remember him for the double-slit experiment, but he did much more.

Born in 1773, he is often described as a physicist, physician and Egyptologist. A child prodigy, he could read with considerable fluency at the age of 2. By 13 he had good knowledge of Latin, Greek, French & Italian. He made various optical instruments. He began an independent study of Hebrew, Chaldean, Syriac, Samaritan, Arabic, Persian, Turkish and Ethiopian languages. By 19 he had mastered Newton's *Principia* and *Optics* and Lavoisier's *Traité élémentaire et chimie.*

In 1793 at the age of 20 he began a medical education, starting first at Bart's in London and then going onto Edinburgh (1794), Göttingen (1795), and Cambridge (1797). In 1800 he received a considerable inheritance and set up a medical practice in London. This never really took off and in 1801 he was appointed professor of natural philosophy at the Royal Institution. Again his lectures were not successful, because they were too technical and detailed for popular audiences and compared unfavourably with his colleague, Humphry Davy. In 1803 here signed his post, but held from then on a number of medical and scientific posts.

Early researches were concerned with the physiology of the eye. He was elected FRS in 1794 (age21) for his explanation of how the ciliary muscles change the shape of the lens to focus on subjects at different distances; and in 1801 he gave the first descriptions of astigmatism and colour sensation.

His most lasting contribution was helping to establish the wave theory of light. He criticised the ideas of Newton and Huygens for their corpuscular theories. He introduced the idea of interference of light, which he explained by the superposition of waves. His best-known demonstration was using the double slit. His views were badly received in England, where opposition to Newton's corpuscular theory was unthinkable. In 1816 his interest in optics was revived by the work of Arago and Fresnel. In a letter to Arago he suggested that light might be propagated as a transverse wave. This allowed polarisation to be explained in the wave theory and gave an explanation of the known optical phenomena. He also gave physical meaning to the constant of proportionality in Hooke'sLaw, which gave rise to it being called *Young's Modulus*.

Directly named

What about the *Fahrenheit* in Woking, Surrey? Admittedly, it is the *Fahrenheit & Firkin,* but that's good enough in a rather poor field of directly named pubs. **Daniel Fahrenheit** was born of German parents in what is now Gdansk, Poland, in 1686. He emigrated to Amsterdam and became a glassblower and instrument maker. Galileo had invented the thermometer in 1600, but Fahrenheit was the first to use mercury as an indicator. His lower fixed point was taken from a mixture of ice and salt, but his upper one was the temperature of the human body, which he put at 96^{o}.

The *Farenheit and Firkin* in Woking, Surrey.

Still in Surrey is the *Armstrong Gun* at Egham, named after **William George Armstrong** who was born in 1810 in Newcastle-upon-Tyne. He was an industrialist and engineer who invented high-pressure hydraulic machinery and revolutionised the design and manufacture of guns. I did not rate him very high in my list of candidates until I happened to visit the house and estate he created at Cragside, Northumberland.

He abandoned his Newcastle law practice in 1847 to devote his time to scientific experimentation. He founded an engineering works to build hydraulic cranes. Because his hydraulic machinery was dependent for power on water mains or reservoirs, he invented, in 1850, a hydraulic accumulator. It comprised a large water-filled cylinder with a piston that could raise water pressure within the cylinder. Thus machinery such as hoists, capstans, turntables, and dock gates could be worked in almost any situation. He next improved gunnery for the British army.

The hydroelectric scheme he developed on his estate enabled him to light the house with electricity, the first home in this country to have that distinction. The then Prince and Princess of Wales in making a tour of the northeast, chose to stay at Armstrong's house rather than the splendid castle at Alnwick which did not have this and other mod cons! Armstrong was elected a fellow of the Royal Society in 1843. He died at his beloved Cragside just after Christmas in 1900.

There is one pub dedicated to ***Sir Isaac Newton***, in Cambridge. It is disappointing that there is not one in Woolsthorpe, near Grantham in Lincolnshire where he was born in 1642. His father, owner of the manor, died three months before Isaac was born. He was left by his mother in the care of his grandmother. He was quiet, unwilling to play with the village boys but was interested in making things. His mother returned to Woolsthorpe in 1656 after the death of her second husband. After two years helping his mother with the family farm, he went to Cambridge University in 1661, where he stayed for nearly forty years. He was forced to return to Woolsthorpe in 1665 because of the plague. Here he began to develop the ideas for which he is so famous: about gravity and also about optics, grinding his own lenses. He also worked out his mathematical ideas of the calculus.

The *Sir Isaac Newton* in Cambridge

When here turned to Cambridge in 1667, he was elected Fellow of his college (Trinity) and, in 1669, Lucasian professor, the post Stephen Hawking has today. He served as an MP for the university for 1689-90 and 1701-2: although not politically very active, he presumably saw it as a way of gaining influence. Charles Montague, who introduced him to court and society circles, influenced his public career. When Montague became Chancellor of the Exchequer, he offered Newton the post of Warden of Royal Mint in 1699. From this time he did virtually no science, except for publishing and revising works already written. He did concern himself with the affairs of the Royal Society, of which he became president in 1703.

At the Mint his first task was to supervise the great re-coinage and also to pursue counterfeiters. He regularly visited suspects in Newgate and other prisons. Between June 1698 and Christmas 1699 he interviewed 200 witnesses on 123 separate occasions. In the same period 27 counterfeiters were executed. The Mint made Newton a wealthy man. Atthetimeofhisdeathhehadaccumulated£30,000 - probably several million in today's currency.

Much of his later life was spent in priority disputes. These arose through his reluctance to publish his own work. A dispute began in 1700 when Leibniz objected to Newtonians referring to him as the "second inventor of calculus". Leibniz asked the Royal Society to conduct an enquiry into the matter in 1712. Newton behaved quite shamelessly. He appointed the committee, decided what evidence it should see, and actually drafted the published report himself. Later, he appealed to the report as an independent justification of his position.

Throughout his life he displayed a keen interest in religion. At his death over 1000 manuscript pages and two completed books, all devoted to religious matters, were discovered. He was a Unitarian, a belief he kept fairly secret, as it would have excluded him both from the Mint and his Lucasian chair. He died in 1727 at the age of 85 after a short illness.

Another notable physicist to have a pub named after him is **Lord Kelvin**. There is one in King's Lynn, Norfolk, but as far as I know neither in his country of birth, Northern Ireland, nor in Scotland with which he was associated for over half a century. William Thomson Kelvin, born in Belfast in 1824, was an extremely precocious child who matriculated at Glasgow University at the age of 10. He went to Cambridge and then returned to Glasgow as professor of natural philosophy, a position he held for 53 years. Throughout his life he was a devout member of the Scottish Free Church.

His work on electromagnetism is second only to Faraday and Maxwell. With Faraday he was responsible for the concept of the electromagnetic field. Particularly important is a fundamental paper of 1847 in which Kelvin drew an analogy between an electrostatic field and incompressible elastic solid. Other innovations included the use of vectors to represent magnetic induction and magnetic force.

His other great area of work was, of course, thermodynamics. In 1852 he set out the fundamentally important law of conservation of energy. He also collaborated with Joule in experimental work. One of the important results of his work was the concept of absolute zero and an absolute scale of temperature. He died in 1907 at the age of 83 at Largs in Ayrshire.

I mentioned Joule. There is not a pub named after him but there is one that was originally owned by the Joule family. It is in Chorlton Street, Manchester. Its present name is *Paddy's Goose.* It was because of the family wealth derived from a brewery that **James Prescott Joule** was able to devote himself to a life of scientific research involving precise measurements. He is reputed on his honeymoon to have measured the difference in temperature between the top and bottom of a waterfall. His best-known work was done in collaboration with Lord Kelvin, *viz*. The *Joule-Kelvin Effect.* It is perhaps particularly appropriate for this lecture that he remained a brewer all his life, showing that beer and physics do probably mix.

The Copthorne Hotel at Dudley, Birmingham has a bar called *Faraday's*. Pictures inside of old physics apparatus and of the man himself, certainly rate it for inclusion. **Michael Faraday's** father was a blacksmith who suffered from poor health. Brought up in poverty, his education, such as it was, ended at 13. He began to work for a bookseller and binder. His interest in science seems to have been aroused by his reading the 127-page entry on electricity in an *Encyclopaedia Britannica*. It stimulated him to perform some simple experiments. The turning point in his life came when he attended some lectures at the Royal Institution in 1812 given by Humphry Davy. He took very full notes that he bound himself. Luck was with him for Davy had an accident and needed some temporary assistance. It was while working with Davy that Faraday showed him the notes he had made. Faraday was to spend the rest of his working life at the Royal Institution. In 1815 he was promoted to the post of superintendent of the apparatus of the laboratory. Ten years later he became director of the lab. In 1833 he was elected to the newly endowed Fullerian Professorship of Chemistry at the Royal Institution. He had earlier turned down the chair of chemistry at University College, London.

Unlike Newton and some others, he wasn't in the money. He made up for the paucity of his salary at the Royal Institution with a part-time lectureship at the Royal Military Academy, Woolwich. Some moves were made for him to have a government pension. He didn't get on with the Prime Minister, Lord Melbourne, who called such pensions 'grosshumbug'. This caused Faraday to refuse the pension. He was a great refuser throughout his life: the chair at UC; he also refused a knighthood and, what is beyond belief, the presidency of the Royal Society–not once but twice! However, there was a happy sequel to the pension business. In 1835 Melbourne apologised, enabling him to accept the pension. From this time on he was much more comfortable financially.

If his financial troubles were over, others weren't. He suffered some kind of breakdown in 1841 and went into the country to rest. In 1844 he resumed his experiments, but by the 1850s his creativity was indecline. He gave his last children's lectures in 1860 and resigned in the following year. He then took up residence in a house at Hampton Court, made available to him by Prince Albert, the consort of Queen Victoria.

This is only a small selection from the material I give in my talk. But I hope it is enough to give a flavour of what I try to do.

The Group's Website:
www.iop.org/IOP/Groups/HP/

The Pioneering of Magnetic Resonance Imaging in Aberdeen*

Professor John Mallard
Aberdeen University

In 1965 I came to the newly created Chair of Medical Physics in Aberdeen: this was the first established one in Scotland. Magnetic Resonance Imaging in medicine began to be conceived when I saw, for the first time, that my know ledge of magnetism, acquired during my early PhD research, might have medical applications after all!

In the early 1970s we turned to Nuclear Magnetic Resonance (NMR), influenced by the work of Damadian in the U. S. A. who began measuring the nuclear magnetic resonance parameter T1, called the spin-lattice relaxation time of the water protons (the hydrogen nuclei) in tissue samples. We found that T1 times were longer for malignant tumour tissue than for normal tissue, but rather less so than the measurements reported by Damadian during that period: the differences between different tissues were related to the water content of the tissues, which varied by about 10%, and the brain showed white matter 40% less than grey matter. This suggested that imaging by NMR might show up the different organs of the body, with grey brain appearing different from white - a distinction hitherto not possible by imaging - as well as possibly showing tumours; and inflammation, which would belong due to its high water content. It became possible to predict what a rabbit T1 image might look like and even a human sectional image was drawn predictively! It turned out to be surprisingly accurate!

In1973, Lauterbur in the U. S. A. suggested away of forming an NMR image. This was that, as well as placing the sample in a uniform standing magnetic field to excite the resonance; another magnetic field is to be applied so that a gradient in field strength is created across a sample, so that one side of it was in higher standing field strengths and, therefore, the protons resonating at higher frequencies than the protons on the other side. Thus the NMR signals from the sample have lower frequencies from the protons on one side and higher frequencies from protons on the other

* Professor Mallard gave a talk entitled The Evolution of Medical Imaging – from Geiger counters to MRI – a personal saga to the History Group on a talk given to the Group on Monday 14th September 1998. This text is adapted with kind permission from a longer article: The Pioneering of Medical Imaging in Aberdeen, *Aberdeen University Review*, LVI, no. 195, Spring 1996.

side. Thus position is coded by frequency. Also, at the position where there is a greater concentration of protons, it will show a stronger signal at the frequency of that position, so that there is a frequency spectrum across the sample related to position. By compounding several spectra taken with the magnetic field gradient at several different angles around the sample, an image could be reconstructed by computed tomography, which was well-known by then.

We were already to the fore in computed tomography, and therefore had all the computer programmes to hand, so we were able to very quickly explore the possibility of NMR imaging. We obtained our first image of a whole mouse in Aberdeen in March 1974 by this method (Fig. 1): it was very exciting and it was a world-first! The mouse was outlined by the proton concentration signals, whilst the liver and brain were shown by the mean T1 values at each point (or pixel, the nickname for picture element) within the outline, averaged over the thickness of the animal. To our astonishment we also saw on the image the very long T1(black) of the inflammation surrounding the broken neck which had to be inflicted to ensure that the animal was completely stationary during the hour necessary to build-up the image. Our first image had shown a sort of pathology!

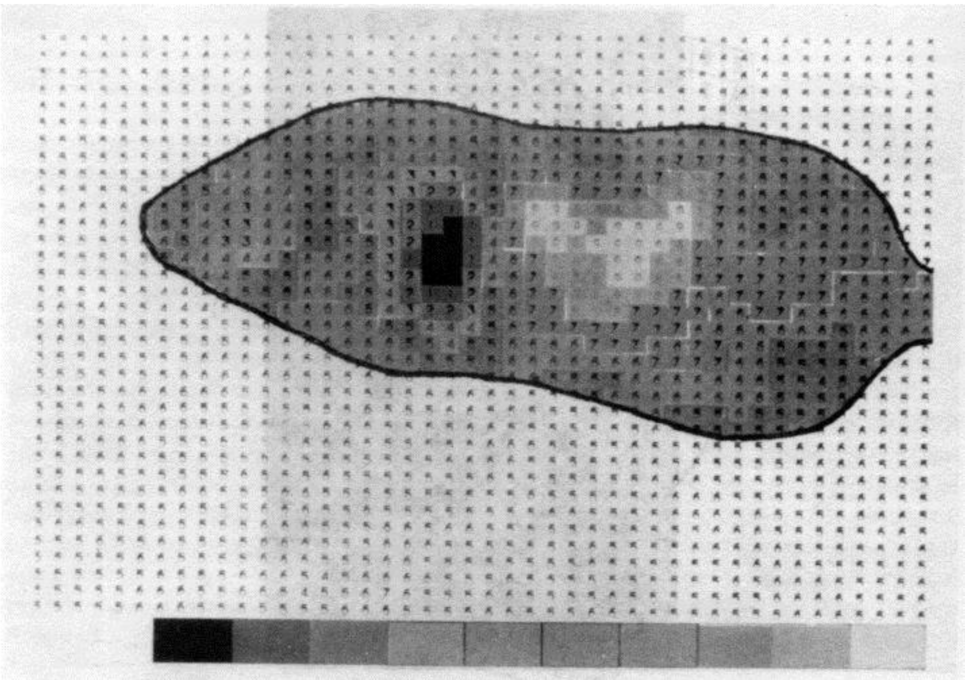


Fig. 1: The first-ever NMR image of a mouse displaying relaxation time information. The liver and brain are seen, and, as is the neck (in black, see text)

My experiences so far had convinced me that if a new imaging technique was to be really useful clinically, it had to image any part of the body - the trunk, as well as the limbs or the head, which, because they were smaller, were easier to achieve. Also, because of the success of the mouse image, it needed to measure the relaxation times from point to point of the image. So, in contrast to most of the other teams, we in Aberdeen decided in 1974 to build a whole body imager, which could image T1, as well as proton concentration. There were times during the next six years until 1980 when this seemed to be a mistake, because the others seemed to be leading the way, but in the end, this strategy was to prove absolutely right!

The others also searching for magnetic resonance imaging included three competing teams in the University of Nottingham Department of Physics!
- one led by Professor Raymond Andrew, who produced the first image of a wrist in 1977
- one led by Peter Mansfield, whose team patented the method of selectively exciting and defining a slice in 1974 and obtained a finger image in 1977, a cross-section of an abdomen in 1978, and who later concentrated on echo-planar fast pulse sequences
- and one led by the late Bill Moore, who built a whole body imager in the early 1980s.

There was also a team led by Ian Young at GEC, London (this company had bought-over the medical imaging programme of EMI, whose early world-wide success with X-ray computed tomography imaging, led by Godfrey Hounsfield, had failed business-wise by the late '70s), who first imaged a human head in 1978.

In addition, there was Raymond Damadian's team in New York, who produced the first human thorax section in 1977.
Although these images were interesting as a laboratory experiment, none of them were anything like good enough to be of use for clinical work.

A prototype was gradually built in the Medical Physics building at Foresterhill, Aberdeen. The magnet needed to image a body trunk was large and a uniformity of field strength of 1 in 10 was vital. Whilst iron-cored and even permanent magnets seemed attractive there were unknown effects of induced eddy currents, and the estimated 6-tonne weight would overload most hospital floors, and did not seem to be the basis for a practicable prototype at that time! The favourable configuration of a vertical field of 0.04T (400 gauss) was the highest we dared to go in order to achieve the uniformity needed at that time. Oxford

Instruments Ltd. Built it as their first one-off and it consisted of four horizontal coils which were air-cooled. Special designs had to be evolved for the coils which carried the pulses of electric current to provide the magnetic field-gradient along the length of the patient (to select the plane of imaging across the patient); and for the magnetic field gradients across the patient from left to right (to position the signals across the patient); and through the patient from back to front (to position the signals from back to front).

By 1979, images were being obtained of volunteers, (including the author!), which were of recognizable shape and showing some internal anatomy, but having serious interfering artefacts due to body movements, such as heartbeats, which made the images of little use for medical purposes. These artefacts were not eliminated until the early months of 1980, when we developed and introduced the very first of the two-dimensional Fourier transform methods, which we nicknamed "spin-warp" imaging.

The spin-warp technique was the real breakthrough for magnetic resonance imaging. There followed a period of imaging ourselves as volunteers, so that we could learn to understand just what was being imaged in each plane across the body. Realistic images with startling anatomical details were seen of soft tissues, which could not be seen easily by X-rays or other techniques.

The first patient was imaged on August 26th 1980 (Fig. 2). Abdominal images which showed details of immediate clinical usefulness of the primary malignant disease (carcinoma of the oesophagus), and its spread to the liver with massive secondary deposits (seen as white), and to the bones of the spine which had not been suspected before in that patient, were obtained. It was a very exciting day! MRI had arrived and was clinically useful and of tremendous potential.

The machine was quickly in full use to investigate patients, who were selected and cared for by Dr F. W. Smith, the consultant in Nuclear Medicine at the Aberdeen Royal Infirmary, who was quick to see the clinical potential of this new method for diagnosis and who eagerly followed it up. It was found very quickly that it was superb for many things other than tumours e. g. multiple sclerosis. It was clear that NMR imaging gave superb contrast between the different soft tissues, a property not possessed by other imaging techniques. It was also unlikely to cause any hazard to the patient, unlike X-rays or radioactivity, since the energy of electromagnetic radiation used in MRI is nine orders of magnitude lower.

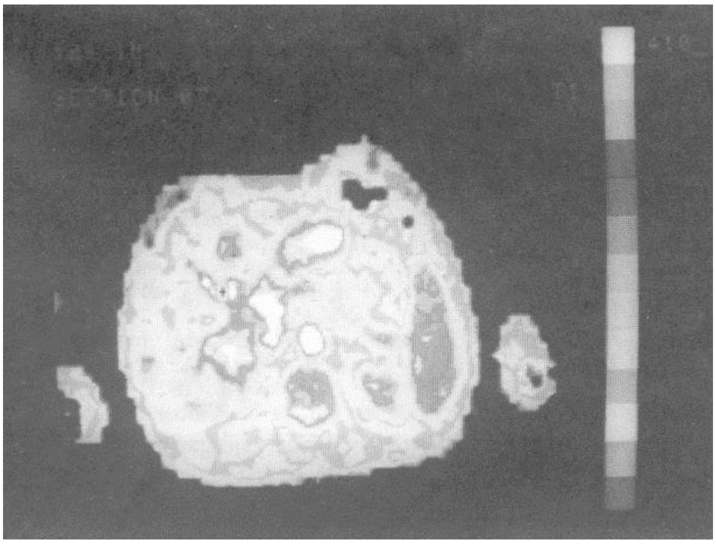


Fig. 2: The world-first clinically useful NMR image. It is an abdominal image which shows massive secondary malignant deposits of very long T1 (T1 > 500 msec) (seen as white) in the liver. The arms at the side of the body are also seen.

Because the machine was in a physics research laboratory several hundred yards from the Aberdeen Royal Infirmary, each of us had to act as porters in all weathers: there is a photograph of Dr Smith wheeling a patient into the Department in a wheel-chair with several inches of snow on the ground! We also had to create a patient waiting facility adjacent to the machine: to do this, the Medical School soap store had to be moved! Over 900 patients were imaged with this machine over 2½ years, which led to many papers being published describing world-first clinical series, each of which showed features which had either not been seen before or which were only acquired with difficulty by other diagnostic procedures. They caused much excitement in the medical fraternity, many hailing the new technique as the biggest step forward in diagnosis since the discovery of X-rays in 1895.

It was clear that our prototype machine was going to be so busy imaging patients that we could not work on it to improve it! In order to develop this technique, which had only just been born, we would have to build another machine! This we called NMR Imager Mark 2, and we needed about £300,000 to build it! Unfortunately, the Medical Research Council would not help us with a further grant, and U. K. industry seemed not to be interested(!) - the only one already in the field (GEC, London) had its own programme of development well on the way. The only way forward, reluctantly, was to accept a grant of £283,000 from the Asahi Chemical Company of Japan to build our Mark 2 and to provide them with our know-how. The agreement was drawn up in collaboration with the British Technology group, who were handling, on behalf of the Government, the Patent Applications which I had insisted be taken out on the various stages of our research, all funded by public monies sometimes with only the reluctant acceptance of the young scientists(who wanted immediate publication!). By the time that these negotiations were drawing to a close, I had been approached by a Sales Manager, whom I had known in early gamma camera days at Nuclear Enterprises Ltd. Of Edinburgh, to set-up a company in Aberdeen to manufacture our machine. The year of 1981 was frantic with the Departmental team beginning to build Mark 2, with three Japanese engineers at their side, learning and faxing the details to Tokyo; and trying to persuade the University, the Scottish Development Agency and financiers locally and in London, to support a company in order to try to keep some manufacture of this wonderful invention for the benefit of Britain: it was a hard slog. This company (M&D Technology Ltd.) was not created until early 1982 with a total investment of only £l.5m; by which time the multinational companies were well on the way with their prototypes, and with General Electric of New York, for example, spending £112m a year on developing it.

The Aberdeen Medical Physics Department team then built Mk. 2, which had a better spatial resolution, and operated at 0.08T (800 gauss): but, in the interests of speed to get it built was basically very similar to the Mk. 1. This machine was installed in a room especially built for it by the Grampian Health Board in the Royal Infirmary itself. It successfully imaged over 9,000 patients from 1982 to 1992, and in that year made way for an up-to-date commercial machine made by the huge Siemens multinational of Germany, operating at 1.0T (10,000 gauss) and with superb spatial resolution and startling soft tissue detail in the images (Fig 3). Over 18,000 patients have been imaged by MRI in Aberdeen at the time of writing.

The Aberdeen Mark 2 design formed the basis of the Japanese machine produced by Asahi, which was sold widely in Japan with over a hundred machines being installed during the 1980s. Asahi eventually joined up with Siemens to produce a much improved machine with a permanent magnet. The Mark 2 was also the basis of the M&D Technology Ltd. Machine in Aberdeen, of which three were installed, and used successfully, only being with drawn at the end of their useful life in the early ‘90s. The one installed at the Edinburgh Royal Infirmary is now in the National Museum of Scotland, and the one at St. Bartholomew’s Hospital is now in the Science Museum, London! Unfortunately, however, this small British Company, which had its premises in Mastrick, failed in 1985, largely due to under-capitalisation, and lack of long-term financial support and understanding by the City of London financiers, and others, which made it impossible for its machine to be redesigned to take account of the stupendous and unprecedentedly expensive products of the major multinationals: these were selling like hot cakes in wealthy countries such as the U. S. A. and W. Germany. Perhaps the biggest tragedy of this failure was the break-up of a tremendous team of thirty or so clever scientists, engineers and technical staff, carefully brought together to create a magnificent piece of modern high-technology: their potential was completely lost to Britain.

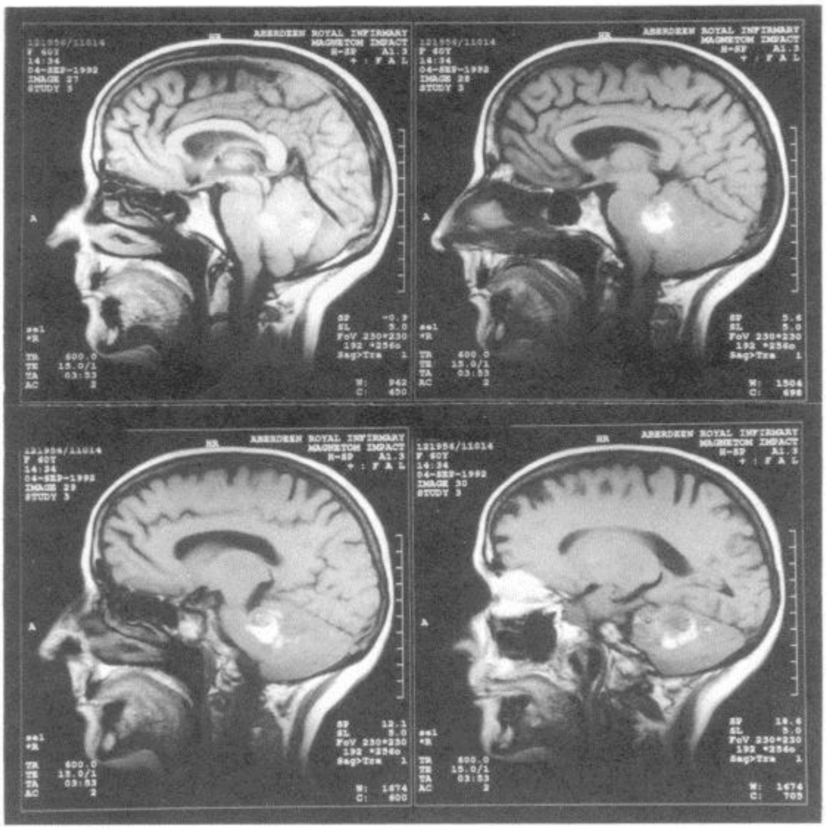


Fig. 3: Brain images taken in 1992 on a commercial machine at1.0T (10,000 gauss). They are four adjacent sagittal slices moving through a cerebellar tumour with oedema (seen as white).

It is necessary to go back to 1981 when GEC, London installed their prototype magnetic resonance imaging machine “Neptune”, operating at 1500 gauss provided by a superconducting magnet at Hammersmith Hospital in London. The superconducting magnet was made by the Oxford Instrument Company, and they created a wonderful niche in mastering this new technology, which set the trend of all the companies to higher magnetic fields. This gave greater signal strength, and enabled finer spatial resolution to be obtained in the images, which therefore showed greater detail. This was just what the radiologists were wanting, the images being more akin to the superb detail of their X-radiographs. The “supercons” also gave a better stability: they were quickly taken up by the competing multinationals, in spite of their great expense, at more than £500,000 a time in the beginning! By 1984 fields of 1500 to 5000 gauss became available, and fantastic detail of the soft-tissue brain structure became possible for the first time (Fig9): one by one the major multinationals came on-stream with superconducting magnet imagers giving up to 1 and 1.5T, but very expensive indeed at more than £lm a time at the beginning, and requiring special suites of room with high structural installation costs. The demand was high in the U. S. A, and wealthy countries, but much lower in Britain and poorer countries! By 1985 many hundred imagers were in use in the U. S. A, Japan, and Germany whilst the U. K, which had developed it, had barely ten! By 1984 our clinical papers were being rejected by editors and referees, because they were not “state of the art”! The university teams in research laboratories were gradually pushed out of the further development of NMR imaging, due to the much greater financial and manpower resources that the major multinationals could bring to bear.

By 1991 there were many thousand machines worldwide, and by the end of the century more than 200 are expected in the U.K. The world is installing about 2000 machines a year, a market of well over £1,200m p. a. The Post Office recently issued an MRI postage stamp as part of a series on “medical inventions”.

Fortunately most of the U. K. research effort which led the world in the ‘70s and early ‘80s, was patented, mostly through the British Technology Group (BTG) because public money had funded the research in the universities. After lengthy and extremely expensive, million dollar lawsuits, skilfully conducted by the BTG in the middle 1980s, one by one the multinationals agreed to pay royalty fees which now bring in several million pounds annually to the U.K. Now that the BTG has been denationalized, it will be more difficult to persuade young scientists to delay publication until the patenting process has begun.

MRI is a very versatile technique with several quite different natural mechanisms for producing imaging contrast: these are the two relaxation times, TI and T2, the proton density, motion, and others, which vary from tissue to tissue and in disease, because the water is related to the biochemical constituents and macromolecules taking part in the living or disease processes. Flow provides another contrast. One can also inject paramagnetic contrast materials which change the magnetic moment of the protons and constituents, and in some pathological conditions increases the imaging contrast: compounds of rare-earth metals such as gadolinium are used, introduced in 1988, and improved clinical accuracy is reported. Very fast pulse sequences have been developed which give images in milli-seconds, and real-time movies of the beating heart.

The great medieval universities were founded in the Middle Ages (e. g. King's College, Aberdeen in 1495), after the very first experiments with lodestone were being carried out by Peter Peregrinus, an Italian military engineer, and the study of magnetism was beginning. One of the very first scientific books was *De Magnete* in 1600 by William Gilbert, a physician to Queen Elizabeth I of England. By way of Helmholz - a physiologist as well as a physicist - Faraday, and James Clerk Maxwell - who was for a time Professor of Natural Philosophy in Aberdeen - the magnificent development of MRI has come for the benefit of mankind!

It's About Time

Displaying Time

Neil Brown
Science Museum, London

This paper is a brief history of the instruments used to measure time. In its original form it was given as a talk at the Science Museum in London, and except where indicated the examples cited are original timekeepers or reconstructions in the Science Museum collection.

From the beginnings of civilisation to the Middle Ages

One of the features distinguishing man from the rest of the animal kingdom is a sense of time, and the earliest instruments for measuring time date from the beginnings of history. Both shadow clocks and water clocks have been found in ancient Egyptian tombs. The shadow clocks divided the period of daylight into twelve "temporal hours", which vary in length according to the time of year. The water clocks or *clepsydrae* are bowls that emptied slowly through a small hole, so that the level of water indicated the time elapsed since the bowl was filled. They could be graduated to show temporal hours by using different scales for different times of year. The water clock found at Karnack and dating from about 1400 B.C. is the best-known example.

Water clocks were probably the most common form of timekeeper from classical times right through the Dark Ages, and they lasted into the 19th century. There is a rare surviving example from about 1700 of a type known as a "falling drum" water clock. Water clocks had the major advantage of working at night or in cloudy weather. There were sun clocks as well. One tantalising surviving relic is a group of four fragments of a sundial and calendrical device dating from Byzantine times, the 5th or 6th century A.D. It is the second oldest geared mechanism known: only the Antikythera mechanism is older. The 11th century Saxon sundial on Kirkdale church in Northumberland is well known. It is graduated to indicate the four Saxon "tides" into which the period of daylight was divided.

The sand glass is another ancient type of timekeeper. It appears to be of medieval origin. Sand glasses were much used on ships, where other early timekeepers did not work. They were also used in churches to time sermons, and by the medical profession to time a patient's pulse. Fire clocks, like the oil-lamp clock that indicated the time from the level of oil remaining in the reservoir, were less common. A different type of fire clock is the Chinese incense clock, in which the fire wends its way round a maze so that the position indicates the time.

Equal-hours sundials

Few people up to medieval times were much concerned with the time of day, except priests, who wanted to know the times of prayer. A system of unequal hours sufficed for everyone except astronomers and astrologers (often one and the same). Unequal hours began to go out of use at about the same time as mechanical clocks came into use. Mechanical clocks measured equal hours but it would be rash to assume that this forced the change. It is at least as likely that whatever fuelled the desire for mechanical clocks also encouraged the change to the system of equal hours that we still use, and prompted the design of sundials to indicate time on this system. The earliest equal-hours sundials are portable ones from the 1450s. One of the earliest in the Science Museum is a complex silver sundial from the late 16th century made by Humphry Cole, one of the first British instrument makers. The number of different types of sundial is astonishing, and is a tribute to the knowledge and skill of renaissance craftsmen. Sundials range in size from the large dials in public places to portable instruments equivalent to a modern wristwatch. Like watches, many portable sundials were as important as status symbols as for their utility.

At night, it is possible to tell time from the position of the moon, using an instrument called a nocturnal. It was not terribly common, and was probably used most by sailors.

Early mechanical clocks

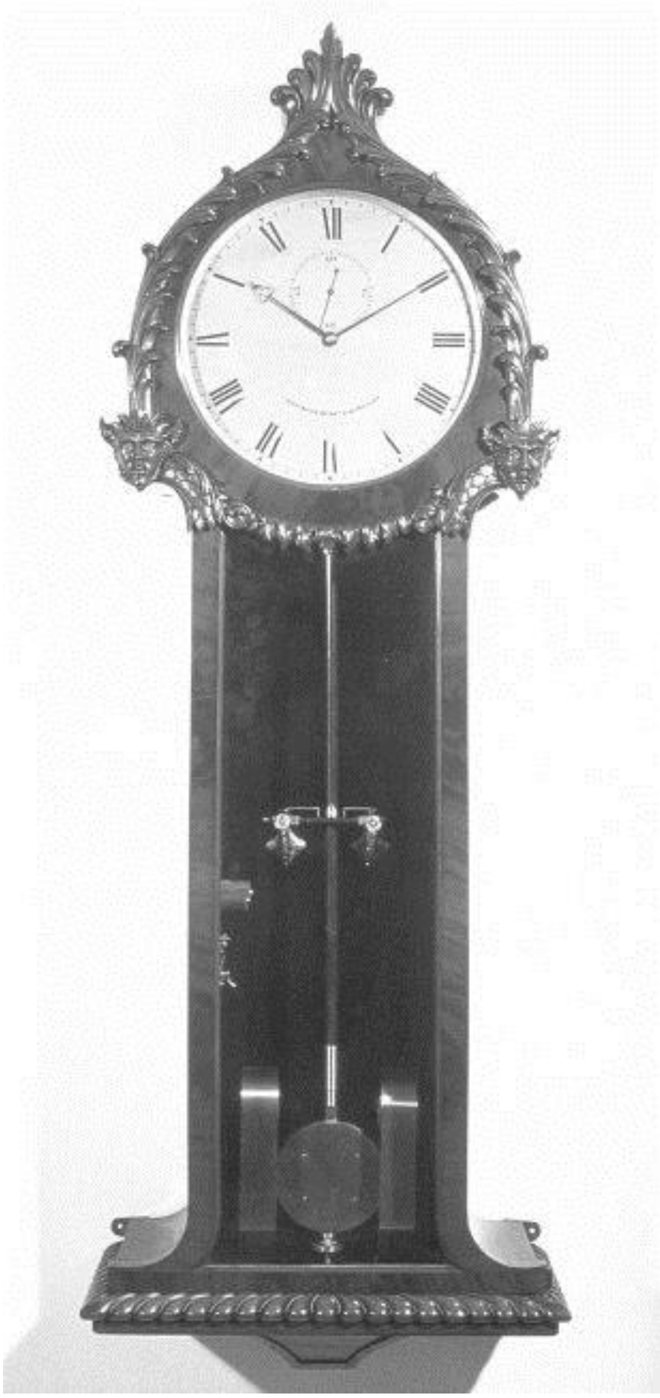
The future of time keeping was with mechanical clocks, but they could not displace other time keepers completely. Until the 19th century the only way of setting a clock right, outside an astronomical observatory, was to use a sundial. The railways created the need for a standard time throughout the country, and railway telegraphs were the first way of disseminating time signals from observatories such as Greenwich. Not until the BBC started broadcasting the six-pips time signal in 1924 could the time be checked easily anywhere in the country.

One of the earliest known mechanical clocks was built by Richard of Wallingford in about 1336. There is a reconstruction of his clock in a private collection. The other famous early clock is the astrarium completed by Giovanni de Dondi in 1364. It was an astronomical computer with dials showing the positions of the sun, moon and planets plus various other indications, as well as the time. The time keeping of all early clocks was governed by the oscillations of either a balance wheel or a weighted bar called a *foliot*. These had no natural period of oscillation. Their rate was affected by every fluctuation in the driving force so they were poor timekeepers, varying by as much as fifteen minutes a day. The clock found in an attic in Dover Castle, now thought to date from about 1600, is one of only two public clocks in Britain with the original foliot.

Britain is fortunate in having two of the oldest surviving public clock mechanisms. Both originally had foliot mechanisms, and both were later converted to pendulum control. One was installed at Salisbury Cathedral by 1386. It was restored in 1956, with a reconstructed foliot, and is now to be seen in the cathedral. The other was installed at Wells Cathedral by 1392, and has been on loan to the Science Museum since 1884. It has the pendulum and anchor escapement fitted about 300 years later, and a few missing pieces have been replaced. The astronomical dial that it used to drive is still in the cathedral, now driven by a more recent mechanism. It is impossible to be certain when these clocks were built or to what extent they areoriginal, but they are certainly old.

Pendulum clocks

The break through in clock design was the use of the pendulum. Galileo proposed a clock mechanism controlled by a pendulum in 1642. We know it from a surviving drawing, and reconstructions have been made from that. Nothing came of Galileo's design, but in 1658 Christiaan Huygens made a pendulum clock, and soon afterwards Saloman Coster began producing pendulum clocks to Huygens' design. Huygens knew that the period of oscillation of a pendulum is independent of the amplitude only for very small amplitudes. In 1659 he added cycloidal cheeks to the top of the pendulum to make the period the same at large amplitudes. This modification was used in some clocks made by Coster, but it was more trouble than it was worth. All the early Huygens clocks used short pendulums, and this was satisfactory for small domestic clocks.



The most significant development in the history of timekeeping: a reconstruction of Huygens' first pendulum clock, as illustrated in his 'Horologium' of 1658.

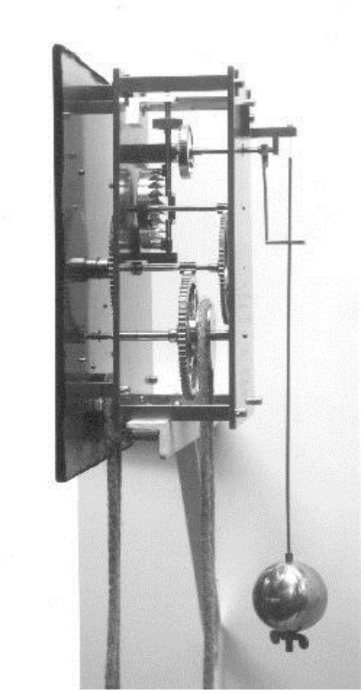
For greater precision a long pendulum is better, and soon came into widespread use. The essential complement to the pendulum, especially along pendulum, was the “anchor” escapement, because it worked with a smaller angle of swing than the verge escapement used in Huygens’ clocks, as in all earlier clocks. The anchor escapement is first seen in a clock made by William Clement in 1671. For precision clocks the “dead-beat” escapement introduced by George Graham in 1715 was superior. It worked much like the anchor escapement but was very precisely designed so that it impulsed the pendulum near its zero position and produced little frictional drag elsewhere. Clockmakers may have lacked formal schooling in physics, but the good ones had an excellent grasp of physical principles.

Balance spring clocks and watches

Huygens published another important horological innovation in 1675, the use of a spiral balance spring to control a balance wheel. Robert Hooke had tried a similar idea a little earlier but failed to bring it in to use. The combination performed the same function as a pendulum but could be used inwatches and portable clocks. The most famous balance-spring watch of all must be John Harrison’s fourth chronometer, that eventually won him the Board of Longitude prize for enabling sailors to calculate their longitude at sea. The Harrison chronometers are at the Old Royal Observatory, Greenwich, part of the National Maritime Museum .

Improvements after the pendulum

Most clocks made during the 18th and 19th centuries, including many by master clock makers such as Thomas Tompion, had straightforward anchor escapements. They are often more distinguished as pieces of furniture than for their clock mechanisms. Nevertheless, during that period there is a complex history of improvements in clocks for precision measurement. The clocks made by Shelton for the Royal Society for observing the transits of Venus in the 1760s are good examples of precision clocks. The Shelton clocks were used on many expeditions and one of them went with Captain Cook. The one now in the Science Museum collection was last used in earnest in the United States in the late 19th century, over a hundred years after it was made.



A fine electric wall clock by Alexander Bain, circa 1850. Bain has a good claim to the title of inventor of the electric clock

Most collections of clocks and watches concentrate on aesthetic aspects, but the Science Museum collection has a different emphasis and is strong in two distinctive areas. One is public clocks, and two early examples, the Dover Castle clock and the Wells Cathedral clock, have been mentioned. The other is electric clocks, starting with a beautiful wall clock made in about 1850 by Alexander Bain, who was the first person to patent an electric clock. From then on there was a multitude of different designs. The endemic problem was obtaining good electrical contact with minimum effect on the swing of the pendulum.

Free pendulum clocks

The best possible mechanical time keeper is a pendulum swinging freely. A clock pendulum cannot be totally free, but the less interference there is with its motion the better. Riefler, in Germany, designed a purely mechanical clock with an early free pendulum 1891, and Riefler clocks had become standard in major observatories. The ultimate pendulum time keeper was the "freependulum" clock devised by William Hamilton Shortt in 1924. It used two pendulums. The "slave" was the pendulum of an almost standard Synchronome electric clock, and it did all the work. Every thirty seconds it triggered an electrical mechanism that gave the free pendulum a very small impulse. As this mechanism reset it operated a synchronising device that gave the slave pendulum an extra impulse if it was a bit late. The Shortt free pendulum in the Science Museum is the prototype and is in a glass case instead of the usual evacuated copper cylinder, so it is not so precise but the pendulum is visible. Shortt clocks were accurate to a few milliseconds per day. A hundred were made, and most of them were used in observatories, but they only lasted about a decade because electronic time keepers took over.

In Russia, where electronics were not so advanced, Fedchenko designed a free pendulum clock that may have been superior to the Shortt clock. It was unknown in the western world, but there is now one at the Old Royal Observatory in Greenwich.

Quartz clocks and atomic clocks

Clocks using vibrating quartz crystals as the time keeping element began to appear at the end of the 1920s, and by the early 1940s they were taking over as time standards. They were an order of magnitude better than free pendulum clocks. They looked nothing like a modern quartz clock or watch: the electronic circuitry usually occupied a couple of 19-inch racks.

A major improvement in quartz clock technique was made by Louis Essen, at the National Physical Laboratory in Teddington. He cut the quartz in a ring, the so-called Essen ring. It was Essen and his colleague Jack Parry who took the next major step, in 1955, when they designed and built the first caesium atomic clock. This measured time from the frequency of the radiation associated with the transition between two particular quantum states of atoms of caesium. Essen and Parry's apparatus was never operated as a standard clock, but it was used to measure the frequency of the caesium transition in terms of the best available astronomical time standards. The figure obtained from those measurements was used some years later to redefine the second in terms of the caesium transition. From then on atomic clocks took over as the standard of time.

Essen and Parry's clock proved at least as good as the best quartz clocks of the period, and they were reckoned to be accurate to a second in 300 years. In the three centuries since the invention of the mechanical clock, accuracy had increased by eight orders of magnitude. In less than 50 years since Essen and Parry's work, atomic clocks have improved by another four orders of magnitude, and radically new techniques are being investigated which should yield similar improvements over the next few decades.

The best of time: the worst of time†

Dr. Christopher Ray
King's College School, Wimbledon

In the last one hundred years, it might be said that consideration progress has been made in our understanding of time. In the classical view of time events throughout the universe are judged against one and the same cosmic time standard. Einstein's insights, expressed forcefully in his work on the special theory of relativity, called such 'absolute' standards into question. His work provoked much anxiety about our understanding of space and time, and some of those anxieties persist.

The 'clock paradox', sometimes recast as the 'paradox of the twins', still causes spirited debates. It is clear that we can account for a difference in ageing (the number of ticks of our own body clock) by reference to the fact that the behaviour of clocks depends upon the path taken through spacetime. Some have argued that it is the inertial force associated with acceleration and deceleration which causes a clock to 'experience' less time than a clock which remains unaccelerated. However, it is a straightforward task to show that in the special theory of relativity accelerations as such are not required. Those who look for material causes for events and processes are consequently discomforted. Of course, many then point to general relativity, saying that only in this wider context can we make sense of the behaviour of clocks.

Special relativity also allows us to consider the possibility of time travel: any signal which travels faster than light away from and then back to a source will reach the source before it was sent! These are the tachyons of Star Trek fame. Even though the hypothesis that such particles might exist appears to belong to science fiction rather than fact, special relativity by itself does not rule them out. The theory does not prohibit 'faster than light' signals; rather it insists that the speed of light is invariant. One must add some additional principle, as, for example, Stephen Hawking does in his *The Large-Scale Structure of Space-Time*. Indeed, Hawking does so because general relativity allows us to consider possible universes in which backwards causation is possible, and he is not inclined to take these too seriously. I discuss many such problems in my

† A synopsis of the illustrated talk given by Christopher Ray at the special meeting on time.

own books *The Evolution of Relativity* and also *Time, Space and Philosophy*.

The recent work of Hawking and many others on black holes and the very early universe, have brought about even more insights into the character of time. Close to a singularity, we should not expect the same of time as we observe in more normal circumstances. The postulation of imaginary time has helped some to provide a better theoretical appreciation of what is happening at these ‘edges’ of spacetime.

The fact that time seems to be asymmetric has been another puzzle: we seem to be moving from past to future – even though many physical laws are indifferent as far as the direction of time is concerned. Ilya Prigogine and his team working in Brussels and Texas believe that with an appropriate perspective on thermodynamics – using many of the techniques learnt in chaos theory – we can explain why we grow older rather than younger. Progogine sets out his ideas clearly in *The End of Certainty*.

However, despite apparent advances, many uncertainties remain – not just about the character and properties of time, but about the very existence of time itself. Julian Barbour argues with passion but not altogether persuasively in *The End of Time* that we now have a good chance of understanding the physical world without any need for time.

The debates about time, evident in the work of early Greek philosophers – Zeno, Aristotle and many others, still rage. Perhaps we have yet to see the best of time.

The Group’s Website:
www.iop.org/IOP/Groups/HP/

Quantum Concepts, past and present

Introduction

Dr. Hugh Montgomery
University of Edinburgh

I would like to welcome everyone to this meeting of the history of physics group, and perhaps I should begin by explaining how the planning for the meeting developed. Our initial intention was to celebrate Max Planck's introduction of the quantum concept in 1900, and to discuss quantum ideas in the early years of the century. However there was a feeling that this might turn out to be too Whiggish. The Whig interpretation of history treats events in the past as if they were "part of a providential preparation for our present idyllic state"; and some of us feel that the present state of quantum theory is not all that idyllic. When the old quantum theory was being applied to atomic physics people knew roughly what they were talking about, but their ideas did not work very well; whereas nowadays the quantum formalism is extremely successful, but it is not entirely clear what people are talking about. The standard interpretation of quantum mechanics was developed mainly by Bohr and Heisenberg in Copenhagen in the late 20's, and it has proved a powerful and versatile guide to research programmes in physics. However a number of physicists, including Einstein and Schrödinger, were never able to accept the Copenhagen Interpretation, and in the last few decades it has come under renewed scrutiny. It has to be conceded that several interpretations of quantum mechanics now exist, and their relative merits are controversial. One can rejoice in this situation or deplore it, but any discussion of the early development of quantum theory can hardly ignore our present position.

The widening gap between the brilliant successes of quantum theory and growing doubts about its interpretation have given rise to inevitable tensions in the subject. It has often been said that when Einstein criticised quantum mechanics, he was trying to save determinism; probably it is closer to the truth to say that he was trying to save reality. The feature of the Copenhagen Interpretation which disturbed him most was its refusal to consider the actual state of an atomic system, as distinct from the investigator's knowledge of that state. His objection is basically philosophical, and this is where the trouble begins. Many physicists regard philosophers as the Thought Police, who are about to demand that all developments in quantum mechanics be halted until all the conceptual problems have been solved. This of course would be nonsense; physics and philosophy have their own methodologies, and each must maintain independent sovereignty over its own domain. (We may wish to be *in* Europe, but we do not wish to be *run by* Europe.) However some physicists go much further than this, and insist that philosophy can have no possible relevance to any conceptual problem in physics. This refusal to consider philosophy is emotional rather than logical, and it is a much more recent phenomenon than many physicists believe. One thinks of Einstein taking his summer holidays on the Baltic in 1918, reading Kant's *Prolegomena* and writing "Bosh" on the margin of every other page. Einstein really cared about the philosophical problems; so did Planck; so did Bohr; so did Heisenberg and so did Schrödinger. The fact that Heisenberg was partly sympathetic to Kant's ideas, while Einstein was bitterly opposed to them, has been wiped from the folk-lore of physics; but it may be very relevant to some of the discussions we shall hear today.

It is not for me to take sides on these issues, but I would like to raise a question that has sometimes been overlooked. To what extent can conceptual problems in physics be resolved by appropriate experimental tests? There is an implicit belief among many physicists that in principle *any* problem will yield to the right experiment. Empirical testing is clearly a vital ingredient in physics, but we need to think much more clearly about what experiments can and cannot do. For example, experiments to test Bell's Inequalities provide evidence against some of Einstein's arguments, but they have not destroyed them completely.

Finally we need to think about the problems of the poor bloody infantry – those of us who have to teach quantum mechanics to students. We do want them to study the subject in detail, so that they can apply it intelligently to physical problems, and this makes it very tempting to gloss over the controversial aspects. Some time ago I was taking a second year course on the subject, and a student came up to me and said: "I think Einstein was quite right; quantum mechanics is a load of rubbish, and I'm going to say so in the exam." I replied along the lines: "Fine, Ken, fine; now what I'll do is set you an extra paper, in which I shall ask you to explain the exact nature of Einstein's criticisms, and of Bohr's responses to them." Needless to say this produced a swift conversion to total orthodoxy on the part of Ken. But the situation worried me; I felt that I had resorted to the latent authoritarianism that underlies so much teaching in physics. We have not yet learnt how to handle controversy in a way that combines disciplined and liberal thought.

The Planck Radiation Law, past and present

Professor Peter Landsberg
University of Southampton

Physics in Berlin,1910-1933

Berlin was the mecca of physics in the period 1910-1933. Wilhelm Wien, famous for his studies of black-body radiation, was the first Nobel Prize winner (in 1911) relevant to today's theme. Of course Wien's major association was with Wurzburg (1910-1920), even though he had had an appointment in Berlin and had spent time in Helmholtz's laboratory there. In fact Röntgen, his predecessor in Wurzburg, had won the first ever Physics Nobel Prize (1901) while there.

Next in the hall of fame in Berlin, next to Planck, was Nernst, who upon attaining the acme of a Berlin appointment in 1914, showed his usual sense of theatre. He decided to come by car (which was then quite rare). He was at the driving wheel in his fur, and his family was bundled in the back. His chief mechanic was ensconced next to Nernst. This was a useful precaution, since according to K Mendelssohn it was suggested that "the discoverer of the theory of galvanic elements had charged his battery with the wrong polarity", leading to a breakdown, and adding an extra night to the journey from Göttingen. Although amusing, this story of Mendelssohn's seems a little unlikely. Incidentally, Nernst was awarded the Nobel Prize for Chemistry in 1920.

Then in 1914 Planck and Nernst collaborated to bring Einstein from Zurich to Berlin, and this brought another Physics Nobel from Zurich to Berlin in 1921. Thus with Planck, Wien (he died in 1928), Nernst and Einstein, Berlin was a good place to pursue research in Physics.

I was born in Berlin in 1922 and left in 1939. Due to the fact that I was so young, and not clever enough to go to university at the tender age of 11 years, I regret that I cannot report on student life at that time from my personal experience. I recall very clearly more general experiences, however: stormtroopers marching through the streets, the Horst Wessel Lied, the Sturmer showcases with their unappetising pictures of Jews, and in later years a real air of panic; and my personal feeling as a boy that the Hitler regime was installed forever. It was with some surprise that I noted later in life that he was in power for no longer than Margaret Thatcher was Prime Minister in Britain! But it is the damage wrought through the war he started (and the incidental damage to German physics) that was irreparable for so many decades. This is what gave the illusion of a much longer period to Hitler's reign.

Although I was unfortunately too young to give a student's report on life in Berlin, I found just such a report by the distinguished mathematician Walter Ledermann, who was a student there from 1928 to 1933. You know what students are like. In spite of many interesting, amusing and even stimulating characteristics they always grumble about lecturers: they do not speak clearly, or their handwriting is bad, the lectures are poorly organised, etc. Ledermann tells us that Planck's lectures were of high intellectual standard, but his blackboard technique made it difficult to take notes, as he wiped out after a few lines everything he had written with a sudden swing of his left arm. To take notes at all, you had apparently to sit on the extreme right of the room!

As to Nernst, Ledermann tells us that when Nernst wanted to show the potential difference between two touching metals, he usually asked the audience to produce a gold sovereign for this purpose. But for students he noted, "I do not expect you will be able to lend me a gold coin, so I took the precaution to bring my own collection." He clapped his hands, and a technician brought in a velvet-covered tray. Nernst picked up the pieces one by one, commenting: "this is the Nobel prize, this is the Bunsen medal, this is the Franklin medal, as far as I know there is only one other copy in Europe; it belonged to my late friend Herr Boltzmann". Ledermann comments, "A most effective performance, despite its boastfulness."

Schrödinger succeeded Planck. An Austrian, he came to work in an open-neck shirt on a summer's day, very unlike a normal Berlin professor of the time. Today things may be quite different and more informal. Anyway, Schrödinger was once stopped by a security guard who thought that he was a political agitator. Ledermann reports that Schrödinger was a scintillating person.

Von Laue (1879-1960) was one of the nine doctoral students of Max Planck's, and had received the Physics Nobel prize for X-ray diffraction by crystals in 1914. He was brought to Berlin by Planck in 1905, and he remained there for most of his professional life. He was a hero for many people because of his courage in opposing the more extreme Nazi measures. His ashes were interred in Göttingen next to those of the great Berlin scientists who preceded him: Planck and Nernst.

The Physics

Below are references to the work discussed in the lecture:

a) How to extend the Planck argument so that it yields not only the black-body distribution law, but also the oscillator energy spectrum and the zero point energy
P T Landsberg Eur. J Phys 2, 208 (1981)

b) How non-equilibrium situations can lead to photons with a non-zero chemical potential
P T Landsberg Recombination in semiconductors, OUP 1991 p.319

c) The appearance of Planck distributions in higher dimensions
P T Landsberg & Ade Vos, J Phys A22, 1073 (1989)

d) The use of black-body radiation to clear up an old problem concerning the relativistic transformation of temperature
P T Landsberg & G E A Matsas, Phys Lett A223, 401(1996)

Alpha rays without particles:
early wave-mechanical account of the tracks in a Wilson chamber

Dr. Michel Bitbol
CNRS, Paris

In 1929, Nevill Mott published a paper entitled The wave mechanics of α-ray tracks. This paper is well-known as a seminal work about the quantum theory of measurement. It shows how it is possible to avoid stating an explicit solution to the measurement problem, and yet maintain a coherent attitude in the use of the quantum formalism, thus giving this problem what I call a performative solution. In his paper Mott followed quite closely the interpretation of wave mechanics which was developed by Charles Galton Darwin a few weeks earlier in 1929, but he made these ideas more concise, and also less metaphysical.

The initial motivation of both Darwin's and Mott's papers was Gamow's theory of radioactive disintegration of 1928. In this theory, the emission of α-rays was explained wave-mechanically by means of potential-barrier penetration. Now, the problem is that as soon as they have emerged from the nucleus, the α-rays appear to have essentially corpuscle[‡]-like properties, for they give rise to tracks in cloud chambers. Charles Galton Darwin's project was then to restore conceptual homogeneity between the explanation of the radioactive emission (which is based on pure wave mechanics) and the account of detection (which apparently must involve corpuscularian categories). He wished to make sense of the α-ray tracks without resorting to the process that consists in imagining that at each observation "the wave [turns] into a particle and then back again [into a wave]". He wanted "to show how a discussion only involving the wave function y would give spontaneously the results which simple intuition would suggest could only be due to particles". As for Mott, he also insisted that, "the wave mechanics unaided ought to be able to predict the possible results of any observation that we could make on a system". But how is it possible? According to Darwin, in order to account for the tracks, the relevant wave function must contain factors corresponding not only to the α-particle, but also to every ionizable atom in the cloud chamber. "Before the very first collision, (the wave function) can be represented as the product of a spherical wave for the α particle, by a set

‡ particle [Ed.]

of more or less stationary waves for the atoms. ... [The] first collision changes this product into a function in which the two types of co-ordinates are inextricably mixed". This is a very clear early statement of what we now call the entanglement of wave functions after Schrödinger's papers of 1935. Darwin even insisted that "the trouble ... in the quantum theory has only arisen through attempts to work with an incomplete y^2, namely a y-function that does not incorporate relevant elements of the measuring device. As for Mott, he noticed that "... we are really dealing with wave functions in the multispace formed by the coordinates both of the α-particle and of every atom in the Wilson chamber".

Now, what about the interpretation of the wave function? According to Mott and Darwin, the quantum mechanical account, including when it uses entangled wave functions, does not provide the slightest element of description of the putative processes underlying the phenomenon; it only enables us "to predict the possible results of any observation". In other words, "interpreting the wave function should give us simply the probability that such and such an atom is ionized".

Then, Mott and Darwin insisted that the multidimensional wave-mechanical account must be pushed as far as possible, and that any reference to corpuscular or discontinuous processes must be delayed as much as possible. This procedure is fully coherent, insofar as it consists in developing continuously the predictive formalism until the stage where a probabilistic prediction is required, rather than mixing up continuous predictive elements with unwarranted discontinuous descriptive stories. In Mott's terms, "Until this final [probabilistic] interpretation is made, no mention should be made of the α-ray being a particle at all". As for Darwin, he took this delay as the pivotal concept of his interpretation of quantum mechanics, and as a sort of leitmotiv. His major aim was to show "how it is possible to postpone speaking of particles", for according to him, "there is no need to invoke particle-like properties in the unobserved parts of any occurrence, since the wave function y will give all the necessary effects". Each entangled wave function can be read as a disjunction of conditional statements, relating one ionization to a series of other ionizations approximately located on the straight line joining the radioactive nucleus and the first ionization. While the probability of the first ionization is evenly distributed, the conditional probability of obtaining an approximately straight track following this first ionization is very high. However, here again, says Darwin, "The decision as to the actual track can be postponed until the wave reaches the uncovered part where the observations are made". Darwin later went even further, in suggesting that it is only at the level of the brain that we are really compelled to stop the chain of entanglements, and that it is our consciousness that so to speak cuts sections of the overall wave-function when it makes observations. He thus anticipated later views of the measurement problem such as Von Neumann's or London's and Bauer's. But this urge to explain how it is that we finally see a single track, in spite of the multiple-track structure of the relevant overall wave function, is a clear sign that Darwin was still tempted by ascribing a partly descriptive status to wave mechanics, rather than a purely predictive one. Mott avoided such speculations; he contented himself with having proved that the probability of having two ionized atoms in the cloud chamber vanishes unless the line that joins them passes near the radioactive atom.

The interpretative strategy used by Heisenberg in his *Physical Principles of the Quantum Theory* to account for the tracks was quite different. Unlike Mott and Darwin (and owing to the influence that Bohr had exerted on him), Heisenberg had no reluctance to jump from corpuscle representation to wave representation and back again whenever it appeared convenient to do so. He considered that including the α particle and the ionizable hydrogen atoms of the cloud chamber within the same compound system, or taking the α particle as the only system and the ionizable atoms as part of the observation device, is a matter of free choice. A cut has to be introduced somewhere between the system and the observation device, but, says Heisenberg after Bohr, the location of this cut is almost arbitrary; it only depends on pragmatic considerations.

Now, what are we to think about this difference between Heisenberg on the one side and Mott and Darwin on the other? Even though their methods are equivalent from a purely pragmatic standpoint, they are not from an intellectual standpoint.

The method of successive wave-packet reductions is usually much simpler, for it consists in using the information afforded by each point-like observation to extract a new wave function for the α particle alone out of the compound wave function of the larger system consisting of the α particle and an ionized atom. The problem is that one usually forgets that successive reductions are by no means changes of the initial wave function, but rather redefinitions of it for practical reasons. As a consequence of this forgetfulness, the discontinuous evolution of the wave function is taken as a sort of descriptive account of the process that gives rise to the track, and this arouses spurious questions about the physical mechanism of the wave packet reduction. By contrast, the method of the entangled wave-functions has the merit of permanently maintaining a clear distinction between the predictive continuous model and the series of predicted discontinuous events. The only question which arises in it concerns not the discontinuous collapse of the wave function, but the progressive shift from a wave-like to a classical probability theory when the system encompassed within the global wave-function grows bigger and bigger. In other words, the question here is that of a progressive loss of interference terms in the probabilistic formalism. As we now know, a plausible answer to that question, but not directly to the question about wave-function collapse, has been provided by the decoherence theories.

There also is another significant intellectual difference between the two attitudes. Heisenberg's insistence that corpuscularian categories are good enough to explain tracks in cloud chambers may be taken as an incentive to forget in the long term Bohr's cogent statement according to which the corpuscular picture is relative to a certain class of experimental situations, or to a certain mode of analysis of experiments, and that one should therefore avoid taking it at ontological face value. By contrast, holding on to the wave-mechanical model until the very moment when the probability of a series of ionizations is to be calculated enables one to bypass completely the corpuscularian categories, and thus to avoid taking them too seriously. A very strong expression of this alternative way of seeing things was provided by Schrödinger: "it is better to regard a particle not as a permanent entity but as an instantaneous event. Sometimes these events form chains that give the illusion of permanent beings".

Hence the title of this paper: all of the phenomena of radioactivity, including the tracks, can be predicted with maximal consistency if one does not make any reference to particles.

Major References

1. N.F. Mott, "The wave mechanics of α-ray tracks", Proceedings of the Royal Society, London, A126, 79-84, 1929; reprinted in: J.A. Wheeler & W.H. Zurek, *Quantum theory and measurement,* Princeton University Press, 1983.
2. C.G. Darwin, "A collision problem in the wave mechanics", Proceedings of the Royal Society, London, A124, 375-394, 1929.

Bohr's contribution to the interpretation of quantum mechanics

Professor Basil Hiley
Birkbeck College, University of London

Abstract

In this talk I discussed those aspects of the interpretation of quantum mechanics that were unique to Bohr, emphasising that he had a deeper view of quantum phenomena than is generally realised and that his account is not presented in many of the textbooks on quantum mechanics. One can easily see why this is so. His view is very different from the traditional view of physics bringing in notions like non-separability and wholeness. These ideas are extremely difficult to defend in the current view of science based as it is on reductionism. I brought these points out by quoting extensively from Bohr's own writings hoping to show exactly how his view significantly differs from what is called standard quantum mechanics.

I then compared and contrasted Bohr's views with the approach that Bohm and I investigated in our book *The Undivided Universe*[1]. The Bohm approach has, unfortunately, been seen as being in direct opposition to the Copenhagen view. I do not see it this way. Bohr's discussions, although denying that any meaning can be given to the idea of a particle trajectory, contain features that became clear to me only when I compared them with the Bohm approach. As a consequence I concluded that this latter approach gives another useful perspective on quantum phenomenon, but a perspective that also contains non-separability and wholeness.

Bohr's Outlook

The talk started by first considering Bohr's account of stationary states. Rather than explain why these orbits are stable, Bohr simply declared that stable orbits exist and because they exist, the classical laws of electrodynamics must be "suspended" for these particular states. This immediately means that we cannot determine experimentally what the electron is doing in a stationary state because to discover these details we need classical electrodynamics and that is what we don't have for stationary orbits. This, together with Heisenberg's uncertainty principle, has been taken to imply that no meaning can be given to any notion of a "particle trajectory".

Bohr actually carries this much further and claims that it is not possible to give any type of picture of quantum processes even *in principle*[2]. When we teach classical mechanics, we always have pictures to call upon and we try to carry on this tradition in quantum mechanics. But we are forced into very unclear notions like "wave-particle duality", giving the impression such a model actually "makes sense". It doesn't and I always feel sorry for the poor confused students. Surely it is either one or the other, not both together? Why can't we say it is neither and admit, along with Bohr, that further analysis is not possible? If you want to insist on giving pictures, then the only consistent and unambiguous view I know is the Bohm approach, but we must not mention that because it is not the accepted view!

But why did Bohr conclude that a detailed analysis of quantum processes is not possible even in principle? He repeatedly explained his position, but never, apparently, with sufficient clarity. He uses phrases like "the indivisibility of the quantum of action"[3], and "the uncontrollable interaction between objects and measuring instruments"[4] to imply that in quantum mechanics we cannot make a sharp separation between the system under observation and the means of observation[3]. That is, that at the quantum level a given measurement cannot define simultaneously *all* the properties of a system unambiguously. Measurement forces a system into an eigenstate of one operator, so that you cannot say anything about the properties described by the complementary variables. Notice this does not mean that our measurement procedure is somehow "ham-fisted". It is that in a particular measurement, the complementary variables cannot even be *defined*[5].

This inseparability of the observed from the means of observation was fundamental for Bohr. He tried to emphasise by re-defining the word "phenomenon". For him the word did not refer to a particular isolated evolving process, independent of the means of observation. He wrote:

> I advocate the application of the word phenomenon exclusively to refer to the observation obtained under specific circumstances, including an account of the *whole* experimental arrangement.[6]

If the actual process cannot be defined in its entirety what can we do? We can tell people what we have done by describing the experiment and what its outcome was[6]. What we cannot do is to give a detailed account of what the particles of the observed system are actually doing.

But physicists instinctively feel that we are in the business of trying to understand the evolution of physical processes independently of whether we decide to measure something or not. This is where the standard approach deserts Bohr and instead adopts the von Neumann view that the wave function is the most complete description of the state of the system, which evolves objectively in space-time following the Schrödinger equation. But taking the wave function to be a function of state necessarily leads to the projection postulate ("collapse of the wave function") and to all the familiar paradoxes of standard quantum mechanics such as the measurement problem, the cat paradox and so on.

I want to stress here that this is very different from what Bohr assumes. He assumes the wave function is merely a symbol that appears in an algorithm that enables us to calculate the probable outcome of a given experimental arrangement and that is all! It does not describe the state of any isolated *thing.* This is a consequence of the indivisibility of the quantum of action and therefore there is no measurement problem for Bohr. If the wave function is merely symbolic, it is meaningless to ask for a physical process to explain its collapse! Recall Bohr often denied the possibility of a description of what is actually going on in space-time.

But why is there no ontology? Could we not develop radically new concepts that would take into account the indivisibility of the quantum of action in a way that would enable us to understand what was going on? Bohr felt we had actually reached the limits of man's powers of perception, of visualising and of concept forming[7]. It should be remembered that Bohr was influenced by the Danish philosopher, Harald Höffding; I wonder to what extent the Kantian notions of *a priori* space and time might have had an influence on these conclusions[8]. This possibility becomes even stronger in view of his remarks about the impossibility of space-time descriptions of quantum phenomena.

According to Bohr then, we cannot provide an ontology for quantum processes. We can simply tell each other what we did in our experiments and what results we got and that is all. In other words we have only an epistemology. But we needn't rely on philosophical arguments to reach such a conclusion. Von Neumann's famous theorem excluding the possibility of a more detailed explanation of quantum phenomena concludes that:

> It is therefore not, as it is often assumed, a question of are-interpretation of quantum mechanics, --- the present system of quantum mechanics would have to be objectively false[9].

Unfortunately all theorems depend on assumptions and if the assumptions are too restrictive, then the conclusions that are drawn from them do not have any validity. This is the weakness of this theorem as Bell eventually demonstrated[10].

Bohm's Approach

I now want to develop a different approach and I will start from a statement made by G. P. Thomson in 1930. He claimed that

> … there should be no objection to postulating laws and quantities, which afforded a convenient and graphic mode of expression, merely because they are not observable without introducing uncertainties. Such an objection would equally apply to the introduction of the electric and magnetic vectors as separate quantities for the only quantity that can really be observed, viz., the force on a charged particle, is compounded from them.[11]

I quote this because this is just what Bohm found by writing the Schrödinger equation as two real equations. These equations are a result of writing the wave function in polar form $\Psi(\boldsymbol{r}, t)=R(\boldsymbol{r}, t)\exp[iS(\boldsymbol{r}, t)/\hbar]$. The real part gives

$$\frac{\partial S}{\partial t} = \frac{(\nabla S)^2}{2m} + Q(\boldsymbol{r},t) + V(\boldsymbol{r},t) \qquad (1)$$

If S were the classical action and Q=0, equation (1) would be the well known classical Hamilton-Jacobi equation. It is simply the equation for the conservation of energy,

$$E = \frac{p^2}{2m} + V(\boldsymbol{r},t) \qquad (2)$$

because of the canonical relations $E = \frac{\partial S_c}{\partial t}$ and $p = \nabla S_c$. Here S_c is the classical action. But in (1) S is the phase of the wave function and $Q(r,t) = -\frac{\hbar^2}{2m}\frac{\nabla^2 R(\boldsymbol{r},t)}{R(\boldsymbol{r},t)}$. Nevertheless in a stationary state $E = \frac{\partial S}{\partial t}$, while $\nabla S = \Re(\Psi^* P_{op}\Psi)$. So if we follow Bohm and identify this latter equation with a particle moving with momentum p_B, we find equation (1) can be written in the form

$$E = \frac{p_B{}^2}{2m} + Q(\boldsymbol{r},t) + V(\boldsymbol{r},t) \qquad (3)$$

Comparison with equation (2) shows immediately that the classical kinetic energy is replaced by two terms, the first looks like a kinetic energy, the second is like an additional potential energy. The suggestion made by Bohm was that we could consider p_B to be the "beable" momentum. The corresponding particle followed a trajectory that could be found by integrating $p_B = m\frac{dv}{dt}$. These trajectories then corresponded to the streamlines of the probability current, which in the classical limit became the classical particle trajectories. The difference in the two sets of trajectories is simply accounted for by the presence of the quantum potential. Since the remaining (imaginary) part of the Schrödinger equation gives the conservation of probability, all the results of standard quantum mechanics follow without modification.

In fact all that is actually necessary to provide an ontology in the Bohm approach is to regard each actual quantum process as being parameterised by a unique p_B. We could consider this to be the momentum of a particle following a specific trajectory as did Bohm or we could be more radical. We could regard this process as being something much more general[12].

Let us return to equation (3) and examine the consequences of the presence of the additional quantum potential, Q. This is totally unlike any classical potential. It has no external source, it need not fall off with distance and it carries information of relevant aspects of the environment[§]. It is this latter fact that brings us back to Bohr. While Bohr focussed on the assumption that we cannot make a sharp separation between the observed system and its means of observation, Bohm's approach shows that if we do make such a distinction, then we must include the quantum potential energy. This term in effect unites them again since the potential contains the information about the measuring

[§] A more detailed account of this potential can be found in Bohm and Hiley (ref. 1) device and shows the process is dependent on the details of the apparatus. A different measuring device will produce a different quantum potential and the particle will behave differently. This then offers an explanation of Bohr's statement[6] quoted above.

Notice the Bohm approach does not only apply to measurement. It applies to any system in interaction with any other system. In other words it is an ontological explanation.

Quantum Non-locality

The final point I made concerned quantum non-locality. In the Bohm approach, for a pair of systems in an entangled state, $\Psi(\boldsymbol{r}_1, \boldsymbol{r}_2, t) = a\phi(\boldsymbol{r}_1, t)\psi(\boldsymbol{r}_2, t) + b\phi(\boldsymbol{r}_2, t)\psi(\boldsymbol{r}_1, t)$, the energy conservation equation reads

$$\frac{\partial S}{\partial t} + \frac{(p_{B_1})^2}{2m} + \frac{(p_{B_2})^2}{2m} + Q(\boldsymbol{r}_1, \boldsymbol{r}_2, t) + V(\boldsymbol{r}_1, \boldsymbol{r}_2, t) = 0 \qquad (4)$$

where

$$p_{B_i} = \Re\left[\Psi^*(\boldsymbol{r}_1, \boldsymbol{r}_2, t)(-i\hbar\nabla_i)\Psi(\boldsymbol{r}_1, \boldsymbol{r}_2, t)\right] \qquad (i=1, 2) \qquad (5)$$

Here we notice that the particles are coupled no matter how far apart they are, even when $V(r_1, r_2, t)=0$. This is an example of quantum non-separability. Notice that the coupling may not vanish even when the particles are very far apart and thus the particles are somehow "together yet apart".

If we measure a property of particle #1, then the corresponding property of particle #2 becomes defined immediately, no matter how far apart the particles are. This comes about because during the measurement, a new quantum potential links particle #1, particle #2 *and* the relevant particles of the apparatus. This linkage occurs even though the apparatus is only localised near particle #1. Thus it is the presence of the apparatus at #1 that defines the outcome of particle #2. This is quantum non-locality.

Bohr recognised that some form of coupling between both particles and the apparatus was necessary and he wrote in answer to Einstein-Podolsky-Rosen

> From our point of view we now see that the wording of the above mentioned criterion of physical reality proposed by

> Einstein, Podolsky and Rosen contains an ambiguity as regards the meaning of the expression “without in any way disturbing a system”. Of course there is no question of a mechanical disturbance of the system under investigation during the last critical stage of the measuring procedure. But even at this stage there is essentially the question of *an influence on the very conditions which define the possible types of predictions regarding the future behaviour of the system* (my italics).[13]

Notice Bohr denies that there is a mechanical disturbance between the two particles but there is an influence. What could he mean by an ‘influence’? Could the quantum potential be regarded as this influence? It certainly is not mechanical in origin as we explained in our book[1]. Could it be that Bohr anticipated the quantum potential but didn’t want to go down that road because it did not fit into his preferred philosophical framework? I left this as a point for discussion.

References

[1] D. Bohm and B. J. Hiley, *The Undivided Universe: an Ontological Interpretation of Quantum Theory*, Routledge, London 1993.

[2] N. Bohr, *Atomic Physics and Human Knowledge*, p. 62, Science Editions, New York, 1961.

[3] ibid. p. 39

[4] ibid. p, 48

[5] ibid. p. 72

[6] ibid. p. 64.

[7] N. Bohr, Light and Life, Nature, 1933, in *Atomic Physics and the Description of Nature*, p. 5, Cambridge University Press, Cambridge, 1934.

[8] N. Bohr, Letter to Geiger, 21 April 1925, Coll. Works, V, p. 353

[9] J. von Neumann, *Mathematical Foundations of Quantum Mechanics*, p325, Princeton University Press, Princeton, 1955

[10] J. S. Bell, *Speakable and Unspeakable in Quantum Mechanics*, Cambridge University Press, Cambridge, 1987.

[11] G. P. Thomson, *The Wave Mechanics of Free Electrons*, p127, 1930.

[12] D. Bohm and B. J. Hiley, *The Undivided Universe: an Ontological Interpretation of Quantum Theory*, Chapter15, Routledge, London, 1993.

[13] N. Bohr, *Phys. Rev.* , **48**, p. 696, (1935).

Future Meeting arranged by this Group

<u>Space</u>

Special half-day meeting

Saturday 12th May 2001, 1.00 pm for 1.15 pm
Science Museum, Exhibition Road, South Kensington, London

1.15 Introduction
1.20 <u>British Rocketry</u> Doug Milland, Curator, Science Museum
2.10 <u>The Apollo Missions</u> Jonathan Allday (author, *Apollo in Perspective*) King's School, Canterbury
3.00 Break and refreshments
3.15 <u>The Search for Extra-Terrestrial Life</u> Jacqueline Mitton, Research Fellow, Royal Astronomical Society
4.05 <u>The Hubble Telescope in Historical Perspective</u> Robin Catchpole, Senior Astronomer, Royal Observatory
4.55 Closing Questions

A number of the talks will be illustrated, and there will be time for questions after each speaker. We do hope you will join us for what promises to be an extremely interesting afternoon.

Those who are interested in touring the Science Museum are also very welcome to join us for an optional tour starting at 11 a.m.

A flyer will be sent out nearer the time, but if you need more information before then, please contact the Honorary Secretary, Sophie Duncan …
Post: The Science Museum, LONDON SW7 2DD
e-mail: s.duncan@nmsi.ac.uk

Other Lectures and Meetings, at home and abroad

This information has kindly been supplied by the BSHS and is their copyright. Nearly all these meetings are open to people who are not members of the society concerned, sometimes at a slightly higher cost. We remind readers to check before departure that the event has not been cancelled.

British Society for the History of Science

Edwardian Science
at Museum of the History of Science, Oxford
on 21 May 2001 (NB this is the confirmed date)
Was there a distinctive "Edwardian" cultural period? What are the implications for history of science? This meeting assesses such questions. The Society's EGM will be held during the meeting. Provisional speakers include: Robert Olby, PeterBowler (on "Creative Evolution and the New Theology: Changing Attitudes to Evolutionism in Edwardian Britain"), Robin Mackie & Gerrylynn Roberts (on "Edwardian Chemistry: A period of Educational, Institutional and Employment Change"), Keith Vernon, (on "Professionalization and Public Service: Scientists, Civil Servants and the Invention of Research, 1900 – 1916"), Ivan Crozier (on "The Political and Social Contexts of Havelock Ellis' Sexology"), Ivor Grattan-Guinness (on "Bertrand Russell on Empire in the 1900s"), William (on "The Domestication of Exotic Entomology: The Hope Museum, 1893 – 1913"), Gregory Radick (on "Hobhouse at the Zoo"), and George Wilkins (on "Sir Norman Lockyer and the British Science Guild"). The meeting has been organised by Professor Robert Olby and Professor David Knight.

Les rêves et les choses. A Joint BSHS/SFHST meeting on the History of Science in Museums
at Musée des arts et métiers, Paris
on 30 June – 1 July 2001
In recent decades many innovations in museums of science have ignored history, to concentrate on an exclusively modernist perspective, even when viewing the past. This has been linked to a disappointingly narrow notion of what should constitute "the public understanding of science". There are now signs of dissatisfaction with this approach and evidence of

aspirations towards a richer public culture of science, in which both history and art will make substantive contributions in their own right. The meeting will take place at one site for such aspirations and will comprise reports and discussion of recent work, and plans (as well as dreams) for the future. It is a joint meeting between the British Society for the History of Science and the Société Francaise d'Histoire des Sciences et des Techniques. The fee covers registration, refreshments and admission to the Museum. Participants will need to book accommodation for themselves, but will be sent advice on hotels. Offers of papers or other presentations should be sent to: Dr Jim Bennett, Museum of the History of Science, Broad Street, Oxford, OX1 3AZ. [jim.bennett@mhs.ox.ac.uk]

Environmental Science and the Industrial City (tbc)
at Manchester (tbc)
on 31 August – 2 September 2001 (tbc)
It is hoped to confirm a conference on this topic shortly.

Postgraduate Workshop
at Magdalen College, Oxford (tbc)
on 3 – 4 January2002 (tbc)
The annual BSHS Postgraduate Workshops provide stimulating and informal opportunities to present papers and discuss history of science.

American Chemical Society

Late Twentieth-Century Chemistry (1958-2000)
at Chicago
on 26 - 30 August 2001
This meeting will be part of the American Chemical Society's National Meeting programme. Further details from Richard E Rice, General Education Program, MSC 1201, James Madison University, Harrisonburg, VA 22807, USA

Anglo-French Naval Historians Eighth Conference

Science and the French and British Navies, 1700 - 1850
at National Maritime Museum
on 30 April – 3 May 2001
The conference is intended to explore the many ways in which science

affected the workings and development of the French and British navies, and the parts that the navies played in the advancement of science. Further details from Helen Jones, Research Administrator, National Maritime Museum, Greenwich, London SE10 9NF. [research@nmm.ac.uk]

Birkbeck College

Robert Hooke Tercentenary Conference
at Birkbeck College
on 7 – 9 July 2003
A major international conference is being organised under the auspices of Gresham College, London, to commemorate the tercentenary of the death of the natural philosopher and polymath Robert Hooke (1635-1703). Sessions will be devoted to the full range of Hooke's life, work, milieu and legacy; there will also be ancillary activities, including visits to buildings designed by him. Offers of papers are invited from those actively engaged in research on Hooke. Please send details, including the proposed title and a synopsis, to the organisers Michael Cooper at [m.a.r.cooper@city.ac.uk] and Michael Hunter at [m.hunter@history.bbk.ac.uk]. Those who would like to attend and wish to be kept informed of plans as they develop should send their details to the administrator Mrs Julie Jones at [julie.jones6@btinternet.com].

British Library

John Evelyn and his Milieu
at the British Library
on 17 – 18 September 2001
John Evelyn (1620-1706) was the quintessential virtuoso, a leading member of the Royal Society in its formative years and a key figure in the naturalisation of European culture in early Enlightenment England. This conference will address a range of themes illustrated by the British Library's unparalleled holdings of Evelyn manuscripts and books. Speakers will include Douglas Chambers, Mordechai Feingold, Antony Griffiths, John Harwood, Steven Pincus and John Spun. Full details will be available shortly. Preliminary enquiries should be addressed to the organisers, Frances Harris at [Frances.Harris@bl.uk] or Michael Hunter at[m.hunter@history.bbk.ac.uk]

British Association

Annual Meeting
at University of Glasgow
on 9 – 7 September 2001
The History of Science Section will be putting on sessions on the history of psychology (Helen Haste, GrahamRichards), on the Lunar Society (Adam Hart-Davis, Jenny Uglow, Jennifer Tann, Desmond King-Hele), as well as sessions on science, war and peace and on the Scottish Enlightenment. Further details from British Association, 23 Savile Road, London, WIX 3NB. Details about the programme or section from Dr Rhodri Hayward, Wellcome Unit for the History of Medicine, University of East Anglia, Norwich, Norfolk, NR4 7TJ.

EURESCO conferences 2001

Three thousand years of adulterations and quality control
at Kalamaki Beach Hotel, Corinth, Greece
on May 18 – 23, 2001
This conference will deal with adulterations and quality control in ancient, mediaeval and modern times up to the introduction of contemporary techniques. Its scope is to investigate the historical evolution of techniques and concepts, as well as their social implications. Further details from POB 10876, 54110 Thessaloniki, Greece. [varella@chem.auth.gr]

Faculty of the History and Philosophy of Medicine and Pharmacy of the Society of Apothecaries

Lectures
at Apothecaries' Hall,
on 25 April and 5 June 2001 at 6 pm
These will be, respectively, Brian Livesley (on old dogmas, new tricks) and David Phillipson (on future apothecaries). Further details from Dee Cook, Society of Apothecaries, Black Friars Lane, London EC4V 6EJ. [archivist@apothecaries.org] or [clerk@apothecaries.org]

German Society for the History of Medicine, Science and Technology

Trends and Perspectives in the History of Medicine, Science and Technology
at Hamburg
on 28 September – 1 October 2001
The German Society for the History of Medicine, Science and Technology will celebrate its centenary at this meeting. Further details from Professor Helmuth Albrecht, Institut für Wissenahafts und Technikgeschichte, TU Bergakademie Freiberg, Nonnengasse22, 09599 Freiberg, Germany. [halbrech@vwl.tu-freiberg.de] or from *http://www.mpiwg-berlin.mpg.de/dggmnt/tagungen/hamburh2001.html*

INHIGEO

Geological Resources and History: Rocks and Dinosaurs
in Lisbon and Aveiro
on 24 June – 1 July 2001
Offers of papers to and further information from Professor Manuel Pinto, Department of Geosciences, University of Aveiro, 3810-Aveiro, Portugal. [mpinto@geo.ua.ptl

Institution of Electrical Engineers

29th Annual Meeting of the History of Technology Group (PNS7)

at Chatham Campus of the University of Greenwich
on 29 June –1 July 2001
While contributions on any subject relevant to the history of electricity and electrical engineering will be welcomed, in view of the historical significance of the Thames estuary and its immediate environs in the development of electrical engineering papers relating to the various industries and establishments that grew up along the banks of the river from submarine telegraph cables of the 1860s to semiconductors and computers would be of especial relevance. Offers of papers should be sent by the end of April 2001 to Dr Colln A Hempstead, 3 Uplands Road, Darlington, Co Durham, DL3 7SZ. [colin.Hempstead@ntlworld.com]

International Leibniz Congress

VII Congress
at Technische Universität Berlin
on 10-14 September 2001
Offers of papers to and further details from Prof Dr Hans Poser, Institut fur Philosophie, Wissenschaftstheorie, Wissenschafts und Technikgeschichte der T U Berlin, Ernst-Reuter Platz 7 (Sekr. Tel 12-1), D-10587 Berlin, Germany.

International Union for the History and Philosophy of Science

XXI International Congress of the History of Science
at Mexico City
on 8-14 July 2001
The theme of this congress will be "Science and Cultural Diversity". To receive further details contact XXI International Congress of the History of Science, Apartado postal 21-873, 04000 México, D.F., México. [xxiichs@servidor.unam.mx] or visit *www.smhct.org*

Science Museum

Locating the Victorians
at the Science Museum
on 12-15 July 2001
The year 2001 marks the sesquicentenary of the Great Exhibition of 1851 and the centenary of the death of Queen Victoria. These anniversaries provide the opportunity to review our interpretation of the culture of the Victorian period. The BSHS will also subsidise Postgraduate BSHS members attending this conference (half of registration cost). Students will need to pay the full registration first and then claim back half the amount by writing, with receipt, to the BSHS Executive Secretary, 31 High Street, Stanford in the Vale, Faringdon OXON, SN7 8LH. Registration can be made via: *http://www.sciencemuseum.org.uk/researchers/victorians*

University of Edinburgh

Geography and Revolution
at University of Edinburgh
on 18-21 July 2001
The importance of spaces and the situated nature of knowledge in understanding the history of intellectual and social change has been acknowledged increasingly by scholars in a variety of disciplines. This conference will bring together an international and interdisciplinary set of speakers to build upon and extend these interests. The connections between geography and revolution – scientific, political and technical – will be explored by scholars from geography, history, and history of science. The speakers will be Mark Bassin, Jerry Brotton, Graham Burnett, Peter Dear, Paul Glennie, Michael Heffernan, John Henry, David Livingstone, Robert Mayhew, James Moore, Nicolaas Rupke, James Ryan, Steven Shapin, Nigel Thrift and Charles Withers. Registration forms may be downloaded directly from the Conference Website at *www.geo.ed.ac.uk*. Further information is available from either of the Conference Conveners: Professor David N Livingstone, School of Geography, The Queen's University of Belfast, Belfast BT7 1NN. [d.livingstone@qub.ac.uk] or Professor Charles W J Withers, Department of Geography, University of Edinburgh, Edinburgh, EH8 9XP. [cwjc@geo.ed.ac.uk]

University of Leeds

Learning and Teaching Support Network workshop on teaching history of science, technology and medicine
at Leeds University
on 30-31 May (tbc)
This workshop will discuss common issues arising from teaching history of science in universities. For more information contact: Dr Graeme Gooday, Associate Director (History & Philosophy of Science) LTSN Philosophy & Religious Studies Subject Centre, School of Philosophy, Division of HPB, University of Leeds, Leeds LS2 9JT, UK. [g.j.n..gooday@leeds.ac.uk] Tel: 0113 233 3274, Fax: 0113 233 3265 *http://www.prs-ltsn.leeds.ac.uk/*

University of Manchester

Representing Emotions: evidence, arousal, analysis

at University of Manchester
on 25-27 May 2001
This international conference is part of Manchester University's 150th Anniversary celebrations. It brings together academics from a diverse range of disciplines to interrogate how emotions have been conceived, analysed and represented from the Renaissance to the present day. Particular emphasis will be given to the changing ways in which artists, musicians and performers have represented the emotions, and the implications of these artistic developments for medical and scientific theories. Speakers have also been asked to reflect on the relationships between institutional and disciplinary discourses relation to this theme. In this way expertise from a range of backgrounds will be brought to bear on questions of systematization and the policing and representing of emotions, in a forum which is unparalleled. Speakers will be Peter Burke, Christine Battersby, Charles Brotman, Otniel Dror, Isabella van Elferen, Christopher Gartner, Penelope Gouk, Michael Heyd, Dalia Judovitz, Marcia Pointon, Graham Richards, Michael Schwarz, Chandak Sengoopta. Further details from Dr Penelope Gouk, CHSTM, Mathematics Tower, University of Manchester, Manchester M13 9PL. [gouk@man.ac.uk]

University of Oxford

Thomas Harriot Lecture
at Champneys Room, Oriel College
on 24 April 2001 at 5pm
The lecturer will be Robert Goulding on "Thomas Harriot, Geometer". Further details from Robert Fox, Modern History Faculty, Broad Street Oxford, OX1 3BD. [robert.fox@history.ox.ac.uk]

Wellcome Trust Centre for the History of Medicine at University College London

Perspectives on the History of Food Safety
at Wellcome Building
on 18 May 2001
The speakers will be: Peter Atkins, Lesley Diack, David Smith, Mike French, Jim Phillips, Nathalie Jas, Arouna Quedraogo, Eve Seguin, Keir Waddington and Patrick Zylberman. Further details from Ms S Bragg, Wellcome Trust Centre for the History of Medicine at University College London, 24 Eversholt Street London, NW1 1AD.