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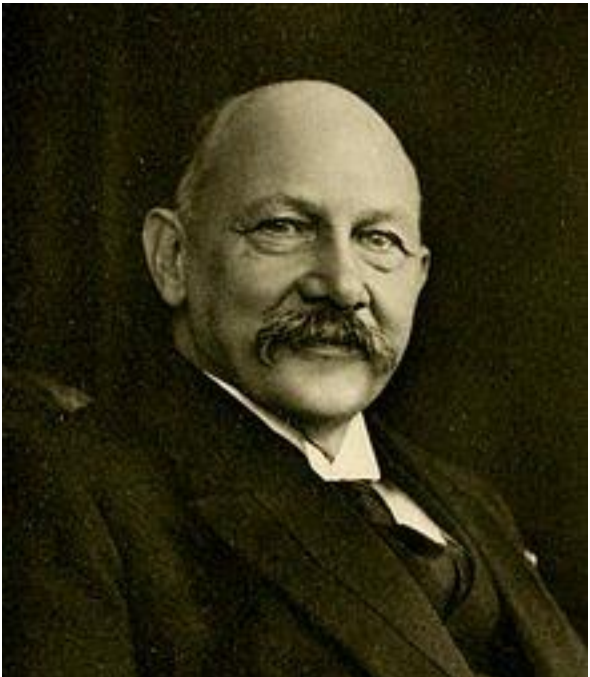
Newsletter



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Cover picture: Heike Kamerlingh Onnes - *taken from Wikipedia*

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Editorial

A Stroll with the Gentlemen of the Royal Society
(Taken from the Proceedings 1850 – 1854)

‘The great success with which the optical researches are treated of in the publications of the RS must make me anxious to lay before the Society a demonstration of my theorem…’

Thus wrote Prof Haidinger of Vienna in 1852, in language that seems very ornate to us today. He concludes his letter with:

‘I have the honour to be, My dear Sir, Your obedient servant, W Haidinger’

I would not suggest a return to such a style but I wonder if we haven’t lost a certain delicacy of touch with the emails of today.

Earlier Henry Fox Talbot – well known for his pioneering work in photography (but was also a physicist) reports on his work on, what we would now call flash photography, capturing images on a spinning disc. For this he needed a power source:

‘…an electric battery, kindly placed at my disposal by Mr Faraday…’

His experiments were apparently successful though unfortunately he gives no quantitative details.

But not all was sweetness and light! At the meeting in May 1853 a mild argument broke out (via letters) between Mr Joule and M Regnault over allegedly mis-represented decimal figures.

I did not feel it desirable to enter upon the laborious investigation…to add a couple of decimal figures… but - a little tongue in cheek?:

‘I feel much gratified that the result arrived at by so eminent an experimentalist as M. Regnault confirms the accuracy in the main of the number I adopted’

The somewhat bizarre paper entitled **'On the vibrations and Tones produced by the contact of bodies having different temperatures'** by John Tyndall, 1854 led to a more acrimonious dispute but always couched in the most polite terms - well almost. The Rev. Baden Powell, VP, reported:

'The peculiar views of Prof. Forbes… were the chief inducement to the resumption of the subject by the author' (Tyndall)

The author then went on to demolish the unhappy Professor Forbes over no less than three experiments. He concludes:

'…an examination of the arguments of Prof. Forbes against the views supported by Faraday… shows the facts adduced against said views become, when duly considered, strong corroborative evidence of their correctness'

In other words - you're wrong! And finally, and delightfully:

Astronomer Royal George Biddell Airy in researching possible correlation between **'Direction of the Wind to the Age (lunar cycle) of the Moon, 1840 to 1847,** as reported by Norwegian seamen, sums up thus:

'…while there is great uncertainty in the verification of an empirical law, even from nearly ninety lunations, it seems very distinctly to negative the asserted law which gave rise to the inquiry.'

Say no more!

Turning to the more formal business of the Society at the December 1851 meeting, admissions were noted of such luminaries as

Thomas Henry Huxley, George Gabriel Stokes and William Thomson.

Also noted was the death of Jens [sic] Christian Oersted whose name appeared *'on the Foreign List'*

It seems political correctness didn't feature much in the mid 19^{th} Century - thankfully!

Malcolm Cooper

Chairman's Report

Five meetings were held during 2015 – possibly a record number for the Group. They were fairly well distributed in space and time: London (March), Leicester (April), Bristol (June), Manchester (September) and Anglesey (November).

The highlight of the meeting held at the Institute of Physics headquarters in London, 'The Lives and Times of Pioneering Women in Physics', organized in conjunction with the Women in Physics Group, was undoubtedly the presence of the granddaughter of Marie Curie, Professor Hélène Langevin-Joliot - a physicist in her own right - who delivered an insightful lecture on the pioneering work of her grandmother. The one-day meeting, attended by around 80, included presentations by six other distinguished speakers, several of whom touched on the desirability of attracting more women into science-based subjects. (see report in this issue – Ed.)

A two-day conference entitled 'From Hooke to Helioseismology' was held at the University of Leicester 9-10 April. This was the second meeting that the history of Physics Group has organized with the British Geological Association, the first being 'Rutherford's Geophysicists' in Cambridge in 2013. The reference to Robert Hooke in the title is not related to his theory of elasticity but rather to his suggestion that earthquakes and the upheaval of rocks account for marine fossils being carried to mountain heights. There was only one presentation on helioseismology, with all other lectures concentrating on seismology studies of the Earth, providing valuable information on its interior structure and on events such as earthquakes and nuclear tests.

In recognition of 2015 being The International Year of Light, a one-day meeting in Bristol with the title 'Focus on Light' was held on the 5th of June in collaboration with the Optics Group. Professor Malcolm Longair opened the meeting with a tribute lecture to James Clerk Maxwell, which included a re-examination of his classic papers on electromagnetism. The remainder of the programme was varied, covering the work of Thomas Young, the physics of colour in biological systems, a history of lighting, herapathite –a polarizing material, photonic crystals, optical fibres and single-photon sources.

'50 Years of Dilution Refrigeration' was the title of a one-day meeting held on 16 September at the University of Manchester jointly with the Low Temperature Group. The idea for this meeting came from our former chairman, Peter Ford, who opened the meeting with a lecture that is reproduced in this Newsletter. The programme of eight talks, which included the history of the development of He-3 refrigerators, was arranged by Andrei Golov.

Our final meeting of the year was a half-day event at the Wylfa Nuclear Power station in Anglesey, held in collaboration with the Nuclear Industry Group and supported by several other organisations. The event was conceived by Geoff Vaughan to mark the closure of the last of the Magnox reactors scheduled for the end of 2016. The presentations - to a large and enthusiastic audience of around 120 - covered the history of the development of British reactors, fuel production, aspects of technical design, maintenance considerations and decommissioning, including containment and storage of radioactive waste. The future of the site, which involves construction of a new Advanced Boiled Water Reactor, was the subject of the final talk. (see report in this issue – Ed.)

A full report on 'From Hooke to Helioseismology' by Sheila Peacock appears in *Astronomy and Geophysics* Vol 56, Issue 5, p 31-33)..

At the AGM held on 16th September, two new members were elected to our committee. We welcome back Dr Peter Ford, a previous Chairman of the Group, and a new member Prof. Keith McEwen.

For 2016, two meetings have already been arranged. These are 'A History of Units from 1791 to 2018' to be held at the National Physical Laboratory in Teddington on 17th March and ' A poetic view of William Rowan Hamilton' – readings of sonnets written by Iggy McGovern in recognition of the life and work of the 19th century mathematical physicist, to be held at IOP headquarters on 14th June. In addition, members may wish to know in advance the location and dates of the '2nd International Conference on the History of Physics'. This conference, which follows the inaugural conference in this series held in Cambridge in 2014, will take place in Pöllau, Austria, 4-6th September 2016. (see report in this issue – Ed.)

We are always pleased to receive the views of members on our activities. Please contact me (ead34@cam.ac.uk) if you have any comments or suggestions.

Professor Edward A Davis

Meeting Reports

The Lives and Times of Pioneering Women in Physics
Report by Chris Green

On Wednesday 4th March 2015, the above conference was held in the Franklin Lecture Theatre at the Institute of Physics in Portland Place, London, organised by History of Physics Group (HoPG) and the Women in Physics Group (WIPG) of the Institute of Physics (IoP), and supported by the Institute of Physics, the London & South East Branch of the IOP and the French Embassy.

After the welcome and greeting by the chairpersons of the two organising groups, **Dr Gillian Butcher** of the WIPG spoke on

The contribution of women to physics: a historical overview.

Dr. Butcher said that the early Greek philosophers including Hippocrates (460-370 BC, the Father of Western Medicine) believed that a woman's womb wanders around her body, causing hysteria and women's health problems, emotional instability and an inability to reason – a view that was expressed even up to the early 20th century! While women such as Hildegard of Bingen (1098-1179 AD) could write on theological, botanical and medicinal texts, in later times, female alchemists could be accused of witchcraft and in 1620 James 1 enacted legislation to make it illegal for his subjects to do anything considered inappropriate to their gender. The rise of technology and printing opened up more opportunities for women, and later the burgeoning philosophical societies sometimes attracted large female audiences. Dr. Butcher noted that progress had not been linear, however; for example, women's take-up of doctorates in science actually fell between the 1920s and the 1960s. In all, from c. 2700 BC right up to the present day, Dr. Butcher listed some 56 women throughout history who have made valuable contributions to science, with varying degrees of public and official recognition. She concluded her overview by stating that throughout history, women have indeed contributed significantly to science, but that it wasn't a linear progress, and depended on time, place and religion.

Early European Women Pioneers

In the first of the lectures on Early European Women Pioneers, **Professor Gerry Lander** spoke on:

Lise Meitner (1878-1968): Pioneer of nuclear fission.

Elise Meitner was born into a well-to-do Jewish Viennese family in November 1878 and excelled at school. She shortened her name from Elise to Lise, as she was always later known. At that time women were not allowed to attend University in Austria, so she took courses privately and passed what was essentially a "private exam" set by the University. Max Planck had visited Vienna in 1905 and encouraged her to come to Berlin, which she did in 1907 and worked (unpaid) as his assistant and also at the Kaiser-Wilhelm Institute of Chemistry. There she met and worked with Otto Hahn, who had begun to work in the new field of "radio-chemistry". Meitner as a physicist, Hahn as a chemist, made a fine team and they made important discoveries over the next two decades, especially in the heavy elements of the periodic table. When Hitler came to power in January 1933, there was no immediate danger for Meitner, as she was protected by her Austrian passport. However, with "Anschluss" in March 1938, she lost this protection, and with two small suitcases she fled Berlin in July 1938 to work in Stockholm.

Meanwhile the irradiation of uranium with neutrons was causing much interest worldwide. Unlike similar experiments on heavy elements such as gold, the results with uranium made no sense at all. Led by Meitner, Berlin had been working on this full time since 1936, with the young chemist Fritz Strassmann playing a key role. After first identifying radium as a possible reaction product, Strassmann & Hahn then reported that barium was produced. They had no idea how this could be explained, and wrote to Meitner hoping she would help. Just after Christmas 1938 Meitner had this information, and was expecting a visit from her nephew Robert Otto Frisch. On a famous "walk through the snow" they realised that the uranium nucleus had become unstable and split. Using Einstein's famous $E = mc^2$, they deduced the energy produced and hence explained fission. Two famous papers were published in *Nature* in early 1939.

Meitner played no role in the forthcoming "race for the bomb". She learnt about Hiroshima from the radio and was horrified. In 1945 Hahn alone received the Nobel Prize for Chemistry. Meitner and Frisch were nominated by many, but unsuccessfully. She worked in Sweden in their nuclear-energy programme, and in 1960 went to live in Cambridge, UK, to be near her nephew Otto Frisch, who was a Professor at the Cavendish. She received many honours, including being elected "Woman of the Year" in 1946 in the USA. She died in 1968 and is buried in a small village in Hampshire with the epitaph "A physicist who never lost her humanity".

Meitner and President Harry S. Truman, 9 February 1946, Washington, D.C. Meitner was honoured as „Woman of the Year" by the National Women's Press Club. They did not discuss nuclear weapons.

In the second lecture on Early European Women Pioneers, by who was probably the star turn, the granddaughter of Marie Curie, **Professor Hélène Langevin-Joliot**, herself a noted nuclear physicist who undertook fundamental research until five years ago, spoke on:

Marie Curie (1867-1934): Pioneer of radioactivity.

Born in 1927, Langevin-Joliot just remembers her grandmother before her death in 1934. Langevin-Joliot spoke about her grandmother's life, talking about Marie Curie's early days in Poland. Marie Sklodowska was born in Warsaw on 7 November 1867, the daughter of a teacher. In 1891, she went to Paris to study physics and mathematics at the Sorbonne where she met Pierre Curie, professor of the School of Physics. Marie Curie describing her first meeting with husband Pierre as a "decisive encounter", saying that he was so taken with Curie's intelligence that "one conversation was enough for Pierre to change his mind about [all] women!" They were married in 1895. Langevin-Joliot described Marie Curie's early struggles to study science and her collaboration with Pierre Curie. "It is difficult to imagine personalities more different: Pierre was as dreamy as Marie was organised, so they complemented each other very well," she said. While Pierre was a professor in the school of chemistry and physics at the Sorbonne, Marie was allowed to work there, and to have a woman in the laboratory in that place was a historical event in France. The Curies worked together investigating strange new phenomena, building on the work of the German physicist Wilhelm Roentgen and the French physicist Henri Becquerel. Marie studied what were known as "uranic rays", questioning whether these were unique to uranium, and began to check all the elements for the same property. She also decided to examine minerals such as pitchblende and chalcolite. Upon discovering that these were even more radioactive (a term that she coined) than pure uranium, she realised that an even more radioactive element than uranium must be present. In July 1898, Marie and Pierre went on to discover a new chemical element polonium, and at the end of the year, with two other scientists, radium. For their research into radiation, they were jointly awarded the 1903 Nobel Prize in Physics with Henri Becquerel, though Marie was due to be left off the nomination until Pierre was alerted to the situation.

Pierre's life was cut short in 1906 when he was knocked down and killed by a carriage. Marie took over his teaching post, becoming the first woman to teach at the Sorbonne, and the university made her director of her own laboratory where she devoted herself to continuing the work that she and Pierre had begun together.

She was denied entrance to the French Academy in 1911, but in that year she was awarded a second Nobel Prize, for Chemistry. In the First World War she created mobile radiography units to examine casualties, and through this work she developed "a combination of self-confidence and diplomacy that would help her to achieve her goals during the rest of her life", Langevin-Joliot said. The Curie's research was crucial in the development of x-rays in surgery. During World War One, Marie helped to equip ambulances with x-ray equipment, which she herself drove to the front lines.

Despite her success, Marie continued to face great opposition from male scientists in France, and she never received significant financial benefits from her work, nor was she elected to the prestigious French Academy of Sciences, despite two Nobel Prizes!

By the late 1920s her health was beginning to deteriorate. She died on 4 July 1934 from leukaemia, caused by exposure to high-energy radiation from her research. The Curies' eldest daughter Irene shared the 1935 Nobel Prize in Chemistry with her husband, Frederic Joliot. They were Hélène Langevin-Joliot's parents.

Professor Langevin-Joliot explained that her grandfather, Pierre Curie, knew the difficulties that his wife, and indeed all women, faced, and in the paper describing the discovery of radium (in 1904) Pierre Curie insisted that Marie be the sole author. There were murmurs that Marie was but a humble assistant of Pierre's, but as she established herself, it became clear to all that this was not the case. She was a most original scientist in her own right, but Langevin-Joliot stressed that Pierre's crucial contribution should not be overlooked!

In 1921 Marie Curie undertook a tour of the US to visit women's universities and to thank those who had donated money to supply one gram of radium for her laboratory, which had been depleted of funds after the war. She also agreed to become vice-president of the International Commission for Intellectual Cooperation of the League of Nations.

Marie Curie's life "showed science as a human adventure", Langevin-Joliot said. "There is a comment of hers that I like very much: 'I have given a great deal of time to science because I wanted to, because I loved research.' Her scientific achievements opened the way for the following generation of women scientists."

British Female Leaders

After lunch, in the first of the lectures on British Female Leaders, Prof. Allan Chapman spoke on:

Mary Somerville, and her work in astronomy and optics, c.1820-1860.

In an interesting and very entertaining talk, he spoke about the phenomenal intellectual gifts of Mary Somerville who was a leading mathematician and astronomer, and after whom Somerville College was named. She was of Scottish origin, born in Jedburgh, the daughter of Vice-Admiral Sir William George Fairfax and was related to several prominent Scottish houses through her mother. Her childhood home was in Burntisland, Fife, but her father sent the 10 year old Mary for a year of tuition at an expensive boarding school in Musselburgh . She returned being able to read and write and could perform simple arithmetic and knew a little French. Following this, she was informally taught elementary geography and astronomy, and was taught Latin by her uncle, Dr Thomas Somerville, who described her as an eager student. Her brother's mathematics tutor also allowed her to attend his lessons unofficially. She also obtained a copy of Euclid's *Elements of Geometry*, and began to teach herself from it. However, her parents forbade Mary from further study, but this did not deter her from studying on her own, although she had to continue in secret. Meanwhile, she continued in the traditional roles of the daughter of a well-connected family and maintained a sweet and polite manner – she was nicknamed "the Rose of Jedburgh" among Edinburgh socialites.

In 1804 she married her distant cousin, the Russian Consul in London, Captain Samuel Greig, son of Admiral Samuel Greig. They had two children and lived in London, but it was not a happy time for Mary – although she could study more easily, her husband did not think much of women's capacity to pursue academic interests. However, he died in 1807 and Mary returned home to Scotland and found her inheritance from Greig gave her the freedom to pursue intellectual interests. In 1812 she married another cousin, Dr William Somerville (1771–1860), inspector of the Army Medical Board. The contrast with Samuel Greig could not have been greater as William Somerville encouraged and greatly aided her in the study of the physical sciences. They had four children. During her marriage she made the acquaintance of the most eminent scientific men of the time, among whom her talents had attracted attention. Before she had acquired general fame, Pierre-Simon Laplace told her, "There have been only three women who have understood me. These are yourself, Mrs Somerville, Caroline Herschel and a Mrs Greig of whom I know nothing" (of course, Somerville was first *and third* of these three). Mary translated the *Mécanique Céleste* of Laplace, and greatly popularised its form, and its publication in 1831, under the title of *The Mechanism of the Heavens*, at once made her famous. She stated "I translated Laplace's work from algebra into common language". Her other works are the *On the Connexion of the Physical Sciences* (1834), *Physical Geography* (1848) and *Molecular and Microscopic Science* (1869). Much of the popularity of her writings was due to her clear and crisp style and the underlying enthusiasm for her subject which pervaded them. From 1835 she received a pension of £300 from government, and in the same year she and Caroline Herschel became the first women members of the Royal Astronomical Society. In 1838 she and her husband went to Italy, where she spent much of the rest of her life. In 1868, four years before her death at age 91, she signed John Stuart Mill's unsuccessful petition for female suffrage.

She died at Naples on 28 November 1872, and was buried there in the English Cemetery. She is commemorated all over the world, but in particular, Somerville College, Oxford, was named after Mary Somerville, as is Somerville House, Burntisland, Fife, where she lived for a time as a child.

In the second lecture on British Female Leaders, **Professor Francis Duck** spoke on:

Edith Stoney (1869-1938): Pioneer of medical physics.

Professor Duck said she had had little historical prominence but had been a pioneer of medical physics, and with her sister Florence, had set up the first radiological service, situated at the Royal Free Hospital. Edith Anne Stoney was born in Dublin in 1869, the daughter of George Johnstone Stoney FRS (1826-1911) , the professor of physics at Queen's College Galway who was the physicist who coined the term 'electron' to describe the fundamental unit of electrical charge so Edith grew up in a family in which achievement was expected.

Edith was educated privately and later went to the Royal College of Science for Ireland, 1888-89, but Trinity College Dublin was not open to women until 1904, so both Edith and her sister Florence went on to higher education in England. In 1889 Edith was awarded the Winkworth Scholarship from Newnham College, Cambridge. She was an outstanding student, taking a 1st Class and ranked equal to the 17th Wrangler in the Mathematics Tripos Part 1 examinations in 1893, followed by a II(ii) in Part 2 the following year. However, though women at the time could attend lectures, sit the exams and obtain passes, they were not allowed to be admitted to degrees at Oxbridge – Cambridge did not admit women to degrees till 1948.

She also became a life member of the British Association for the Advancement of Science. In doing so she joined a small minority of women in the BAAS at that time, about 200 of a total membership of about 5000. It would be another 20 years before the first women would become either a section chair or a council member. By now she was starting to discover the difficulties facing women scientists in an overwhelmingly male profession, and increasingly concentrated her efforts by working in exclusively female organisations.

Edith was appointed as lecturer in physics at the London School of Medicine for Women at an initial salary of £100 per annum. So far then, this is the teaching of physics to medical students (for example, Bernoulli's equation for anaesthetic gases) rather than the development of Medical Physics in its own right. Edith was personally responsible for the physics course and laboratory. An ex-student later wrote:

"Her lectures on physics mostly developed into informal talks, during which Miss Stoney, usually in a blue pinafore, scratched on a blackboard with coloured chalks, turning anxiously at intervals to ask "have you taken my point?" She was perhaps too good a mathematician ... to understand the difficulties of the average medical student, but experience had taught her how distressing these could be.

In 1901, the Royal Free Hospital created a new part-time position of medical electrician, and her sister, Florence Stoney was appointed. The two sisters set about selecting, purchasing and installing x-ray equipment and, the following April, Florence opened the new radiological imaging service. Edith still did not formally have a degree, because it will be recalled that women were excluded from graduation at Cambridge until 1948. Trinity College Dublin redressed this injustice by granting such women *ad eundem* degrees, and Edith Stoney rectified her own position in 1905, when she was one of the first batch of 6 women to graduate from Trinity College Dublin *ad eundem*, based on achievement at another university, and was awarded both BA and MA, recommending other Cambridge women to follow her example. Women who took advantage of the *ad eundem* degrees at Trinity College Dublin were known as "Steamboat Ladies"!

Britain declared war on Germany on 4 August 1914 and by October, Florence was working in a hospital in Europe, exposed to real danger. The sisters offered their services to the British Red Cross at the War office in London, to provide a radiological service to support the troops in Europe. They even had a complete x-ray system, prepared and ready to use. Their offer was refused, because they were women.

Following her resignation from the LSMW, Edith was free to make her own contribution to the war. She offered her unique skills in radiological physics to the Scottish Women's Hospitals (SWH), an organisation formed in 1914 to give medical support in the field of battle

By mid-June 1915 the 250-bed SWH tented hospital was set up near the front line at Troyes. Edith ran the x-ray department. Her mathematical skills were used in devising and using stereoscopic x-ray methods to locate bullets and shrapnel for the surgeons, and carefully calibrated x-ray exposure was necessary to distinguish the small soft tissue changes associated with gas gangrene. A thumbnail sketch of her at this time:

"*A learned scientist, no longer young, a mere wraith of a woman, but her physical endurance seemed to be infinite; she could carry heavy loads of equipment, repair electric wires sitting astride ridge tents in a howling gale, and work tirelessly on an almost starvation diet".*

The Serbian authorities awarded her the Order of St Sava in recognition of her services. By 1918 Edith's health was suffering badly, and she finally resigned from the unit on 24th October. Her war service was recognized by further medals, the *Croix de Guerre* and *Médaille des épidémies,* from the French, and the Victory and British War Medals from Britain.

Returning to England and with no pension and no medical qualification, she instead returned to academic life as lecturer in physics in 1919 at King's College for Women in the Household and Social Science department, which she held until her retirement in 1925.

In the early 1930s Florence developed spinal cancer, and died, aged 62, on 7 October 1932. Edith's reaction to her sister's death was later summed up by a close friend, Dr Lisa Potter: "She was devoted to her sister Dr Florence Stoney, and never really recovered from the shock of her death" [17]. Nevertheless, she continued to promote science for women.

Edith died, aged 69 years, on 25 June 1938 at her home in Bournemouth.

She was a strong advocate of education and training for women, creating a fund that allowed young graduate women to spend time on scientific research overseas. At a time when medical physics was still struggling to become an identified profession, Edith Stoney stands out as one of its most able pioneers.

The final lecture in this report was on:

The first female physics professors in the UK, Daphne Jackson (1936-1991) and Gillian Gehring

This was uniquely special in that it was given by Professor Gillian Gehring herself, who knew Daphne Jackson personally. Daphne Frances Jackson was born in Peterborough on 23rd September 1936, and went to the local grammar school, Peterborough County Grammar School for Girls from where she was able to apply to take physics at Imperial College in London. She was one of only two female students on the course alongside 88 males.

Prof. Daphne Jackson (1936-91), became head of the physics department at the University of Surrey at the age of 34. She had published 80 papers on nuclear physics, had been head of quantum physics at Imperial College, and as a “hobby” had set up a scheme to enable female physicists to re-establish their careers after a break. Now called the Daphne Jackson Trust, it had helped more than 250 women to restart their careers, she said. In 1989 Prof. Gehring herself became only the second female physics professor in the UK, and a portrait of her had been unveiled in the Firth Hall at the University of Sheffield on 2 March by Prof. Athene Donald, she said, in order to bring some balance in the gender of role models on display.

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Kate Crennell talked about prominent women in crystallography whose lives spanned the years 1903 to 2012: Rosalind Franklin, Kathleen Lonsdale, Dorothy Hodgkin, Helen Megaw and Louise Johnson.

The text of this talk was unfortunately not available at the time of going to press but may be included in a future issue - Editor

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Disclaimer

Manchester Dilution Refrigerator Conference

Report by Peter Ford

A one day meeting took place on Wednesday 16th September to mark fifty years since the first successful operation of a helium dilution refrigerator in the Physics Department of the University of Manchester. Peter Ford, who had worked on this project, gave a lecture describing the principles of the dilution refrigerator and the events leading up to its operation when it first reached a temperature of 0.065K. The idea for a helium dilution refrigerator was the brainchild of the Harwell based physicist Heinz London. The first realisation was achieved at Leiden in the group led by Krijn Taconis. Rudolf de Bruyn Ouboter, who was a member of that group, described the initial work and subsequent developments. Their first refrigerator only achieved a temperature of about 0.22K for reasons which were suggested in the publication by the Manchester group.

Following the initial success, rapid progress was made in the development of the refrigerator and prominent among those involved was John Wheatley of the University of California at San Diego. The prolific contributions in ultra-low temperature physics made by this remarkable man were described by Oscar Vilches who worked with him for a number of years. A major advance for improving the performance of the dilution refrigerator was the development of sintered "step" heat exchangers having very large surface areas, which enabled temperatures of less than 0.01K to be reached. George Pickett of the University of Lancaster lectured on "Dilution Refrigerators for Millikelvin Termperatures" and in particular described the pioneering work of Giorgio Frossati of the University of Leiden who developed silver powder heat exchangers.

Part of the conference was devoted to the commercial development of the dilution refrigerator. This began almost immediately following the initial success at Manchester by the Oxford Instruments Company who collaborated closely with Heinz London. The work of this Company over some fifty years was described by Graham Batey and Vladimir Mikheev both of whom are current employees. An important breakthrough has been the production of dilution refrigerators which have not required the use of cryogenic fluids and this has considerably increased their applications. In addition to Oxford Instruments, the work of other commercial cryogenic companies namely ICE, Bluefors, Cryogenic and Janis was presented by a member of their personnel.

The dilution refrigerator has been used in areas which are way outside ultra-low temperature physics. We had two fascinating talks firstly by Tapio Niinikoski, who has retired from CERN in Geneva, who lectured on “Powerful Dilution Refrigerators for Particle Physics Experiments” and then Alain Benoit of Grenoble on “Dilution Cooling for Space Applications”.

The conference was attended by some eighty delegates and was excellently organised by Andrei Golov of the University of Manchester, who put together the programme, and to whom I would like to extend our thanks and appreciation.

Groaner’s Corner

Heisenberg and Schrödinger are swiftly driving along when they are stopped by a police car. A policewoman comes over and says coldly
‘Do you know what speed you were going at Sir?’

‘Absolutely no idea’ says Heisenberg stiffly, ‘but I know exactly where I am!’

Ah, says the policewoman to herself, I’ve got a right wise guy here.

‘I’ll have to search your vehicle’ she says and steps round to the rear of the car, flings open the boot, and carefully lifts out an object. She struts round to the passenger side and knocks on the window.

Schrödinger leans over and whispers to Heisenberg, with a twinkle in his eye, ‘Aren’t our policewomen getting younger these days!’

‘Did you know you have a dead cat in your boot?’ she says in a disgusted tone.

‘No, I didn’t’ says Schrödinger beaming, ‘but I do now!’

Anonymous

"Farewell to Magnox" *

Report by Jim Grozier

This meeting was held to mark the end of an era – the era of electricity generation by Magnox nuclear reactors in the UK. It was held on 28th October at Wylfa on the Anglesey coast – Wylfa, in 1971 the last of the eleven Magnox sites in the UK to open, the most powerful, at 980 megawatts of electricity, and the last to close, in December 2015.

The meeting was not actually held inside the reactor complex – it was at the Sports & Social Club, just outside the fence – but the symbolic power of holding it on site was clear. There is some irony in the fact that, from the headlands at either end of Cemaes Bay, one can see in one direction the looming bulk of the nuclear power station, and in another direction a group of giant wind turbines. Which of these is the energy source of the future? Which is the way forward? Or do we need both? In 1971 the answer would have been clear; nowadays it is anything but.

There were six talks: three on various aspects of the Magnox reactors, one on Magnox fuel production, one on waste management, and one on plans for "Wylfa Newydd" – the new Advanced Boiling Water reactor that is planned for this site in the mid-2020s.

The Magnox reactors were the first generation of nuclear reactors in the UK; Calder Hall, in Cumbria, was actually the first in the world to feed electricity to a national grid on an industrial scale, in 1956. The eight reactors at Calder Hall and Chapelcross in Dumfries – the first two Magnox stations – were originally known by the codename PIPPA (Pressurised Pile Producing Power and Plutonium); they were indeed "dual-purpose", but their primary function was seen as plutonium production for weapons. These two were followed by the remaining sites: Berkeley, Bradwell, Hunterston, Hinkley Point, Dungeness, Trawsfynnydd, Sizewell, Oldbury and Wylfa. All these sites had twin reactors. They came on-stream progressively, between 1962 and 1971. They did produce some plutonium, which is described as "civil plutonium" and is ring-fenced for civil use, awaiting a reactor capable of using it; but their main purpose was to generate electricity.

The fuel consisted of unenriched, metallic uranium clad in the magnesium alloy casings that gave the Magnox design its name. These fuel rods were all made at the Springfields factory near Preston; Bob McKenzie of Westinghouse, who gave a detailed talk on fuel production, was keen to point out that this should not be confused with the fictional Springfield nuclear plant featured in “The Simpsons”! The plant was designed, manufactured, built and commissioned in just 18 months.

Earlier in the afternoon, Malcolm Grimston of Imperial College had given a historical narrative that stretched back to the discovery of radioactivity and beyond, including Ernest Rutherford’s assessment in 1932 that it was not feasible to get energy from the nucleus, and the (new to me) story of how Germany gave up trying to produce plutonium in 1942 after the failure of its experimental pile – now known to be due to the presence of neutron-absorbing boron impurities in the graphite moderator.

Malcolm pointed out that one of the problems with the Magnox reactors was the absence of economies of scale: they were all, in a sense, prototypes – not only different from one another, but continually evolving on an individual basis, to meet each new challenge. Ted Hopper of Magnox Ltd (now a part of the Cavendish Fluor Partnership) took us through this evolution, and highlighted some of these challenges. They included the effect of neutron bombardment on steel pressure vessels, raising the temperature at which the transition from brittle to ductile took place in the steel, which was ultimately solved by moving to concrete pressure vessels. Also the carbon dioxide coolant, normally regarded as chemically inert, reacted with the graphite moderator to produce carbon monoxide, lowering the moderator density. This effect was mitigated by a slight enrichment of the ^{235}U content of the uranium fuel, up to 0.8% from its natural concentration of 0.7%, which does not sound like much, but, as Ted pointed out, represents a 14% improvement in fuel efficiency, which would be very welcome in a car engine!

Bob Clayton, formerly Engineering Manager at Wylfa and Chief Engineer of Magnox, gave a detailed talk about the Wylfa boilers. These presented many engineering challenges, because they had to be accommodated in the narrow space between the reactor core and the wall of the spherical pressure vessel. This necessitated using small pipes with many bends – not an ideal design for a boiler, and one which was to present problems during the life of the reactors. Leaks could be detected by monitoring the moisture level in the coolant gas, and selectively closing parts of the boiler until the leak was located. This could all be done remotely, but of course repairing a leak needed human intervention – and the boiler area was an extremely hostile environment.

Alun Ellis of Radioactive Waste Management (part of the Nuclear Decommissioning Authority) took on the “poisoned chalice” of waste management. He outlined the various levels of radioactive waste, and what is being done with it. The Drigg storage facility is OK for low level waste, but intermediate and high level wastes require geological disposal; no progress has been made on this issue to date in the UK. (Alun kept an admirably straight face when he announced that fission products “don’t remain radioactive for ever ... just for hundreds of thousands of years”. Well, that’s all right then!) The search is on, not just for a suitable geological site, but also for a “willing community” to accept the waste – and, not surprisingly, none has yet been found. This issue continues to blight the nuclear industry’s claim to “green” credentials.

What stands out, for me, about the Magnox story are the achievements of the engineers, often working to deadlines that must have seemed impossible to meet. The sheer ingenuity of the Springfields plant came across in a vintage video clip shown by Bob McKenzie during his talk. Equally impressive was the degree of automation that was achieved when a new plant was built, reducing the workforce by a factor of five and introducing robots to do most of the handling. Other success stories that unfolded during the meeting were the leak detection techniques used on the boilers, and the introduction of “on-load continuous refuelling” to avoid disruptive shutdowns.

The Magnox reactors ended with Wylfa. For the second generation of UK reactors, the AGR (Advanced Gas-Cooled Reactor) design was chosen instead, mainly to increase the temperature of the steam so that more efficient turbines could be used. In their combined lifetimes, the Magnox fleet generated a petawatt-hour (10^{12} kilowatt-hours) of electricity, and there were no major accidents.

* This meeting was organised jointly by the IOP Nuclear Industry and History of Physics Groups

Feature Articles

Newton, Huygens and Thomas Young's Interpretation

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(From a talk presented at Bristol on 5 June 2015 as part of the History of Physics Group's contribution to 'Focus on Light', a conference celebrating the International Year of Light, 2015)

Thomas Young famously established the wave theory of light using the principle of interference. Working at the Royal Institution, he obtained interference patterns by superimposing two coherent light beams. Young was not a professional scientist. He was a professional physician with the interests and encyclopaedic knowledge of a polymath. His special talent was in making extraordinary connections between different ideas which led to major breakthroughs in the areas in which he studied. His forte was a kind of lateral thinking based on parallel processing from a massive array of facts.

Because he didn't follow up with extensive mathematical developments of his breakthrough discoveries, Young has sometimes been considered, wrongly, as a dilettante with negligible significance for the overall development of science. However, the single-minded pursuit of an idea to its completion is very different from creating the initial breakthrough conception, and the kind of thinking that Young was able to produce has had a very significant impact on science precisely because it stems from a relatively rare kind of talent and frequently leads to quite unexpected results.

Historians have made much of the fact that Young's work in establishing the wave theory of light was soon overtaken by the beautiful and complete mathematical theory of his great French contemporary, Augustin Fresnel. They have even sometimes implied that Fresnel knew nothing of Young's work and discovered for himself the principle of interference, the significance of Huygens' work, and the necessity of transverse waves for polarization. The documentary evidence and the direct involvement of Young's friend François Arago suggest otherwise, and it is clear that it was Young who set the agenda for the explanation of interference, diffraction and polarization which was set out so masterfully in Fresnel's work. In particular, it was Arago, who had a deep knowledge of Young's work, who persuaded Fresnel to take up wave theory and to specifically aim at the problem of diffraction.

Astonishingly, Young not only set the agenda for early nineteenth century wave optics, he also set the entire tone for the historiography of optical theory up to the present day, and even contributed to the way in which other major scientific theories have been viewed. Young saw clearly that the key aspect of the theory of light was a competition between rival theories of particles or corpuscles and waves. This was, of course, a relatively old issue but Young's work brought it sharply into focus, perhaps for the first time.

One of the key aspects was the explanation of the law of refraction, first published by Descartes, $\sin i / \sin r = n$ (a constant refractive index). In a corpuscular or emission theory, a force perpendicular to the boundary makes the particles travel faster vertically in the denser medium, making the refractive index, $n = \sin i / \sin r = v_2 / v_1$. The argument comes from Descartes's own analysis. At a later date it became associated with the principle of least action, which first emerged in the mid-eighteenth century in the work of Maupertuis and Euler.

Wave theorists, by contrast, realised that wavefronts are perpendicular to 'rays' of light, and the distance between successive wavefronts decreases in the denser medium, reducing the speed, and making the refractive index $n = \sin i / \sin r = v_1 / v_2$. This argument is often thought to have been introduced by Christiaan Huygens, and his is certainly the most sophisticated version. However, it predates him by several decades and seems to have originated in the work of the philosopher, Thomas Hobbes. Pierre de Fermat then associated it with the principle of least time.

The two refraction conditions are often presented as the obvious consequences of the corpuscular and wave theories but they are not. Robert Hooke contrived to use his pulse or wave theory to find the Cartesian condition v_2 / v_1 because he made his wavefronts oblique to accommodate a theory of colour which disagreed with Newton's. Maignan and Barrow found the alternative condition v_1 / v_2 from a corpuscular theory with the particles acting as a fluid or in a fluid aether.[1]

As Thomas Young saw it, the two main protagonists were Newton (for particles) and Huygens (for waves), these contributions dating mainly from the 1670s, thirty or forty years after Descartes and Hobbes. Newton has always been considered one of the corpuscular theorists, supposedly believing in $n = \sin i / \sin r = v_2 / v_1$, but his real views on light were much more subtle. In reality, he found it difficult to reduce optics to mechanical terms, though he made several attempts, especially in his early optical lectures. The problem was that his two main experiments seemed to give conflicting information.

In the first he passed white light through a prism, observed that it dispersed into a spectrum with colours from red to violet, but that, on passing rays of any of the individual colours through a second prism, there was no further dispersion. Clearly, for him, the colour-forming property of a ray of light was not due to a modification by the prism, but was related to the kind of *conserved quantity*, like momentum or mass, that he had found important in mechanics. Rays of light were in some sense real objects, like particles, with conserved mechanical properties like mass and momentum.

The second experiment seemed to suggest something entirely different, even opposed, to the first. He took up some early observations by Boyle and Hooke on what we would now call interference fringes in thin films, and set up a quantitative experiment by observing the fringes in the film between a glass plate and a convex objective lens from a telescope. He was able to show that the fringes were periodic with a mathematically definable property related to the modern concept of wavelength.

At first he conceived of the waves that this implied being induced in the medium by the rays of light, but work on thick films subsequently showed that the periodicity was intrinsic to the rays themselves and was maintained coherently over thousands of vibrations. He eventually came up with describing the periodicity as a kind of 'fit', which was in some fundamental sense probabilistic or indeterminate. 'A ray of light has paroxysms of reflection and refraction and indeterminate ones at that.'[2]

What was Newton to make of this contradiction? The answer lies in a way of thinking that was completely different to that of any of his contemporaries, and completely misunderstood by them. Newton's practice was not to try to reconcile seemingly contradictory positions with a mechanistic hypothesis, a model-dependent theory. The two properties were conceived in abstract terms and, though they seemed to suggest contrary natures for light, they had both to be fundamentally valid, so the seeming contradiction had a fundamental meaning to be discovered at a later date.

Newton's early theorising suggested the *momentum ratio* $n = \sin i / \sin r = p_2 / p_1$, and *inferentially* $n = v_2 / v_1$. In Proposition 94 of the *Principia*, he derived the condition for particles which *may or not be* like those of light. Descartes had derived his result by assuming that there was no horizontal force, without any consideration of what the vertical force would be. Newton's argument assumed a refractive index dependent on the incident velocity.

$$\frac{\sin i}{\sin r} = \sqrt{1 + \frac{k}{v_i^2}}$$

Expressed in terms of a vertical force with potential ϕ (an integral in Newton's theory) this becomes

$$\frac{\sin i}{\sin r} = \sqrt{1 - \frac{2\phi}{c^2}}\ .$$

Newton's Proposition 39 (equating ϕ to $\frac{1}{2}v^2$) allows us to write this as

$$n = \frac{\sin i}{\sin r} = \sqrt{1 - \frac{v^2}{c^2}}$$

or

$$n = \frac{u}{c} = \sqrt{1 - \frac{v^2}{c^2}}$$

We may note the similarity of this expression to the inverse γ factor of special relativity, and also the implication that

$$u^2 = c^2 - v^2.$$

Dynamic arguments like this were used by people who claimed to be Newton's followers in corpuscular mechanics, in particular Robert Smith and A.-C. Clairaut. Despite this, Newton, in his works, nowhere said that light *was* corpuscular. It was not his style to produce fundamental arguments based on hypotheses, however plausible. In fact, by the time he published his *Opticks* in 1704, he had discovered strong reasons to doubt the whole theory. This was because he couldn't find a proper mathematical description of dispersion.

If a mathematical theory of optics was possible, then it had to apply to dispersion. Dispersion occurs because different refractive indices are associated with different colours. A mechanical explanation of refraction in a medium would seem to require dispersion via a change in momentum. The coloured rays might be distinguished either according to their masses or their velocities. In the spirit of Proposition 94, Newton tried several velocity models of dispersion, red faster than blue, blue faster than red, etc. He asked John Flamsteed, the Astronomer Royal, to make a crucial test. Did Jupiter's satellites look red or blue when they were eclipsed? Flamsteed had no idea what this was about, but he reported that they didn't. Newton decided that particles with different speeds wasn't the answer.[3] Exactly the same evidence presented itself on two further occasions: in the middle of the eighteenth century and with Fresnel in the early nineteenth – with the same result.

Newton never doubted the conservation of momentum, and he always believed that the momentum increased in the denser medium, but he had problems with the definition of momentum as mass × velocity, with the implication that $n = \sin i \,/\, \sin r = v_2 \,/\, v_1$ was no longer necessarily valid. It never appeared in this form in the *Opticks*. In that work, Newton avoided the velocity relation in deriving $n = \sin i \,/\, \sin r$. Subsequent commentators thought this was an oversight, but it was really a deliberate omission. In fact, Newton tried instead a mass model of dispersion in which the force involved depended in some way inversely as the mass,[3] suggesting that in some respect the 'mass' of a light corpuscle was not a fixed quantity in the same way as that of a real material particle.

Now, Newton was not only the creator of the modern mechanics of material particles. He was also the creator of the modern development of wave theory. He gave it mathematical treatment for the first time in Book II, Section VIII of the *Principia*, showing its relation to simple harmonic motion, defining frequency and wavelength, and giving two key formulae for the velocity of waves:

$$c = \lambda v \qquad \text{and} \qquad c = \sqrt{\frac{k}{\rho}}\,.$$

Euler later bodily adapted Section VIII from Newton's more geometrical approach to the modern algebraic style, while d'Alembert subsequently discovered the wave equation, the third key equation in wave theory.

Newton included diagrams in the *Principia* clearly showing wave diffraction and, in the section on the tides in Book III, gave, as Young noted, a succinct exposition of the principle of interference. He also studied what we *now* call optical diffraction, which had been observed by Francesco Grimaldi in 1665, providing the most accurate experiments for more than a hundred years, but no one at that time recognised what it really was. This was because no one really appreciated the importance of transverse waves in a medium. Wave theories of light tended to be defined in terms of longitudinal pressure waves, like those of sound.

Newton repeatedly denied that light could be simply a pressure wave as it showed only straight line motion and did not diverge into the unmoved spaces. In his experiments on single-slit diffraction, Newton never found the light to bend into the shadow, as the diffraction explanation would seem to require. However, late in his lifetime two researchers in France saw what we now call the Poisson spot behind a disc-like object without realising its significance. Newton's own very careful experiments led him to explain the fringes he observed in diffraction as the result of a force inducing a transverse eel-like motion in the rays of light.

Disregarding earlier wave theorists, such as Hooke, Young saw the principal opposition to Newton's particle or corpuscular theory as coming from Christiaan Huygens. Huygens' theory is most distinctive from that of his predecessors in using his famous construction. Physically, every wave motion was assumed to produce innumerable secondary wavefronts whose common tangent defined the new wavefront as being perpendicular to the direction of propagation. Huygens was able to use this to derive the v_2 / v_1 refraction condition and the principle of least time.

Huygens' rays were not always normal to spherical wavefronts, as they had been for earlier wave theorists; they were lines drawn from the original centre of the wave motion to the points of each of the secondary wavelets on their common tangents. This meant that the wavefronts did not have to be assumed to be spherical. And one new fact could now be uniquely explained by Huygens' theory. Erasmus Bartholin had observed that Iceland spar produced two refracted rays, the ordinary ray, obeying the usual law of refraction, and the extraordinary ray which did not. Rotating the crystal made the extraordinary ray rotate about a normal to the crystal facet upon which the light was incident. Huygens was able to account for this by proposing that the wavefront for the extraordinary ray in Iceland spar had an elliptical rather than spherical surface, produced by a second aethereal pulse in the crystal.

Double refraction was the main subject at the only meeting between Newton and Huygens in 1689. It turned out to be the Achilles' heel in Newton's optics, and Newton was well aware of the problem it posed for him. He could not find an explanation, though he complained about Huygens' theory requiring two aethers. The problem he had with Huygens' work was that it assumed a physical hypothesis which, for him, was at variance with the facts. There could only be one aether at best, and light travelled forward in a straight line in contradiction to Huygens' theory.

But on another aspect of the behaviour of Iceland spar, Newton had an explanation while Huygens didn't. Huygens himself had shown that using two crystals of Iceland spar and, rotating one with respect to the other, he could make either of the rays disappear. Newton claimed that this was due to a 'polar' property of the rays of light, assuming it had some kind of material nature. Now, polarization would become a major turning-point in the creation and reception of the wave theory, and, looking at this, we can see how it was Newton who defined the whole problem for Young.

Young was Cambridge-educated and was certainly familiar with Newton's major works, the *Principia* and the *Opticks*. He had read Proposition 94 in Book I of the *Principia*. This is the only time in his published work that Newton used the Cartesian argument. But here he specifically states that he is investigating a hypothetical case for particles which may or may not be like those of light. But Young wouldn't have concerned himself with such niceties, because Newton in Query 29 of the *Opticks* would have given the impression that he was describing his own hypothesis of the nature of light corpuscles: 'Are not the Rays of Light very small Bodies emitted from shining Substances? For such Bodies will pass through uniform Mediums in right Lines without bending into the Shadow, which is the Nature of the Rays of Light. They will also be capable of several properties, and be able to conserve their Properties unchanged in passing through several Mediums, which is another Condition of the rays of light.'

Young would have seen in Queries 24-29 that Newton was having trouble with double refraction, and when his friend William Wollaston published work on Iceland spar in 1802, Young immediately noted that it supported the Huygens explanation of double refraction. This created the chain of events which ultimately led to the Fresnel theory, in which the classical wave theory achieved perfection.

When a French version of Wollaston's work was published, Laplace, the major Newtonian optician of the period, immediately recognised the threat to his programme of describing all physical phenomena in terms of action-at-a-distance between particles, and in 1807 he set one of his protégés, Etienne Malus, to work on the problem. The latter managed to show that, using Huygens' construction, but with the principle of least action replacing the principle of least time used by Huygens, the corpuscular explanation of light could be made to yield the relevant equations. The corpuscular theory was saved, together with the Laplacean programme, but only for a decade.

It was also Malus, who followed up Newton's suggestion on polarization, deriving the word from Newton's 'polar' virtue. However, Young thought Newton's explanation was a hand-waving one – a fudged, purely 'verbal' attempt at explanation, without scientific content. And it was he who first came up with the explanation that polarization occurred because light waves must be transverse. Young clearly thought that Huygens' explanation of double refraction was such an outstanding result that it overrode all imperfections in the Huygens theory.

There were, in fact, many wave theories of light, including a very sophisticated one by Leonhard Euler, based on the mechanical wave theory he had derived from Newton. From Young's point of view, trying to explain interference and diffraction, there was a lot wrong with Huygens's theory, as it explained virtually nothing else. Huygens' wave theory was based on random pulses – he didn't accept periodicity. He wrote: 'But as the percussions at the centres of these waves possess no regular succession, it must not be supposed that the waves follow one another at equal distance.'[4] So the concepts of wavelength and frequency didn't apply. It was *Newton's* experiments, not Huygens', that gave Young his values of wavelength.

In principle, this meant also that there was no such thing as phase velocity, the physical property predicted to decrease in the denser medium in the wave theory. There was also no concept of interference or diffraction. Grimaldi's experiments were denied. In Huygens' view, for example, the waves or secondary wavelets crossing over each other should not be allowed to interfere. Huygens' principle, in truth, was not really concerned with 'physical' wavelets at all. He made the assumption, to justify rectilinear propagation, that the secondary waves would only be perceived at their common tangent. Taken to its logical conclusion, it would make points not on wavefronts assume an intensity infinitely greater than those on the wavefronts. And Huygens' pulses were longitudinal, so polarization was also impossible.

Despite the brilliance of Huygens' then little-known construction, his theory was not the obvious way to construct an explanation of light. There were other theories better able to explain the interference and diffraction phenomena with which Young's examination of the wave theory had begun. However, Young had read Newton and *Newton had invoked Huygens' explanation of double refraction*, even quoting him in French in Query 28, though not referring to his construction. Young was so encyclopaedic in his reading that he would certainly have turned to read Huygens, and so had his attention drawn to Huygens' construction.

Through Arago's criticism of Malus's theory, in 1811, which specifically referred to Young's explanation of Newton's rings (1811), and through his encouraging the young Fresnel to take up the wave theory in 1814, and his suggestion to Fresnel in 1815 that the problem of diffraction was the main one to be solved by a wave theory, the Huygens version of the wave theory was naturally foregrounded and the Huygens construction adopted. It was thus through Young that Huygens became significant. But it wasn't obvious that this was the way that wave theory should have proceeded. Fresnel's theory only worked when he introduced a seemingly arbitrary obliquity factor which overcame the problem in Huygens' theory of forward progression.

Young's account of the development of optical theory was not meant to be historical in the strictest sense. He used 'Newton' and 'Huygens' almost as 'counters' to represent corpuscular and wave theories, though neither, as he well knew, was a truly accurate description. Young also emphasized the difference in the velocity ratios for refraction that was eventually put to the test. Generally, but not uniquely, theories based on corpuscular ideas, using least action, tended to invert the velocities that were characteristic of wave theories, based on least time, and vice versa. Conversions between the theories were often made by a simple inversion, and it happened both ways at different times. Hamilton showed that the principles were interchangeable and equally applicable to optics as early as 1827.

Many earlier histories proclaimed that Foucault's experiment in 1850 showed that light travelled faster in air than in water, and so seemed to show that the corpuscular theory was wrong and the wave theory right. But there were always problems with this. Foucault measured the group velocity not the phase velocity, which is the one predicted by the wave theory. They can be completely different.

The issue was eventually solved by wave-particle duality. De Broglie's relation gave us $p = h/\lambda$, and so involved a combination of least time and least action, and the reciprocal nature of the particle momentum and the phase velocity of the waves. By this argument, photon momentum should increase on refraction. But controversies over whether photon momentum increased or decreased on refraction continued throughout the twentieth century, involving such physicists as Minkowski and Abraham. The issue was finally resolved as late as 2010 by Barnett and Loudon.[5,6] The canonical momentum increases, the kinetic momentum decreases.

Since the inversion of velocities is such a general principle, it is interesting to note that Newton came remarkably close to $p = h/\lambda$ in a draft version of the Newton's rings experiment in the 1660s: 'the difference of … ye interjected medium belonging to each circle [proportional to wavelength] are reciprocally as … ye motions [momenta] of ye rays in that medium'.[7] Poisson later pointed out that Fresnel's integrals required particle speed to be inversely proportional to wavelength. This is not the only place where Newton seems to have been on the right lines, despite his failure to explain double refraction.

Young's historiography has helped to pitch the theory of optics as a series of revolutionary developments, seeing the classical wave theory and the quantum theory as new beginnings. We have been persuaded that the corpuscular theory of light must be rejected, because the photon does not behave like a material particle. This is the first of the revolutions which then leads to the second in which even the classical wave theory must be overturned. If we take this line, we have to construct our history so that the corpuscular theory is wrong, even though Hamilton was able to use it to set off the development which ultimately led to Schrödinger's quantum mechanics.

But we needn't have written our history like this. In the strictest sense, we see that nearly all of *Newton*'s positions are 'correct' in modern (quantum) terms, and this was because he created general explanations, based on abstractions and a systematic rejection of mechanistic hypotheses. Thus, the corpuscular theory (in Newton's version) correctly required a change in *momentum*, not velocity. The mass model of dispersion in terms of the momentum argument of Proposition 94 has the same mathematical structure as would later be created by 'relativistic mass' (γm). The photon does have preferred directions of polarization, through its spin. The fits have the same kind of probabilistic nature as quantum particles. Reflection is due to the whole surface, as Newton also supposed.

Newton's explanations again seem to anticipate the pilot wave and the superluminal phase wave. He understood the interconvertibility of light and matter, and, while recognising that light must have some kind of material nature, he also saw that light 'particles' and those of material bodies followed different laws. We see the distinction today in terms of the difference between 'massless' gauge bosons and massive fermions. Even the Newtonian explanation of diffraction in terms of a force has been shown by Sir Michael Berry to equate to the Bohmian quantum potential, if we use a construction (cotidal lines) first introduced by Young. The streamlines which are 'contours of constant phase of the total wave' rather than conventional wavefronts drawn normal to the rays, do 'indeed wriggle like an eel, as the result of non-Newtonian forces acting from edges etc.'[8,9]

Most famous of all, is his clear use of a dualistic theory to explain the co-existence of seemingly contradictory results from his two experiments. In first recognising this, Young, as always, was ahead of the game. He wrote in an article in 1817: 'Whether, therefore, light may consist in the projection of detached particles with a certain velocity, as some of the most celebrated philosophers of modern times assert, or whether in the undulations of a certain ethereal medium as Hooke and Huygens maintained, or whether, as Sir Isaac Newton believed, both of these causes are concerned in the phenomena ... .'[10] None of this is a 'coincidence'. It is the result of using non-hypothetical methods, based on an Ockhamist abstraction from the data.

The development of optics did not need to have followed the path created by the Huygenian method, which Young revived and which Fresnel then employed. It could have remained 'Newtonian' throughout. It could have been based on the characteristic function of Hamilton which did not distinguish between wave and particle theories, and which then led to Hamiltonian dynamics and Schrödinger's version of quantum mechanics. Hamilton started off as a corpuscular theorist and was immediately able to transfer over to the wave theory by inverting velocities when that became successful.

Fresnel's theory is one of the most perfect ever devised – almost to the point where it gives a false impression of what physical theories are usually like. As far as I know, it is the only really general theory based on a model and not purely on abstractions, like Maxwell's theory or quantum mechanics. Huygens' construction is an extraordinary piece of mathematical physics because it is so singular. It is not obvious that waves should do this. In fact, it shouldn't work. It doesn't explain why waves travel forward in a straight line. But it is a great addition to the techniques available to the physicist, and it was ultimately through Young's influence that it played such a prominent part in optics in the nineteenth century.

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More groans

Two atoms meet on the street. One says to the other

'Great to see you - how are you?'
'Oh not too good, I'm afraid' says the second atom dejectedly
'What's the matter?' asks the first
'I think I've lost one of my electrons!'
'Are you sure?'

'I'm positive!'
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The Principles behind the Helium Dilution Refrigerator and its First Success at Manchester University

A Personal Reflection by Peter Ford

This is my own account of the events leading to the first successful operation of a helium dilution refrigerator in Manchester in June 1965.

I had obtained a BSc Honours Degree in Physics from the University of Birmingham in 1963. During my final year I attended a low temperature physics course given by Professor Joe Vinen, who had very recently come to Birmingham from Cambridge to take up a chair in the Physics Department there and begin work in low temperature physics. The lectures first aroused my interest in low temperature physics. The names of Joe Vinen and Henry Hall are inextricably linked together through their pioneering studies at Cambridge on second sound in uniformly rotating liquid helium carried out between the years 1955-58 (1). Henry Hall had already obtained a Chair in Physics at Manchester University.

I came to Manchester in September 1963 to study for a PhD in Henry Hall's Low Temperature Physics Group working under the supervision of Dr Eric Mendoza. I had visited him two months previously when I first learnt about "The Harwell Dilution Refrigerator" and I was given a short document about it, which had been produced by a chemical process - this being the pre-photocopying era. The document was marked "Confidential". I recall Eric telling me that if such a dilution refrigerator, which involved a mixture of liquid helium-4 and its rare isotope liquid helium-3, could be made to work, then it would be the most important technical advance in low temperature physics since the War. I think that I realised immediately the importance of the proposal and also of Eric's prophetic words "If it could be made to work". Trying to achieve this occupied much of my efforts, as well as those of several other people, over the next two years.

The helium-4 atom is characterised by having a very simple and stable structure. The nucleus contains two protons and two neutrons and has no resultant angular momentum or magnetic moment. The two orbiting electrons completely fill the innermost K shell and are firmly bound. Helium proved to be the most difficult of all the elements to liquefy. The first liquefaction was carried out by the Dutch scientist Heike Kamerlingh Onnes, and his co-workers, at Leiden University in Holland in 1908, and occurred at 4.2K (2,3). Helium is an inert atom and the reason for the very low boiling point is a consequence of the weak Van der Waals attractive forces between helium atoms and only at low temperatures are they sufficiently strong to overcome the disruptive influence of thermal agitation. For some twenty five years after the first liquefaction of helium, Leiden was the only place in the world capable of reaching temperatures close to the absolute zero of temperature and hence they had a monopoly to study a wide open field in physics. In the process Kamerlingh Onnes and his co-workers discovered superconductivity in 1911 (2,4) and studied it intensively. However, during the 1920s it was realised that helium itself had some strange properties which took place around 2.18K. Willem Keesom, who succeeded Onnes as head of the Leiden laboratory, observed a striking specific heat anomaly occurring at this temperature and below it helium appeared to enter a strange new state, which became known as the superfluid state in which part of the helium seemed to flow with no viscosity or heat capacity and appeared to have an enormously high thermal conductivity, several orders of magnitude greater than copper at that temperature (1,2,5).

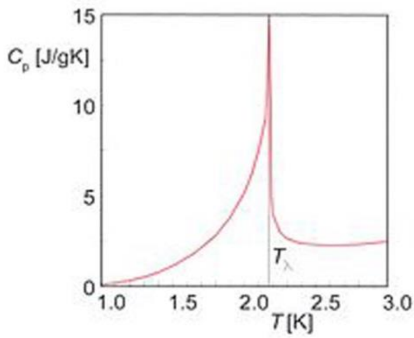


Figure 1 The Specific Heat of Liquid helium-4 under its Saturated Vapour Pressure.

It is fortunate for physics and essential for the helium dilution refrigerator that besides helium-4 there exists also a rare stable isotope helium-3. The electronic structure of both isotopes is identical and hence the interaction between helium-3 atoms must be the same as that between helium-4 atoms. However, the nucleus of helium-3 contains two protons but only one neutron and has a net spin of one half as well as a magnetic moment. As a result of possessing an odd number of particles, helium-3 obeys Fermi-Dirac statistics whereas the common helium-4 obeys Bose-Einstein statistics. This difference in statistics, due to the difference in the number of particles in the nucleus, reveals itself in the radically different behaviour of helium-4 and helium-3 in the liquid state. People have likened the Bohr-Rutherford model of an atom as an orange, representing the nucleus, sitting in the middle of St Paul’s Cathedral. Last year I attended a Carol Service in St Paul’s Cathedral and was very aware of its vast size. I find it amazing that the difference of one neutron within the nucleus gives rise to such remarkably different properties of helium-3 and helium-4. Helium-3 behaves as a Fermi liquid and at low temperatures has a finite heat capacity and viscosity and obeys Curie's law of magnetism. There was widespread interest as to whether helium-3 would become superfluid and this was eagerly sought after during the 1950s and 60s. It was finally first discovered by Doug Osheroff, Bob Richardson and David Lee at Cornell University in 1972 at a temperature of about 2 milli-K (0.002K). Superfluidity in helium-3 was found to be a much richer and more complex phenomenon than that found in helium-4 and has subsequently been extensively studied.

The behaviour of mixtures of helium-3 and helium-4 has also been widely studied and is crucial to the understanding of the Helium Dilution Refrigerator. It can best be appreciated by reference to the phase diagram of a mixture.

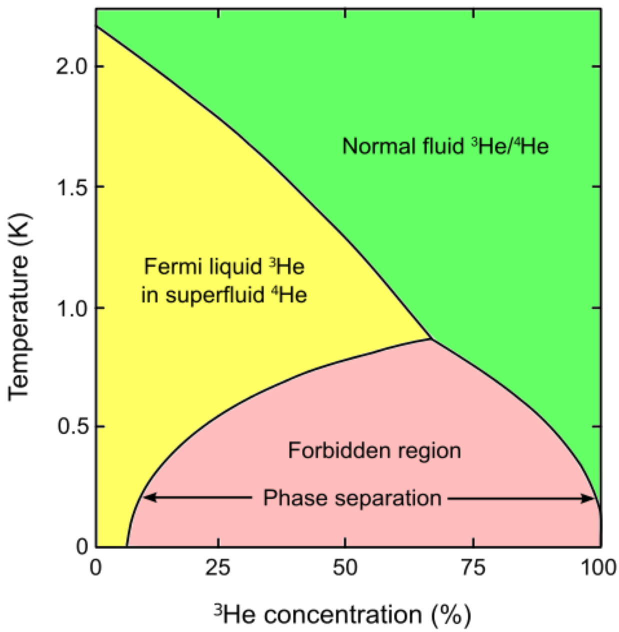


Figure 2 The Phase Separation Diagram for a Liquid Helium-3 – Liquid Helium-4 Mixture near the Absolute Zero of Temperature.

Above a temperature of about 0.86K, the two isotopes are miscible in all proportions although it will not be superfluid if it contains more than a certain amount of helium-3. When it is cooled to a temperature below 0.86K, there is a separation into two components. This separation becomes more marked as the temperature decreases and can be thought of as a consequence of the third law of thermodynamics, which requires a state of perfect order at the absolute zero of temperature. The lighter helium-3 rich phase floats on top of the heavier helium-4 rich phase and there is a visible boundary between the two layers. At a temperature of about 0.1K above the absolute zero of temperature, one can see from Figure 2, that there is essentially pure helium-3 in the upper phase and a concentration of about 6% helium-3 in the lower phase.

This situation gives rise to the proposal for a Helium Dilution Refrigerator, which was first put forward in a seminal paper by London, Clarke and Mendoza (6), following experiments which had been carried out at Harwell. They suggested that in the lower phase the superfluid helium-4 has virtually no entropy and viscosity and can therefore be regarded as a background matrix in which the helium-3 atoms can move. This can be thought of as a "quasi-gas". By contrast the upper phase, containing almost pure helium-3, can be thought of as a "quasi-liquid". There is a latent heat associated with helium-3 passing from the upper "quasi-liquid" phase to the lower "quasi-gas" phase and this would be expected to produce a cooling in a similar manner to that experienced by a liquid on evaporation. This is the basic principle behind the operation of the helium dilution refrigerator.

The fact that that there is just over 6% of helium-3 in the dilute phase at the absolute zero of temperature is crucial for the operation of the dilution refrigerator. This was first discovered by David Edwards and his colleagues (7) at the Ohio State University in the USA. According to classical physics one would have expected a complete phase separation between the two isotopes. The finite solubility of helium-3 in the lower, helium-4 rich phase, at the absolute zero implies that a helium-3 atom must have a lower energy when placed into pure liquid helium-4 than it would have in pure liquid helium-3.

Because these are quantum liquids, the helium-3 atoms obey Fermi-Dirac statistics and so the Pauli Exclusion Principle applies. As such, when helium-3 atoms are added sequentially into a helium-4 environment at the absolute zero they must go into successively higher energy states. Eventually a concentration is reached where there is no energy advantage for a helium-3 atom to be in a liquid helium-4 environment instead of a liquid helium-3 environment. This occurs at a solubility of 6% helium-3 in helium-4.

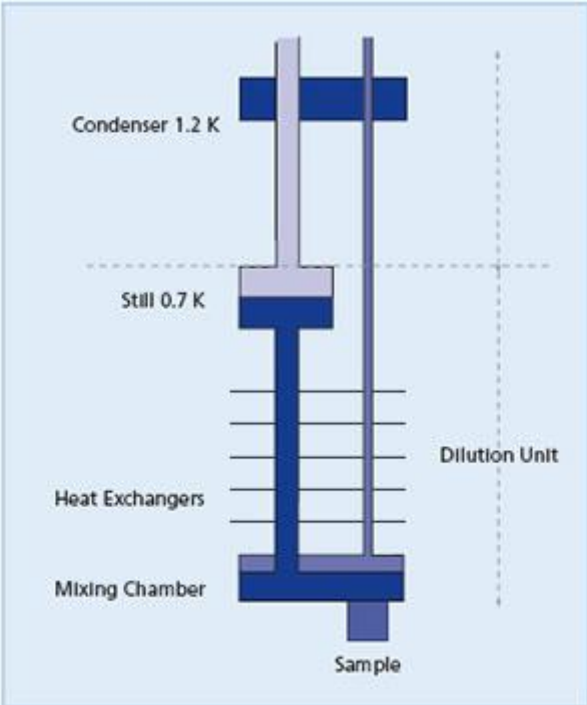


Figure 3 Flow Diagram of the Helium Dilution Refrigerator.

In a similar manner to a domestic refrigerator, the helium dilution refrigerator also operates with a closed cycle. The phase separation and cooling take place in the **mixing chamber**, which is the lowest temperature of the refrigerator. Experiments are carried out by attaching samples to it. On leaving the mixing chamber the helium-3 in the dilute phase flows towards the **still** passing through a series of **heat exchangers**. The still is maintained at a temperature of about 0.6K and it is here that the helium-3 is re-concentrated. At this temperature the vapour pressure of helium-3 is several orders of magnitude greater than helium-4 and when the liquid surface of the still is pumped the helium-3 is vaporised preferentially.

Almost pure helium-3 gas is circulated around the refrigerator at room temperature using a closed vacuum system. It is then re-condensed by allowing it to pass through a separate **helium-4 condenser bath**, which is maintained at a temperature of around 1.2K using a separate pumping system. A **flow impedance**, in the form of a fine capillary tube, is used to maintain a sufficiently high pressure in the region of the 1.2K condenser bath so as to enable the helium-3 vapour to condense.
Finally, the almost pure liquid helium-3 is cooled by the heat exchangers and then flows into the top of the mixing chamber to complete the cycle. Under ideal conditions, only the helium-3 is circulated with the helium-4 providing a background matrix.

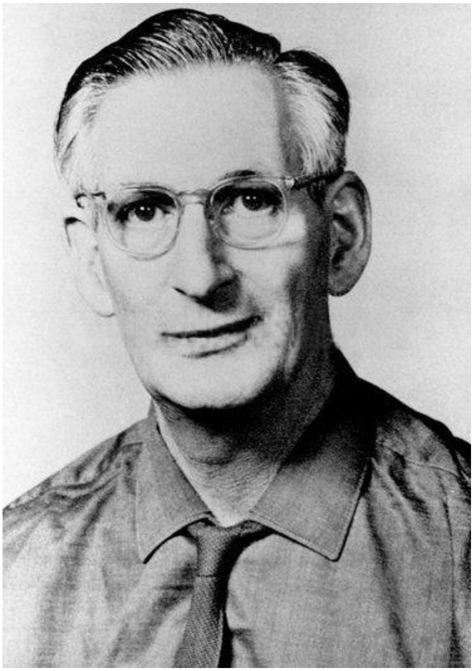


Figure 4 Heinz London.

The Helium Dilution Refrigerator is generally regarded as the brainchild of Heinz London (8). He was born in the city of Bonn into a prosperous, liberal German-Jewish family. His father was a Professor of Mathematics at the University of Bonn, who died of a heart condition when Heinz was nine years old. He studied for a doctorate in Franz (later Sir Francis) Simon's research group in low temperature physics at the University of Breslau. In 1933, with the rise of the German Nazi Party under Hitler, Simon was forced to leave Germany. He moved, with most of his group, to the Clarendon Laboratory in Oxford at the invitation of and with assistance from Frederick Lindemann, who was Professor of Experimental Philosophy at the University of Oxford and Director of the Clarendon Laboratory. Lindemann subsequently became Viscount Cherwell and during the Second World War was the Chief

Scientific Advisor to Winston Churchill. Simon also brought to Oxford from Breslau his nephew Kurt Mendelssohn and Nicolas Kurti. He was joined a year later by Heinz London who then lived close to his brother Fritz who was also carrying out research in Oxford. Simon and his group established a powerful research group in low temperature physics at the Clarendon Laboratory, a situation which continues to this day.

During the War, Heinz London was involved in the isotopic separation of uranium-235 from uranium-238, which was required for the development of the atomic bomb. This was one of the most difficult isotopic separations ever attempted. In view of this, it is perhaps ironic that the separation of a mixture of helium-3 and helium-4 necessary for the operation of a dilution refrigerator should occur spontaneously below a temperature of 0.86K. Both Fritz and Heinz London were experts on electromagnetism and thermodynamics and among other things they jointly carried out pioneering work on the theory of superconductivity in the 1930s. Heinz London is alleged to have said that he was "prepared to die" for the second law of thermodynamics.

As early as 1951, at a Conference in Oxford, Heinz London pointed out that at very low temperatures, when helium-4 was superfluid, a mixture of few percent of helium-3 in helium-4 could be thought of as a "gas" of helium-3. If this "gas" was further diluted by adding more superfluid helium-4 it would be analogous to the adiabatic expansion of a gas and a cooling effect should take place. This concept resonates way back to 1877 when the Frenchman Louis Cailletet first liquefied nitrogen and oxygen by the adiabatic expansion of the gas (2,3).

Heinz London had been working at Harwell since 1945 and in 1962, following on from the experiments carried out by London, Clarke and Mendoza, Eric Mendoza was given a contract by Harwell to develop a Helium Dilution Refrigerator at the University of Manchester, where Eric was a senior lecturer at the time. It was this machine (shown in fig. 5) that I began working on in September 1963 as a PhD student working mainly alongside Dr Dafydd Phillips who came to Manchester having spent the previous year at the National Research Council at Ottawa in Canada.

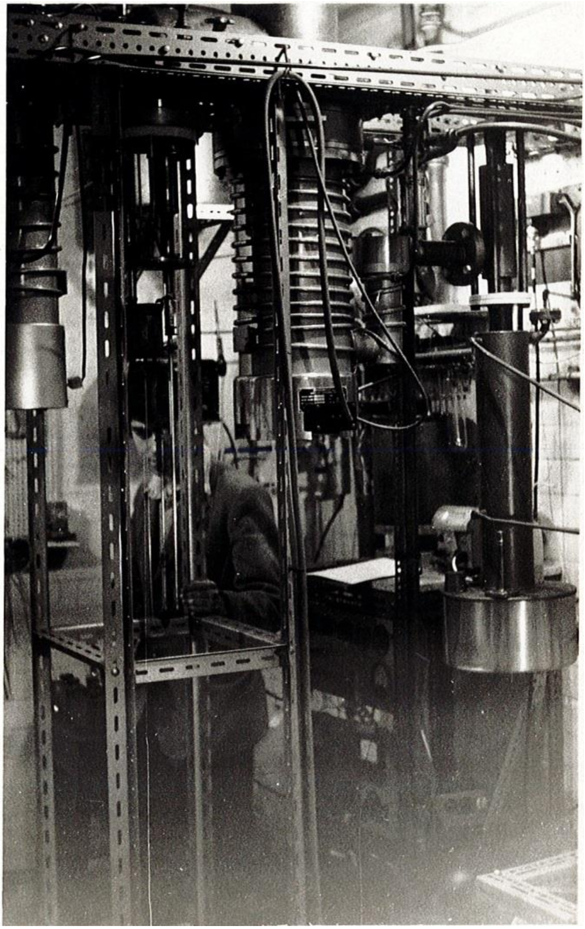


Figure 5 A general view of the apparatus showing on the right the pump for the helium-3 connected to a powerful diffusion ejector pump for circulating the helium-3. At the extreme left at the top is the diffusion pump for pumping the condenser to 1.2K Peering through the Dexion framework is a youthful me.

Perhaps the best summing up of this time Dafydd and I, together with Eric Mendoza, spent working on the machine are the words that Eric wrote for the Royal Society Biographical Memoir for Heinz London (8) written in

1971 by David Shoenberg, whom I once recall coming to Manchester University and being shown the apparatus by Eric
"The experiments occupied the best part of three years and were disastrous from the start. We know now that it could never have worked because of convection instabilities at the bottom, but in fact we never got that far. The main difficulty was simply that it had been badly constructed, the brazing of the stainless steel was bad and our choice of big mercury pumps was ill advised. No sooner did we detect one leak than another opened up. This was all very disheartening, particularly as we had no mass spectrometer leak detector to begin with, so that leak detection was terribly slow; it was only after two years that I managed to find money to buy a superannuated model from the Linac group at Manchester – but by that time it was too late. We wasted a lot of time on an elaborate gas-handling set up, being obsessed by losing any helium-3. We also constructed a needlessly complicated temperature measuring cell down in the mixing chamber, for Heinz insisted that there must be no argument about the temperature we attained, and that only a paramagnetic salt with very elaborately designed coils was good enough as a thermometer. While all this was in progress, we heard that the Leiden group under Taconis was also constructing a refrigerator". (8)

I would concur with everything that Eric Mendoza wrote. The apparatus in Manchester was certainly a leviathan. We worked in the basement of the Bragg Building in Manchester University located in Coupland Street. This was before the present day strictures on health and safety but even in those days some concerns were expressed by the Technical Manager of the Physical Laboratory as to the suitability of the room for this project. A hand operated lift was installed to carry the 25 litre helium dewars from the ground floor down to the basement.

As Eric wrote, we certainly experienced huge problems with leaks both in the cryostat and the gas handling apparatus, which led Eric to once recite the jingle *"A leak a week is all we seek".* At times this was reality if not rather optimistic. To measure the flow rate we used a rather elaborate Kronberger Flow Meter (9), which was essentially a Wheatstone Bridge arrangement where one arm consisted of the helium-3 gas flowing through, which would become unbalanced if the flow rate increased or decreased. At the time Hans Kronberger was head of the Daresbury Laboratory outside Liverpool and I remember Eric and me once visiting him in his palatial office. I recall that we had two massive, loud explosions of the glass dewars, which had been stored horizontally. I now realise that this is the worst possible thing to do, since enormous stresses and strains were set up within them. The dewars were big, roughly five feet long and a foot in diameter, and people from neighbouring laboratories came rushing in to see whether we were still alive and alright. We were fine apart from being severely shaken and there were shards of glass everywhere. In those days we did not wear safety goggles. In addition, one of my roles was to support the large vacuum can surrounding the dilution refrigerator assembly while Dafydd stood on a chair and soldered it into position firstly using soft solder and later, through Dafydd's insistence, with Woods metal. This could also be rather hazardous for me when bits of solder dropped down onto my head and hands. In the eighteen months that I was working on the apparatus with Dafydd Phillips we never reached the stage of attempting to circulate helium-3.

In the early 1960s few Universities had any sort of higher degree based on course work. In my first year at Manchester University we all had to do the rather exotically named "Diploma of Advanced Studies in Science". This involved extra lectures in Quantum Mechanics, Solid State Physics and Mathematics as well as writing a short report on your research project and one sat two written examinations and a viva on the project. I remember receiving lectures from Sam Edwards both on Quantum Mechanics and Solid State Physics. He later became Sir Samuel and head of the EPSRC. I used to see him in later years at various functions, mainly those run by the Institute of Physics, and he was always extremely friendly and we talked about the "good old days". Many of the lectures were held in the very fine lecture theatre on the first floor of the Schuster Building in Coupland Street, which was dismantled after the Physics Department moved to its present site on the other side of Oxford Road in 1967 much to the dismay and annoyance of several people, including myself, who had attended lectures there. Outside the lecture theatre was the bust to Sir Arthur Schuster, currently on the ground floor in the present Schuster Building, and I remember that one student always deposited his motorcycle crash helmet on it before attending lectures.

The Diploma of Advanced Studies in Science could be awarded with Distinction. Most of us, including myself, obtained the Diploma with Distinction so perhaps this was not that elitist after all.

I liked Manchester as a City which I thought was vibrant and dynamic. In 1963, post War austerity was coming to an end. However, the clean air act had not come into force and all public buildings in Manchester including the University were jet black due to years of soot. In the time that I was there, between the years 1963-65, I witnessed the rise and rise of Manchester

United and saw playing at Old Trafford such legendary footballers as George Best, Bobby Charlton and Dennis Law. There was also the Free Trade Hall and the Halle Orchestra with their famous conductor Sir John Barbarolli. At a more primitive level I saw stock car racing, speedway and professional wrestling at Bellevue Manchester.

For the first three weeks of my stay in Manchester, I lived in the Hulme Hall of Residence in Rusholme and in the evenings I used to study in the rather fine library that they had there. It was only recently that I discovered that some fifty years earlier Niels Bohr had worked in the same library while developing his atomic theory. I moved into the brand new Moberley Tower Hall of Residence built above Burlington Street in the centre of the University, which was designed as a post-graduate hall of residence. There was an excellent atmosphere among the students at that time and a few of them I still know. It was usefully situated if I was working late in the laboratory or had to go in late at night to top up a dewar with liquid nitrogen or check on the apparatus. I believe that I lived on the twelfth floor and this gave a panoramic view over the Manchester area and on a clear day I could see the Jodrell Bank Radio Telescope in the far distance in Cheshire. Moberley Tower I felt was not an attractive building to look at and was demolished some five years ago as student requirements changed.

In 1964, Eric Mendoza was appointed Chair of Physics at Bangor which was part of the University of Wales. I remember going to Bangor with him to visit it and thought that it was a beautiful place. He was also writing his classic book with Brian Flowers on *Properties of Matter* and I recall several times when Brian Flowers came to see Eric to discuss the book. I believe that the book is still in print and widely read. At the time Brian Flowers, later Sir Brian, was the Langworthy Professor of Physics and later he also headed up the EPSRC. In addition, he became Rector of Imperial College, London and ended up as Lord Flowers of Queen's Gate after the road where his residence was situated as Rector of Imperial College.

Early in 1965 work on the original Harwell Dilution Refrigerator was abandoned at Manchester and Eric departed to take up his Chair of Physics at Bangor. In addition, Dafydd Phillips left and joined The Oxford Instrument Company Ltd. While there he worked on dilution refrigerators. The United Kingdom Atomic Energy Authority (U.K.A.E.A.) held patents covering the principles of the dilution refrigerator and refrigerators of this type were manufactured under licence by the Oxford Instruments Company Ltd. Dafydd also worked closely with Heinz London who maintained an active interest in the problems associated with the refrigerator right up to his death in 1970. Heinz London acted as a consultant to the Oxford Instrument Company Ltd and together with Dafydd he developed an osmotic pressure gauge to measure absolute temperature in the milli-degree region. Heinz London died just as this development was coming to fruition. .

During the time that I was at Manchester University, Henry Hall had shown interest in the dilution refrigerator and after the original Harwell Dilution Refrigerator had been abandoned he decided to try and build one by adding an attachment to an existing helium-3 cryostat. This was also a massive piece of equipment which had been developed over many years and had the undoubted advantage of being in excellent working order. It was also very much in line with the ideas of Heinz London when he was drawing up a patent specification that the dilution refrigerator would be an additional stage to a helium-3 cryostat. I worked with Henry and also his excellent research student Keith Thompson, who was a Manchester Physics graduate, from about March 1965 to the end of July of that year, when I left to embark on a D.Phil at the University of Sussex.

I was very impressed by the way that Henry Hall and Keith Thompson, admittedly with my help, succeeded in getting a helium dilution refrigerator working successfully in a remarkably short space of time. We did, however, experience several problems the solving of which were important in the development of the dilution refrigerator. One of the more subtle of these was a convective instability which occurred in the dilute region between the mixing chamber and the still. This was mentioned in Eric Mendoza's account of the first dilution refrigerator given in the Royal Society Biography of Heinz London. The work of London, Clarke and Mendoza had shown that in a dilute mixture of helium-3 in helium-4, the helium-3 would flow at constant osmotic pressure, which meant that the product of the concentration of helium-3 (X3) and the absolute temperature (T) was constant:

$$X3T\text{=constant.}$$

If the concentration of the helium-3 was 6% at the mixing chamber at a temperature of 0.1K, then the constancy of the osmotic pressure meant that at the still, which was maintained at a temperature of about 0.6K, there would be a concentration of helium-3 of only 1%. The still is always placed above the mixing chamber and since helium-3 is lighter than helium-4, the density of the mixture at the still is greater than that at the mixing chamber. This can give rise to a convective instability in which the mixing chamber would initially begin to cool down and then the temperature would suddenly rise in a catastrophic manner due to this convective instability. This possible effect was first pointed out to us by Heinz London who also worked out the criteria which were required to minimise it. The tendency for convective instability was dampened by the viscosity and diffusion of the helium-3 and also by having a long path connecting the mixing chamber with the still as well as a small diameter of the connecting tube of around 1mm. Following London's suggestion, we were able to eliminate the effects of convective instability, by modifying the configuration of the refrigerator. A communication at that time from Taconis of the Leiden Group in Holland suggested to us that the reason why their dilution refrigerator, which they discussed at the 9th International Low Temperature Physics Conference held in Columbus Ohio (10), failed to attain a temperature below 0.2K was also due to the effects of convective instability.

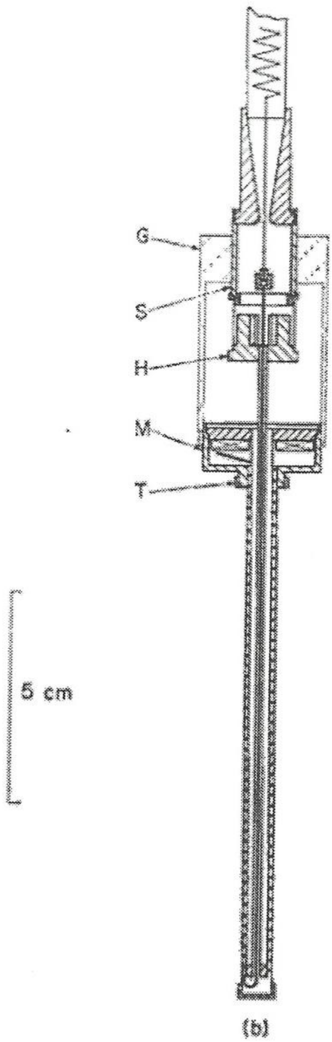
The second major problem was due to the Kapitza boundary effect. Although the thermal conductivity of superfluid helium is enormously high, problems can arise when one wishes to transfer heat into or out of a solid body into the helium. This gives rise to the Kapitza boundary effect and is a temperature discontinuity at the surface of a solid when heat flows from the solid into the liquid. It becomes important for temperatures below about 0.6K and is crucial in the design of the heat exchangers. Much of the subsequent rapid improvement to the lowest temperature obtained and improved performance of the dilution refrigerator lay in developing much better heat exchangers. What is required is a large surface area of contact between the dilute helium-3 moving out from the mixing chamber towards the still and the incoming concentrated helium-3 about to enter the mixing chamber to complete the refrigerator cycle. In our first successful refrigerator the heat exchanger consisted of two concentric tubes.

The principle behind the dilution refrigerator requires that the superfluid helium-4 is essentially a static medium through which passes helium-3. If some helium-4 is also circulated, this will result in the degrading of the performance of the dilution refrigerator. A problem did occur at the still which was maintained at a temperature of 0.6K through a heater. At this temperature the vapour pressure of helium-3 is several orders of magnitude greater than helium-4 so that almost pure helium-3 is pumped off to be recycled. However, initially there was a tendency for the heater to boil off the superfluid helium-4 film around the walls of the still. Again this was due to the Kapitza boundary effect whereby the heat from the still heater was not entering the region around the surface of the dilute liquid mixture.

This problem was overcome by placing a short section of stainless steel between the heater and the still and introducing some copper posts up to the surface of the still where the re-concentration of the helium-3 was taking place.

The last problem to overcome was that the cooling in the dilution refrigerator took place at the interface between the concentrated and dilute regions of helium-3 within the mixing chamber. For practical purposes this cooling has to be extracted from the mixing chamber to the experiment attached to the mixing chamber. Again this was hindered by the Kapitza boundary effect. In order to reduce this, the mixing chamber, which was made from copper, had fine grooves inserted in it to enhance the surface area of contact between the mixing chamber and the experiment.

Figure 6, which is taken from Figure 2b of Reference 12, shows the final successful design of the helium dilution refrigerator, which took into account and overcame the problems associated with the refrigerator which I have referred to. This dilution refrigerator reached a lowest temperature of 65 milliK (0.065K) as measured by a cerium magnesium nitrate paramagnetic salt and was first achieved around June 1965.



(b)

Figure 6 The final version of the dilution refrigerator which first reached a temperature of 0.065K. S: Still; H: Still Heater; G: Graphite Precooling Link and Mechanical Support; M: Mixing Chamber; T: Screw thread for attachment of the load. (After Figure 2b of reference 12)

I wrote up a Masters Thesis on the dilution refrigerators and I was awarded an MSc from the University of Manchester in December 1965. The external examiner was Heinz London and in those days at Manchester University there was not a viva for an MSc degree by thesis. I transferred to the newly founded University of Sussex, which had established a flourishing low temperature group under Professor Douglas Brewer who had come from the Clarendon Laboratory in Oxford. Here I worked on an aspect of dilute magnetic alloys, The Kondo Effect, which involved making resistance measurements on alloys such as parts per million of iron in gold down to temperatures of about 0.5K using a conventional helium-3 cryostat. I also recall that this cryostat gave lots of problems for several months before we were able to resolve them. I finally received a D.Phil from Sussex University in the middle of 1969 and several publications in the Physical Review and other journals stemmed from this work. During the 1960s and 70s the Kondo effect was an important area of condensed matter physics and studied extensively both experimentally and theoretically.

Shortly after I left Manchester, Henry Hall presented a preliminary account of the dilution refrigerator at the St Andrews Symposium on Superfluid Helium in August 1965 (11). A more detailed publication appeared in the journal Cryogenics in April 1966, which was co-authored by Henry, Keith Thompson and me (12). A Helium-3 - Helium-4 Dilution Refrigerator, developed by the Oxford Instrument Company Ltd, was featured at the Institute of Physics Annual Exhibition in 1966, which was held at Alexandra Palace in North London.

I returned to Manchester five months later in December 1965, to receive my MSc degree, and again the following month to attend the Institute of Physics Solid State Physics Conference. It was at this Conference that I heard a lecture given by Professor John Wheatley of Urbana, Illinois, who was the winner of the Simon Memorial Prize. During the course of that lecture I was staggered at the progress that he and his group had made in developing the dilution refrigerator in the intervening few months. One of the most memorable moments for me occurred just before the lecture when a small entourage entered the lecture theatre and sat in the front row. In the middle of it was a little old lady who was Lady Simon. Just over twenty years later I heard a lecture at Imperial College, London in December 1986 which was given by the Russian Physicist Yuri Sharvin, who was the winner of the Simon Memorial Prize. Again, just before the lecture started a small entourage entered the lecture theatre and sat in the front row.

In the middle of it sat a little old lady who again was Lady Simon. I believe that she lived to be 104 years old.

During the second half of the twentieth century, I had a tenuous association with the Simon Memorial Prize. Heinz London was the winner of the first Prize in 1959 and Henry Hall and Joe Vinen won it in 1963, for their work on second sound in uniformly rotating superfluid helium. I heard the 1968 Simon Memorial Lecture at the University of Sussex given by Kurt Mendelssohn from the University of Oxford. Mendelssohn was the first person to liquefy helium in England in 1933. He also wrote the book *The Quest for Absolute Zero*, which I have always felt was one of the most readable and interesting books on physics and influenced me to study the subject. The second edition has an account of the dilution refrigerator and is also dedicated to Heinz London. In 1973, I spent nearly a year at the University of Paris at Orsay in a rather abortive attempt to reconnect with the Helium Dilution Refrigerator working in the Group of Eric Varoquaux. In December 1992 I attended the Institute of Physics Condensed Matter Physics Conference at the University of Sheffield and heard the Simon Memorial Prize Lecture given by Olivier Avenal and Eric Varoquaux. Finally, in 1998, I attended the Simon Memorial Prize Lecture given by George Pickett and Anthony Guenault of the University of Lancaster: “In recognition of their outstanding contributions to the field of low temperature physics”. This was held at the Manchester Institute of Science and Technology (UMIST) before it amalgamated with the University of Manchester a few years later.

About five years ago I was attending a meeting at Manchester University and made a lonely pilgrimage to see the Bragg Building in Coupland Street and have a look at the old laboratories. The Bragg building had been opened in 1931 by Lord Rutherford and named after Sir Lawrence Bragg, who at that time was the Langworthy Professor of Physics at Manchester University having succeeded Lord Rutherford when the latter moved to become head of the Cavendish Laboratory in Cambridge. The building, of course, was still there but instead it had been renamed the Martin Harris Building for Drama and Music. The inside of the building had been totally transformed and while wandering around I was accosted by a secretary whom I told that some forty five years ago where we were standing was a huge piece of equipment which had produced a temperature less than one tenth of a degree above the absolute zero of temperature using a novel technique which had been pioneered in Manchester. I do not think that she was in the least bit impressed.

I recall almost immediately afterwards going along a corridor and opening a fire safety door, something which I am sure never existed in the 1960s, and it was like entering a time warp since there was an open space leading to the stairs down to the basement, which I had used on numerous occasions and it all looked exactly as it was all those years previously. The room in which I worked had been converted into a battery storage room and the rest of the area, which had been full of offices and laboratories, was now the music library.

By chance I happen to be a good friend of Sir Lawrence Bragg's elder daughter, Lady Margaret Heath, who also lives in Bath. Last year we wrote a letter to the office of the President and Vice-Chancellor of the University of Manchester, Professor Dame Nancy Rothwell, suggesting that a University Blue Plaque should be erected on the Martin Harris building commemorating Sir Lawrence. At the start of this year we received a letter from the University Historian and Heritage Manager, Dr James Hopkins, saying that the suggestion had been enthusiastically approved and would be fast tracked. The unveiling is due to take place on Thursday December 10th which should have been exactly 100 years to the day since Sir Lawrence and his father, Sir William Bragg, were jointly awarded the 1915 Nobel Prize for Physics for their work on X-Ray diffraction. In reality the award ceremony for the Nobel Prize was delayed because of the First World War. Sir Lawrence finally gave his Nobel Lecture in Stockholm on September 6th 1922.

That concludes my account of the first dilution refrigerator at the University of Manchester. In the intervening years I did see some of the principal characters involved. Eric Mendoza was a practicing Jew and Manchester has a very large Jewish community. He spent about ten years at the University of Bangor before moving to Jerusalem. In 1977 I attended a physics conference in Haifa, Israel and afterwards spent a few days in Jerusalem. I arranged to see Eric again who was living in the Jewish Quarter of the city. It was a very amicable re-meeting and I felt that he was genuinely pleased to see me. He died several years ago at around ninety years of age. I met Dafydd Phillips on a few occasions. Once, rather surprisingly, I bumped into him at the Ruhr University in Bochum, Germany. The last time we met was at a meeting in Cambridge in 1994 to mark the hundredth anniversary of the birth of the eminent Russian physicist Peter Kapitza. I saw Henry Hall at various meetings in England when as always he was very friendly. The last time was at the British Association Meeting at the University of East Anglia in 2006 when I heard his cheery, fog-horn like voice say "Hello Peter" and waving to me from about fifty yards away.

There are two other people whom I have not mentioned so far and to whom I would like to pay tribute. The first is Gill West, who was an extremely skilled technician working within the low temperature physics group at Manchester University. He was able to make the delicate and intricate parts of the dilution refrigerator that Henry Hall requested to the required precision. Without Gill's ability we might well not have succeeded. The second is Hugh Montgomery, who at that time in the 1960s was working at Harwell in the group of Heinz London. He sometimes came to visit us at Manchester to discuss the progress on the original refrigerator, as did Heinz London on one occasion, and his advice and suggestions about the original dilution refrigerator was always sound and prescient. I met him again on a few occasions in the 1990s after I finally returned to England and first became involved in the History of Physics Group of the Institute of Physics.

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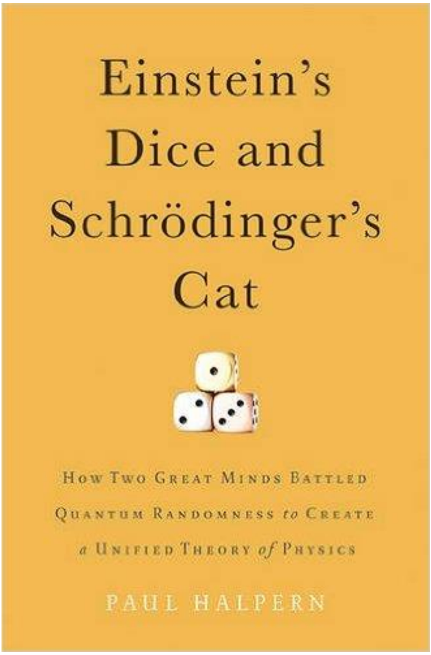


Einstein's Dice and Schrödinger's Cat

Paul Halpern

Basic Books (New York) 2015
ISBN 978-0465075713
288pp £20

Reviewed by:
Cormac O'Raifeartaigh - Waterford Institute of Technology

In January 1947, Erwin Schrödinger, Nobel laureate and Senior Professor of Physics at the Dublin Institute for Advanced Studies, announced at a seminar at the Royal Irish Academy that he had made an important breakthrough in unified field theory, a fearsome problem in modern physics that had challenged Einstein for many years. The seminar was attended by the great and the good of Irish academia, including the prime minister Eamon de Valera, who had persuaded the Austrian-born Schrödinger to take up a position at the Dublin Institute a decade before. Schrödinger's announcement was breathlessly reported in Irish media outlets such as *The Irish Press* and *The Irish Times* the next day, under headlines such as 'Scientist at Irish Institute succeeds where Einstein failed'.

The story was quickly picked by the international media, and Einstein was pressed by the *New York Times* to respond. Respond he did, pointing out in a rather brusque press release that Schrödinger's 'breakthrough' was merely a reformulation of ideas that had already been proposed, and scolding the press for inappropriate hype. Einstein's response generated further press coverage, not least from the *Irish Times* satirist Brian O'Nolan, who sardonically asked "What does Einstein know of the meaning of words? Very little, I should say". Meanwhile, Schrödinger accepted Einstein's criticism, but the incident led to a temporary cooling of relations between the two great scientists and erstwhile colleagues.

This interesting media contretemps between Einstein and Schrödinger is the central scene of the book **'Einstein's Dice and Schrödinger's Cat',** by the American physicist and science writer Paul Halpern. Intrigued by his discovery of a box of press clippings describing the incident at the Albert Einstein Archive at Princeton, the author reconsiders the dispute between the two giants of 20^{th} century physics, setting it in the context of their lengthy collaboration in matters of science.

Indeed, the title of Halpern's book refers directly to Einstein and Schrödinger's allied stance against the emerging orthodox view of the new quantum physics. While each played a seminal role in the discovery of the strange behaviour of nature on the quantum scale, each distrusted the orthodox or 'Copenhagen' interpretation of quantum theory that emerged in the late 1920s. In Einstein's case, his "God does not play dice" mantra neatly summarized his rejection of the inherent randomness of nature implied by the Copenhagen interpretation. As for Schrödinger, a famous thought experiment involving a cat in a box highlighted difficulties with the consensus view that a quantum entity only acquires a well-defined energy state on observation.

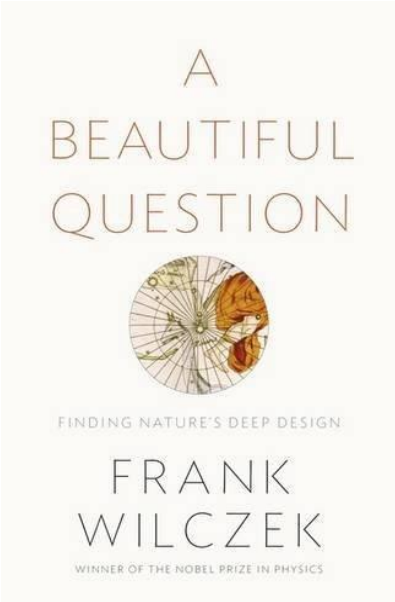
The reader is thus brought on a brief tour of quantum physics, relativity and the search for a unified field theory, enmeshed throughout with potted biographies of Einstein and Schrödinger. Many aspects of this story have been told elsewhere, but Halpern's account includes an unusual and interesting emphasis on the interaction between the two great scientists, from their friendship as colleagues in Berlin during the golden years of the Prussian Academy to their travails due to the rise of the Nazis, from their exile at the Institutes for Advanced Study in Princeton and Dublin respectively to their gradual isolation from the physics community due to their stance on quantum physics.

A most unusual aspect of the book is the use of a substantial correspondence between Einstein and Schrödinger ranging over several decades, providing many illuminating insights into their approach to the philosophy of physics. Much of this material is new, even to historians of science, as it was translated by the author from handwritten letters on the Albert Einstein Archive that are not widely available.

Halpern also does a thorough job on the science, although it is not a light read for readers unfamiliar with fundamental concepts of quantum physics and general relativity. Indeed, it could be argued that the level of detail somewhat masks an important theme of the book, the great (and mistaken) excitement felt by Einstein and Schrödinger in turn as they mistook ever more sophisticated formulations of general relativity as important milestones in the quest for a theory of everything.

One puzzling aspect of the book is a slight difference in narrative style between the description of the central scene – the press spat between Einstein and Schrödinger - and the careful historical approach of the rest of the book. The author's account of the build-up to the dispute seems rather speculative, peppered with unsupported statements such as " Schrödinger was a brilliant man but not a particularly brave one....he yearned to be admired....and faced immense pressure to justify his salary". In addition, the description of the dispute itself relies heavily on contemporaneous newspaper reports, leaving the reader to wonder whether the incident was something of a media storm in a teacup. For example, it is known that Einstein wrote directly to Schrödinger soon after the latter's ill-fated seminar at the Royal Irish Academy, outlining his view of the 'breakthrough'. Meanwhile, Schrödinger wrote to Einstein, apologizing for his hyperbole and the resultant press coverage. Such communication between the main actors hardly constitutes "a media war that tore apart their decades-long friendship'', as stated in the opening line of the book. It's also worth noting that Einstein himself erroneously announced a 'solution' to the problem of unified field theory on several occasions over the years, so it is unlikely that he bore any lasting grudge against Schrödinger for similar hubris. Indeed, the two resumed their correspondence on matters of physics in the years after the incident.

All in all, a well-researched tale of an intriguing kerfuffle between two of the greatest scientists of the 20th century. The story will be a compelling read for anyone with an interest in theoretical physics or in the interaction of scientists with the media.



A Beautiful Question:
Finding Nature's Deep Design

Frank Wilczek

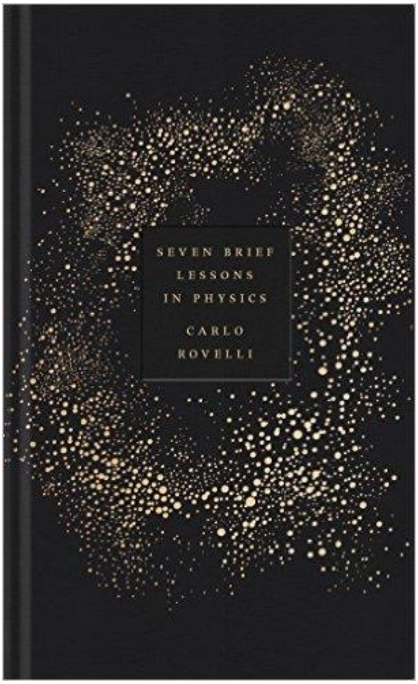
Penguin/Random House 2015
ISBN 978-846-14701-2
448pp Hardback £ 25

Reviewed by:
Derry W Jones, University of Bradford

Frank Wilczek, a theoretical physicist awarded the Nobel prize for earlier work, has composed a meditation, as he calls it, that embraces long-term history, recent advances in quantum physics and even the nature of the universe. If the book's 430 pages looks overpowering, despite its innocent title, the main text is a mere 331 pages and most subsections of 2-3 pages have well-spaced sub-headings. Within the text are 40 diagrams – photographs, graphs and drawings – but a greater inducement to the reader is a set of about 50 superbly reproduced colour plates. These illustrate the breadth of the author's outlook, which extends to philosophy; plates encompass classical and other artistic paintings by Fra Angelico, Claude Monet and Salvador Dali as well as drawings specifically for the book, some by the author. These include diagrams explaining Pythagoras's theorem or the geometry of perspective and the mixing of colours red and green or blue and yellow. *A Beautiful Question* contains no equations, apart from e=mc2 and yx=-xy, and even Maxwell's equations for electricity, magnetism and light are represented in pictures [plate N].

Wilczek's approach to quantum theory follows a broadly historical sequence beginning with allusions to the ideas of Pythagoras, Plato and Aristotle. It is developed in successive chapters via the notions of Copernicus, Leonardo da Vinci, Galileo and Newton and on to Faraday, Maxwell (his favourite physicist) and Einstein. Special note is made of the mathematical physicist Emmy Noether (1882-1935) who, as a Jew, had to flee from Nazi rule. She linked mathematical symmetry with unchanging physical quantities: time translational symmetry. Before his 25 years at the Mint, Newton worked furiously at Cambridge for 25 years (terminated by bubonic plague, 1665-6). He envisaged gravitation on the Earth and moon, leading to tides, precession of the Earth and the motion of comets. Wilczek calls Newton's prism analysis of light and its reversal the chemistry of light. Analogies with light and the harmonics of sound (even Babbage touched on this musical connection) are formulated. Despite the familiar names and Wilczek's clear scholarly treatment, this is a challenging book converging to contemporary theoretical physics and perhaps in a direction favouring his answer to the title. Much could be read by those uninitiated in physics. Symmetry, supersymmetry and Quantum Chromodynamics (QCD) [or 'quantum electrodynamics on steroids'] are emphasized before Wilczek returns to symmetry and simplicity. There are four fundamental forces: gravity, electromagnetism, strong forces and weak forces (which transform protons into neutrons, as on the Sun). Ultimately Wilczek gets to the problems of nuclear physics including the separation of hadrons (or fermions) into baryons and mesons (bound states of quark and antiquark) and the Higgs particle and field. He holds that computer power has introduced a new kind of physics, additional to theoretical and experimental, namely numerical experimental or 'solving hard equations'. In the approach to contemporary Core Theory physics (his term for the Standard Model), one becomes aware of a brighter brain than that of most of us.

At the beginning, Wilczek asks: Does the world embody beautiful ideas? Is the world a work of art? Early spiritually-based searchers found beauty inherent in the physical world. Is beauty primarily symmetry and economy of means? Does symmetry really include the arts? The sense of beauty needs to be broadened to recognize that the equations from the Core Theory describing the physical world, or at least the world of science and engineering, are beautiful. If beauty is in the eye of the beholder, is the world-making Artisan constrained by a desire for beauty? Wilczek seems to answer his question by concluding that the physical world embodies beauty. But he also notes that the physical world is home to squalor, suffering and strife and that we should not forget these two aspects.



Seven Brief Lessons in Physics

Carlo Rovelli

Allen Lane *2014*
ISBN-13: *978-0241235966*
Hardback, *£9.99*

Reviewed by Peter Rowlands
University of Liverpool

Many of us became physicists because we had an insatiable desire to know how the world was structured and how things happened in it. We had years of rigorous training to master the experimental and theoretical techniques and to acquire the bank of knowledge needed to make the necessary connections. It was difficult for us then and it is still is. The knowledge available to us was accumulated slowly by the efforts of many thousands of individuals before us over several centuries. Recounting the struggle to acquire this knowledge is what makes the history of physics so fascinating.

History is incidental to Carlo Rovelli's seven brief lessons, but we are conscious of it as we read his account. We are also aware that the story remains unfinished; many things are still unexplained, and some of the things he describes are speculation rather than fact. We feel, however, that physics is so important to the whole human race that we should make every effort to communicate the most important results to as many people as possible, and to show how physics is a very different kind of process to what they may imagine, with imagination and conjecture playing very significant roles alongside rigorous experimental testing and mathematical theory.

Rovelli's seven lessons deal with general relativity; quantum mechanics; cosmology; fundamental particles; quantum gravity; probability, thermodynamics and black holes; and the role of human beings as both created by and creators of this world described by physics. To a large extent these are the ones we would expect – the established theories at the current frontiers of knowledge. The choice of the more speculative loop quantum gravity for the fifth lesson no doubt reflects the author's own theoretical interests, but it is important, in any case, for a physicist to make clear to readers 'who know little or nothing about modern science' (p. vii) that it isn't a progression from certainty to certainty, and that there are huge gaps in our knowledge and that ideas are always open to question.

To be successful at explaining such difficult ideas to a presumed lay audience requires a special skill, and an ability to create analogies between physical ideas and more familiar things. Only a completely lay person could tell you whether Rovelli is successful in this, but I think his descriptions are well done and appropriate for the context. Sometimes, attempting to explain aspects of science to non-scientists can lead scientists themselves to gain a better understanding of the meaning of their work. The chapter I most appreciated was the sixth, dealing with thermodynamics, where the thinking seems to reach a deeper level of profundity, going beyond simple popularisation.

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Forthcoming Meetings

A History of Units from 1791 to 2018

In anticipation of the redefinition of the kilogram in terms of Planck's Constant in 2018, this meeting will look back at the beginnings of the metric system, and at the evolution of metrology in mass, time, temperature and resistance measurement since that time.

National Physical Laboratory, Hampton Road, Teddington, Middlesex,

Contact details: Dr Jim Grozier Email: j.r.grozier@btinternet.com
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2nd International Conference on the History of Physics

I am pleased to report that a second International HoP Conference will be held in Pöllau, Steiermark, Austria on September 5th, 6th, and 7th of September 2016. It is being organised by the EPS and 'Echophysics'.

The very successful inaugural conference, held at Trinity College Cambridge, brought together professional historians of science, physicists, and others interested in various aspects of physics history, with the goal of promoting interdisciplinary exchanges and raising the profile of the subject to its rightful place in physics education and research.

The conference title '*From past endeavours to new insights'*, reflects the leading theme of the importance of history in the teaching, learning and pursuit of physics, with the underlying thought that a unifying central topic can provide a backdrop to wider discussions and exchange of ideas related to other areas and periods. Nevertheless, submissions on all aspects of physics history will be welcome.

Please note that another conference is being organised for the 7th, 8th and 9th of September at the same venue on science and literature.

Further details and website will be available soon - Editor

Don’t wait for the *next* newsletter – *search the archive!*

Over the last two years or so, we have been gradually building up an **on-line newsletter archive** on our website. If you have only recently joined the Group, you will have missed many excellent articles; or maybe you have been a member for some time, but have mislaid your old newsletters. Well, now you can read them on-line! At the time of this newsletter going to press, issues 12 (Spring 1999) onwards had been uploaded, with a couple of gaps; our intention is to eventually have all of them on-line.

As a taster, here are some of the more interesting articles you might like to peruse. However, all tastes are different, so if you don’t see anything you like here, don’t be put off - there’s plenty more!

My Early Years as a Physicist in Poland
Joseph Rotblat
No. 13 (Spring 2000)

The Pioneering of Magnetic Resonance Imaging in Aberdeen
John Mallard
No. 14 (Spring 2001)

60 Years of Medical Physics seen through the eyes of one who went through it
Sidney Osborn.
No. 17 (Winter 2004)

Newton’s missing experiment?
Vicente Aboites
No. 18 (Summer 2005)

The University of Aberdeen Natural Philosophy Collection of Historical Scientific Instruments
John Reid
No. 20 (July 2006)

400 Years of the Telescope
John Reid
No. 26 (August 2009) and No. 27 (March 2010)

Early Days In Particle Physics
D. F. Falla
No. 28 (October 2010)

To find the archive:
Go to our web page at:

http://www.iop.org/activity/groups/subject/hp/index.html;

click on "Newsletter" in the menu on the left, then click on "newsletter archive". You will then see the past issues listed in reverse date order. Click on any one of these to download it in PDF format. The contents page of each newsletter is bookmarked, so that you can go straight to the article you want.

We are also working on an index, to enable you to find interesting articles by subject. So:

Wanted!

Articles, Letters, Queries

- *long or short*

wanted for *your* Newsletter

Send to Malcolm Cooper, Editor

email: mcooper@physics.org

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