

Institute *of* **Physics**

History of Physics Group

Newsletter

2002

No. 15

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Editorial

Welcome to the 2002 Newsletter. Scientific walks feature several times in this issue. Kate Crennell has walked the Oxford Science Walk and the Bath Scientific Heritage Trail for us, and her illustrated accounts make interesting reading. The leaflets are readily available for you to do the walks yourself, if you're in the area. In addition, the group is organising a walk around Oxford on March 9th, specifically to do with the history of physics, and finishing with a lecture – do come along!

We have another blue plaque for another physicist – this time John Henry Poynting. You can read Peter Rowlands' account of the unveiling and article about the man on page 33. And for a brief portrait of Ernest Marsden, always in Rutherford's shadow, turn to Malcolm Cooper's article on page 23.

We had two successful meetings in the past year. The first one was on **Space**, back in May at the Science Museum in London. Read Jacqueline Mitton's talk on UFOs, Aliens and Life in Space on page 38.

Starting on page 44, we have a thorough write-up of December's meeting on **The Nobel Century**. We have the Laureate's view from Sir Joseph Rotblat (The Importance of being a Nobel Laureate), the Swedish perspective from Sven Widmalm (The Nobel Century: reflections on a curious phenomenon) and some Cautionary Tales from Peter Rowlands to close.

As well as the Oxford walk, the Group has several other plans for the coming year: we have a Dirac Centenary meeting in Brighton in April and another meeting in October. We will also be trying something a bit different by organising a residential weekend in September. So if you've not been along to one of the meetings before, why not come along this year. And if you're a regular attender, you know you're always welcome.

Dates for your diary:

- Saturday March 9th OXFORD WALK
- Tuesday April 9th DIRAC CENTENARY MEETING
- 7th or 14th September RESIDENTIAL WEEKEND
- Saturday October 19th MEETING (subject TBC)

Lucy Gibson

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Future Meetings arranged by this Group

Oxford History Walk

Saturday 9th March 2002

A guided tour round sites of historical Physics interest in Oxford (flyer with further information sent round separately; old01@ic.ac.uk).

Dirac Centenary Celebration

Tuesday 9th April 2002

The Brighton Centre, Brighton, UK (Part of this year's IOP Congress)

Speakers:

Helge Kragh	<u>The Life and Times of Dirac</u>
Sir Roger Penrose	<u>Dirac's influence</u>
Neil Turok	<u>Anti-matter (and cosmology)</u>

In addition, the 12:00 IOP Plenary Lecture will be:

Sir Chris Llewellyn Smith	<u>Particle Physics and Dirac</u>
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*For further information: Conferences Department, Institute of Physics,
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Residential Weekend

Weekend of 7th or 14th September 2002

History of Physics residential weekend, with invited speakers and visits.
Details will be sent out separately nearer the time.

Chairman's Report

Given to the Group at the AGM, Saturday 1st December 2001

I would like to present my Chairman's Report for the year since the last Annual General Meeting, and I will naturally start with our Membership number for the Group, which today stands at 531, the highest we have been and 25% more than two years ago. So that is faster than the growth in the membership of the Institute as a whole. However, on the other hand, we attract only 1.5% of the Institute's members to the group, so we have lots of opportunity to grow. Another concern has to be the small percentage of our members that come to our meetings, such as the one we have just had - the programmes are invariably excellent, but we only get some 5-10% of our members at any of our meetings. Perhaps we could discuss what we can do about it during our general discussion.

We had two 1/2-day meetings during the year, both of which were most enjoyable. Hugh Montgomery organised a meeting here at Portland Place on Quantum Concepts, past and present. We had talks not only from Hugh but one from Peter Landsberg from Southampton on the Planck Radiation Law, from Michel Bitbol of CNRS on early wave mechanical accounts of α -ray tracks in the Wilson Cloud Chamber and from Basil Hiley from Birkbeck on Bohr's contribution to the interpretation of Quantum Mechanics. There was then a roundtable. That is not an easy area, but the meeting went very well.

In May, Christopher Ray organised a half-day meeting on Space held, thanks to Sophie Duncan, at the Science Museum who also arranged that we had a tour of the Space Gallery guided by Doug Millard, its Curator. At the meeting itself, Doug told us the history of British rocketry. We had a talk from Jonathan Allday from King's School, Canterbury on the Apollo Missions and by Robin Catchpole from the Royal Observatory on the Hubble Telescope. We had a delightful talk by Jacqueline Mitton on the Search for Extra-Terrestrial Life - convincing us that we have not yet met any aliens. (Of course PhysicsWeb pointed out only 2 days ago, that 76 planets have now been detected outside the solar system and the first evidence was announced that one has an atmosphere - not very encouragingly of sodium vapour! But it's early days.)

We, of course, organised meetings of our Committee around these scientific meetings, just as we had one this morning - and that will lead to some of the items we would like to discuss later.

Issue 14 of our Newsletter, again impressively edited by Lucy Gibson, came out in the Spring, again a very full Newsletter.

Coming to Elections and the like, as is noted in the Minutes of the last AGM, Olivia Davies was elected to the Committee and we said our thankyou's to my predecessor as Chairman and former Secretary and Treasurer and real midwife of our Group, John Roche.

I think that is all I want to say - except for a very important fact, and that is that two long-serving members of our Committee retire today: Hugh Montgomery and Stuart Leadstone. They have been, over the years, stalwart members of our Group, generating excellent ideas and advice and working hard for the Group - I very much hope they will continue to do so. So can I invite you all to join me in thanking them and wishing them well.

Ian Butterworth, Chairman

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

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.

Stuart Leadstone: an appreciation

Dr. Hugh Montgomery & Dr. John Roche

Stuart Leadstone's retirement from the committee of the History of Physics Group marks the departure of the last of its founding members, and he will be sorely missed. For the last 18 years he has supported the work of the committee in an enthusiastic but unassuming way, and his energy and shrewdness have been invaluable.

Stuart graduated in physics and theoretical physics in 1961, and he threw himself wholeheartedly into physics education. His 35 years at the chalkface have brought him experience of a variety of different types of school, and recently he has taught at the Atlantic College in Wales and at Banchory Academy in Scotland, where he is now Principal Teacher of Physics. He also keeps closely in touch with the Nat. Phil. Department in Aberdeen University. He has won various teaching awards, reflecting his concern for conceptual clarity and rigour in physics, most notably the Institute of Physics Teacher of the Year Award in 1998.

Those of us who have tried to teach physics know that the failure rate among teachers is rather high, even among those of us who enjoy it and think we are doing rather well. Stuart's success has several causes. He has a natural keenness and dislike of laziness which makes students realise that something is expected of them, but he never for a moment pretends that the subject is easy, and never tries to impress the class with his own expertise. This gives him a rapport with students that they value highly, as several of his ex-pupils have told me.

Another reason why Stuart is a good teacher is that he treats physics as a human enterprise, and has a long-standing interest in its history. He does not often introduce history consciously into his physics lessons, and my guess is that it guides him in what not to say, rather than in what to say. However he does regard physics as a subject for the imagination, not merely for logical manipulation.

Stuart has organised meetings of the History of Physics Group in Cardiff, in London, in Edinburgh (3 meetings) and in Oxford. His meetings have always been characterised by enthusiasm, efficiency and a good attendance. He carried out excellent work at Group committee meetings

and contributed many pieces for the History of Physics Group Newsletter. He is also a strong promoter of archival work in the history of physics in Scotland. His retirement is due to his present heavy workload, not to any loss of interest. Banchory does not lie quite at the centre of the UK, but let's hope that he will find time to contribute to at least some of our meetings in the future.

The following is a selection of some of Stuart's publications:

- Maskelyne's Schiehallion Experiment of 1774. *Physics Education* **9** 452 (1974)
- The Photoelectric Effect – A Suitable Case for Surgery? *Physicists Look Back: Studies in the History of Physics* (Ed. J. Roche) Adam Hilger p.169 (1990)
- Abnormal View of Reflection and Refraction. *School Science Review* **76** 114 (1995)

Dr. Hugh Montgomery: an appreciation

Dr. John Roche

Dr Hugh Montgomery joined the committee of the History of Physics Group in 1994. He graduated in physics at Oxford in 1952 and worked there under Sir Francis Simon in the low temperature physics laboratory, receiving his doctorate in 1956. He then spent 10 years at Harwell and subsequently joined the physics department at Edinburgh (then known as the Department of Natural Philosophy) where he remained until 1997.

During his career Hugh developed a particular interest in the interpretation of quantum mechanics and later in some knotty problems in electromagnetism. While an undergraduate he attended a course in the history of science and always maintained an interest in the subject afterwards. He also became interested in the philosophical aspects of certain physics concepts, particularly the philosophy of space, which he studied for a term at Cambridge under Mary Hesse.

Hugh was memorable at committee meetings for his quiet charm, his carefully considered suggestions and his intense interest in all of the activities of the group. He served on many meeting planning teams, he chaired meetings, and wrote for the Newsletter. Hugh has the rare quality of "presence", and will be much missed from the committee.

The Oxford Science Walk

Kate Crennell

The Oxford Science Walk text Sophie Huxley, illustrations Edith Gollnast
Published 1993, 20 pages, ISBN 0 9522671 0 1 by "Science Walk
Publications"

Abstract

This booklet describes a walk around Oxford looking at places associated with scientists from the time of the founding of the University in the thirteenth century to modern science in the twentieth. The places are mostly buildings where scientists worked or memorials but the only scientific instruments to be seen are inside the Museums. There is a brief paragraph about the achievements of each scientist and directions how to get from one place to the next.

Starting the walk

The first page has a useful sketch map with the walking route marked on it; each site has a number which is shown on the map and in the booklet. Helpful directions are given explaining how to get from one site to the next and where to look for the interesting memorial. You can start anywhere, however the booklet begins at the Botanical Gardens. This is probably because the author, Sophie Huxley, is a biologist and a descendent of the T. H. Huxley who, in the nineteenth century, confronted Samuel Wilberforce, Bishop of Oxford, in a debate in the University Museum on the validity of the theory of evolution and the alternative story of creation in the Bible, the so-called "Apes and Angels" debate.

After looking in the Gardens and the memorial to the discoverers of penicillin I went into the University Parks to look for the plaque in honour of James Sadler, the first English aeronaut who flew in a hot air balloon in 1784. This is where I found that the elegant line drawings which illustrate this booklet are not all true representations of the memorials. James Sadler's memorial has a small balloon on it, but it is not as elegant as the one drawn in the booklet.

Sundials abound in Oxford which is strange because down in the Thames valley the skies are often grey and overcast. The first one mentioned in the booklet is that constructed in 1629 on the external wall of Merton College Chapel which uses a brass bullet set in the corner of the adjacent

buttress as a *gnomon*. It is not known who made it, either John Bainbridge, first Savilian Professor of Astronomy, or Henry Briggs, who succeeded Sir Henry Savile as Professor of Geometry. Memorials to both men can be found in the College Chapel.



Fig. 1

Figure 1 shows the next sundial on the walk, this one is in the Great Quad of All Souls. It was designed by Christopher Wren. He was a mathematician and astronomer as well as an architect. In Oxford he built the Sheldonian Theatre with its remarkable ceiling, Queen's College Chapel and the Tom Tower in Christ Church, where the heaviest bell in Oxfordshire, Great Tom, weighing over 6 tons, is still rung daily at 9.05pm for 101 strokes. Christopher Wren was also one of those who attended meetings of the Oxford Philosophical Society in Wadham College in the mid seventeenth century together with other scientists who had moved to Oxford because of the unrest in London during the Commonwealth. Some of these scientists later founded the Royal Society in London.

Perhaps the skies were clearer in the days when the Radcliffe Observatory was built in 1773. There are no telescopes there now, it is used by Green College and is closed to visitors. It can be seen just to the North of the Radcliffe Infirmary in Woodstock Road.

Other scientists mentioned in the booklet which I do not have space to describe in detail are: Robert Boyle and Robert Hooke, Edmund Halley, Dorothy Hodgkin and numerous biologists. The Museum of the History of Science in Broad Street and the University Museum in Parks Road (<http://www.oum.ox.ac.uk/> with a useful set of links to other Museums) are well worth visiting; you can easily spend a whole afternoon in either of them.

More recent sites not mentioned in the booklet

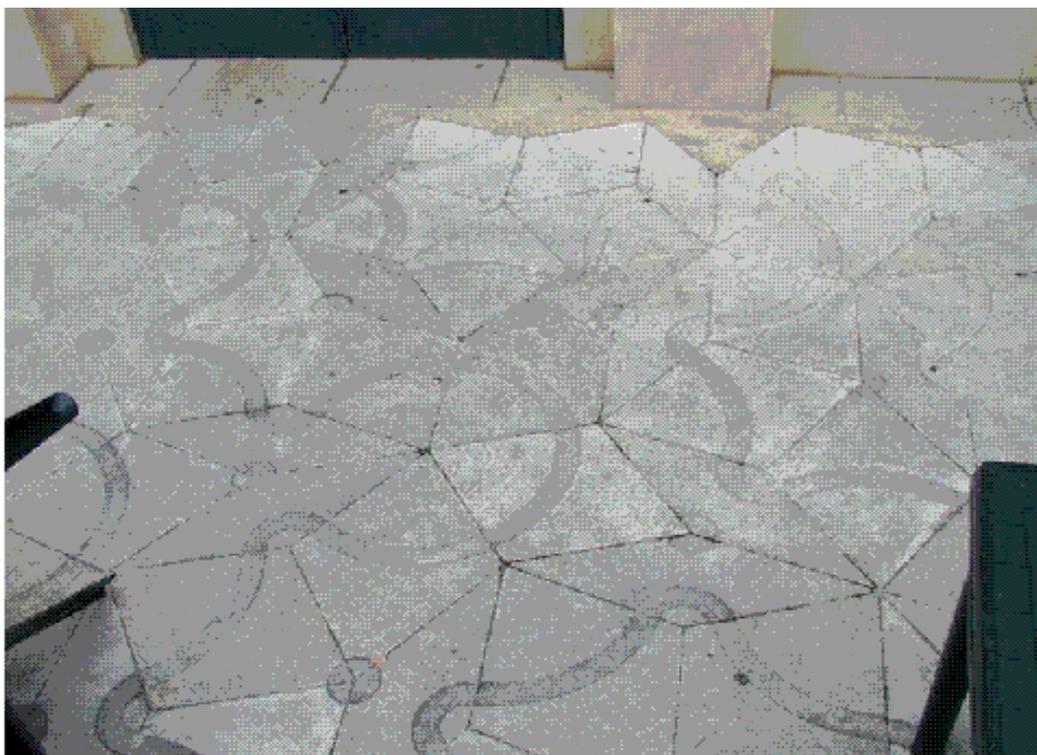


Fig. 2

Roger Penrose was a fellow of Wadham College and when the undergraduates wanted an extension to their patio bar, he persuaded the college to decorate it with a “penrose tiling” of kites and darts as shown in the photograph above.

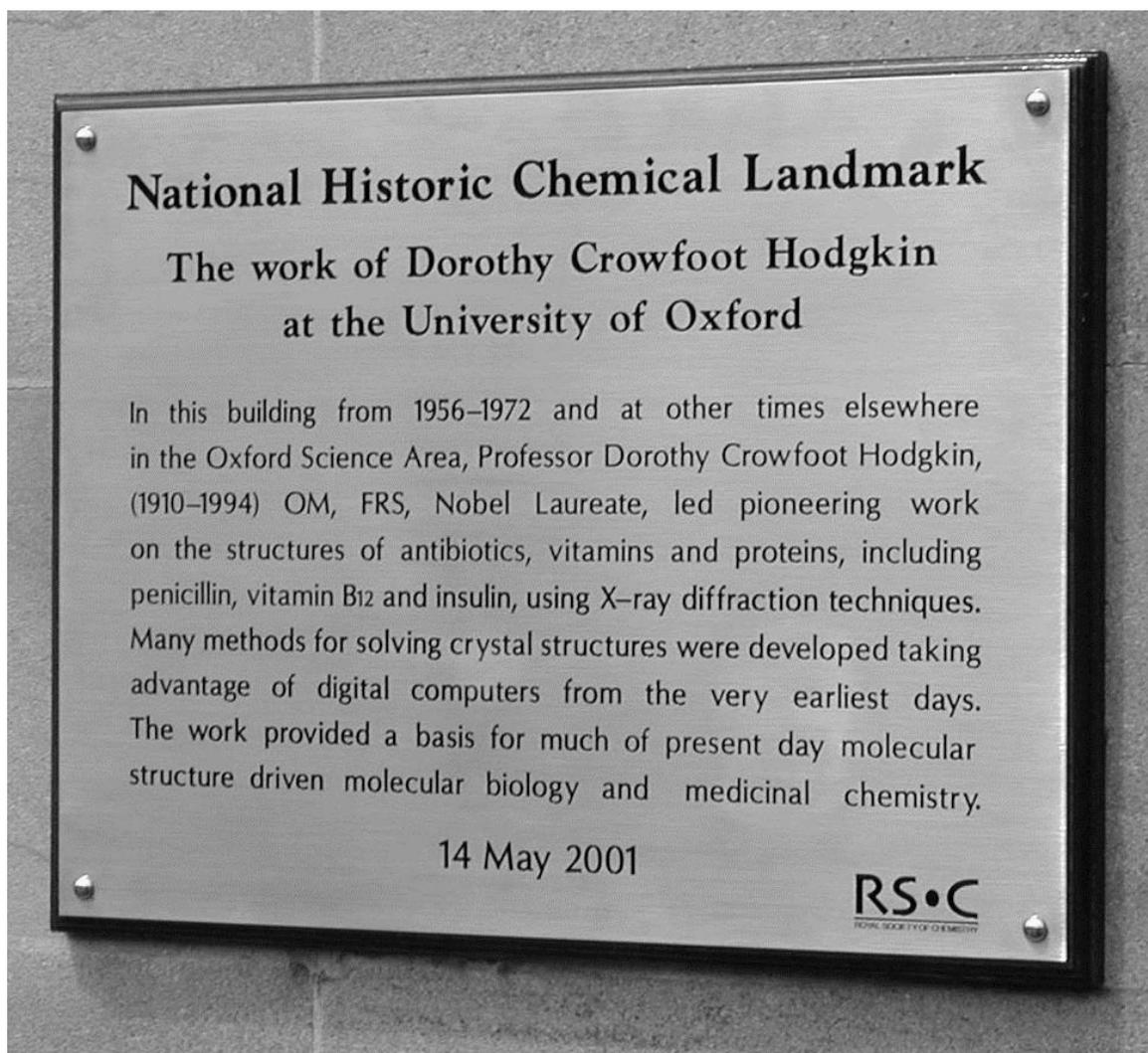


Fig 3

Further along Parks Road near the pedestrian crossing at South Parks Road, on the wall of the entrance to the Chemistry building, is a new memorial (Fig. 3) to the work of the Nobel prize winning crystallographer Dorothy Hodgkin, which was unveiled in May 2001 by the Royal Society of Chemistry. (Further details can be found on the website of the British Crystallographic Association at <http://bca.cryst.bbk.ac.uk/BCA/CNews/2001/RSC.html>.)

Continuing along Parks Road one reaches the Physics Department and the two buildings of the Clarendon laboratory, where much pioneering research in low temperature physics was carried out between the World Wars in the twentieth century. The latest building between the 2 older Clarendon buildings is the Sir Martin Wood Lecture theatre, donated by

the founder of Oxford Instruments which specialises in the production of high field low temperature magnets.

If you have children with you on your walk they may like to visit “CuriOxCity” a “hands on” interactive exhibition for young people in George Street near the Old Fire Station.



Fig.4

A modern sundial erected in the Pocock garden of Christ Church (Fig. 4), not normally open to the public but it may be seen by prior arrangement with the college.

Twentieth century industry is represented by a small plaque on the wall at first floor level near the Law Courts which reads "William Morris, Motor manufacturer and trader".

I enjoyed this walk very much and would be happy to advise members who want to walk along it for themselves.

Practicalities

Availability: The booklet is available from the Oxford Tourist Information Centre, some of the Museums and the bookshops (e.g. Blackwells) in the City. It has now been mounted on the website of the Museum of the History of Science (<http://www.mhs.ox.ac.uk/features/walk/index.htm>) where you can take a virtual walk by clicking on a map on the screen and then printing the web pages. If you have a fast connection to the Internet this site is tolerable, if you have not, you will find yourself waiting a long time to download images which merely have text in them. If you turn off the images, the site is unusable. It is a pity that the creator of the Web pages did not take the opportunity to update the walk with sites of interest erected more recently. The booklet may also be ordered from the Museum of the History of Science price £3.95 inclusive of Postage & Packing. It is a convenient format (size 125mm x 215mm) and easy to carry around on the walk.

Getting to and around Oxford: Oxford is well served by public transport; it is probably best to arrive by train. Driving in Oxford is a nightmare of one way streets, pedestrian precincts and traffic jams. The central car parks are expensive and often full; the “Park and Ride” large car parks on the outskirts of the City are no longer free and the price of a bus ticket to the centre seems to go up every few months. Once there you can buy one day bus “Rover tickets” which allow you to take any bus within the city limits, or you can get a ticket for the open topped tourist bus and get on and off anywhere on its route. Undergraduates cycle on the “cycle tracks” denoted with green paint on the edges of many of the roads, but there are few cycle parks so it is probably better to walk. This can be tiring; fortunately there are many coffee shops where you can rest to plan the remainder of your walk, or pubs with historical connections, frequented by authors such as C. S. Lewis whose science fiction novels include *Out of the Silent Planet* or where Colin Dexter’s “Inspector Morse” liked to drink his pint of beer.

The authors state, “there is about an hour of actual walking”, but I found that if you want to examine the places in detail it takes much longer than that. Even if you know your way around Oxford some of the memorials are hard to find, so perhaps plan to spend a weekend there. Some sites can be seen at any time, others are inside colleges which are only open to visitors in the afternoons. There is a list of Museums and their opening times on the back page, but it is best to check these in advance either on the world wide web or at the tourist information centre and then plan your route.

The Bath Scientific Heritage Trail

Kate Crennell

The Bath Scientific Heritage Trail was launched at the Bath Royal Literary and Scientific Institution (BRLSI) on 19th January 2001, accompanied by an exhibition of photographs and artefacts. It is the result of a collaboration between the BRLSI and the West of England branch of the British Association for the Understanding of Science.

Getting Started

The poster has a map, with a scale but no arrow indicating the direction of North. There is no suggested order in which to visit the sites, nor exact instructions on how to get from one to the next. Instead red circles on the map are joined by a red line to a description of the site. The introduction says:

'Bath is so much more than the baths, Royal Crescent, and Jane Austen. In earlier times, before science and engineering turned professional, such invention and discovery was undertaken by wealthy fashionable people (Gentlemen essentially). Such people went to fashionable places such as Bath, so it is perhaps no great surprise that Bath was a real hotbed of science and engineering during the 18th and 19th centuries.'

Figure 2 shows where I started, at the Beazer Gardens Maze beside the Avon river near Poultney Bridge. This is an elliptical maze created in 1984 by Adrian Fisher when the theme of the Bath Festival of the Arts was "The Maze". The circular centre is a mosaic showing a copy of "Sulis", a Gorgon's head carving found on a pediment of the nearby Roman temple.

You may find it easier to start at the BRLSI in Queen Square which has a wide range of activities and exhibitions and helpful staff. It is built on the site of the house of Dr William Oliver (1695-1764), who was a founder of the Mineral Water Hospital and invented the "Bath Oliver" biscuit as part of his cure for rheumatic diseases.



Fig. 2

The Roman Baths are fed by local hot springs which contain many minerals. Richard Strutt (1847-1927) and Sir James Dewar (1842-1923, inventor of the Thermos flask) detected helium in these waters. Sir William Ramsey (1852-1916, a Noble Prize winner for chemistry) detected radium.

My impression was that most of this trail is concerned with Victorian industrial innovation and engineering projects such as the building of the nearby Kennet and Avon Canal. Some day I should like to visit the Claverton Pumping Station and the Dundas Aqueduct which carries the Kennet and Avon Canal across the Avon Valley at Limpley Stoke over the river and the railway.

In the city I looked for the lodgings of William Smith (1769-1839) who came to Bath as a surveyor for mining companies, while staying in Bath he dictated his work "The order of the strata". He was the first person to realise that rock and sediment were laid down in a regular sequence identified by the fossils contained in the layers. The leaflet gives some addresses where he lived and had his offices, but little is left of his activities there. I spent some time searching for the address and finally decided that it had been obliterated by a smooth Bath stone modern office building, without even a small commemorative plaque.

The highlight of the trail for a physicist is the Museum in the Herschel House, (open afternoons only) where William Herschel, the discoverer of Uranus, and his sister Caroline, a comet watcher, lived for some years. His telescope is on view but was too big for me to photograph easily inside the small room. He was an accomplished musician and conducted the first performance in Bath of Handel's "Messiah".



Fig. 3

Figure 3 shows his music room and small telescope. The background music which was playing in the entrance hall was one of his compositions, which can be purchased on a CD in the shop.

Outside in the garden where he did his observing is a sundial, a sculpture of William and Caroline and fixed to the wall a *GREEN* plaque (Figure 4, not one of their usual blue ones) erected by the IoP which reads:

*Here lived Scientist and Musician Sir William Herschel, 1738 – 1822,
from where he found the planet Uranus, March 13th 1781. He also
discovered infra-red radiation in 1800 ~ and his sister Caroline Herschel
an early woman scientist, 1750 – 1848, hunter of comets*



Fig. 4

The last place I visited was the Museum of Bath at Work which houses the engineering collection of the Victorian entrepreneur Jonathan Burdett Bowler, who manufactured ginger beer in stone bottles. Figure 5 shows his office resplendent with typewriter and clock.

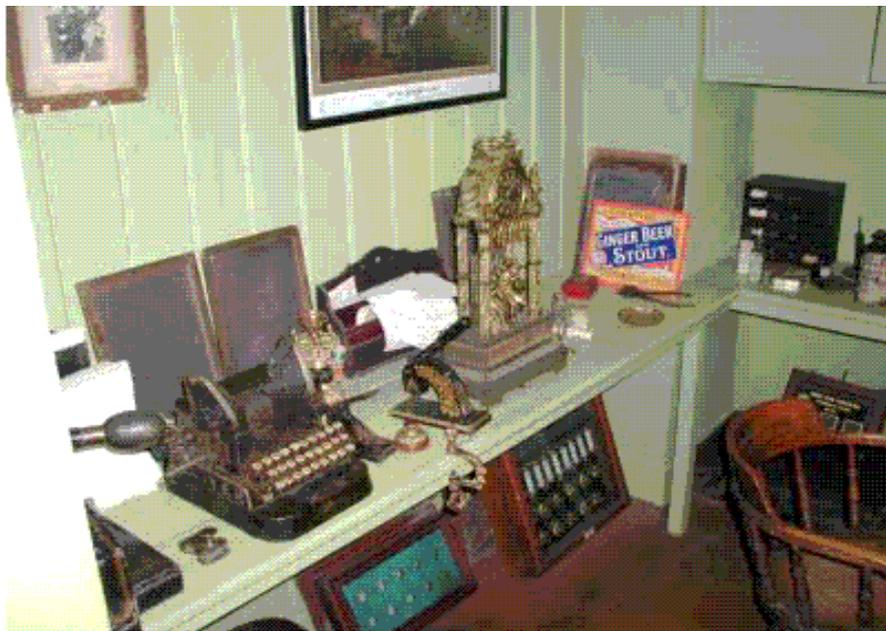


Fig. 5

Other industries represented there are weaving, power generation, brewing, car making and ship building.

There is plenty to see on this trail, if you wish to stay longer than a day you might enquire of the Landmark Trust, who maintain flats in several houses in the city, Elton House just round the corner from Abbey Square, Marshal Wade's House is in the square itself above the National Trust shop, or if you prefer staying in a folly, you can rent a holiday apartment for a few days in the Beckford's Tower on Lansdown Hill two miles north of the city. The tower is also open to visitors at weekends between March and October.



Further information:

Bath Preservation Trust for the Herschel House, Beckford's Tower and the Building of Bath Museum tel:(0225) 311342 Web site <http://www.bath-preservation-trust.org.uk>

Museum of Bath at Work tel:(01225) 318 348 Web site <http://www.bath-at-work.org.uk>

Bath Royal Literary and Scientific Institution (BRLSI) tel:(01225) 312 084; website <http://brisi.bath.ac.uk/brisi.html>

British Association for the Advancement of Science West of England branch website <http://www.ba-west.org.uk>; Send an email to trail@ba-west.org.uk for current availability of the poster.

The Landmark Trust tel: (01628) 825 925 website <http://www.landmarktrust.co.uk>

Practicalities

The leaflet describing the trail is an A2 sized poster printed in full colour on both sides of glossy paper with photographic illustrations. It is normally folded down to become 100mm x 210mm so it can easily be carried in a pocket but if the day is at all windy the page becomes unwieldy and somehow the information you want always seems to be on the other side of the paper.

Ten thousand copies of the poster were printed and freely distributed in June 2001 thanks to financial support from Copus, the Committee for the Public Understanding of Science, and the Institute of Physics. Two images of the two sides of the poster, size just under 500kB, may be downloaded from their web site (at <http://www.ba-west.org.uk/maps.htm>) unfortunately, it is too low a resolution for the text to be legible. It is probably better to get one of the glossy leaflets, which were freely distributed at tourist sites throughout Bath. To check on current availability contact by email trail@ba-west.org.uk

Getting to Bath

Like Oxford, Bath has a good train service. It is one of the stops on the old GWR (God's Wonderful Railway) built by I. K. Brunel in the nineteenth century. Like Oxford, Bath is ruined for the motorist by traffic jams, it too has many one way streets for cars, pedestrian precincts and scarce, expensive car parks. Unlike Oxford, Bath is hilly, so you need to be fit to cycle. Unfortunately many of the sites in this poster are well outside the city limits, so you will need a car to visit some of them. I looked only at some of the parts which can be walked to within the city.

Like Oxford you can ride on an open topped "Guide Friday" tourist bus with a commentary. You can also buy a "Smart Pass" which will enable you to get reduced fares at many of the popular attractions and museums including the Roman Baths, and the Jane Austen Centre; details from www.bathpass.com or tel: 0870242 9988. This is probably worth getting, since most of the places of interest charge a modest entrance fee. The museums are mostly privately owned commercial businesses which are open all day. Most have associated coffee shops.

A comparison of the two walks

Costs: The Oxford Booklet costs £3.95 from the Museum of the History of Science. The Bath leaflet is free but may no longer be available. Most of the Oxford Museums belong to the University. Entrance is free although you are asked to give a donation towards their upkeep; and their opening hours may be only afternoons as they are working academic institutions. Bath museums and the Abbey mostly charge a few pounds entrance fee, but they are open for more of the day and a “Bath pass” is available which entitles you to reduced entrance fees.

The booklet/poster: The area of paper in each is about the same, but the Oxford booklet is easier to handle while walking in windy weather. Neither have complete maps, the Oxford one marks North but lacks a scale, the Bath one has a scale but no orientation information. Neither have an index to people or places. The Oxford booklet assigns a number to each site, which is marked on the map and printed in the booklet. The map has a dotted line showing the path through the city between the sites. The text gives detailed instructions. The Bath map marks sites with a red circle joined by a red line to the description. The Oxford booklet has black and white line drawings, the Bath poster coloured photographs, but uses a larger text size. There is much more information about each person in the Oxford booklet.

Websites: The Oxford walk can be downloaded from the website if you have a fast communications line which can load such a lot of images. If you are partially sighted or on a slow line and turn off the images the site is unusable. The Bath site has a nice introduction but the resolution of the poster images they provide is too low for the text to be readable.

Getting there and travel within the city: Both cities are choked with car traffic and have good rail connections. They both have plenty of service and tourist buses. The Oxford walk is only within the city and can be covered on foot. Many places described in the Bath poster are miles from the city so you need a car to visit them.

Historical sites: Bath is older than Oxford but has not been developed so continuously. There was development in Roman times, about 100 A.D and again in Georgian times and then in Victorian industrial areas. Any Roman buildings in Oxford have long since been built over; some colleges date from the 13th century and there has been continuous development since.

A Sketch of Ernest Marsden

Malcolm Cooper



**Fig. 1: Sir Ernest Marsden
CMG, CBE, DSc, FRS, FRSNZ
1898-1970**

Introduction

Sir Ernest Marsden is little known within the physics community in the UK, and probably not at all amongst the general public and yet he made a vital contribution to atomic physics in the early 20th century and continued to apply the rigour and discipline of his scientific training in many other areas of activity throughout his life. Like many others, he had come under the powerful influence of Sir Ernest Rutherford but it is clear that Marsden was able to channel the inspiration derived from Rutherford to reinforce his own vibrant and dynamic personality, especially regarding his direct approach to tackling problems. This, and his infectious enthusiasm for any task he undertook, has been his legacy for which he is well remembered in New Zealand and should perhaps be more well known elsewhere.

If Sir Ernest Marsden is remembered at all, it is as one of the two people who actually carried out the alpha particle scattering experiment, but often with the caveat, “Of course it was Rutherford who ...”. That Rutherford was the great mind behind the concept is beyond doubt and yet such a meagre description of Marsden’s role would be to unfairly diminish his part in the story.

The early 20th century model of the atom is rightly remembered as the Rutherford-Bohr model; Neils Bohr introducing the concept of quantum electron energies in 1913 to the earlier Rutherford model - one in which the bulk of the atomic mass was confined to a small central body. Perhaps, as has been suggested, Rutherford had had some idea in the back of his mind that the structure of the atom was not as J. J. Thompson and Lord Kelvin had put forward, and had already put Geiger and Marsden to work investigating the scattering effects of metal foils on alpha particles. Or maybe it was just a hunch at the time that the mass and charge of an atom were concentrated in some unknown way which led him to further investigate these effects. In any event it was specifically to the young Marsden that Rutherford had turned to speak the much quoted line, “See if you can get some effect of alpha particles directly reflected from a metal surface”.

Marsden, then still an undergraduate, could have had no real idea of the possible significance that these experiments were to have, but nevertheless met the challenge with enthusiasm and a thoroughness which was to typify his whole life. He later said, “I do not think he (Rutherford) expected any such result ... yet if I missed any positive result it would be an *unforgivable sin*.” [my italics]

Although Marsden continued working under the guidance of research assistant, Hans Geiger, as part of the final year undergraduate course at Manchester at that time, it seems he performed the experiments not only with great care but also with considerable independence. Sir James Chadwick was to later comment, “Observations by Marsden confirmed and amplified in joint work with Geiger ...” and E. N. Da C Andrade said of the 1961 Rutherford Jubilee, “This session was thus, in a way, a Rutherford-Marsden celebration”.

The experiment is very well known of course but perhaps a brief review of its aims is worthwhile. It consisted of three parts, each to investigate* :

1. The amount reflected from different metals ranging in atomic weight from aluminium to lead.
2. The amount of reflection from gold of different thickness.
3. The fraction of reflected particles.

The final results, as published in the 1909 paper, fell out beautifully. In Experiment 1 the effect clearly varied as the atomic weight $A^{3/2}$ and not as $A^{1/2}$ as was first thought. Experiment 2 showed that the number of particles reflected increased with the thickness (up to a limit of about 10^{-5} m) indicating that the effect was not just a surface one; and of course, not forgetting the famous result of Experiment 3 that about one in eight thousand particles was reflected through 90 degrees or more.

Incidentally, not everything was plain sailing. Initially, measurements for the silver sample in Experiment 1 did not fit at all well. Marsden, however, quickly traced the problem to its source. He had been given a silver coin by a colleague, S Antonoff, with the assurance that Russian coinage was much purer than British. This may well have been the case normally but Antonoff had failed to mention that this same coin had already been used in an experiment and had consequently become contaminated with polonium!

Thus, Rutherford was brought to the idea of the nucleus by the results so carefully found by Marsden and which were formalised in his joint paper with Geiger in 1909, which, incidentally, also represented the first concrete evidence supporting such a scheme. Marsden had avoided “the unforgivable sin”.

* I am using the language of the paper here.

The year after publication he took a job as a lecturer at the East London College and during his absence, Geiger continued working on Rutherford's hypothesis. Only a year later, however, he returned, perhaps discontent leaving at such a crucial time, and plunged back into intensive experimental work testing the new theory.

By now Marsden himself had a number of young men working under his guidance and fruitful years followed during which he published many papers concerning a whole series of experiments which further confirmed Rutherford's predictions. These were nearly all joint papers with such as C. G. Darwin, A. B. Wood, R. H. Wilson, W. C. Lantsberry and many others. Marsden had proved himself a formidable experimenter - one to whom Rutherford could and did turn with great confidence.

It seems extraordinary then, when in 1915 he left England to take up the post of Professor of Physics, Victoria College, Wellington, New Zealand. There are a number of reasons, however, why Marsden, after such a profound start in research physics, should decide to leave Manchester, Rutherford and the cutting edge of experimental atomic physics at such a point in his career and move to faraway New Zealand.

Many can bear witness to Rutherford's powerful personality and there can be little doubt that it was Marsden's utter dedication to Rutherford which lay at the heart of his decision.

The previous incumbent at Victoria College, Prof. T. H. Laby (of Kaye & Laby), disappointed with the lack of research facilities at Wellington, had resigned and moved to Melbourne leaving the chair vacant. Rutherford, highly regarded in NZ, of course, was returning from a BA meeting in Sydney and called in to Wellington at the request of the College Council to discuss Laby's successor. He obviously had great influence with the council and his advice must have been quite unequivocal since apparently they appointed Marsden there and then without further advertising!

He could never have refused. And in any case he was a man of boundless energy, restless and was well suited to this adventure, the first of many journeys in his globe trotting life.

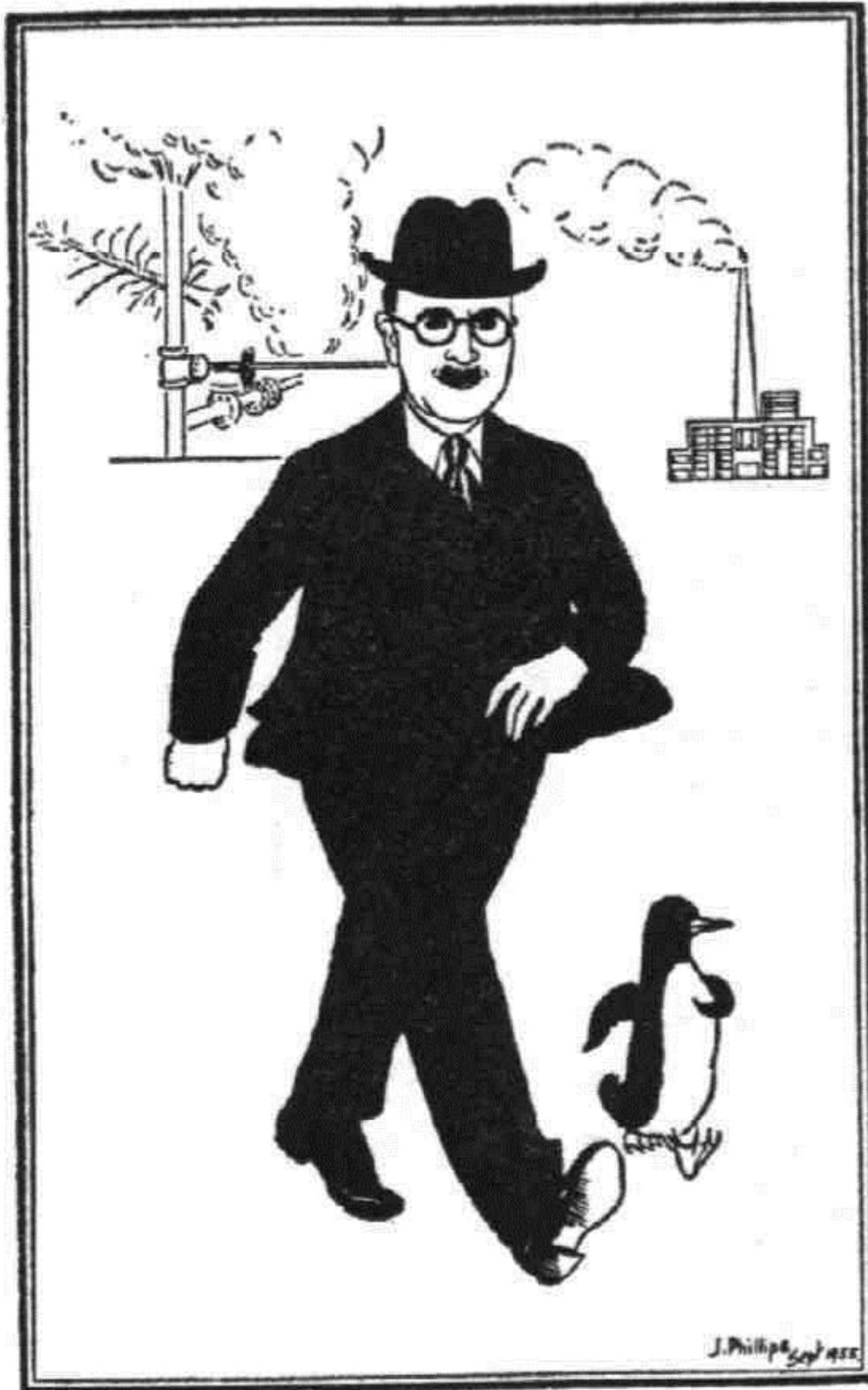


Fig. 2

He arrived in Wellington in the spring of 1915 but by the spring of 1916 he was on his way back to Europe having volunteered for overseas service with the Armed Forces. In 1917 he was sent to the France and was wounded in action. He was awarded the Military Cross and was mentioned in Despatches for Gallantry in the Field. It is a terrible irony that at one point he was in the trenches fighting opposite his old friend and colleague Hans Geiger.

After the war he returned to his duties in Wellington. His stay at there was relatively short but even so he re-organised the Physics Dept, and instigated a new building at a cost of £20,000 (in competition with the library); his talents at gaining funding were prodigious. And, of course, he had his lecturing commitments.

He must have brought a breath of fresh air to lectures, at least he is said to have had a breezy style of lecturing (no pun intended) and would be prancing about the lecture theatre operating equipment and writing on the blackboard much to the delight of his students. With his contagious enthusiasm, he certainly seems to have had the knack of stimulating their interest and persuading them to give of their best.

But in other ways perhaps his temperament was not entirely suited to the rather enclosed life of academia. or maybe his itchy feet got the better of him. In any event, in 1922, he took the post of Assistant Director of Education, a decision he later confessed to Rutherford was somewhat ill founded.

So it was only four years later when, in 1926, he was yet again on the move. Following the success of the British DSIR, there was wide support in both countries for a similar organisation to be set up in New Zealand. Sir Frank Heath, the Permanent Secretary of the British DSIR was requested to investigate the situation and report to the British Government. The outcome of this was an Act of Parliament which made provision for just such an organisation. Heath's report also recommended Marsden to head it.

Thus it was that in the August of 1926, Marsden became its first "Secretary". Showing typical political acuity he himself had set the job title - he considered it sufficiently vague and thus would not be hidebound by any troublesome constraints implied in a more specific title! This time though, he abandoned his own maxim that "a man should change jobs every ten years in order to keep interested" and remained for over 20 years.

It was here that he made his most significant contributions in areas other than pure research, and again his dynamic personality served him well to confront a wealth of problems, seen and unseen. For example the New Zealand economy was very dependent on exports of meat and dairy produce and it must be remembered that the foodstuffs production and preservation technologies of today were not available then. Meat would go bad in transit, butter wouldn't spread well and grain often made rather poor bread. Some of these may sound trivial but it gives an idea of the range of problems he was confronted with.

He quickly identified the need for detailed studies to be made and responded to the challenge by pioneering the setting up of organised research in the form of various groups such as: The Wheat Research Institute, The Dairy Research Institute, The Wool Industries Research Institute, The Soil Bureau and many, many others. There seemed to be no problem to which he would not turn his attention.

By means of these various organisations operating under the control of the DSIR he was able to bring scientific method to industry and consequently improvements were made in all these areas and in a relatively short time. He was not the sort of person to "hang about". It is said that, "Marsden marshalled the DSIR into gear with such a speed that the laymen were breathless". Furthermore, with the stimulation of applied biology in, for example, plant research, and by the revolutionary idea that agriculture was indeed an industry, Marsden had provided the drive and means to unify and direct the efforts of hitherto disparate areas of industry, farming and academic scientific research into a very powerful and effective force.

During these dozen or so years he had worked very hard and accomplished much and now at the age of 50 had to face another world war. Not surprisingly he met the demands of World War II and addressed the problems of defence with characteristic vigour and tactics. He was sent by the NZ government to Bawdsey Manor, England to a gathering of physicists to hear about the newly invented Radar. Not only did he learn all about it, but also he managed to return with a quantity of prototype equipment which gave them a head start over many other countries (including Australia and the USA) in radar production. He had also managed to get one of his teams over to MIT and learn about the latest microwave technology and thus was able to produce and supply radar sets to US forces before their own production was properly underway.

But he did not fare very well with military formality and protocol. It is said that he frequently left the office wearing full uniform complete with a trilby. Worse still he once scandalised some guards at a defence post by acknowledging their salute by politely raising his lieutenant colonel's cap!

Uniforms apart, he was undoubtedly capable of considerable diplomacy. An excellent example of his technique was shown when during a wartime project, university engineers had rebelled against military authority. Marsden's solution was as follows:

To the commanding officer he said,
"This young fellow's got you cold. Yours maths won't bear investigation, I think you should make your peace with him"

And to the engineer:

"Look, this man's your commanding officer. If it comes to a court martial, I can do nothing for you!"

Apparently it worked!

He was never above a little deception to get the outcome he wanted. During the early research into radar he had two teams - one in Wellington and one in Christchurch. He would travel between the two reporting to one team how well ahead the others were. And *vice-versa*! Indeed, he became so well known for these kinds of ruses that he was nicknamed "toffee apple" on account of the fact that he often preceded his requests with some incentive or other.

His enthusiasm and single-mindedness involved him in a great many schemes - too many some have said. There is some substance to his critics' claim that he often promised more than he could deliver. However his promises were never empty ones but were borne of his sheer enthusiasm and ebullience.

As may be imagined he was not able to devise and implement these various schemes unchallenged but had to deal with a bureaucracy which sometimes tested his considerable political skills and endurance to the limit. In fact it was partly this that persuaded him ultimately to resign from the DSIR and to choose to finish his working life as scientific advisor for the NZ government in London.

Throughout the period of his working life he managed to publish nearly 90 papers; many of those published after 1926 were formalising work done at the DSIR. However, he never abandoned his early interest in radioactivity and he put out several papers on the possible connection between (ionising) radiation and cancer as late as 1965.

It was said earlier that he had left Rutherford and the cutting edge of research but it is also true to say that wherever the cutting edge may be, he was convinced it needed a sure and guiding hand and he did his utmost to propitiate this wherever possible. In his own words at the Rutherford Jubilee, “ ... let us make some effort to strive for conditions ... under which future Rutherfords may arise with the same international sympathy and understanding”.

His overriding concerns were always to apply the methods of rigour and flexibility of the scientific approach to many differing situations and to promote the ethos in which these aims may be achieved. There is little doubt that he greatly enriched the development of his country of adoption and is fondly remembered today as “Ernie” - the inspiration for such organisations as “The Marsden Fund” - a NZ Government research fund. But he has perhaps been too neglected in his country of birth and it is hoped that others will focus on the values, goals and aspirations that were so dear to his heart.

References:

- *Rutherford at Manchester*, Ed. J. B. Birks 1962 Sir Ernest Marsden-80th Birthday Book
- *Neils Bohrs Times*, A Pais, Biographical Memoirs- CA Fleming, Royal Society

Brief Curriculum Vitae

- 19th Feb 1898 Born, 68 Hermitage St, Rishton, Lancashire
- 1906 - 1909 Studied for a physics degree at the University of Manchester
- 1909 Published seminal paper with Hans Geiger – *On the diffuse reflections of alpha particles*
- 1909 - 1914 Except for a brief period at the East London College, he remained at Manchester, working with Geiger under the tutelage of Rutherford, and published 19 papers on radioactivity mostly concerned with the various effects of alpha particles.
- 1914 Awarded DSc (Manchester)
- 1914 - 1922 Professor of Physics, Victoria University College, Wellington, New Zealand
- 1916 - 1918 (Military Service)
- 1922 - 1926 Assistant Director of Education, New Zealand
- 1926 - 1947 General Secretary of the NZ Department of Scientific and Industrial Research
- 1947 - 1954 NZ Government Scientific Adviser, London
- 1954 Officially retired, but continued working and publishing papers until 1966.
- 15th Dec 1970 Died, Lowry Bay, Wellington, New Zealand

Interested in Ernest Rutherford?

www.rutherford.org.nz

**A well-crafted website about the man, his life and work,
developed by Dr John Campbell at the Physics Department of
the University of Canterbury in Christchurch, New Zealand.**

Blue Plaques for Physicists: John Henry Poynting

Dr. Peter Rowlands

There is now an Institute of Physics plaque to John Henry Poynting at Monton Unitarian Church in Greater Manchester. The Church dates back to 1875 and was erected by the physicist's father, the Reverend Thomas Elford Poynting. The unveiling took place on Saturday, 17th March 2001. Following an informal tour of the Church and grounds, Dr Richard Potton, Chairman of the Manchester Branch, introduced Dr Peter Rowlands, who gave a brief account of Poynting's career and scientific achievement. Then, Mrs Elizabeth Ratcliffe, grand-daughter of the physicist, formally unveiled the plaque, while Mr Julian Poynting, his great-nephew, gave a vote of thanks, and the Reverend Brenda Catherall, Minister of Monton Unitarian Church gave a Prayer of Dedication. Approximately sixty people attended, including many members of the Poynting family.



Mrs Elizabeth Ratcliffe and Dr. Richard Potton.

Location

Take M602 to junction 2, A576 towards Eccles. After a few hundred yards you are signposted left to Monton, ending up at Monton Green where the church is clearly visible.

John Henry Poynting (1852-1914)



JH Poynting

John Henry Poynting was born in the parsonage at Monton, Lancashire, on 9th September 1852, the youngest son of an equally remarkable father. Thomas Elford Poynting was a self-educated teacher from the West Country who took up a position at Flowery Fields in Manchester in 1841. He obviously didn't think that the name implied Earthly Paradise for, we are told in *The Inquirer* (1878), "He used in later life to describe the dread with which he contemplated settling among the fierce and half civilised people of the North of whose uncouth manners and violent deeds he had heard." However, he accepted the post of Minister at the Unitarian chapel in Monton in 1846 and remained there for 32 years; during his time there (1875) he built the magnificent church at Monton Green, which has now been superbly restored with the help of English Heritage. T. E. Poynting had a great interest in science, especially physics, and wrote educational works on science. The son's early education was at his father's school near the church. From there he went to Owens College, Manchester (1867-1872), gaining a BSc from the University of London in 1876, and Trinity College, Cambridge (1872-1876), where he graduated as third wrangler.

He then returned to Manchester as a demonstrator in the Physics Department (1876-78), during which period he lived with his father and attended the new church. While at Manchester, he began a lifelong friendship with J. J. Thomson, with whom he subsequently completed a large *A Text-book of Physics* in 4 volumes. At Manchester, he also produced two statistical studies on drunkenness (in which Manchester and Salford come out very badly, perhaps confirming his father's apprehensions after all!), and began a project to measure Newton's universal gravitational constant G (or, equivalently, the density of the Earth). In 1878 he became a fellow of Trinity College, Cambridge, working in the Cavendish Laboratory under James Clerk Maxwell. (He was awarded a Cambridge ScD in 1887.) His father died during that year and we have a record of the son attending the funeral at Monton. (Thomas Elford Poynting's is one of fifteen family graves in the churchyard.) His final career move was to become first Professor of Physics at Mason College, Birmingham (subsequently, the University), in 1880. In the same year, he married Maria Adney Cropper, daughter of a Unitarian minister at Stand, near Manchester, by whom he had three children.

Poynting remained at Birmingham until his death, at Edgbaston, on 30th March 1914. It was there that he found an atmosphere conducive to doing all his most important scientific work. Elected FRS in 1888, he was awarded the Adams Prize from Cambridge, University, in 1892, for an essay on "The Mean Density of the Earth", and the Hopkins Prize of

Cambridge Physical Society in 1903. He received the Royal Medal of the Royal Society in 1905, and was Bakerian Lecturer in 1910. He served as President of the Physical Society in 1905 and as Vice-President of the Royal Society from 1910 to 1911.

Poynting was equally outstanding as a theoretical scientist and an experimentalist. His greatest discovery was the Poynting theorem (1883), an equation representing the energy balance in a closed region of an electromagnetic field, from which comes an expression for the flow of energy (the Poynting flux), represented by the Poynting vector ($\mathbf{S} = \mathbf{E} \times \mathbf{H}$). This had many remarkable consequences, including the fact that energy was carried outside a wire in an electric circuit and not inside it with the current. According to J. J. Thomson, it 'revolutionized ideas about motion of energy in the electric field'. The theorem certainly made possible a vast extension of Maxwell's electromagnetic theory. It is, also, as everyone knows, the source of one of the great physics jokes ('the pointing vector') – and there aren't many of those! In the years immediately following this discovery, Poynting applied Faraday's tubes of force to the energy relations of the electric circuit (1885), and his own ideas on energy to the Leyden jar (1886).

In the same year Poynting produced a remarkable paper, in the *Mason College Magazine*, partly based on an earlier discussion by Felix Eberly, in which, 22 years before Einstein's theory of relativity, he discussed how time, measured by light signals, would seem to be extended to a space traveller approaching, say, somewhere like Sirius, at close to the speed of light, the speed of light, and how time would be effectively reversed if one exceeded it, with the second law of thermodynamics being violated, so that stones would suddenly find 'the ground beneath them grow too hot to hold them', and so be 'thrust up into the air to fall into schoolboys' hands', while 'rivers would run up-hill'. He says: 'It would require a Jules Verne to describe life as it would be in such a system.' Poynting had, however, shown that he had a vivid scientific imagination of his own.

As an experimenter, Poynting managed to measure the gravitational constant (G), using a relatively ordinary type of beam balance to obtain a value which agrees with the modern value to 4 parts in a thousand (1891). He showed, in 1905, that gravitational attractions between crystals were not dependent on orientation or temperature. In optical work, carried out with his colleague, Guy Barlow, he found that the reflection of light from a partially absorbing surface caused a tangential stress along the surface, and that refraction of a beam of light through a prism caused a torque on

the prism (1905); while the emission of heat radiation by a body caused it to recoil in the same way as a gun firing a bullet (1909). The delicate apparatus which Poynting used to discover these entirely new effects, together with the beam balance he used for his gravitational work, is still preserved and displayed in the Poynting Laboratory at Birmingham, now named after him.

Poynting made contributions in many other fields, including a theory of change of state (1881) and another on osmotic pressure (1891), but one area of particular interest was astronomy. He was a keen observer and his telescope still survives. He discovered the retarding effect produced by the transfer of momentum between photons and particles in motion about the Sun, now called the Poynting-Roberston effect, which results in small particles of dust being driven by radiation into the body of the Sun (1903), and calculated that the temperature of Mars would be too low to support life as we now know it. A lunar crater has been named the Poynting crater in his honour. He also had a great interest in the philosophy of physics, partly stemming from his Unitarian background and his father's educational influence. In 1893, he argued against the finality of mechanistic models in physics, and, six years later, gave an extensive account of his views on the philosophy of physics in a Presidential Address to Section A of the BA. According to his friend Thomson, he was a "very successful" teacher, and he was, from all accounts, very well liked by his colleagues. All known portraits of Poynting show him wearing a ring instead of a knot in his tie. After his death, his colleague Guy Barlow wore Poynting's ring in his own tie for the rest of his life.

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

Space

UFOs, Aliens and Life in Space

*Dr. Jacqueline Mitton
Royal Astronomical Society*

It's very strange, but large numbers of people believe that Earth has been visited by extraterrestrial beings. These aliens are supposed to travel in spacecraft - often saucer or cigar shaped - and they give us earthlings a convenient instant explanation for anything in the sky that doesn't immediately look familiar.

It was H. G. Wells' story about the invasion of Earth by Martians that launched the career of the alien. Wells was not the first person ever to write about extraterrestrials, but the immense success of his 1897 story *War of the Worlds* brought the idea of aliens to the public at large, particularly the idea of aliens capable of visiting Earth.

Wells was inspired by the charismatic American businessman Percival Lowell, who built an observatory in Arizona especially to observe Mars starting in 1894. Lowell had been intrigued by reports of *canali* - apparently straight line features or channels - on Mars. Lowell claimed he could see martian canals and dreamt up an imaginary civilization struggling to survive as more of the planet became desert. His books were read widely and his ideas caught on. Lowell's ideas about canals and martian civilizations were discredited by the 1920s but the popularity of aliens has been maintained by stories, movies, and sheer human fascination with the idea.

The modern era of the alien UFO began in 1947 when off-the-cuff remarks made to a reporter by an American private aviator, Kenneth Arnold, gave birth to the flying saucer legend. Before the 1940s there had been sporadic reported "sightings" of one kind and another, but communications were slower and people did not have so much radio and TV. "They flew like a saucer would if you skipped it across the water", Kenneth Arnold said, puzzling over a V-shaped formation of flying objects he had seen from his private plane. And that's how an innocent flock of what were almost certainly migrating white pelicans, glinting in the Sun as they flew, started the flying saucer craze in 1947. Arnold's

words were mis-reported. He'd seen "flying saucers". When just a few days later, an US army airbase needed to explain wreckage from a secret experiment that had gone wrong, someone had the bright idea of saying it was one of those 'flying saucers'. The claim was quickly withdrawn and the wreckage put on display. But the seeds of suspicion about a conspiracy had been sown, seeds that would grow out of all recognition years later, into the legend of Roswell and the hit TV series, *The X-files*.

Within a few weeks of Arnold's report, nine out of ten Americans said they were familiar with the idea of flying saucers though, at this time, few linked them with alien visitations. That was an idea pushed by the media and science fiction enthusiasts. Arnold was happy enough to go along with it. With large numbers of reports pouring in, the US Air Force initiated a series of investigations that ran between 1948 and 1969 to sift through the sightings - presumably motivated by a genuine concern of a real security threat of some kind that might be uncovered from so many observations.

During the early years, there were some who thought there might be something in the extraterrestrial hypothesis. Some of them wrote very persuasively and fuelled the controversy. Some, such as George Adamski, either saw fame and profit to be made from the gullible public, or suffered from delusions. In his 1953 book, *Flying saucers have landed*, Adamski became the first person to make a serious claim to have met an extraterrestrial being. In general, scientists have been very reluctant to get involved in the UFO controversy, fearing that they would be ridiculed or would harm their professional reputations, a reaction which remains the same today. Yet almost all "sightings" have natural or human explanations, and those that so far do not deserve scientific investigation.

Human-origin "UFOs" include planes, satellites, balloons, searchlights, and rocket trails. One real incident in Australia was almost certainly a chance combination of aircraft lights and Venus. Brilliant Venus, or a bright star twinkling strongly, low in the sky are the most common explanations for "unusual" lights in the sky. But there are many other natural phenomenon that can give rise to weird effects.

Meteors, or shooting stars, can be surprisingly bright and look coloured. They can leave a trail that persists for minutes and even make sounds. Auroras may not be recognised when they make exceptional appearances outside their normal polar zone. A painting done recently in Yorkshire is supposed to show an orange UFO with lights beaming downwards - glimpsed behind dark storm clouds. But looking again, maybe it is just

sunset colours on high clouds with crepuscular rays.

Some of the strangest sky effects are due to sunlight passing through or reflected from ice crystals in the atmosphere. Sundogs, or *parhelia* are not uncommon - colourful bright spots in cirrus cloud, often in pairs, either side of the Sun. The phenomenon called “the spectre of the Brocken” is in fact the three-dimensional shadow of a human in fog but looks like a terrifying alien.

Could any of the strange things people see, however few, be of extraterrestrial origin? With no hardware to analyse, no alien bodies, nor even any unambiguous images, why prefer an alien explanation to an earthly one? The answer, I suspect, lies deep in the human psyche and has little to do with scientific evidence for or against.

The title of Steven Spielberg’s famous 1977 movie *Close Encounters of the Third Kind* was a term coined by an American astronomy professor, J. Allen Hynek. Hynek was drawn into the subject by chance. He happened to be at a university near an Air Force base in Ohio in the 1960s. He was drafted in to sift through hundreds of supposed sightings and so he tried to categorise them - a time-honoured way scientists deal with data. And it was he who in 1972 first used the expression “close encounters”. There were close encounters of the first, second and third kinds, representing increasing levels of interaction with the purported UFO or aliens. Eventually, Hynek became an agnostic - admitting that he did not totally discount the alien theory of at least some UFOs. But he was trained as an astronomer. Skills as a detective or a psychologist might have been more useful in dealing with his witnesses.

By the 1980s, obsession with aliens was taking a new twist. Hundreds of people were claiming not just to have seen aliens but to have been abducted by them. In 1987, the horror novelist Whitley Strieber published a book, *Communion*, describing what he claimed was a real abduction by aliens. It became an international best seller and popularised the image of the grey alien with large black eyes. Abduction experiences may well be real – but more in the mind than in three-dimensional space.

We are still waiting for any substantiated evidence of alien visitors to Earth. But could there be alien life anywhere in space, even if it has not travelled to our planet? It is difficult to imagine what cosmic life forms might be like, since we have experience of only one example - Earth life. We’ve come to realise just how diverse and adaptable Earth life is, though there are certain minimum requirements. That is why Earth has

life and the Moon, such a near neighbour, does not.

Evidence from our own planet shows that life can hide itself away in unsuspected places - such as in a translucent Antarctic rock, where nearly a centimetre under the surface one may find a community of black lichen, white fungus and green cyanobacteria, releasing nutrients from the rock and sheltering from extremes of cold, dryness and exposure to ultraviolet radiation.

Of all the planets in the solar system, Mars remains the greatest hope for finding evidence of life, past or present. As late as the 1950s, some scientists believed that there could be swathes of surface vegetation on Mars. Now we know there is no such vegetation - it is too dry and cold. The flurry of excitement generated in 1996 by unusual microscopic forms and chemistry in a martian meteorite has subsided as contamination on Earth is recognized as a problem and a variety of non-biological explanation have been advanced for the formations. Such problems are good reason for looking for life *in situ* on Mars, and a motivation behind the Beagle 2 Lander, conceived and built by British scientist, which will go on Mars Express (2003). A sample return mission to Mars is unlikely before 2008.

Microscopic life on Mars cannot yet be ruled out. Who can tell what is lurking beneath the surface, where Mars' pitifully small reservoir of water is trapped in the rocks, if life can find niches in the most hostile terrain of Antarctica? It is clear though that large forms of life do not exist on Mars. If not living organisms, maybe there are fossils from a long past era when Mars was wetter and more Earth-like.

Elsewhere in the solar system, optimists have focused on Jupiter's moon Europa. Close-up images support the theory that tidal energy keeps water beneath the surface in a liquid form, where otherwise it would be frozen rock hard. Only a space mission may tell us one day whether life exists deep under Europa's icy crust.

What about life beyond the solar system? Our ideas on possible habitats for life are based on our own experience. The obvious thing to do is to look for Earth-like planets. Until a few years ago, we did not even know for sure there were any extrasolar planets. Now about 70 have been found and regular discoveries are no longer headline news. Though all found so far are likely to be gas giants like Jupiter their existence could mean that there are smaller planets in the same system, or rocky moons. Projects to find Earth-like planets using arrays of telescopes in space are under study

by NASA and ESA. These would have some capability to look for the chemical signature of life - particularly oxygen. The main focus will be on planets in the potential life zone of the parent star - where it's not too hot and not too cold. But perhaps the more intriguing question is whether there is intelligent life anywhere.

The Search for Extraterrestrial Intelligence - SETI for short – centres around the idea that artificially created radio signals, like those we generate, can be distinguished from the natural radio emission normally picked up by radio astronomers. Other civilizations may even be trying to communicate with us. One day they might become aware of our emissions. However, the signals from Earth have been generated over a very short time -40 years or so. This means signals have travelled no more than 40 light years, and the early ones were not strong. Extraterrestrials would have had difficulty picking us up yet!

One characteristic of artificial signals is that they span a very narrow range of frequency (the reason why you can tune into particular radio programmes without too much interference). The search involves not only looking at different stars or directions in space, but checking every conceivable narrow frequency band. The task is immense, and has only become feasible at all with the development of receivers that can be attached to large radio telescopes and are capable of taking data from billions of channels at a time.

The data processing task is unbelievable. But now, anyone with a home computer can help out. SETI at home was a brilliant concept to harness the spare computing power of personal computers worldwide when they are standing idle. Participants download software which operates as a screensaver. It brings in chunks of data over the internet, from the home of the SERENDIP project in Berkeley, California, and sends back the results. To date, more than three million people worldwide are taking part. It has effectively become the world's largest supercomputer. (To join or find out more, go to setiathome.berkeley.edu)

What are the chances we are not alone? Who can tell? We just don't have enough information. Statistically it seems implausible that only Earth has life, considering the universe as a whole. It's also worth remembering for what a short part of Earth's history it has had technologically capable life. The first life emerged nearly 4 billion years ago yet the first multicellular organisms only date back just over 500 million years. Our human ancestors appeared about 2 million years ago. The technology to send radio signals and travel into space is but a few decades old - nothing on

the cosmic timescale.

If ET is present in our vicinity of our Galaxy, why is there not abundant evidence of him? There are a variety of possible reasons, for example, the aliens may not have yet invented a way of crossing interstellar space. Or they may not be interested in exploring space. Who knows, they may live on a perpetually cloudy planet. A more sinister theory is that a galactic club of advanced aliens know about us but have set aside our planet as a kind of zoo or nature reserve, and have deliberately left us alone - so far. Even if ET exists, we may never know it if he is too far away to communicate or be detectable by us. There can be only a slim chance of it ever happening, but if extraterrestrial signals are discovered, it would be one of the most profound and significant scientific discoveries of all time.

Niels Bohr Archive

*“The family of Niels Bohr has decided to release all documents deposited at the Niels Bohr Archive, either written or dictated by Niels Bohr, pertaining specifically to the **meeting between Bohr and Heisenberg in September 1941**. There are in all eleven documents. The decision has been made in order to avoid possible misunderstandings regarding the contents of the documents. The documents supplement and confirm previously published statements of Bohr's recollections of the meeting, especially those of his son, Aage Bohr.*

The documents have now been organised, transcribed and translated into English at the Niels Bohr Archive. Because of the overwhelming interest in the material, it has been decided that the material should be published in full instead of being made available to scholars upon individual application, as is normal practice at the Niels Bohr Archive. This has been done by placing facsimiles, transcriptions and translations on this website

*An illustrated edition of the transcriptions and translations is published in the journal *Naturens Verden*, Vol. 84, No. 8 - 9. Reprints can be obtained from the Niels Bohr Archive for USD8/EUR8/DKK50 (prepaid).”*

Finn Aaserud, 6th February 2002

www.nba.nbi.dk/

The Nobel Century

Introduction

*Prof. Ian Butterworth
Imperial College*

I would like to welcome you all to this half-day meeting to celebrate the establishment of the Nobel Prizes, the first of which were awarded a hundred years ago in 1901.

Alfred Nobel was born in 1833, the son of Immanuel Nobel, an engineer and inventor whose career oscillated between periods of great business success and bankruptcy so he kept moving from one country to another. Alfred was a very bright boy - at 17 he was fluent in Swedish, Russian, French, English and German with interests in English literature as well as physics and chemistry. But rather introverted. His father disapproved of his reading so much poetry and packed him off to study chemical engineering with visits to Sweden, (the family was at that point in St. Petersburg), to Germany, the US and France. And it was in Paris that he met the young Italian chemist, Ascanio Sobrero who had invented nitroglycerine - a very powerful but unstable explosive. Nobel was intrigued and set out to make nitroglycerine a practical explosive, experimenting firstly with his father in St. Petersburg, and then back in Sweden when his father returned there in one of his periods of bankruptcy. The experiments were dangerous, in one of which his brother Emil was killed, and eventually the Stockholm authorities forbade any further experiments with nitroglycerine within the city area. But by 1867 he had patented the process which mixed nitroglycerine with silica to produce a paste which could be shaped so as to be inserted into drilling holes and had patented the detonator to fire the explosive which he called dynamite. He proved a very successful businessman with factories or laboratories in some 90 places.

But he stayed somewhat introverted and never married - and when he died in 1896 and his will was opened it was found unexpectedly that his fortune was to be used to establish international prizes in Physics, Chemistry, Physiology or Medicine, Literature and Peace. They were the

first truly international prizes. The first in physics went to the German Roentgen in 1901 for his discovery of what he called X-rays.

Today we have three speakers who will say something about the Nobel Prizes - and it is a very great pleasure to introduce as our first speaker, Sir Joseph Rotblat who together with the Pugwash Conferences on Science and World Affairs was awarded the Nobel Peace Prize for 1995 for their efforts to diminish the part played by nuclear arms in international politics and, in the longer run, to eliminate such arms. Sir Joseph ...

The Importance of being a Nobel Laureate

Sir Joseph Rotblat

In a few days from now a group of the most famous and respected figures in the world of science and culture will assemble in Stockholm and Oslo to celebrate the centenary of the event that made them famous and respected: the institution of Nobel Prizes. All living Nobel Laureates, 254 of them, plus representatives from 16 peace organizations, have been invited to the celebrations, and more than 90 per cent have accepted. This will be a unique occasion, which, in my opinion, should be taken not just for jollification but for serious thought about the state of affairs in science and society, and the role this august group should play in them. But since I am addressing the History of Physics Group of the Institute of Physics, I will start with some historical data about the Nobel Prize, with particular reference to the Prize in Physics.

The Nobel Prizes were instituted by the Swede, Alfred Nobel, who died on 10th December 1896, at the age of 63. The date of his death, 10th December, is important, because this is the day on which the Nobel Prizes are presented each year. He bequeathed his vast fortune, acquired from his various inventions and business deals, to set up a fund, the interest from which “shall be annually distributed in the form of prizes to those who, during the preceding year, shall have conferred the greatest benefit on mankind.” The qualification “during the preceding year” was never adhered to strictly, the interpretation of it being the year in which the impact of the discovery became fully appreciated. Indeed, awards are usually made many years after the prize-winning work was done. The

longest delay was 53 years; it was in Physics, to Ernst Ruska. The average delay is about 20 years. No wonder that the best advice given on how to win a Nobel is: “live to a very old age.”

Alfred Nobel was a complex, even a controversial character, but I see him as a great believer in the power of the human intellect, whether expressed in the advances in science or in the written word. He was also concerned about the perils to humanity that may result from the advances in science. Having invented dynamite, at that time the most powerful explosive, and worrying about the heavy casualties in a war in which dynamite were used, he encouraged those who worked towards the prevention of war by establishing a prize in Peace. He could not have foreseen that 50 years later another invention would make dynamite seem like a mere toy: the invention of nuclear weapons with a destructive power a million times greater. But this, of course, makes the need to avoid war much more compelling.

Alfred Nobel was quite specific about the awards in his will of 1895. It is all contained in one paragraph, and because of its historical value I will read it out to you:

The said interest shall be divided into five equal parts, which shall be apportioned as follows: one part to the person who shall have made the most important discovery or invention within the field of physics; one part to the person who shall have made the most important chemical discovery or improvement; one part to the person who shall have made the most important discovery within the domain of physiology or medicine; one part to the person who shall have produced in the field of literature the most outstanding work in an ideal direction; and one part to the person who shall have done the most or the best work for fraternity between nations, for the abolition or reduction of standing armies and for the holding and promotion of peace congresses.

He went on to stipulate who should make the awards:

The prizes for physics and chemistry shall be awarded by the Swedish Academy of Sciences; that for physiology or medical work by the Caroline Institute in Stockholm; that for literature by the Academy in Stockholm, and that for champions of peace by a committee of five persons to be selected by the Norwegian Storting [the Norwegian Parliament].

But having given the executive function to the Scandinavian countries, he ensured the international character of the prizes in the final sentence of that paragraph:

It is my express wish that in awarding the prizes no consideration whatsoever shall be given to the nationality of the candidates, but that the most worthy shall receive the prize, whether he be a Scandinavian or not.

The Nobel Foundation, the body set up to administer the will, tried to adhere strictly to its terms, but nevertheless a few changes have been made over the course of time, mainly in the number of awards in each category. The will stipulates one person in each, but almost from the beginning exceptions were made to the rule. Thus, in 1902, the Physics prize was shared by Hendrik Lorentz and Pieter Zeeman, for their work on the Zeeman effect. And in the following year, 1903, the Physics Prize was shared by three people: Henri Becquerel and the couple, Marie and Pierre Curie, for work on radioactivity. As time went on, sharing became more frequent, and in 1968 the Foundation formally decided that up to three persons could receive the Prize annually in each category. This has been adopted in all categories except for literature, in which – with a few exceptions – only one person is awarded the prize in a year. This is surprising, considering that Literature comprises not only books of fiction, but poetry, drama, essays, criticism, philosophy and history, with many deserving candidates in each of these.

A more substantial deviation was in the Peace category where, apart from individuals, international organizations were made eligible for the prize. Sixteen such organizations have been given the honour. In chronological order, they are: Institute of International Law; International Peace Bureau; International Committee of the Red Cross (three times); International Office for Refugees; Friends Service Council; American Friends Service Committee; UN High Commission for Refugees (twice); UN Children's Fund (UNICEF); International Labour Organization; Amnesty International; International Physicians for the Prevention of Nuclear War; UN Peacekeeping Forces; Pugwash Conferences on Science and World Affairs; International Campaign to Ban Landmines; Médecin Sans Frontiers; and, this year, the United Nations. Initially, in a given year the Peace Prize was given either to individuals, up to three, or to organizations. Recently, the rules seem to have changed in that both an individual and an organization can share the prize in a given year. Thus, this year's prize was shared by the United Nations and its Secretary-General Kofi Annan.

A major change occurred in 1969 when a sixth category of awards was initiated: Economic Sciences. This was introduced after complaints that a whole class of disciplines, the political sciences, that had grown rapidly in the second half of the 20th century, was completely ignored. One might point out that even among the natural sciences some disciplines, such as astronomy or geophysics, were excluded. A particularly glaring omission is Mathematics, although the Field's Medal is recognized as equivalent to the Nobel. Since no financial provision was made in Nobel's will for the prize in Economics, the Bank of Sweden has set up a special fund for this purpose. This prize too is under the jurisdiction of the Nobel Foundation and the awarding body is the Swedish Academy of Sciences. But I have noticed that the traditional members of the Nobel Foundation view the economic prize with disdain, as something not quite on a par with the other categories.

The selection of candidates follows a complex procedure. Nominations, which have to be in before the end of January each year, can be made by individuals or organizations, according to strict rules for each category. Thus, the nominations for Physics can be made by the following: Members of the Nobel Committee for Physics; Members of the Swedish Academy of Sciences; former recipients of the Nobel Prize in Physics; Professors in Physics in Scandinavian universities; Professors of Physics in a number of other universities, each year selected by the Swedish Academy of Sciences. In addition, the Academy may also invite individual physicists to submit nominations.

A large number of nominations, of the order of 200 in each category, are nowadays made each year (previous nominations are not automatically carried on). The nominations are scrutinized by a committee of five persons for each category, and are gradually sifted to shorter and shorter lists. Eventually, by summer, the candidates are chosen, and their names submitted, first to the physicists in the Academy, and eventually to all members of the Academy. With all this procedure, it is surprising that no serious leakages occur about the selection; secrecy is an important element in the process, but of course rumours flow freely.

Let me now give a few statistical data about the awards (Table I). During the 100 years a total of 708 individuals have been honoured. The actual number of awards is slightly larger, because several scientists have received more than one prize. The numbers differ between the categories, reflecting the differences in the number – from one to three – awarded each year. In the natural sciences there were about 160 in each. In other categories the numbers are much smaller. In Literature, for the reason

given before; in Peace, because in some years organizations rather than individuals were selected; and in Economics because this started much later.

Category	Total No.	Alive now	Mean Age	Number of Countries
Physics	163	64	52	17
Chemistry	134	53	55	20
Physiology or Medicine	175	63	56	18
Literature	99	19	64	30
Peace	88	24	63	31
Economic Sciences	49	31	69	7
	708	254		

Table 1: Nobel Laureates

I believe that out of the total of 708 Nobelists, 254 are alive now, though I cannot guarantee they are all *corpus mentis*. There is not much difference in the survival rates among the science categories, but in Literature and Peace the mortality rate seems to be significantly higher. This has nothing to do with professional risks – although there have been three assassinations of Peace Laureates – but it reflects the ages at which the awards are made in the different categories.

The ages of the Nobelists when they receive the Prize cover a very wide range, from 25 to 87 years. In Physics, for example, they range from 25 - the age at which Laurence Bragg received the prize together with his father - to 84, when the Russian, Peter Kapitza got the prize. The mean age for each category is shown in the Table, and is of interest for another reason. I have heard it said that for each discipline there is an age at which the best work is done by a scientist. The youngest occurs in mathematics; next comes physics, then chemistry, biology, and finally the social sciences. Amazingly, this is exactly the sequence found for Nobelists. Considering the wide spread of ages within each category this is no doubt spurious, but it still confirms the popular belief. Writers and peacemakers come in between the natural and social scientists.

The last column in the Table gives the number of countries from which the Nobelists came, in each category. This does not mean the country of one's birth, but the country in which one resided at the time the work which led to the Nobel was done. Scientists are much on the move and this leads to controversy. There is a story about Einstein, who was asked to explain the theory of relativity in simple terms: "You can take my nationality as an example of relativity," he said. "If my theory will prove correct, the Swiss will claim that I am Swiss, and the Germans that I am German. But should it prove wrong, the Swiss will say that I am German, and the Germans that I am a Jew." In this Table Einstein is listed under Switzerland.

As seen in the Table, in respect of geographic distribution too there is a significant difference between the categories. In the hard sciences, high quality work is restricted to a small number of countries, whereas the spread is much larger in Literature and Peace.

The country distribution of the Physics Prize is shown in Table 2. The bulk of the prizes went to three countries: the United States, Germany, and UK. In the early years the USA had very few prizes. A dramatic change occurred after 1932, when Hitler came to power in Germany, causing an exodus of scientists of Jewish origin, mainly to the USA. Whether this is responsible for the dramatic increase of prizes to America is difficult to say, but the fact is that in Physics as well as in all the other categories, Nobelists of Jewish origin seem to have received a disproportionately high share, about 20 per cent, of the total number of prizes. No wonder Hitler didn't like the Nobel Prize.

A brief comment on the gender aspect. Women play a dismal role among the Nobelists, only about four per cent of the total, roughly the same percentage as in the Fellowships of the Royal Society. And I see no sign of an increase in the number of women Nobelists in the recent years.

Some oddities in the allocation of prizes may be of interest to historians of science. Thus, Lord Rutherford, an experimental physicist *par excellence*, received his prize not in Physics but in Chemistry. So did a number of other physicists. Albert Einstein received a prize for his contribution to the understanding of the photoelectric effect, but not for his theories of relativity, for which he is most famous. On the other hand, Marie Curie, the real discoverer of radioactivity, received two prizes, the first in Physics and the second, eight years later, in Chemistry.

USA	70
Germany	21
UK	20
France	11
Russia (USSR)	8
Netherlands	8
Sweden	4
Switzerland	4
Austria	3
Denmark	3
Italy	3
Japan	3
Canada	2
China	2
India	1
Ireland	1
Pakistan	1

Table 2: Nobel Prizes in Physics by country

There were three other cases of two prizes being given to one person. One was in Physics, to John Bardeen of the USA, for his work on transistors and later for the theory of superconductivity.

Another scientist who got two prizes, and narrowly missed a third, was Linus Pauling. His first prize was in Chemistry in 1954, for his work on chemical bonding. The second prize, in 1962, was in Peace, for his campaign, mainly among scientists, against the testing of nuclear weapons. And it was his anti-nuclear stand that prevented him becoming the only person to be awarded three Nobels. Pauling was very close to the discovery of the structure of DNA. He planned a visit to Maurice Wilkins' laboratory in King's College, London, to examine X-ray crystallography photographs that would have confirmed his hypothesis. But as a suspected communist sympathizer, his passport was revoked by the US authorities. So it was Watson and Crick who had a look at the photographs, and this led them to the discovery of the double helix structure of DNA, and to the Nobel Prize in Medicine, which they shared with Maurice Wilkins.

Linus Pauling did not give up his dreams of a prize in Medicine; he thought that this would come from his work on vitamin-C, which, he believed, would be a cure of cancer if taken in sufficiently high doses. He kept prescribing ever larger doses; he himself eventually took 17 grams a day, but all the same he died of cancer.

With the large number of candidates competing for a few places, many scientists deserving a Nobel have to be left in the cold, but there is one omission that I deplore: the non-award of a Physics prize for the discovery of nuclear fission. Fission is a purely physical phenomenon, but the only Nobel awarded for it was in Chemistry, to the German scientists Otto Hahn. Hahn had established the chemical identity of some of the fission products, and for this he deserved the prize in chemistry, though it should have been shared with his collaborator Fritz Strassmann. But neither of them understood the nature of their finding. Hahn made this quite clear in a letter he wrote to Lise Meitner, an Austrian physicist who had been working with Hahn and Strassmann for many years, before she was forced by Hitler's decrees to flee to Sweden. As it happened, her nephew, Otto Robert Frisch, who worked at that time with Niels Bohr in Copenhagen, came to Sweden to spend Christmas with his aunt. She showed him the letter from Hahn, saying that he definitely established that barium was a product of the bombardment of uranium with neutrons. Frisch immediately saw the physical explanation of this finding, namely, that the uranium nucleus, with so many protons in it exerting repulsive Coulomb forces, is very nearly unstable, so that a hit by a neutron is sufficient to break it up. Frisch coined the name fission for this process, and immediately after returning to Copenhagen carried out the experiment that showed the emission of a relatively large amount of energy in the process. Together with Lise Meitner, they published their findings in *Nature*, and immediately after this several scientists, including myself, working in different laboratories, observed the emission of neutrons at fission. This opened the way to a chain reaction and the practical utilization of nuclear energy; and all that followed from this. Surely, Robert Frisch, perhaps together with Lise Meitner, deserved the physics prize. I nominated him for the prize several times, as did several others, but without success.

Frisch was a real genius, one of the most talented physicists that I ever came across, and it is very sad that no recognition came to him. Lise Meitner, although with no Nobel, received many prestigious awards during her life, but not Frisch, and after his death in 1972 he has been almost forgotten. This is shameful. Perhaps some of the historians of

science present here will undertake to write the biography of that great physicist.

I mentioned earlier that guesses are made every year about the likely winners; but reliance on such guesses can sometimes be pushed too far. This was the case with the Nobel Peace Prize for 1995. I knew that I had been nominated that year but did not expect to be successful. The reason was that in England there was a firm belief that John Major, the Prime Minister at the time, would get the prize for his efforts to bring peace to Northern Ireland, which incidentally came to nothing. I don't know how the rumour started, but it was strong enough for the Tory Party to take it as a certainty. That fateful day, Friday 13th October, was the last day of the Tory Annual Conference in Blackpool. John Major was scheduled to give the main speech, intended to rouse the flagging spirits of the members. The start of the session was postponed to coincide with the announcement of the award in Oslo, so that John Major would enter the hall with great fanfare, as a Nobel Laureate. Imagine the anticlimax when the winners turned out to be myself and the Pugwash Conferences.

Let me now turn to some general comments on the Nobel Prizes, as seen today. The last century saw tremendous changes in all walks of life, so much so that the world today is entirely different from the world a hundred years ago, when the first Nobels were awarded. To a very large degree, these changes have been the consequence of applying scientific findings, and, by positive feedback, they resulted in a huge increase in the volume of scientific research and in the number of people pursuing it. During the 100 years, the number of scientists and technicians has grown exponentially, with a doubling time of about 13 years, so that at the end of the century there were a hundred times more of them than at the beginning. During the same period, the world population, although growing very fast, increased by less than fourfold. Of course, the number of "masterminds" does not increase linearly with the total scientific manpower, but the number of scientists deserving a Nobel, according to the standards of the early years, has certainly increased greatly. Yet, no more than three scientists can be honoured in each category. This makes the task of the selectors very difficult; they have either to raise the standards, or use different criteria, which are not clearly understood by the candidates, often resulting in frustration and anger.

Claims have been made that the Nobel Prize itself may have a negative effect on the progress of science. The prestige of the Nobel is so high that areas of research likely to lead to a prize attract disproportionately young, ambitious scientists, thus distorting the natural development of science.

Another criticism is that the Nobel Prize affects a fundamental characteristic of science: openness. In the pursuit of a Nobel, some scientists have become so fearful of their ideas being stolen by others in the field that they carry out their work in great secrecy, until they are ready for publication. Others try to protect themselves by taking out patents, a practice frowned upon by the scientific community, as it restricts the free flow of research.

An apparent polarization of views emerges about the Nobel. Some would like to see it abolished altogether, as an element corrupting science and scientists. Others see it as a desperately needed symbol of authority and coherence in an age when all standards are under attack. The prevailing view is that the Nobel Prizes should continue, but be modified in some ways to take account of the changes that have taken place in the nature of scientific research.

An example of this is the so-called “big science” in which Nobel-ranking discoveries are likely to be made. Advances in these are achieved by the joint efforts of huge teams, numbering tens of scientists and technologists. It is becoming increasingly invidious to pick out one to three individuals for the honour. One suggestion was to introduce into the Physics category the practice of awarding the prize to an institution, as is done in the Peace category. Some will object to this on the grounds that it would dilute the standing of the Prize, as well as the money that goes with it (the current value of the prize is about a million dollars).

The very large number of deserving but unsuccessful candidates calls for some measure of recognition for them. Some already follow the practice of film stars, who in the description of their careers never fail to mention the fact that they have been nominated X times for an Oscar. I have seen CVs of personalities in which they describe themselves as having been nominated several times for a Nobel Peace Prize. This is not so bad as the inscription I saw on a chemist shop in India: “R.S. Kumar, B.Sc. (failed).” But it might be worth considering instituting a class of honorary Nobelists. The committees, which select the winners, should each year perhaps announce a list of 5-10 names, in each category, of those who came very near to being selected. It would bring some kudos, although no money.

Let me conclude this talk with remarks about the social responsibilities of scientists, and, in particular of Nobelists. The scientific community can be said to form a kind of a republic. A republic without a territory –

indeed, transcending national frontiers – but with its own laws, language, methodology, and ethics. But the citizens of the republic of science are also citizens of the world community, and it is the interaction between these two communities that is the crux of the problem. Scientists are in general insufficiently aware of their wider social obligations.

At a time when science has acquired a dominant role in the day-to-day life of people and in the affairs of states; at a time when the applications of science may put the very existence of the human species into peril, members of the scientific community all too frequently appear to be oblivious to the changed status of their profession. Many of them shrug their shoulders when asked about their social responsibilities. Worse still, some are actively engaged in inventing or improving instruments of mass destruction. Often they do this not for any real or perceived need for security, but for the sheer exhilaration that comes from innovative work; the same exhilaration that every scientist experiences when advancing the frontiers of knowledge. In the republic of science there appears to be a continuum of motivations, from the sublime quest for the truth to the diabolical contrivance of lethal gadgets.

This *laissez faire* attitude has its roots in history, a remnant of the distant past when science was largely the pursuit of gentlemen of leisure; and the elitist societies they set up took pride in distancing themselves from any practical applications deriving from their work. Without a physical territory, they provided for themselves a virtual territory, the ivory tower, in which they sheltered pretending that their work had nothing to do with the welfare of people.

Such an attitude was perhaps justified at a time when scientific findings and their practical applications were well separated in time and in space. It would take decades before an application was found for a discovery, and then it would be taken up by different people, mostly engineers, in polytechnics or industrial laboratories.

All this has changed. The distinction between pure and applied science is nowadays difficult to discern in many areas of research. Practical applications often follow immediately after scientific discoveries, and are pursued by the same people, in the same place. And, as already mentioned, these applications have a direct and immense impact on the affairs of the world community.

All has changed, I said, except the mentality of many scientists. They still live in the ivory tower. They still disclaim any responsibility for the

consequences of their work. The only obligation on the scientist - they argue - is to make the results of research known to the public. What the public does with them is its business, not that of the scientist.

Quite apart from the amorality - I would say, immorality - of such a policy, it is also against the self-interest of scientists. Scientific research is nowadays a very expensive pursuit, in some areas extremely costly, and the general public, through elected governments, has the means to control research, either by withholding the purse, or by imposing restrictive regulations harmful to science. It is most important that science improves its public image, that scientists regain the respect of the community by presenting a human face, by clearly showing an interest in, and understanding of, the needs of the human society.

Accepting responsibility for one's deeds is of course incumbent on all citizens, not just on scientists. It is becoming even more important in the world of ever-increasing interdependence in which we live, an interdependence itself the consequence of scientific and technological advance. Belonging to a group bestows great benefits to its members, but by the same token it imposes obligations on them. No subgroup should claim exemption from these obligations.

The subgroup of scientists should certainly not seek such exemption. On the contrary, the social responsibilities fall more heavily on them, for the very reason that their work carries consequences of the utmost gravity. In my opinion, this responsibility should rest particularly with the acknowledged leaders of the scientific community, the Nobel Laureates.

Sir Michael Atiyah, the former President of the Royal Society, said in his Schrödinger Lecture: "If you create something, you should be concerned with the consequences. This should apply as much to making scientific discoveries as it does to having children." And, referring to the fact that scientists have a better understanding of the technical problems, he made the succinct observation: "knowledge brings responsibility."

I suggest a parallel dictum: *status brings responsibility*.

Deservedly or not, the Nobel Prize carries a uniquely high prestige; it is recognized as the acme of the honours that a scientist can receive. An aura of all-embracing sagacity attaches to the Nobelists; their opinions, advice and endorsement are sought in all sorts of matters. I believe that this high status makes it obligatory on them to take a more active part in societal affairs, especially on issues of world import.

It is sometimes said that outside their own field scientists are naïve and, in any case, not wiser than the average citizen. While this may be true in a few cases, it is certainly not true in general. To achieve the standards of a Nobel Prize requires a high degree of intelligence, perspicacity and discernment; and these are the very attributes that are necessary for a rational analysis of problems facing the world community.

I am not suggesting that scientists should enter into politics. They should stick to their profession, but as citizens they should pay attention to the evolving societal scene, and if this leads them to specific conclusions they should make these known, from individuals writing letters to the press, to issuing joint public statements. Voicing their views in public may help other citizens to clarify their thinking. In the first instance, of course, this should refer to the relation between science and society, the ethical aspects of scientific progress, and the avoidance of dangers that may arise from the applications of science.

During next week's celebrations in Stockholm and Oslo, a statement will be issued, signed by a considerable number of Nobelists, mainly from the sciences, on the dangers facing the world from global warming, weaponization, and terrorist attacks. This may be the beginning of concerted actions by Nobel Laureates, along the lines I described to you.

The Nobel Centenary: reflections on a curious phenomenon

*Professor Sven Widmalm
University of Uppsala*

What's in a prize? Well, its name, for one thing, is important. I don't think a million dollars would smell as sweet if the prize was not that of the Swedish cosmopolitan inventor, industrialist, writer, philosopher, philanthrope and misanthrope Alfred Nobel. Its centenary is celebrated this year.

A hundred years ago much of the interest generated by the prize, and probably also much of the status that it achieved early on, was due to the

fact that the sum awarded was rather large: about 30 yearly salaries for a Swedish professor, a sum comparable to the present one. Today, I think the sheer size of the award is still important but more important still is the status, the magic of Nobel's name.

There are no other awards that immediately confer upon the recipient the same degree of eminence, a publicly recognized stamp of genius. I think this is most true for the sciences, to a lesser degree for literature and peace, where history has more severely corrected some decisions, and to an even lesser degree for economics where the very idea of a Nobel prize has sometimes been met with criticism and incomprehension. Recent events in Sweden illustrate the kind of controversy that sometimes surrounds the literature and economics prizes: the decision in 2001 to give the award to V. S. Naipaul has been severely criticised on political grounds, because of Naipaul's allegedly hostile attitude to Islam. And during the autumn there was a public attack on the Nobel Foundation by relatives of Alfred Nobel who do not want his name to be associated with the The Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel, the so-called Nobel prize in economics (founded in 1968). Nobel's relatives seem to think the kind of economics usually awarded by this prize is of little intellectual significance.

Criticisms like these have more rarely been levelled against the Nobel science prizes. There are of course cases when decisions have been chastised for being unfair. In 2001, for instance, the cultural minister in France complained about the chemistry prize decision because he thought a Frenchman deserved to share the award – the minister also complained, and there are probably a few who would agree, that it always seems to be Americans or scientists working in America who get awarded these days. But the Swedish Academy of Sciences has a firm reputation for awarding solid achievements and the only instance that I know of where it has been severely criticized on moral grounds concerns the award in 1919 to Fritz Haber for an invention that has often been hailed as one of the most important invention of the 20th century: that of nitrogen fixation which made possible the industrial production of artificial fertilizers, but unfortunately also explosives.

In medicine a few awards have been considered dubious: awards for light therapy, early cancer research, the discovery of insulin or lobotomy. As for physics, I can think of no Nobel award that has really stirred up important controversies. I think it is a fair assessment that the very high status of the Nobel prize in general, what I would like to call the Nobel Phenomenon, relies to a large extent on the successful track record of the science prizes and among them probably most of all on physics.

Having said that, I don't want to imply that the Academy of Sciences' decisions are fair in an objective sense of the word. The Academy has certainly been very good at surfing on the rising tide of physics in the 20th century; possibly it has done more than that, and has helped shape this discipline, at least its status hierarchy. Because, after all, what exactly *is* physics? Judging by the string of Nobel prize awards (166 to date, if I have counted right) physics is mostly atomic, nuclear and particle physics – that is, microphysics, in the sense that research dealing with the most fundamental level of matter attainable for experimentation and theorizing at a particular period in time has been given most attention. To a lesser extent, condensed matter physics and technological developments, mostly of instruments, have also been awarded. There has been little room for astrophysics or atmospheric physics, and whereas the borders between chemistry and medicine are fairly porous, there are no indications that the Swedish Academy of Sciences recognize any connection between physics and the life sciences, despite the fact that physics supposedly has made a deep impact on molecular biology after the war and that it has had important medical uses which physicists have helped develop. Alfred Nobel's will, where it is stated that discoveries should benefit mankind, has been heeded only insofar as you think that important discoveries in the discipline of physics, like the discovery of elementary particles, *always* benefit mankind.

As to the interesting question, of whether experimental or theoretical physics is most important when gauged by the Nobel standard, the answer must be experimental physics – though there are interesting historical differences here as we shall see. I think it is fair to say, however, that microphysics has been to physics what neoclassical theory has been to economics, at least when it comes to awarding Nobel prizes.

The Nobel prize, being a hundred years old, has naturally attracted some attention from historians of science, though not as much – I dare say – as if it had been American, British or for example French. The Swedish language is inaccessible to most scholars in History of Science outside of Scandinavia and if you want to analyse the activities behind the scenes that eventually lead to someone being awarded you have to dig into the archives. The archives of the Nobel Foundation are open to certified academic scholars since the 1970s, though only for prizes fifty years old or more. There you can follow some of the discussions, check out who nominated who and so on. Unfortunately, however, not much of the discussions in committees and in the Academy has been recorded. If you

want to find out more you have to use private or institutional archives and much of this material is in Swedish.

The question is, however, what you learn about the Nobel Phenomenon if you subject it to historical inquiry. It depends of course on the questions you ask, and there are many possibilities. One possible question is this: why have the fortunes of theoretical physics varied in the way that they have over the years? I have made a brief and not very exact survey of the relative number of physics prizes that has been awarded to theoretical and experimental work respectively and there seems to be some fairly marked trends.

It has often been said that an experimentalist tradition dominated Swedish physics a hundred years ago – some say it does so still – and that this was reflected in the choice of early Nobel Laureates, like Wilhelm Conrad Röntgen, the Curies or Albert Michelson. My investigation (which builds on material easily accessible at the so-called Nobel e-museum, www.nobel.se) bears this out to some extent: theoretical work was awarded less often before the 20s, though it was by no means overlooked completely. During the 1920s and the early 1930s theoretical atomic physics was acknowledged with some Nobel prizes and the proportion of theory prizes rose from about a third to about fifty per cent of the total number. It is of course well known that Einstein was not given the award for relativity but for the law of the photoelectric effect; nevertheless, the Academy somewhat obliquely recognized the work for which Einstein had become world famous by saying that the award was also for Einstein's "services to theoretical physics". Many of the household names of the quantum revolution were given the award in these years: Bohr, de Broglie, Heisenberg, Dirac and Schrödinger. The citation for Heisenberg is, I think, particularly nice: "for the creation of quantum mechanics, the application of which has, *inter alia*, led to the discovery of the allotropic forms of hydrogen". This is, to my knowledge, the only time a physics prize is justified by reference to creativity rather than discovery, and you sense that the latter part of the citation is a saving clause, assuring us that discoveries were indeed also being made.

In the mid-30s there was, however, a drastic change in the fortunes of theoretical physics. According to my reckoning, though I might have misinterpreted one or two prizes, there were few such awards before the 1960s: experimental methods, discoveries and instruments were rewarded. Two of the theoretical awards – Pauli's in 1945 and Born's in 1954, really belonged to the formative phase of quantum physics.

In the 1960s and 70s there was again a rich harvest of theoretical prizes, this time associated with the consolidation of Quantum electrodynamics, chromodynamics and the standard model. Feynman, Tomonaga and Schwinger were awarded in 1965, Gell-Mann in 1969, and Glashow, Salam, Weinberg in 1979. Theory actually outnumbered experiment during these decades (though only just).

But the reward to Glashow, Salam, and Weinberg sort of put the cap on this wave of theoretical flourishing in the Nobel context. The 1980s saw theory dwindle and the 1990s were completely dominated by experiment – to an extent that actually exceeded the early years of the century. During these decades there were awards for many experimental discoveries and techniques. In 1984 the prize actually went to a project: Rubbia and Van der Meer were awarded for their “decisive contribution to the large project” which led to the discovery of the W and Z; last year there was an award in communications technology.

So here is a result, a preliminary one but nevertheless: it seems as if there have been periods of waning and waxing interest in theory among the scientists who award the Nobel prize in physics. Possibly this can be related to changes in the development of theoretical quantum physics, probably to other causes as well.

Now, if we look at the prize as historians, are phenomena like these interesting? And how would you go about explaining them? I won't attempt to do that in this particular case because I don't have the broad grasp of 20th-century physics necessary for the task. But I can suggest different hypotheses. The obvious one is that the Academy in Stockholm simply follows public opinion in the physics community as it is mediated by the nominators. This would for example explain why Einstein finally got the prize in 1922 despite – as historians have shown – rather strong opposition among leading members of the Nobel committee of physics in the Swedish academy. The number of nominations was large, and that was that. It was a collective decision to place Einstein where he belonged, on the top of the heap. According to this interpretation the Academy, by its choices, simply *reflect* the general will of the physics community.

On the other hand there are similar cases where the nominee was not awarded, the most well-known probably being Henri Poincaré, who had a very large number of nominations. It would have been difficult to keep Einstein at bay for very long I would imagine – though one never knows, as the Academy of Literature managed not to award for example Tolstoy, Strindberg and Joyce, and as Mendeleev was not awarded in chemistry.

Nevertheless, the fact that Einstein was awarded in the year 1922 had much to do with internal scheming in the Academy of Sciences. Other worthy winners, that were not quite as famous, were probably outmanoeuvred by similar mechanisms, and that is after all not surprising at all unless you think that the number of Nobel prizes happen to coincide with the number of worthy winners, or that the Academy in Stockholm is infallible – both thoughts equally absurd.

This suggests another interpretation: that the Academy and its Nobel committee actually wields substantial power in the world of physics. As there are at any given time a number of people working in the domain of the physical sciences who have made important contributions, the Academy actually can, to some extent, decide what lines of work are to be considered of the highest merit, what branches of physics that are to be given most publicity. For they are the architects and the high priests of the pantheon of the exact sciences. I do not think anybody would want to suggest that Swedish scientists are presumptuous enough to attempt to wage such influence by themselves. But take the case of Niels Bohr. He got the prize in 1922, right after his old friend and benefactor C. W. Oseen had joined the Academy's physics committee. Bohr was at the centre of an important network of theoretical physicists, many of whom got Nobel prizes during the inter war years and beyond. Furthermore, as historian Finn Aaserud has shown, Bohr was probably the most successful nominator of Nobel laureates during this period, proposing 19 candidates of whom 15 were awarded. It is easy to see the Academy as a vehicle for the Bohr network as it established itself at the centre of theoretical physics, and also the Bohr network as a resource for the Academy, looking for candidates that would enhance rather than diminish the status of their prizes. In this interpretation we are looking at something akin to a cybernetic feedback system with the Swedish Academy of Sciences as a major player in the game of scientific prestige.

I would opt for a weak version of the cybernetic interpretation; the Academy of Sciences has probably helped boost the glorious career of microphysics during the 20th century, not least by strategically emphasizing theoretical reorientations as they occurred, but they could not have done it without substantial support from powerful scientific networks like Bohr's.

Robert Friedman has, in his new book *The Politics of Excellence*, revealed, in fascinating detail, the inner workings of the Nobel committees in physics and chemistry and the Academy more broadly in the decades before the Second World War, but there is there little to

suggest that the Swedish scientists changed the course of scientific history. Friedman has however done a good job in demystifying the Nobel prize. It is clear from his, and similar accounts by for instance Elisabeth Crawford, that what went on behind the scenes when decisions were made about Nobel awards is not very different from any other kind of committee work.

Yes, just as horrible as any other kind of committee work. There were alliances, sometimes between friends and sometimes between foes to support or suppress candidates. There was infighting and foul play, all very interesting. But what does it tell us about the historical role of the prize?

Friedman tries to understand the prize from the perspective of Swedish history of science and his book is a valuable contribution in that field. But this does not show why the prize has been of major concern to international science or international media. Explaining why someone got the prize a certain year is simply not historically very interesting. Statistics can be used to map scientific elites but this does not help explain the impact of the prize, unless we think the prize created the elites, a theory for which the evidence is weak. Had Nobel not done so, someone else would probably have invented the Nobel prize; the elites would have gone about their business anyhow. If Einstein, Bohr, Heisenberg and Dirac had not been awarded it is certain that the status of the prize would have suffered rather than that of the scientists.

One may think the secret of the prize is hidden in the archives of the Nobel foundation, but, to quote Gertrude Stein, when you get there there's no there there. By which I mean that among the masses of bleak paperwork there are no revelations that explain the Nobel phenomenon.

Nevertheless the Nobel phenomenon ought to be explained because it is obviously important – for the public image of science if nothing else. I think attention must be turned from the Nobel population – nominators, nominees, laureates, the Swedish academy of sciences – to what I would like to call the Nobel constituency, those who support the Nobel phenomenon in different ways. It is they, after all, rather than the Nobel committees, who decide that it is important by acknowledging the status of the prize.

So why is it that CNN broadcast live from the Nobel festivities in the Stockholm Town Hall, and why do ministers of culture and powerful American universities and industries care about the decisions made by a

scientific clique in Sweden? There are no easy answers here because once you broaden the scope to include the Nobel constituency all sorts of interests and other phenomena present themselves, asking to be included in the analysis. Recently a special issue of the journal *Minerva* on the history of the Nobel prize was published, and the contributions there illustrate the variety of historical questions that emerge from a broader approach to Nobel historiography.

The historian of science Morris Low discusses the impact of the prize in Japan. In that country there has, at least since the war, been a tremendous interest in the prize. Many Japanese seem to have taken the idea that Nobel prize winners constitute a super-ultra-elite very seriously indeed, and there have been perennial discussions about the relatively poor showing of Japan in the competition for prizes. Morris Low shows among other things that this discussion must be seen in relation to more important problems having to do with the eventual accommodation of Japan to Western, more specifically American, cultural, scientific and industrial ideals. Here the Nobel prize stands for the highest achievements of Western culture and the ability to win prizes is seen as an indicator of successful accommodation to Western ideals.

Historian Abigail O'Sullivan instead looks at how the prize has functioned in the context of closure with relation to scientific discovery. Henry Halett Dale was awarded in 1936 for work proving the chemical transmission of nerve impulses, but O'Sullivan shows that the notion of discovery in this case is a very simplistic way of describing a complex process involving a number of researchers. Considering the importance of the notion of discovery for the Nobel prize – it occurs in Nobel's will and is used frequently in citations for awards, denoting both theoretical and experimental advances – O'Sullivan's analysis points to a real problem and a very interesting aspect of the Nobel phenomenon. It keeps alive the notion that scientific progress depends on outstanding work by a relatively small number of individuals rather than say networks of researchers or large groups producing massive quantities of data by the use of heavy machinery and sophisticated electronics.

Historian John Krige actually suggests, in an analysis of Rubbia's and van der Meer's 1984 prize in physics, that on that occasion, at least, the Academy of Sciences actually acknowledged the collaborative nature of modern science by explicitly awarding a "project". Krige argues that this term was used especially to designate Rubbia's part in the work that led to the discovery of vector bosons – that his particular talent for initiating and pushing forward a large and complex work effort was awarded rather

than any particular technical or scientific insight of his. By this interpretation it could be said that this prize was awarded to CERN in a way similar to what is often the case in the category of peace.

If we accept this analysis I think it is also clear that the experiment has not been repeated: as far as I know no other physics prizes have been explicitly awarded for project leadership since. Many recognize that the individualistic ethos suggested by awarding only very few scientists is basically unfair a lot of the time. The Nobel prize reminds us that scientists are individuals, that the system of science does not work like a machine, as policy discussions sometimes seem to indicate. But it does more: by putting only a few men and the odd woman in the limelight it has an ideological significance, promoting the ideology of individualism in a way that perhaps does not enhance the public understanding of scientific research.

Anyhow these examples illustrate some of the interesting functions of the Nobel prize: it is used to gauge civilizational advance as measured by the Western, or nowadays the American standard, and it promotes an individualistic image of research. A comparison with Literature is interesting. The literature prize was once of a similar nature; most laureates were European or American, the prize was a celebration of Western culture. Since the 1950s this has changed to some extent as the ambition to spread awards geographically, culturally, and linguistically has increased. Last year's award to a naturalized Englishman from the Caribbean and of Indian descent who has written about these cultures but also about Africa and the American South symbolizes an effort toward what is often called multiculturalism that is totally absent in the sciences (not to speak of economics).

The science prizes are not multiculturalist. Because the sciences are supposed to be objective.

The idea of objectivity is important for the status of the Nobel prize. Peace and literature may not be objective but the sciences supposedly guarantee that there is something solid among all this cultural and political arbitrariness – not least physics is thought to put solid ground under humanity's feet. I do believe the Nobel prize has been important in promoting this idea, and that scientists and others have been able to use it to that effect. Objectivity has not self evidently been a characteristic of scientific research, not even during the relatively short history of the Nobel prize. If we, and not least the general public, stop believing in it, objectivity may possibly be undermined. This was the case some 80 years

ago, during and immediately after the First World War. Many of the best scientists in England, France and Germany participated in the war-time propaganda, accusing one another of, for example, racial inferiority. After the war Western scientists went along with the politicians and instituted a new world order from which the Central Powers were excluded. Scientific objectivity became a matter of political contention under these circumstances, and a general distrust of science and technology spread among the population in Europe. With the coming of Nazism, a new war, and then a cold war, political divisions continued to hinder international scientific exchange and to gnaw at the notion of an un-political objective science.

But Sweden was neutral during the First World War, it took a neutral stance during the interwar years, during the Second World War, and during the cold war. Stockholm was a neutral place, where impartial scientific judgements could presumably be made. This political situation has probably done as much as anything to preserve the role of Stockholm as the high church of science. Maybe that role will change with the end of the cold war and as Sweden has joined the EU, losing much of its neutral credibility in the process. At least the French cultural minister seems to think it is time for a change: a European prize should perhaps be given more often to Europeans? Or maybe it should be given more often for technological advances, like integrated circuits, in order to encourage the development of industry? Surely Brussels would like that.

It is possible that, as the political foundation of the Nobel phenomenon evaporates – I'm thinking of Swedish neutrality, but also of governments' decreasing interest in sponsoring fundamental research rather than goal-oriented mode 2 European-framework technoscience – it is possible that under these circumstances the Nobel prize will become marginalized, or technology oriented, or changed in some other fundamental way. And if that happens I predict that we will not miss the old Nobel prize much. Because the power of the Nobel phenomenon depends not on the skilful choice of laureates by the scientists in Stockholm, even though they *have* been skilful, or on the brilliance of the researchers who get awarded, even though they *have* been brilliant, but rather on our, the Nobel constituency's *faith* in its importance.

Further reading

The point of departure for all Nobel history is Elisabeth Crawford, *The Beginnings of the Nobel Institution: The Science Prizes, 1901-1915* (Cambridge, 1984). This book has now been complemented by Robert Marc Friedman, *The Politics of Excellence: Behind the Nobel Prize in*

Science (New York, 2001), covering the period to the end of the Second World War (for physics and chemistry prizes). About the Nobel prize and Swedish neutrality after the First World War, see Sven Widmalm, "Science and Neutrality: The Nobel prizes of 1919 and Scientific Internationalism in Sweden", *Minerva* 33:4 (1995), 339-60. A comprehensive account of Danish Nobel history, containing the analysis of Bohr's influence by Finn Aaserud discussed above, is Henry Nielsen & Keld Nielsen, eds., *Neighbouring Nobel: The History of Thirteen Danish Nobel prizes* (Aarhus, 2001). *Minerva* 39:4 (2001) is a special issue on the history of the Nobel prize, containing the papers by Low, O'Sullivan and Krige discussed above, as well as some further reflections on the prize, in the introduction, by Sven Widmalm. Useful statistics about the "Nobel population", i.e. nominators and nominees, for the early decades of the prize's history are gathered in Elisabeth Crawford, J. L. Heilbron & Rebecca Ullrich, eds., *The Nobel Population 1901-1937: A Census of the Nominators and Nominees for the Prizes in Physics and Chemistry* (Berkeley, 1987). For a critical discussion about the individualism inherent in the Nobel system, see Trisha Gura, "Eyes on the prize", *Nature*, vol. 413, Oct. 2001, 560-64.

The Nobel e-museum at www.nobel.se is an invaluable source for those interested in Nobel history or trivia.

The Nobel Prize: some cautionary tales

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In my opinion, the Nobel Prize has conferred a great deal of benefit on physics. It gives the subject a high profile at least once a year. It suggests that, even though it's a long shot, there's possibly a financial benefit in being a physicist. It enables people who don't know the first thing about physics to understand that an individual can be a celebrity in a subject that's a long way from popular culture. Alfred Nobel was good enough to leave a large sum of money in his will for financially rewarding contributors to what most people would regard as worthy pursuits – Physics, Chemistry, Physiology or Medicine, Literature and Peace (he can't be blamed for the subsequent extension of his legacy to include the

‘dismal science’ of economics!) – and the executors have every right to award that money to whomsoever they think fit.

On the whole they have made a good job, at least in the three science areas, of what is obviously a difficult and thankless task. If we think of the history of twentieth century physics, it’s hard to think of any of the really great names who aren’t on the Nobel list: Planck, Einstein, Rutherford, Bohr, Heisenberg, Schrödinger, Dirac, Fermi, and many others. And whatever anyone may say or claim, most of us would be very happy to receive one. The money alone would be extremely useful. One well-known physicist has gone on record as saying that he wishes that there was an opportunity to turn down a Nobel award without creating a problem, because of the subsequent inconvenience the award caused him. ‘Surely, you are joking Mr Feynman’. It’s hard to think of anyone in the last fifty years who did more to milk the status that the Nobel Prize conferred upon him.

Even if we don’t get one ourselves, we can bask in the reflected glory that members of our own University or even our own Department have received the Prize. The Liverpool Physics Department have had three winners – we would have had four if we hadn’t turned down Schrödinger, who was looking for a bolt-hole from Dublin after the controversy caused by the publication of his *What is life?* Recently, I had the honour of giving a lecture in the University of Gdansk. My host, who had been to Liverpool and had seen the promotional posters on display there, introduced me as coming from a Department that had already had three Nobel Prize winners. I am sure that did nothing to reduce my credibility to the audience!

Of course, some particular awards will inevitably be controversial, and I will mention a few. There will always be individual complaints. But the awards in Physics do not seem to be hugely controversial, and there is a large-scale consulting procedure in any case. The statistics for Physics and Chemistry awards from 1900 to 1950 seem to suggest that bias in nominations, for example, from certain countries, has not biased the deliberations of the Nobel Committee.

So why do I consider the Nobel Prize controversial? The answer is that, while the executors of Nobel’s will have the perfect right to give the Prize to anyone they like, this does not mean that their decisions should be used as the criteria for what is significant in scientific history. All too often it appears that they are, and this is seriously damaging to the progress of historical scholarship.

The Nobel Prize cannot be considered as the simple measure of scientific excellence that it is often assumed to be. As with all such measures, we have to take into account the biases and prejudices which inform its decisions. Up to about 1912, for instance, there was a strong bias in favour of experimental work as opposed to theory. The award to Planck in 1913 was a signal of a change in policy. This is why Poincaré, who must be considered as one of the most important theorists of the period, never received the prize, for, by the time this prejudice was removed, he was dead.

Even in the case of subsequent awards, there has always been the idea that a good piece of theorising requires experimental proof, and awards have been made for such confirmations even when it was almost obvious that they were going to come. It is interesting to reflect that had the Nobel Prize been awarded in the seventeenth or eighteenth century, then the bias towards experimental proof would have excluded Newton as a recipient, because the expedition to Lapland which confirmed his figure of the Earth didn't take place until 1736, nine years after he was dead, and the delayed return of Halley's comet didn't take place until 1758, after Halley himself was dead. Similarly, in the nineteenth century, Maxwell died too soon to see his work vindicated by the experiments of Hertz, while, at the beginning of the twentieth century, Boltzmann gave in to depression and committed suicide just as his work was about to be vindicated by Einstein's work on Brownian motion.

Any major prize, no matter how fairly awarded, and however objectively it may be assessed, will necessarily always generate controversy; and, though, as we have said, the physics prize has not been hugely controversial, there have been a number of controversies associated with it, often historical rather than contemporary, and not necessarily involving the participants directly. The first award itself was controversial, though historically it seems an open and shut case. Röntgen, in historical terms, incontrovertibly discovered X-rays. Though not really introducing any truly novel principle into physics (being just extremely short wavelength electromagnetic waves of the type envisaged in Maxwell's theory), they inaugurated a new type of high energy investigation in physics, which proved to be of immense importance, and they also had immediate, and lasting, medical applications. The prize was awarded because of the sensational nature of the discovery – the penetrating photographs of hands, etc, which accompanied the first publication. In the following year, Becquerel made the much less sensational but much more *physically* significant discovery of

radioactivity, which involved unheard of physical phenomena, entirely new forces, and a vast new source of energy previously unimagined. However, he only got to share one third of a prize, with Pierre and Marie Curie.

However, this is not what was controversial about the award. What was controversial was that Philipp Lenard, a much younger and more highly regarded physicist, felt that he should have made the discovery, rather than the middle-aged second-rater from Würzburg. After all, in looking at cathode rays in evacuated discharge tubes at high voltages, he had been careful to shield his apparatus from outside influences by surrounding it with lead. Because Röntgen hadn't, the stray radiation from his tubes had caused some crystals of barium platino cyanide he had carelessly left lying around to glow in the dark, and he had observed one particular version of a whole group of radiations that Lenard had felt was his special province. When Lenard came to Liverpool for the British Association meeting of 1896, he had been given a tremendous and sympathetic reception, with the implication that he was the real discoverer. Though subsequent events gradually proved that he was not, he still had enough credibility to be awarded a Prize of his own in 1905.

Lenard was not the only physicist who got a Prize by persistently complaining. Max Born was a later example, though perhaps with more justification. Born was the director of the Göttingen laboratory, in 1925, when Heisenberg came to him with his strange way of avoiding the anomalies in the Sommerfeld approach to spectra. Born recognized it immediately for what it was – a system of matrix mechanics – and added the probability interpretation and much of the formalism. His contribution was certainly major and without him Heisenberg's version of quantum mechanics might never have got off the ground. However, when Heisenberg got the Prize in 1932, Born was very put out and complained that he should have been up there at the Stockholm ceremony alongside his protégé, and he persisted in doing so for many years to come. Eventually, to Heisenberg's great relief, he received the Prize for 1954.

Many controversies, as we have said, are generated historically rather than contemporaneously. There is, for example, the feminist angle. Only eight women were nominated for prizes in physics and chemistry between 1901 and 1950 and only three were awarded. Proportionately, this is par for the course, but the low total numbers suggest that we should ask the question whether this reflects a bias in the nominators or judges, rather than just a bias in society making it difficult for a woman to pursue a career in science. A few individual examples have been used to back up

the claim of bias among the judges. We will come to these, but first of all a counter-example. Marie Curie was, in this period, the only individual to have won prizes in both physics and chemistry, but it would be absurd to suppose from this that she was a more significant scientist than those who only gained one – Einstein, for example, or Rutherford, or Bohr, each of whom made many contributions for which they could have been cited.

Within this period, the most outstanding case for the feminists would appear to be that of Lise Meitner, who was excluded from the Chemistry Prize awarded to Otto Hahn alone, in 1945, for the discovery of nuclear fission. The case has some merit in that it was Meitner, who, along with her nephew, Otto Frisch, explained Hahn's experiments in this way. However, it is by no means clear-cut. Meitner had partnered much of Hahn's work, but, for political reasons, had left Germany by the time that Hahn did the crucial experiment. This is unfortunate but it doesn't make her an actual partner in an experiment that neither Hahn nor anyone else knew was going to result in this discovery when he carried it out.

If one says that Meitner should have been awarded the Prize for the theory, that is arguable, but the theory is physics, not chemistry, while Hahn's experiment was carried out using specifically *chemical* skills. Also, if Meitner was honoured for the theory, then Frisch would have to be also, as it was at the very least an equal partnership and (if Joe Rotblat is to be believed) biased if anything towards Frisch. Now if you honour Frisch in 1945, you are also honouring the man who co-authored the memorandum setting in progress the Manhattan Project, and then contributing significantly to its success, which, in 1945, would be a hugely political statement which one wouldn't have expected anyone to be prepared to make, certainly not in neutral Sweden, especially when the person they had definitely chosen to honour, Otto Hahn, was at the time a prisoner of the British authorities, and being recorded discussing Germany's failed attempt at the same end. So, the solution was to honour Hahn for a *lifetime's* achievements, ignoring entirely his collaborator in the fission experiments, Fritz Strassmann. It might have been possible, though still very tricky, to have given Chemistry to Hahn and Strassmann, and Physics to Meitner and Frisch, but even this would have been interpreted as being heavily political.

Ironically, there *was* a woman who might have been considered for an award on the basis of nuclear fission, and had received nominations. This was Ida Noddack, who after Fermi's supposed "transuranic" experiments of 1934, had suggested that the experiments might involve the breaking up of a nucleus, rather than a higher synthesis. But the Nobel Committee,

rightly or wrongly (and I think wrongly), do not generally make awards on the basis of what they would call “speculation”. You have to work out the theory fully, include numerical calculations, and even, if possible, carry out experiments of your own, as Meitner and Frisch did, and as Chadwick, for example, did with the neutron.

One of the later examples is easily dismissed. In 1956 Lee and Yang put forward the theory that certain experiments on the decay of K mesons could be explained on the understanding that parity (or spatial symmetry) was not conserved in the weak interaction. At least three teams of experimenters instigated research programmes to put the theory to the test of confirmation. The one that published first, marginally ahead of the others, had five members, four of whom, Ambler, Hayward, Hoppes and Hudson, were men, and one of whom, C. S. Wu, was a woman. There was no actual ‘team leader’, and the names would normally have been arranged in alphabetical order. However, chivalry (!), it would seem, dictated that Wu came first, leading to the pure mythology that it was “Wu’s experiment”. Even if it had been, of course, it was only a confirmation – plenty of experiments already pointed to the nonconservation result. Lee and Yang had made the breakthrough, and were rightly awarded the prize for 1957 – one of the quickest ever given. However, it is said anecdotally that others (one of whom, I have heard, was an American named Lord) had attempted to put forward this view at conferences, only to be shouted down, a common phenomenon in twentieth century physics, as we shall see; and the Nobel Committee, rightly or wrongly, take no heed of people who can’t get themselves heard. Yang, certainly, had already made his reputation with the Yang-Mills theory, and would have been considered to have had the authority to make this kind of pronouncement. It is perhaps this kind of thing that ought to concern us more as historians of science.

Personally, I don’t believe there’s any deliberate or systematic anti-female bias in the Nobel awards, though such an attitude could certainly have been ingrained in certain influential individuals in the past, and maybe still persists. But there are undoubtedly one or two cases which have been particularly profiled because they involve female scientists, though there have been very similar cases involving men. The one who I think has been hardest done to is Jocelyn Bell, who was set to work on using a radiotelescope set up at Cambridge by Antony Hewish. The case here isn’t, I think, about female / male relations, but about research student and supervisor. In principle, a research student is an apprentice who receives tuition in research techniques by carrying out a supervised project, and there are varying degrees of direction, with an almost

indefinable amount of discussion and collaboration between the two parties. Occasionally, a graduate student may come up with an idea that has no real connection with the official project and so be entitled or allowed to claim sole authorship of the work. A good example of this is Brian Josephson's discovery of the Josephson effect, also done at Cambridge during this period. However, on other occasions, the supervisor may define the project, give regular advice, and then help to interpret any findings, especially if they are unusual or unexpected.

In the case of Bell and Hewish, Hewish designed the radiotelescope (which was a special one with an array of detectors, rather than a single dish) with which Bell observed an unusual signal while working on a programme devised by Hewish. This turned out to be the first pulsar or neutron star. It is virtually impossible now to separate out how much the two individuals contributed to making and interpreting the discovery, but it is clear that Bell's role was as rather more than that of an operative working under exact direction from a supervisor, and the discovery was quite unexpected. It has been said that Nobel Prizes are not given to research students, but that is a gross undervaluation of the originality they often display and the contributions they frequently make to discoveries. On the other hand, one shouldn't assume that the individual who happened to be there when the first blip appeared on the detector must be proclaimed, as popular myth would have it, as the sole "discoverer". In the particular case discussed, I think a fairer assessment would have said that Bell should have had a share in the Prize for a very significant contribution to a major discovery brought about as one of the results of an important programme instigated by Hewish. Controversy over this issue has been largely avoided, I believe, because Bell has, to her great credit, refused to make negative comments on the issue.

With the growth of huge collaborations in certain areas, it has become increasingly difficult for the Committee to maintain the tradition of awarding a single Prize to no more than three recipients. There were difficulties even before this development. For example, in the case of renormalization, Tomonaga, Schwinger and Feynman had to be recognized, but many people think that Dyson's work was equally important. I have discussed some other cases with a colleague who is involved in Nobel nominations. In the case of CP violation, I am told, Cronin and Fitch, who won the Prize, were the basic creators of the experiment, Christenson was a research student acting under instruction, while Turlay was a visiting fellow who had the good luck to be involved. In the case of the quantum Hall effect, the discovery was basically von Klitzing's, although the crucial paper had two co-authors, because the

idea was implicit (though not fully recognized) within his earlier work. (Even so, I have heard that the co-authors don't necessarily share this view.) Such problems necessarily arise. It's like those bridge problems which you can sort out afterwards when you can see all four hands, and have time to analyse, but not when you're in the act of playing. What happens, though, in a case like the discovery of the *W* and *Z* particles, where there might be as many as 500 contributing scientists? My colleague tells me that, in such cases, it is very difficult, but in this particular case, it was clear that van der Meer had devised the crucial piece of apparatus, while Rubbia was the inspiration and the driving force for the whole project.

Apportioning exact credit for joint work will always be a problem. When Bardeen and Brattain, working in Shockley's team at Bell Labs, more or less accidentally discovered the point-contact transistor, thus achieving the amplification effect that Shockley (correctly) believed should have been arrived at more systematically using the principle of the junction transistor, every effort was made by the Laboratory to play down the fact that he had not been directly involved in the experiment, as it was clear that the future he had planned so carefully did not lie in the direction of the kind of device which had pre-empted him. Official photographs of the "discoverers of the transistor" invariably showed Shockley alongside (or, more usually, between) his two colleagues. The future did lie with Shockley's devices, and it always surprised me that he had only published his work on them in *Bell Systems Technical Journal*. Then, I discovered that it had been rejected by *Physical Review*. Shockley's policy, however, worked; despite the point-contact setback, he shared the Prize with his two colleagues in 1956. The same did not apply to the sole investigator Theodore Maiman, who first produced a working laser in 1960, but who was forced to publish in a newspaper. Maiman never received a Prize, but Townes, who had already produced a maser, shared one along with two Russian theorists.

The other high-profile case involving a female scientist has, I think, rather less merit than that of Jocelyn Bell. It doesn't involve a Physics Prize but the 1962 Prize for Physiology or Medicine to Francis Crick, James Watson and Maurice Wilkins for the discovery, in 1953, of the double helical structure of DNA, partly using physical techniques. The name missing here, of course, is that of Rosalind Franklin, who had died in 1958. The most important fact here is that Crick and Watson actually discovered the structure, though they used data provided by Franklin and Wilkins, and more by Franklin (though unknowingly) than by Wilkins. Some commentators give the impression that there was a conspiracy by

the Nobel Committee to delay the award until after Franklin died, though they could have hardly been expected to know that this was going to happen! The delay (which wasn't particularly long by their standards) is much more likely have been due to the absolute necessity of such a revolutionary theory being established with certainty before Prizes are awarded. This had more or less happened by 1961, when the genetic code was revealed, principally through the further work of Francis Crick. Though, with Franklin no longer eligible, Wilkins was included in the award, I have to say that, in my opinion, data is not theory, even though in retrospect it appears crucial. Franklin made a significant contribution to the discovery, but you can't claim that someone with the right data is on the point of discovering the theory that explains it, as history has shown that this, on very many previous occasions, has been very far from the case. The people whose data led Chadwick onto the discovery of the neutron were not honoured, because they had no idea of the explanation, though Chadwick himself might not have been honoured if he hadn't backed up the neutron hypothesis with his own data.

As we have seen in discussing contributions to a collaborative effort, the feminist angle is not the only source of controversy. The antiproton award of 1956 has always been controversial. Everyone knew the particle was there. Segré and Chamberlain built a machine specifically to see it, at rather large expense. As soon as they switched on, they found what they were looking for, to no one's great surprise. Antimatter was already known to exist through the discovery of the positron and various mesons, which were antiparticles of others. So no great breakthrough was made. In addition, one of the team, Tom Ypsilantis, claimed he had done all the real work, and even took his case to the lawcourts – he lost, but legal judgements don't always correspond with historical ones. While this award was being made for what many might consider “hole filling”, other, probably more important contributions, received no recognition. Rochester and Butler, for example, had discovered the first of the strange particles in 1947, and these led on ultimately to the discovery of particle generations, but the Prize astonishingly never came to them. Perhaps the Committee thought they were fortunate – in the right place at the right time – but this has not been a consideration in other cases.

The whole of twentieth century physics was carried out while Nobel Prizes were being awarded and in a sense the history of the award is also the history of physics in this period. While not necessarily conveying an unbiased view of twentieth century physics, the Nobel awards do give us an indication of the way that physics was *perceived* to be created in certain quarters. Certain disturbing trends stand out.

One is the bias I have already mentioned against so-called “speculation”. In principle, this is really a bias against analytical or inductive thinking. It is so strong that words like “fear” and “courage” are used in describing reactions to it. Soddy, for example, I have read, had “the courage” to suggest that isotopes or nuclides of the same chemical nature but different mass might really exist. Why does it take “courage” to put forward such an idea. “Insight”, yes, but why “courage”? It reminds me of that sketch in *Yes, Minister* where Humphry Appleby replies to one of Jim Hacker’s proposals, “That would be very *courageous*, Prime Minister.” “Courageous?” says Hacker, now thoroughly alarmed, before immediately finding words to back off.

“Courage” implies that the instantaneous reaction to an inductively-derived idea is often not only negative, but also personally devastating. One might be branded immediately as a “speculator” or, even worse, a “dissident”, and one’s whole career might be blown in one moment of speculative folly. How else can you explain how someone as eminent as James Chadwick could on at least *four* occasions pass up the opportunity of claiming one of the major discoveries of twentieth-century physics, each of which came attached with a Nobel Prize. In 1914, in Berlin, he speculated privately on the possibility that von Laue’s X-ray diffraction might be done with electrons. Then, in 1928, he observed that Skobelczyn’s cloud-chamber pictures showed tracks in the wrong direction; he wrote to Skobelczyn, but, not getting a clear answer, he didn’t pursue the matter. In 1929, he asked Rutherford, as conference chairman, for a few minutes from the floor, where he could speak about the possibility of explaining some other pictures by particles intermediate in mass between electrons and protons, but completely bottled it when called upon to speak, and mumbled something inconsequential. Finally, in 1934, working with Goldhaber on a paper on the photodisintegration of nuclei, he or Rutherford at the last minute struck out some remarks on the efficiency of slow neutron absorption with the comment: ‘Let’s not speculate.’

Why not? What is “speculation” but inductive thinking. But always, such thinking brings immediately condemnation. Why otherwise would Rutherford say to Soddy about their nuclear transformation theory: “For Mike’s sake, Soddy, don’t say transmutation; they’ll have our heads for alchemists”? Why did J. J. Thomson, in referring to his first presentation on the electron theory, say: “They thought I was pulling their legs”? Rutherford was lucky, when he was at Manchester, that he had a useful outlet for presenting ideas such as the atomic nucleus in the proceedings

of the Manchester Literary and Philosophical Society. He could then have his cake and eat it. By then, of course, he was already famous, but even he suffered a major bruising in the 1920s when he tried to put forward a structural theory of the atomic nucleus based on the shell model of the atom, with energy levels, etc.

Chadwick wasn't the only one who forfeited a major discovery by striking it out of the paper at the last minute. The same happened to Bose, though it was actually Einstein who did the striking out. Bose had had his paper on what we now call Bose-Einstein statistics rejected by *Philosophical Magazine*, presumably because "speculative" (i.e. inductive) work was not accepted, especially if the author was completely unheard of and living in India. The same issue of the magazine, however, readily accepted a paper by Bohr, who was by then established, even though it proposed abandoning the conservation laws of energy and momentum to explain beta decay.

In desperation, Bose sent his paper to Einstein, who was impressed enough to further develop the work. However, he struck out Bose's reference to photon spin before publishing a version of the paper in a German journal. Because of Einstein's support, the work was accepted, but Bose failed to get the Nobel Prize, although, ironically, the Prize for 2001 went to the experimental discoverers of Bose-Einstein condensates.

The spin story itself is very complicated. Spin is one of the most fundamental ideas in all of physics, especially the $\frac{1}{2}$ spin of the electron. Uhlenbeck and Goudsmidt first published this idea to explain the Stern-Gerlach experiment. Astonishingly, it is said that they tried to cancel the note that they had sent for publication, but were informed that it was too late to do so. The reason for this was apparently fear, and, in particular, fear of the man who had set himself up, and was accepted as, the arbiter of what was then good practice in theoretical physics, namely Wolfgang Pauli. Pauli could make or break careers, and frequently did so. He was apparently utterly devastating on anything with which he disagreed, and many people were afraid to publish simply because of what he would say or do. Others had apparently also thought of the concept of electron spin, but again didn't have the courage to go public with it. What, we may imagine, would Pauli have done if someone else had come up with the idea of the neutrino? Perhaps because of its complicated prehistory, no one received a Prize for the theory of spin, but, perhaps again reflecting the experimental bias of the Nobel Committee, Stern received the Prize for 1943.

One man who Pauli did manage to break was Baron Ernst Stueckelberg, who must be the unluckiest man in Nobel Prize history, as he was denied the credit for ideas which would have won him *three* different prizes. The first, in the early 1930s, was the idea of an intermediate boson of the Yukawa type to explain the strong interaction. Pauli prevented it from being published. So the next time he had a good idea, Stueckelberg published it in a more obscure fashion, in French. This was a basic scheme for renormalization, but, because of the way in which it was published, it had no influence. By the 1950s Stueckelberg had become a little more established and he then came up with the idea of the renormalization group, which he published in two short papers, one in French and one in English. However, no one took any real interest in this idea until the 1970s when Kenneth Wilson developed it, along with other important theoretical ideas. Wilson, of course, got a prize (on his own); Stueckelberg didn't.

A classic example involves the Prize for 1983, which involves not just one, but *two* cases of the 'unacceptable face' of twentieth-century science. One of these reflects favourably on the Nobel award; one, in my opinion, does not. One of the recipients was Chandrasekhar, who was honoured for his discovery, more than fifty years earlier, of the Chandrasekhar limit for white dwarf stars. Fifty years is a very long time to wait, and the story does not reflect well on the scientific establishment. Chandrasekhar's idea was announced in 1930, but was immediately attacked by Eddington, then at the height of his influence. For a few years Chandrasekhar tried to promote the idea at conferences, but was invariably attacked by Eddington in the most devastating manner. A whole group of significant figures – Fowler, Bohr, Dirac, Russell – supported Chandrasekhar in private but weren't prepared to back him in public. Russell even chaired a meeting in which he allowed Eddington to attack but overruled Chandrasekhar's attempt to reply, even though, just before the meeting, he had said that he knew that Eddington was wrong. Chandrasekhar was effectively driven out of the field and it was not until 1973 that any award actually mentioned his most significant contribution. The Nobel Prize had, at least, belatedly made amends.

But the other half of the Prize conceals an even more disturbing case. This was awarded to William Fowler for his work on nuclear synthesis in stars. However, the theory for which Fowler was honoured was widely known to be largely the work of Fred Hoyle, while Fowler provided the supporting observational evidence. The big breakthrough – the idea that really mattered – was undoubtedly Hoyle's. One can only conclude that he was excluded from the Prize by objections to his abrasive personality,

and to his dissident views on many topics, especially on the fashionable subject of the Big Bang. Hoyle probably had more enemies than any other scientist alive, and one can believe that some people might have thought that giving him the Prize would provide support for his despised Steady State cosmology. But why, then, give the Prize to Fowler unless one intended deliberately to insult Hoyle?

I came across a book recently called *Philip's Science and Technology, People, Dates and Times*, and, as I was idly flicking through the pages of the biographical section, I chanced upon that of Fowler, which commended him for his 'theories' of stellar nuclear synthesis. I immediately looked up Hoyle's. It didn't mention this work at all, but gave a diatribe about all the sins he had committed against the establishment, regarding the steady state cosmology, the origin of life, etc. So, the Nobel award to Fowler had established that *he* was the originator of the theory of stellar nuclear synthesis. History had been rewritten.

There is, regrettably, an element in some historical accounts to rely on the assessments of such third parties as the Nobel Prize Committee. Scientists, who did their main work, many years earlier, somehow get promoted out of nothingness when they get their Nobel award. Other awards then follow, in the same way as they did for the general whose first medal came by accident, while the rest followed automatically. I have two very similar-looking biographical dictionaries of scientists. The Larousse dictionary is a splendid work, full of original information on scientists who are new to me; the Oxford one, I regret to say, is, by its own stated policy, confined to people who have won awards, or have effects named after them, etc., irrespective of their real importance. It is consequently, in my opinion, of significantly reduced value as a research document; it may even be said to have negative value, in perpetuating merely stereotypical information.

In the final analysis, the Nobel Committee have a very difficult job to do, and they have mostly done it rather well. They have inevitably made what we, with all the advantages of hindsight, may decide were wrong decisions, but these have been relatively few; and it is more a matter of emphasis regarding what we would have preferred ourselves – always debatable territory. Even in the one case where it would seem that a deliberate injustice was done, it may have been prejudice from the network of advisers rather than from the Committee itself which swayed the decision. To a large extent, the awards have not been controversial,

and probably much less so than similarly high profile awards for literary, artistic or media achievement.

In recent years, one can recognize that an “Oscar-ish” element has crept into the way some research establishments announce their findings; deliberately premature and overstated claims, sensationalized or dramatized announcements, often in the media, “personality cults”, are often direct bids for Nobel status. There have been occasional attempts to undermine a rival’s announcement, often with evidence that might be considered incomplete. There have been signs of “news management”. There may even be the odd stitch-up. (I don’t personally believe in some of the stories of absolutely simultaneous and independent discovery made in recent years.) But the Nobel Committee have largely managed to resist such pernicious influences.

Nevertheless, the awards should not be taken at face value as equivalent to a history of physics in the twentieth century. Some major topics have been completely excluded (for example, geophysics), and others only sparsely represented (for example, mathematical physics and applied physics). The emphasis has been on areas which contemporaries considered important. Historical analysis may decide that the real priorities should have been elsewhere. It is our duty as historians to resist the idea that the Nobel Prize is a kind of “canonization”, that one winner is as good as another, that it (rather than the actual science) is the highlight of a scientist’s career; and that the large number of scientists who don’t receive it have somehow “failed”. We should not allow the unhonoured collaborators in major researches to be airbrushed out of scientific history. The Nobel Prize for Physics is a fine institution, with an excellent record for promoting the interests of physics and for honouring deserving members of the physics community, but it is not the final arbiter of scientific excellence and should not be used as a substitute for doing genuine historical research.

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