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Cover picture: Tide gauge for recording local tides. William Thomson?
Taken from 'Pioneers of Science' Oliver Lodge. Macmillan 1919

Contents

Editorial		2
Meeting Reports		
Group AGM – Chairman’s Report		4
Cambridge conference		5
Physicists & Mathematicians of Belfast		7
News		
Meeting: From Hooke to Helioseismology		8
Features		
Faraday’s ‘Blue Plaque’	by Michael Jewess	9
The Wheatstone Wave Machine	by Robert W. Whitworth	16
Book Reviews		
Is Water H₂O – Hasok Chang		17
The German Physical Society in the Third Reich		23
	Edited by Hoffmann and Walker	
Stop Press		
EPS Historic sites	by Colin Latimer	30
Committee and contacts		32

Disclaimer

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Editorial

We learn by the mistakes of others?

A much favoured tool to promote the history of physics is the anniversary - and a very useful one it is too. Meetings, talks, visits may be organised centring on 'so many years since the birth, death, discovery of....' which are undoubtedly effective in furthering the cause and judging by the activities of our group over many years, seem to be enjoyable and stimulating occasions. Blue Plaques create another way of promoting the genesis and developments in the field of physics - this time aimed more at the general public - sometimes necessarily condensing a lifetime's toil into a few pithy words. Again, another worthy pursuit - both of which I fully support. But is there a danger that it can become an end in itself, that it can become too introspective?

Moving into a much grander sphere of activity this theme may be expanded into the 'conference'. Having been closely involved in the recent conference in Cambridge (see page xx for a report) it was interesting to draw comparisons. I think it fair to say that it was a great success bringing together Historians, Physicists and those hybrid creatures physicists with a passionate commitment to the history of their discipline - the 'Phystorians' (if I may coin such a word!). And passion was much in evidence particularly at the informal breaks for coffee and meals. Passion and communication - vital ingredients for the food of physics. So there is little doubt that these gatherings are of enormous value, operating at many different levels but I wondered how the history of physics could be more externalised, to thrust or at least diffuse outwards.

One thing which springs to mind to bring the history of physics right to the heart of physics - rather than simply a fascinating but ancillary role in the physics community - is its use in the teaching (or learning, if you will) of physics. Much has been written on this subject including the early assertions of Mach and Duhem in its favour and a quick trawl of the internet reveals many arguments put forward.

It can, for example, give insights into the human nature of scientists, illuminating the perhaps unexpected spiritual, ethical and even moral dilemmas they might face. On a more ‘down to earth’ level it can be argued that exposure to its history presents an environment in which students can better appreciate some aspects of the nature of physics – e.g. that it may be tentative, approximate, of limited validity, even falsifiable or just plain wrong!

Of course there are those who argue that such exposure to false ideas can be damaging and in any case involve a good deal of time effort and cost but being aware of mistakes made – some by much revered players – can yield understanding of how physics (and indeed any of the natural sciences) is investigated. In the September issue of Physics World Cormac O’Raifeartaigh presents a fascinating article about Einstein’s (ultimately incorrect) attempt to construct a ‘steady state’ model of the universe but as O’Raifeartaigh says: *‘it is a fundamental tenet of historical research that unsuccessful ideas can be of great importance in understanding how theories develop’*. And surely the value of understanding how theories develop is not restricted to historians.

It has been said that there is a vast amount of literature on teaching physics through history but very little about real experiences so I should like to pose these questions to our members (and indeed non-members) – what is your experience of this? Does it work? Is it practical? Please send your comments to the email address below.

M Cooper
Editor

mcooper@physics.org

Meeting Reports

Chairman's Report

Two scientific meetings were held in 2014. The first, entitled '*Physicists and Mathematicians of Belfast*', took place at Queen's University, Belfast in June. It was organised by Andrew Whitaker, assisted by Mark McCartney. The one-day meeting included lectures on Joseph Larmor, James Thomson (elder brother of Lord Thomson) Ernest Walton, Dan Bradley, Thomas Andrews, PG Tait, David Bates and John Bell.

The second meeting was an '*International Conference on the History of Physics*', held in Cambridge in September. This was the most ambitious project undertaken by the Group in recent times and required over a year of planning. Grants from the Institute of Physics, the EPS History of Physics Group, the James Clerk Maxwell Foundation and several publishing companies enabled the registration fee to be set at a low level for the two-day event, which attracted over 130 participants. A report on the conference is included in this Newsletter.

An AGM was held on 23 October at the IOP headquarters. This meeting was followed by a presentation by Kate Meehan on the new IOP building to be built near Kings Cross, and a talk by Dr Andy Gregory '*How ubiquitous are cosmological questions (and answers)?*'

Several meetings are planned for 2015, including '*Women in Physics*', '*From Hooke to Helioseismology*', '*The Helium 3 Dilution Refrigerator*' and a joint meeting with the Nuclear Industry Group'. When available, further details will be posted on the Group's web pages:

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

Professor Edward A Davis
Chair, History of Physics Group

Report on International Conference on the History of Physics 4-5 September 2014



The Wren Library Trinity College – photo by Jehangir Cama

This conference, held at Trinity College Cambridge, brought together professional historians of science, physicists, science museum staff, 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 leading theme of the conference was *Electromagnetism: the Road to Power* and several of the invited and contributed talks were on this topic, for example Hasok Chang's lecture on *The early history of electricity from Volta onwards*, Halge Kragh's on *Ludvig Lorenz, electromagnetism, and the theory of telephone currents*, and Paolo Brenni on *The induction coil; the natural and social history of a physics instrument*. However, submissions on all aspects of physics history were invited and indeed the opening talk by John Heilbron, with the title *History of physics, and what it's good for* was a wide-ranging *tour de force* covering physics from the time of the Ancient Greeks.

A session chaired by Jim Bennett, *The science museum: a discussion of contemporary issues around the presentation of science and its history in museums, galleries and science centres*, included contributions from Ian Blatchford, Director of the London Science Museum, and Ken Arnold, Director of Wellcome Collection, which led to a lively debate. A video depicting the life and work of Henrietta Leavitt, narrated by Pangratios Papacosta, was also shown as part of the proceedings.

In addition to 23 invited or contributed oral presentations, 29 posters were displayed in Nevile's Court under the Wren library in Trinity College during an evening session, which preceded the conference dinner. During the conference, special exhibits were on show in the Wren library relating to Isaac Newton, James Clerk Maxwell, J.J. Thomson and Lord Rayleigh.

A book fair was held during one day. Exhibitors included Taylor & Francis, the Institute of Physics, The Eagle Bookshop (Bedford) and Heffers of Cambridge. Delegates were given discount vouchers for use in both Heffers and the Cambridge University Press bookshops.

Taylor & Francis, who publish *The 'Philosophical Magazine* (first published 1798) were giving away a reproduction of four classic papers from the journal to introduce their archive covering over 200 years of content. The reprints included ones from J.C. Maxwell of (1861), J.J.Thomson (1897) and E. Rutherford (1919). The Eagle Bookshop was selling second-hand books, including a fine copy of the two-volume 'History of the Theories of Ether and Electricity' which was relevant to the main topics of this conference, and an elegant reproduction of Hooke's 'Micrographia', a huge and beautiful expensive leather-bound volume.

The History of Physics Group of the Institute of Physics had a display of some of their previous Newsletters.

On the Saturday morning after the conference, delegates were invited to join a tour of the Cavendish Laboratory museum or to take an excursion to Woolsthorpe Manor, the birthplace of Sir Isaac Newton.

The Steering Committee of the Conference (Edward Davis, Malcolm Cooper, Denis Weaire, Peter Schuster (EPS) and Stephen Elliott (Trinity College)) was supported by an Advisory/Programme Committee comprising 27 persons from 10 countries. Claire Garland of the IOP played a big part in the arrangements and was on site during the conference to assist delegates and to make sure everything ran smoothly.

It was the intention of the organisers that this conference would be the first of an ongoing series and already there are plans for a second conference to be held in Austria in 2016.

Edward Davis and Kate Crennell

‘Physicists and Mathematicians of Belfast’ Queen’s University Belfast 25th June 2014

‘Across the years it may be said that physicists and mathematicians from Belfast have made substantial contributions to their disciplines, certainly at least commensurate with its size and status. Its present healthy state leads one to have confidence that this success will certainly continue.’

Eight talks were given at the event, which took place at Queen’s University Belfast. Talks on Joseph Larmor, James Thomson, David Bates and John Bell were given by local scientists. We were particularly pleased to welcome Brian Cathcart, who has written extensively about Ernest Walton to talk about him; Elizabeth Lewis, who has completed a thesis about Peter Tait at the University of St. Andrews, to share her knowledge; and Peter Ford, who has a deep interest in low temperature physics, to talk about Thomas Andrews.

It was especially good that Donal Bradley, son of Dan and himself an eminent physicist, was willing to come to Belfast to talk about his father. The attendance at the meeting was around fifty and the atmosphere was very appreciative.

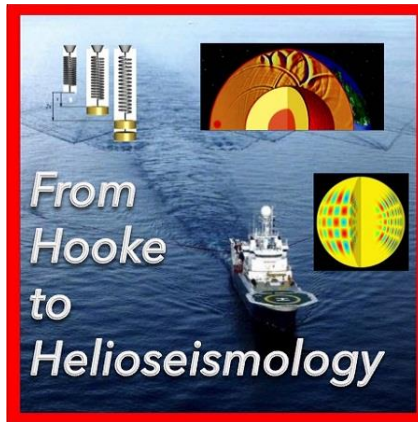
The organisers would like to thank the following organisations for financial support and other help: the History of Physics Group of the Institute of Physics, the British Society for the History of Mathematics, and Queen’s University Belfast.

Mark McCartney
Andrew Whitaker

A special issue devoted to this meeting is currently in preparation - to be published soon.

Editor

News



9th April 2015 – 10th April 2015

The two-day conference will be held at College Court, University of Leicester,

It will contain sessions on Earthquakes, Instruments & Institutions, Controlled-Source Seismology & Crustal Studies, and Passive Seismology. Invited speakers include Roger Musson, John McCloskey, Ian Main, Isabelle Ryder, Chris Browitt, Tim Owen, Alan Douglas, Paul Denton, David Bamford, Randy Keller, Anton Ziolkowski, Robert White, Richard Hobbs, Joanna Morgan, Patrick Bermingham, Mike Kendall, Stewart Fishwick, Catherine Rychert, James Wookey, Andrew Curtis, Michael Thompson and Walter Mooney.

The full programme may be seen at:

<http://www2.le.ac.uk/departments/geology/news/from-hooke-to-helioseismology>

For accommodation see: <https://collegetcourt.co.uk/>

Last Booking Date is 20th March 2015

Faraday's "blue" plaque – commemorating a remarkable master as well as a remarkable servant *

*Michael Jewess
Harwell, Oxfordshire*

“Memorial tablets” (the predecessors of “blue plaques”) were first proposed by William Ewart, MP in 1863. The proposal was taken up by the Society of Arts [1] [2], which erected 35 plaques between 1867 and 1901 [3]. (The scheme passed to the London County Council in 1901 and is now with English Heritage.) Of the 35 plaques, one erected in 1875-6 [4] can still be seen at N^o 48 Blandford Street in the Marylebone area of London. The plaque (Figure 1) states that Michael Faraday (1791-1867), Man of Science, was an apprentice there. Other plaques erected in 1875-6 along with Faraday’s, or earlier, included those honouring Samuel Johnson, David Garrick, Horatio Nelson, Benjamin Franklin, Sir Joshua Reynolds, and Napoleon III.

The plaque is a red-brown colour – not blue! – with white lettering. Close inspection of the “pattern border” reveals in its upper half the words “ERECTED BY THE”, and in its lower half the words “SOCIETY OF ARTS”.



Figure 1: the plaque on 3rd January 2013.

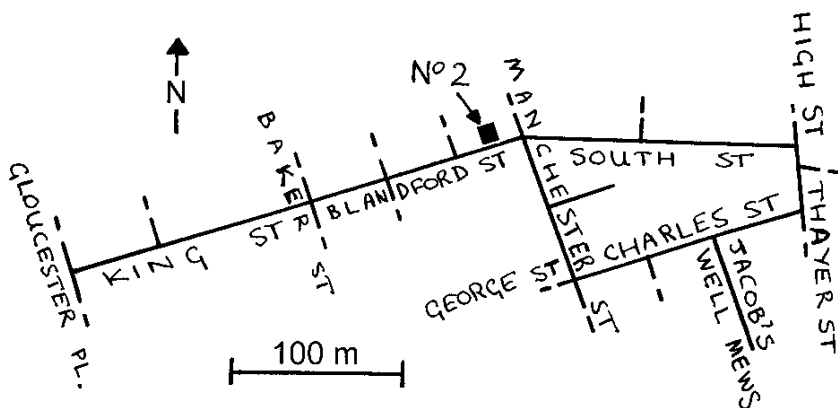
The story of how Faraday’s apprenticeship as a bookbinder in Blandford Street led to his being taken on by Sir Humphry Davy at the Royal Institution in 1813 is a romantic one [5]-[8].

In mid-1791, the Faraday family left Outhgill in Westmorland seeking better financial prospects in the London area. (Outhgill, OS square NY 78 01, now in Cumbria, still has a “Faraday Cottage”.)

* My thanks to the author for permission to re-print this article which first appeared in the Royal Society of Chemistry Historical Group Newsletter – Editor.

Initially, the family moved to rooms near the Elephant and Castle inn, Newington; the family then consisted of James Faraday, a blacksmith, his wife Margaret (pregnant with Michael), and children Elizabeth and Robert. Michael Faraday was born on 22 September 1791. By 1796, the family was living in Jacob's Well Mews in Marylebone. It was while there that Michael received rudimentary schooling, and from there, on 7 October 1805, that he was taken on as an apprentice bookbinder by George Riebau of N^o 2 Blandford Street, a few streets away (Figure 2). Despite the proximity of Riebau's shop to the parental home, the norm would have been for Faraday to take up residence with his master.

Figure 2: modern street plan with *former* street names according to ref. [9]. King St, Blandford St, and South St are now all called Blandford St, numbered east-west. Riebau's shop was N^o 2 Blandford St, OS reference TQ 281(6) 815(5) by GPS near the front door, and is now numbered 48. High St is now called *Marylebone* High St. George St and Charles St are now both called George St. The other street names are unchanged. Portman Square is three blocks south-south-east of King St.



Any lingering doubt that the modern N^o 48 was formerly Riebau's shop at N^o 2 is dispelled by the close resemblance between a contemporary drawing (Figure 3), and a modern photograph (Figure 4). The irregular spacing of the first-floor windows distinguishes the house from other houses nearby.



Figure 3: Riebau's bookshop [10]. "N° 2" appears above "RIEBAU", and "Blandford St" below.



Figure 4: N° 48 Blandford St on 20 December 2012. The house comprises two shops and two apartments, and has three front entrance doors.

Riebau recognised Faraday's talent and generously facilitated Faraday's intellectual development. At the same time, Faraday's training as a bookbinder developed his skill with his hands. Without Riebau, science would almost certainly have been deprived of one of its greatest thinkers and experimentalists.

Riebau encouraged Faraday to read scientific books that passed through the shop, including Lavoisier's *Elements of chemistry*, Jane Marcet's *Conversations on chemistry*, and Thomas Thomson's four-volume *System of chemistry*. For electricity, Faraday used the *Encyclopaedia Britannica* article by James Tytler and *The dictionary of arts and sciences*. In early 1810, Faraday began to attend the lectures of Mr Richard Tatum at 53 Dorset Street off Fleet Street [2, L30], not to be confused with the Dorset Street in Marylebone. Faraday also attended meetings of the City Philosophical Society which Tatum had established (in the 1820s, the CPS was to be informally absorbed by the Society of Arts [2]). Faraday received tuition to improve his writing from Edward Magrath of the Society. He attended Sir Humphry Davy's lectures at the Royal Institution in Albemarle Street on 29 February, 14 March, 8 April, and 10 April 1812. Faraday made visits to bridges and waterworks to improve his general knowledge of civic and industrial installations. Riebau's encouragement extended to allowing Faraday to use a back room as a laboratory outside working hours.

The earliest of Faraday's amateur experimental work for which there is evidence was the construction of a working electrostatic generator [11]. This was made by Faraday with money and materials from Riebau, his father, and his brother, which dates the commencement of the project before the death of Faraday's father on 30 October 1810. Unfortunately, the original papers used by the author of ref. [11], those of "the late Sir James South", are now lost [12]. South had somehow acquired the machine itself, and had shown it to Faraday in his later years, who had been "much affected at the sight of the favourite of his boyhood". Unlike South's papers, the machine survives (and is on display at the Royal Institution). In the course of making the machine, Faraday had dissolved sealing wax in an unspecified solvent and then used the solution to coat the two corks which closed the ends of a large glass cylinder and allowed it to be fixed onto the machine's rotating axis. Faraday later said that his dissolution and precipitation of the wax was his "first chemical experiment".

Evidence of Faraday's later amateur experimental work comes from a remarkable correspondence between Faraday, then in the last three months of his 7-year apprenticeship, and Benjamin Abbott. Abbott was a city clerk of good education whom Faraday had met through the City Philosophical Society and who lived in Long Lane, Bermondsey. Between 12 July and 1 October 1812, Faraday dealt at

length with scientific matters in ten letters to Abbott [L3-5, L7-13], and received a comparable number of (now-lost) letters from Abbott. As well as engaging in wide-ranging general discussion (from mechanics to the elemental nature of chlorine), Faraday describes his construction of two voltaic piles and his electrolysis of aqueous solutions, moreover in a style that gives the reader confidence in the accuracy of his observations. Abbott was doing his own electrical experiments, only tantalisingly referred to by Faraday. The principal reason for the correspondence – the two men met frequently – was set out in Faraday’s first letter begun on the afternoon of Sunday 12 July 1812 [L3]. Faraday justified communication by “Epistolations” (his own neologism) as “improving the mind of the person who writes, & the person who receives”. Faraday sought among other things to improve his “Grammar &c” and his ability to express himself. He noted that ideas “generated and formed in the head” became “clear and distinct” in writing.

But on 7 October 1812, Faraday’s apprenticeship with Riebau ceased, and he took up a journeyman position with Henry de la Roche, of “King Street, Portman Square”. At this period, before postal districts, it was normal to locate minor streets by reference to a nearby major street or square, which allows identification of King Street with that in Marylebone, *ie* the westernmost section of the modern Blandford Street (Figure 2). The work load was high, and Faraday had presumably lost the part-time laboratory in Riebau’s back room. In consequence, the correspondence became one-sided for the next five months, with Abbott writing numerous letters to Faraday but Faraday writing only two to Abbott, neither specifically discussing science [L14, L16]. Faraday wrote to another City Philosophical Society friend, T Huxtable, “I must resign philosophy entirely to those who are more fortunate in the possession of time and means..... I am at present in very low spirits” [L15]. Faraday later recalled having thought that “trade” was “vicious and selfish” and having imagined that “the service of Science ... made its pursuers amiable and liberal” [L419].

Ironically, Faraday’s escape from the bookbinding trade was assisted by his skill in that very trade. Faraday had presented to Riebau bound volumes of his notes of Tatum’s lectures. Through Riebau, these were seen by a Mr Dance who lived in adjacent Manchester Street (Figure 2) at N^o 17. It was Dance [13], a member of the Royal Institution, who gave Faraday his tickets to Davy’s lectures in early 1812. With the encouragement or recommendation of Dance and/or Riebau, Faraday may have procured a meeting with Davy in early October 1812. At any rate, Faraday was sufficiently known to Davy that when the latter injured his eye in late October 1812 (a nitrogen trichloride explosion), he chose Faraday to serve as an amanuensis while he recovered. (Presumably Faraday did this outside his working

hours for de la Roche.) And at some time during the last four months of 1812, Davy saw Faraday's bound volume of the notes he had taken of his own, *Davy's*, lectures. Davy had in May 1812 resigned from paid employment with the Royal Institution but was still an honorary professor. In a letter to Faraday of 24 December 1812 [L17], Davy warmly promised to do what he could for him: "It would gratify me to be of any service to you. I wish it may be in my power." Faraday was an obvious choice should any junior post become available at the RI.

On 19 February 1813, such a post did become available when the laboratory assistant at the Royal Institution, William Payne, attacked the Institution's instrument maker, John Newman, and was sacked. On 22 February 1813, Davy sent a note to Faraday inviting him to a job interview. On 1 March 1813, Faraday was appointed Payne's replacement by the Institution's managers with rooms in Albemarle Street.

On 8 March 1813, from his new situation, Faraday resumed writing to the tolerant Abbott, "in the expectation of a recommenced & reinvigorated correspondence" [L18] [14]. In this letter, he reverted to scientific matters, namely the work he had done for Davy on the extraction of sugar from beetroot and the preparation of carbon disulfide. Now a full-time worker in a properly equipped laboratory, Faraday's talents, already so evident during his apprenticeship with Riebau, could begin to develop fully.

The author thanks Professor Frank James of the Royal Institution, Ms Freddie Magner of Faradays Property Consultants, and Mr Malcolm Thick, FRHistS for their help. Refs [2] and [4] were kindly made available by the RSA archive, RSA, London.

This article first appeared in the Royal Society of Chemistry Historical Group *Newsletter and summary of papers*, summer 2013, **64**, 35-41. The RSC HG website is <http://www.chem.qmul.ac.uk/rschg/>.

Notes and references

1. The Society of Arts was established in 1754 and became the Royal Society of Arts (RSA) in 1908. Its full name is The (Royal) Society for the Encouragement of Arts, Manufactures and Commerce. The RSA today is focused particularly on social and economic innovation, and despite its strong support of industrial design

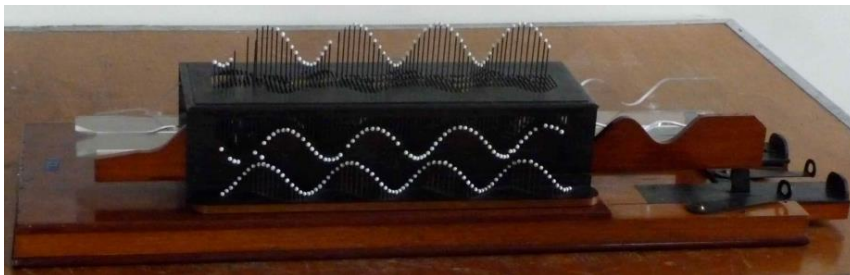
is less directly concerned with science and technology than in the nineteenth century. Faraday was chairman of its Chemical Committee for most of the period from 1826 to 1838 [2].

2. Frank A J L James, *RSA Journal*, February 1992, **140** (No 5426), 192-199, "Michael Faraday, the City Philosophical Society, and the Society of Arts".
3. English Heritage, *History of the blue plaques scheme*, <http://www.english-heritage.org.uk/discover/blue-plaques/about/history> (accessed 20 March 2013).
4. "Annual general meeting – report", *Journal of the Society of Arts*, June 1876, **24** (No 1232), 787-792.
5. L Pearce Williams, *Michael Faraday – a biography* (Chapman & Hall, London, 1965; Da Capo paperback reprint, New York, 1987), 8-29 (Da Capo edition).
6. Frank A J L James (ed.), *The correspondence of Michael Faraday, Volume 1, 1811 – December 1831, Letters 1-524* (Institution of Electrical Engineers, London, 1991), xxvii-xxxii.
7. Peter Day (ed.), *The philosopher's tree* (IOP Publishing, Bristol, 1999), 1-18.
8. James Hamilton, *Faraday – the life* (Harper Collins, London, 2002), 5-16 and 27-43.
9. Benjamin Rees Davies, *London. 1852. Drawn & engraved expressly for the Post Office directory* (British Museum Crace collection, viewable under www.bl.uk/onlinegallery/onlineex/crace/1/007000000000007u00269000.html, accessed 20 March 2013).
10. Henry Bence-Jones, *The life and letters of Faraday* (2 volumes, 1st and 2nd editions, London 1870), 9 (either edition).
11. John Hall Gladstone, *The Argonaut*, 1875, **2**, 33-35, "Faraday's first electrical machine".
12. James (ed.), footnote 1 to Letter 2.
13. There is disagreement as to which Mr Dance of 17 Manchester St.
14. The last letter from Faraday to Abbott in James (ed.) is Letter 247 of 16 December 1824.

"L" references in the text indicate the number given to a letter in James (ed.).

The Wheatstone Wave Machine

*Dr. Robert W. Whitworth
University of Birmingham*



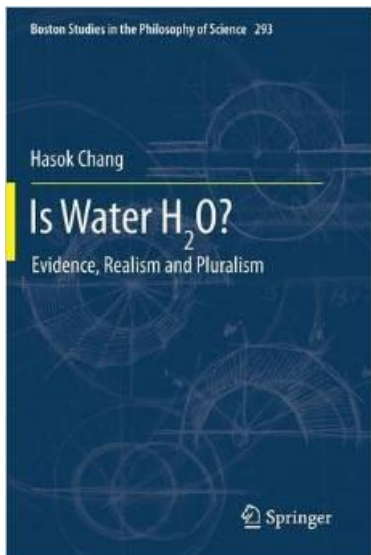
When the History of Physics Group visited the Collection of Historic Instruments at the University of Birmingham in 2010, I showed them a device obviously designed to demonstrate wave motion, but none of us could think up how it worked. In due course I learnt more about this machine, which had been invented by Sir Charles Wheatstone around 1840.

The problem was that our instrument was seriously incomplete, and the bits we had were not intended to work together. Eventually, after studying a complete but non-working model in the Science Museum store, I was able to restore our machine to working order, even if not to perfect condition.

It was devised to demonstrate plane and circularly polarised transverse waves, interference and the addition of such waves. This was well before Maxwell's theory of the nature of light. The machine is a joy to watch and you can see it for yourself in the YouTube video we have made. I think that, like me, you will admire the ingenuity of the man who could devise such a complicated machine so early in the history of wave motion, and also please spare a moment to think about the instrument maker John Newman who built it.

<http://www.youtube.com/watch?v=bw4R5qXalww>

Book Reviews



Is Water H₂O? Evidence, Realism and Pluralism

Hasok Chang

Springer
 ISBN 978-94-007-3931-1
 Paperback/Hardback

2012
 340 pp

Reviewed by Jim Grozier

Introduction

This book might, at first sight, be considered a strange choice for review in a publication dedicated to the history of physics. After all, surely the question of what water is made of, and the history of how that question was answered, come under the heading of *chemistry* rather than physics? Nevertheless, its author, who was trained in physics, represents an exciting new breed of historian-philosopher of science, and I think that any publication of his is worth reading, as an example of a new approach to the history of science.

The book starts with a brief introduction, in which Chang takes the opportunity to outline his approach, to which he gives the unfortunate name “complementary science”, thus running the risk of being bracketed, however unjustifiably, with “complementary medicine”. Its aim, he says, is “to address scientific questions that science itself neglects”, and in a historical context that often means re-examining scientific theories normally thought to have been superseded by better ones. It is a

truism that *any* historical study of science will conclude that what actually happened is more complicated than has been naïvely supposed. To that can be added the concept of *lost* knowledge – knowledge that has not necessarily been discredited, but which simply disappears when one theory supersedes another. *Is Water H₂O?* reveals the fascinating story of the changes which underwent our understanding of the composition of matter in the late 18th and early 19th centuries.

There are five chapters, three of which follow different strands of the H₂O story, followed by two which generalise these case-studies into a coherent historical philosophy of science. The Introduction also explains the structure of the chapters, each of which has three components: an “engaging surface-level introduction and summary, intended to be accessible to non-specialists”, a “full exposition of my position, without constraints”, and finally, “esoteric details and anticipated objections that would interest specialists”. The reader is invited to select only those sections that are appropriate to his or her needs. Thus the book should appeal to a wide range of readers; the Introduction describes the primary intended audience as “academic communities in the history and philosophy of science”, but by following this advice it can be “accessible to wider audiences”.

Water and Phlogiston

The first case study locates the debate about water in the Chemical Revolution which occurred at the end of the 18th century. Before Lavoisier, water had been considered to be an element since ancient times, and questions about its composition therefore did not arise. This elemental view of water became part of the *phlogistonist* approach, which dominated chemistry in the mid-18th century, in which combustion and other chemical changes were seen in terms of the transfer of phlogiston, an “imponderable” (weightless) substance. Phlogiston was one of a number of “principles” which at that time were thought to account for various phenomena including electricity, heat, combustion and light. It was a natural constituent of many substances, particularly metals, and was given off when these substances were burned. But this theory was challenged by Lavoisier and his colleagues, who explained combustion instead as the *absorption* of something (oxygen) by the metal. For Lavoisier, water was a compound of hydrogen and oxygen, and of course this coincides with the modern view.

However, Chang’s detailed investigation of the Chemical Revolution reveals that the passage from phlogiston to oxygen, and hence from elemental to compound water, was anything but straightforward. Apart from the fact that many of Lavoisier’s assumptions (such as his explanation of heat in terms of another imponderable principle, *caloric*, and his belief that all acids contained oxygen)

were later found to be incorrect, Chang highlights some aspects of the phlogiston theory which were later re-adopted under different names, and whose premature death may have held back the progress of science in the 19th century. For instance, it transpires that the phlogistonists were beginning to associate phlogiston with electricity, and as early as 1780, the chemist John Elliott actually suggested that it should be renamed “electron”; indeed, the modern theory of combustion regards metals as *losing electrons* when burned to form oxides, and Chang speculates that if the phlogiston theory had survived a little longer, there might have been attempts to isolate it from the metals which were thought to contain it, resulting perhaps in the discovery of the photo-electric effect a century earlier than it actually occurred.

Electrolysis

In 1800 the Voltaic pile arrived, and was almost immediately used to decompose water by electrolysis. The second chapter of the book is concerned with the age of electrochemistry, which the new battery ushered in.

Electrolysis ought to have counted as decisive evidence for the compound nature of water; after all, hydrogen and oxygen were given off, in almost the same quantity by mass as the reduction in the amount of water. But there was a snag: the two gases appeared in different places; specifically, at the electrodes – hydrogen at the cathode and oxygen at the anode – regardless of how far apart these were. This became known as *the distance problem*. There was no generally accepted theory to explain it, and the phenomenon was even used to argue once again that water was an element, which combined with negative electricity to produce hydrogen, and positive electricity to produce oxygen. (The phlogistonists had described the gases as *phlogisticated* and *dephlogisticated water* – another example of how close the concepts of phlogiston and electricity were.)

The exact mechanism of electrolysis remained uncertain for most of the nineteenth century, which Chang characterises as a period of *plurality* – with several competing theories coexisting until the work of Arrhenius on ionic dissociation. For him this pluralism was beneficial, if only because Arrhenius’s breakthrough rested on the work of at least three other scientists, all of whom had conflicting theories – so all three theories were needed to produce the successful synthesis.

Towards the end of the chapter, Chang investigates some claims about electrolysis made by Joseph Priestley, who, as one of the principal advocates of the phlogiston theory, could not accept that water was a compound. He reported his own attempts to electrolyse water, which ran counter to the experiences of other scientists of the time and were thus dismissed, along with his suggestion that the oxygen given off

is coming out of solution, and not from the water itself. But Hasok Chang is a historian who, not content to simply study old manuscripts, is happy to repeat the experiments themselves, and has done so in this case, obtaining similar results. These can be encompassed within the modern understanding of electrolysis, but were in conflict with the prevailing theory in Priestley's time, and were thus forgotten.

Getting the Formula Right

With the exception of adherents to one discredited theory which still held water to be an element, a consensus emerged during the 19th century that it was indeed a compound of hydrogen and oxygen, even though there was disagreement about how electrolysis actually worked. The next step was therefore to decide in what proportion the two gases combined. This could not be deduced from the ratio of the weights in which they combined, because that process also required a knowledge of atomic weights. The atomic weight of oxygen, relative to hydrogen, was widely believed to be 8, because this was the ratio in which they combined and hence it would lead to the simplest possible formula, HO. This was the formula favoured by Dalton, among others. An investigation of the *volumes* in which the gases combined could, and eventually did, resolve the ambiguity, but this not only required the assumption of the EVEN (equal volumes – equal numbers) hypothesis but also an appreciation of the diatomic nature of the two gases. It was not until 1860 that a consensus could be said to have taken hold, with atomic weights and molecular formulae taking on appearances we would recognise today, after a decade in which atomic chemistry underwent what some have described as a “revolution”; Chang points out that before that time there was a prolonged period when pluralism was the order of the day, with various different approaches coexisting, and maintains that these different ways of understanding chemical combination and molecular structure *all* contributed to the final synthesis.

Pluralism and Active Realism

The last two chapters take a more philosophical turn. First, taking hydrogen as an example, Chang invites us to consider the arbitrariness of the accepted idea of the atom as the most natural building-block, rather than the ion or the molecule. He points out that there are very close parallels between our modern understanding of what happens at the cathode in electrolysis, and the phlogistonists' idea that hydrogen is a compound of water and phlogiston, especially if we make the aforementioned correspondence between phlogiston and electricity. The explanation of the same phenomenon by means of two or more completely different models should not be an unfamiliar phenomenon to us. For me, it

conjures up all sorts of parallels, such as the very long coexistence of the “current” and “pole” models of magnetism; Chang cites the two ways of doing classical mechanics (the Lagrangian and Newtonian formulations) and the two approaches to quantum mechanics (Heisenberg and Schrodinger). Often we find that each of such models has some advantage, depending on what we want to use it for. This is the basis of Chang’s espousal of pluralism. But pluralism would appear to be at odds with realism, the idea that our theories (including the “unobservables” that most theories incorporate) are literally true and are not just mechanisms for getting the right answer – if we wish to maintain two or more incompatible approaches to nature, we surely cannot claim that either one of them is “real”. Chang gets out of that conundrum with his doctrine of *active scientific realism*, which solves the problem by retreating just a little from the sort of correspondence between scientific theories and a single “absolute” truth that is implied by conventional realism. However, he makes a distinction between *epistemic* and *metaphysical* pluralism: while the former allows for the coexistence of different paths towards the truth, he is not arguing that there are actually multiple truths – just that whatever really is “out there” is not sufficiently visible to us to justify the choice of a single path. Additionally, active realism stresses that the question of which particular scientific lens we should choose to view the world depends on what bit of it we are viewing, and the exact purpose of our investigation. We are all surely well aware that “outdated” theories such as Newtonian mechanics and geometric optics still have their uses; Chang’s major innovation here is to highlight the advantages of other, older, viewpoints which are conventionally thought to be justifiably long dead and buried.

Overall assessment of the book

Not being a member of the book’s primary intended audience (academic communities in the history and philosophy of science) I find myself in some difficulty when trying to assess it. I am – at least at present – more a “fellow-traveller” of such communities than a member, but felt it necessary to read the whole book, thus ignoring the author’s detailed advice described above. Not surprisingly I found some of the “esoteric details” a little hard going, but I can well understand their inclusion. The basic historical material is, however, a revelation, and ought to be read by anyone who thinks they know the story. I particularly enjoyed the exercises in “counterfactual history”, where the author postulates how different the history of science might have been if theories such as phlogistonism had not been prematurely killed off.

Having read the book, however, the title – and particularly the tense in which it is set – remains something of a puzzle. Chang quotes the philosopher Robin Hendry on the *current* state of our knowledge of macroscopic bodies of water as “complex and dynamic congeries of different molecular species, in which there is a constant dissociation of individual molecules, re-association of ions, and formation, growth, and dissociation of oligomers”, but clearly that is not really what the book is about; nevertheless the absence of any reference to history in the title or sub-title may give the impression that it is. But the fourth chapter opens with the words “So, is water H₂O?”, and in saying this, the author does seem to be suggesting that a full appreciation of the history can still have relevance for the modern view.

Erratum

The article in Issue 31 ‘Developing school electron-physics apparatus in the 1960s and 1970s: a personal account*’ by Dr Dennis S Beard had unfortunately lost the asterisk footnote which read:

‘The a/c below has been assembled and redrafted from many long and detailed letters from DSB (born 1922 and of reduced agility) to DWJ, who apologises for any remaining errors.’

Editor

EDITED BY
DIETER HOFFMANN
MARK WALKER

**THE GERMAN
PHYSICAL SOCIETY
IN THE
THIRD REICH**

Physicists between Autonomy
and Accommodation



The German Physical Society in the Third Reich: Physicists between Autonomy and Accommodation

Edited by Dieter Hoffmann and Mark Walker

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***Reviewed by Emeritus Professor Derry W Jones
Applied Sciences, University of Bradford***

Germany in the 1920s, first in the Berlin colloquia and then in Munich and elsewhere, was at the centre of physics, as well as being significant in chemistry and biochemistry. Among the most telling pages in this 2006 book, newly translated into English, are those of the Appendix devoted to members who left the DPG (the German Physical Society) during 1933-1938 as victims of racial or political discrimination (various euphemisms were used at the time). Among the better known names in this list, which includes several existing or future Nobel prizewinners, are Einstein (one-time President of the DPG), Lande, Mendelssohn, M Polanyi, Paneth, H Kuhn, Weissenberg, Boni, Elasser, Fajans (physical chemist), Minkowski, Szilard, Fröhlich, Peierls, Jaffe, Karman, Kurti, London, Meitner, Simon and Wigner. Such a brain drain - 348 members of the DPG are said to have had to resign or leave - could not but have an enormous detrimental effect on physics, especially theoretical, in Germany, even without concomitant adverse political influences. Some went to the UK, supported by the Academic Assistance Council, stimulating physics and physical chemistry at Oxford, Edinburgh and Manchester, for example, but the majority ended in the USA, where, among other achievements, some joined American and British scientists on the Manhattan Project in the 1940s. A few, who were on their second emigration, having been

drawn from Hungary to German scientific education and research, perhaps resettled the more rapidly. Germany lost more physicists, applied ones especially, immediately after World War II to military research in the USA and USSR as well as a few disenchanted by the harsh political and material conditions for science in 1945-8.

There have been many studies of scientific aspects of the effect of the National Socialist (NS) regime, but *The German Physical Society in the Third Reich* is the first book resulting from studies over some years by experienced science historians of the DPG and its leaders and editors. The subtitle refers to the tension between accommodating to and compromising with the NS movement (which would increasingly require military research; the four-year re-armament plan began in 1936), and retaining some degree of independence. Although there are 11 separate essays, the arrangement during the preparation time of three workshops, involving participants additional to the authors, helped the editors achieve three results: a consistent approach, minimal unnecessary overlap, and a representation of diverse views. Most of the non-editing authors (four from Germany and one from an American institution) are professionally knowledgeable about the history of physics, while essays on the corresponding mathematics and chemistry organizations are contributed by science historians from Denmark and Israel, respectively. The editors, Mark Walker from Schenectady and Dieter Hoffmann from Berlin, each of whom is also author of one of the essays, have published and edited books on German science before, during and after the Nazi period.

The *Deutsche Physikalische Gesellschaft* (DPG), or its Berlin predecessor, was established in 1845 and claims to be the oldest physics society in the world. Its reputation was assured in the early part of the 20th century by Planck and Einstein while in the 1920s Heisenberg was at the centre of advances. In the opening essay, Mark Walker outlines the political background to the dictatorship period. The appointment of Hitler as chancellor 1933 may not have affected day-to-day activities of the DPG but the NS Law for the Restoration of the Civil Service, a euphemism for the purging of political opponents and non-Aryans, had a profound effect since most non-industrial scientists were civil servants. Many physicists lost their jobs or left the country under this, and the 1935 Nuremberg Laws, banning Jews from public life; the DPG only formally excluded Jews from 1938. Science *per se* was less of a NS target than the universities.

Starting in 1935, the Aryan or German Physics (*Deutsche Physik*) Movement, opposed the work of Einstein (who was subjected to anti-Semitic attacks even during the Weimar Republic) and other *Judische Physik*, but had rather few adherents (many fewer than those physicists who came to embrace National

Socialism). It was led by the Nobel Laureates Philipp Lenard (already retired and earlier a rival of JJ Thomson) and Johannes Stark; the latter had supported Hitler before he came to power and advocated the leadership principle (*Führerprinzip*). Both joined the Nazi party. Stark wanted to dominate the DPG through election in 1933 of himself or a favoured candidate as President but von Laue successfully opposed this attempt at central control of physics journals and research by getting an industrial (Osram) physicist, Karl Mey, elected. After 1936, succumbing to NS rivals, Stark had little influence on German physics but the bias of Aryan ('homage to fact') over modern theoretical physics ('Jewish advocates of Opinion') persisted in the appointment of some chairs. Thus, as Michael Eckert recounts, in 1939 Arnold Sommerfeld's theoretical physics chair at Munich was eventually filled by a mathematical aerodynamicist, Wilhelm Müller, and the successor to the experimental physics chair held by Jonathan Zenneck (chair of DPG in 1935-37 and again in 1939, replacing the departed 1936 Chemistry Nobel laureate Debye) was Rudolf Tomaschek, a pupil of Lennard. Even Heisenberg, who in 1933 had expected the Nazis to fail, was harassed as a 'White Jew' in 1937 – Stark attacked him in *Das Schwarze Korps* - but was rehabilitated through an early school link with Heinrich Himmler of all people. Eckert concludes that the DPG's later reaction to Aryan Physics, a struggle within the physics community, was partly to distract attention from its other co-operation and alignment with NS ideology.

Despite the efforts of some young NS physicists to further Nazify the DPG through Party member Abraham Esau, Carl Ramsauer, an industrial (AEG) experimental physicist experienced in armaments research and discoverer of the Ramsauer electron-scattering effect, was elected President in 1940. Industrial physicists were likely to be more acceptable than academics to the regime. Although Ramsauer had been an assistant to Lenard, he did not join the Party. However he instituted the NS leadership principle into the DPG and continued its gradual 'self-co-ordination and self-synchronisation'. In his essay on the Ramsauer era, Hoffmann writes that Ramsauer combined traditional patriotism, political opportunism, and pragmatic calculation. Popular adulation of a charismatic Führer appeared to peak in Oct 1941, after the early successes in the invasion of Russia. The DPG made a pact with the military-industrial complex over self-mobilisation of physicists for war-related research but re-established a degree of independence over academic appointments on merit. Ramsauer was conscious of the decline in physics in Germany compared with that of the Allies in the 1940s, partly perhaps because of a difference in hierarchical research structure. He campaigned in 1943-44 for greater appeal of an academic career, for setting up of the *Informationstelle Deutsche Physikalische* for public understanding of science, and for encouragement of bright students to study physics in schools or act as young radar assistants. Indoctrination and physical

fitness for the young had been emphasized in education at the expense of science. By 1942, Ramsauer was supporting modern theoretical physics; Aryan physics was no longer mentioned. He and the DPG were service for weapons research. Lack of dynamism of the NS ministry of education under Bernhard Rust led to an anecdotal definition of the Rust unit as the time between two decrees and their retraction. Tackling the question of whether the DPG retained its freedom rather than collaborating or capitulating, Richard Beyler wonders whether it is the freedom of the financial underwriters of the professorial elite or just freedom (not anarchy) to investigate what is worth studying? [Even in America in the 1940s funding concentrated research on military and later biomedical areas while McCarthyism and the Cold War also influenced research choices.] In 1939, the DPG stalled until 1940 (and recorded only in 1941) in amending its statutes to fulfill the requirements of the Reich Ministry of Science, Education and Culture (REM). Apart from excluding Jewish members, the DPG statutes had to introduce the *Führerprinzip* and to concede confirmation of important decisions to REM. On balance under Ramsauer, by trying to avoid conflicts, the DPG gained prestige and retained some freedom (it had acquiesced to the loss of Jewish members) but the Nazi dictatorship profited. In Jan, 1945, the centenary of the DPG was celebrated in a cold Reichstaguter in Berlin months before it and the DPG were no more.

The DPG provided some indirect encouragement of fundamental physics research during the time of the socialist state, Gerhard Simonsohn's long essay surveys research through the annual conventions (or conferences) and, in turn, each of the several journals of the DPG. A totalitarian regime tries to dictate behaviour in all aspects of life. In practice, not all research by ordinary physicists was connected with war projects and there may have been some advantage in physics being in the shadow of chemistry, the economic importance of which was evident; there was no 'Aryan Chemistry' distraction. The very facade of research into basic principles helped serve the regime by suggesting a degree of normality. Applied or technical physics in industry also yielded some basic spin-off. At Siemens they were even during the war constructing a betatron which would provide a profitable post-war medical radiation source. Nuclear research continued to be a topic at all the 1930s conventions and be published openly (although radar research was secret) and the neutron chain reaction was discussed freely in 1939. During the war, rather few physicists worked on Heisenberg's uranium project. Recent US research on nuclear fission was reviewed in Germany in 1942 and, remarkably, in August, 1944, *Physikalische Blätter* quoted a Swedish report of a new uranium bomb of unimaginable potency!

In her essay on the German Chemical Society (DChG) and the Association of

German Chemists (VdCh), Ute Eichmann records that the rapid fulfillment of the 1933 anti-Semitic measures, ousting Jewish members, had a dramatic effect on laboratories and the editorial offices of chemical journals. The DChG in 1933 and the VdCh in 1938 joined the NS German Workers' Party, something that the DPG never did. Despite belittling the importance of science in 1933 ('we can do without physics and chemistry'), in 1936 Hitler recognized 'chemists and inventors in Germany'. Although Richard Kuhn, who presided over the DChG 1938-45, was not a party member, he had in 1938 refused the Chemistry Nobel as inappropriate for Aryans and he advocated unconditional support for Hitler. Volker Remmert describes how the smaller German Mathematical Association (DMV) proved its reliability to the REM and collaborated with the NS agencies.

Klaus Hentschel assesses the mentality of German physicists 1945-49. Some residual NS mentality remained and, rather than feeling liberated or a sense of regret, many Germans apparently considered that Hitler's worst offence was to lose the war; Nazi-ism was a good thing, albeit badly executed. There were travel restrictions, a fear of entering the Russian Zone as East and West were separating, and a lack of communication between scientists in individual Occupation Zones until the merger of the British and American Zones in 1947. In 1946 the Soviet Military Administration in Germany claimed to have recruited 2000 scientists and engineers (including families) to the East. Returning to Germany to investigate calculating-machine developments in July, 1947, the mathematician Richard Courant, who had been one of the first to leave the DPG and Germany in 1933, encountered educated Germans who were demoralized, insensitive, accusing and aggressive against Allied policy. Of course, cities were in ruins, there was an influx of refugees, and conditions were harsh so that everyone was cold and hungry but there was "distrust, bitterness and sentimentality", the title of Hentschel's essay. Other visitors in 1948 were astonished at finding self-pity and the absence of compassion, remorse, shame or even recognition of the havoc wrought in Europe by the Nazis; denazification would be very difficult. The GDP in the British *Zone* was set up in Oct, 1946, with Max von Laue as Chairman; that in the GDR began in 1952. The regional physical societies consolidated into a national association in the 1950s but the name of the DPG was formally resumed in the FRG only in 1963, by which time rebuilding was under way.

Finally, in his essay "Cleanliness among our circle of colleagues", Gerhard Rammer looks back at how the DPG regarded those physicists who had gone with the NS flow. Most 'brain-drain' departures just after the war to America or Russia were for weapons development as the national opponents realigned, but some DPG members left because of disquiet at the way professionally well-qualified NS

physicists returned to university appointments. Rammer refutes the interpretation by Ursula Martius in 1947 that post-war policy continued the cause of National Socialism; she was conscious of the presence of former Nazi supporters at the Gottingen physics meeting. It was more a call for the promotion of physics in Germany and, more generally, of German science and culture. The DPG in the British Zone resolved that to ensure the cleanliness of the essay title only physicists who had damaged the reputations of colleagues or institutions should be considered for exclusion. There were three elements: amnesty; integration and rehabilitation of the politically incriminated; and demarcation and exclusion of unacceptable colleagues. Few physicists were punished for their overzealous collaboration. They included the astronomer Wilhelm Fuhrer (who had been an SS member), Rudolf Mentzel (former officer in the REM, interned 1945-7), Stark, and the applied mathematician Wilhelm Muller (a fellow traveller). Two committed suicide but most had their prison terms commuted, usually to a fine. Many whitewash certificates were issued by von Laue and Otto Hahn. Despite having been trained by Jewish professors, Herbert Stuart was an enthusiastic Nazi, a member of the SA from 1933 and of the NSDAP, who vigorously supported Hitler in 1938; but von Lane still kept in touch. Stuart had to leave his Hanover post in 1947 but, after trying to revive his career in 1951, reached a full chair again at Mainz in 1955.

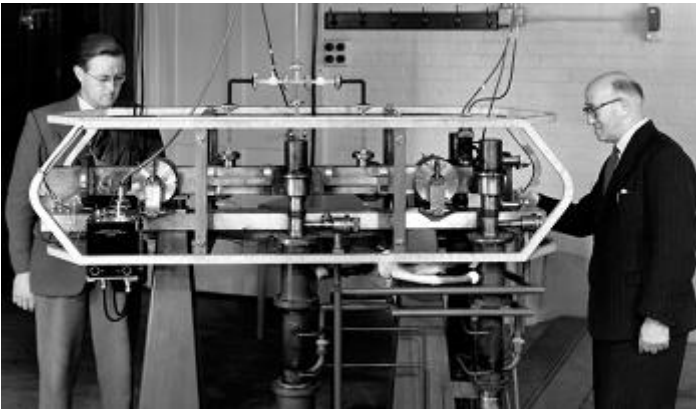
What are the conclusions and surprises? Evidently, the DPG offered only little resistance but did not capitulate to the NS dictatorship; what was the alternative? If all is now revealed, the attention paid to the uranium project (the ostensible justification for the Manhattan project) seems surprisingly small but the appreciable continuation of basic research is unexpected. Conscious as we now are of Germany's reconstruction, economic boom and effective absorption of the DDR into the FRG, it is salutary to read of the low ebb to which Germany and its physicists sank 1945-50. More encouraging, on the other side, is to see the respect in which the Nobel prizewinner Max von Laue (1879-1960) was held for his opposition to the Nazis. He had ended his term as President of the DPG in 1933 but campaigned for Mey against Stark as successor. Even he wrote to Otto Hahn as late as 1954 that the Germans had been the victims, impoverished by the Allies. But other quotes describe him as 'champion of freedom' (Chicago Hon D Sc, 1948) and the only German physicist who behaved decently in NS times'. Becoming in 1946 the first chair of the GDP in the British Zone, he, condemned the real culprits, resisted probing criticisms and rebuffed denazification. As German physics sought to regain its reputation, he materially helped reestablishment of international scientific relations (and managed to get to crystallography meetings in England as early as 1946 and 1948).

Ann Hentschel's translation into fine English yields a very readable book, a remarkably coherent entity for a multi-author volume. She favours a few uncommon or infelicitous usages such as autarky, co-optation, epigonic, immanent, actor for historical participant and the verb contract (for contains?). There are 27 half-page figures, mostly portraits of the leading physicists. The index is excellent on names but almost non-existent on topics. In his Foreword, Eberhard Umbach, 2006 President of the DPG, rightly states that the volume is more than a history of the DPG and of physics during the Third Reich: it is an 'act against forgetting'. The essays together provide a valuable analysis of the history, context and introspection of the German Physical Society, positioned as it was between physics research and dictatorial politics. Hoffmann and Walker's sizeable monograph is recommended to historians of physics and is a reminder to the more general reader about how professional scientists can react under a repressive regime.

Stop Press**EPS Historic Sites**

Colin Latimer, Queens University Belfast

The European Physical Society has officially designated two laboratories as EPS Historic Sites. The National Physical Laboratory in Teddington and the Blackett Laboratory at Imperial College have both been recognised in this way and commemorative plaques unveiled in the past year. This EPS distinction is designed to recognise places such as laboratories, buildings, institution and universities associated with an event, discovery or research work that has made an important and exceptional contribution to physics. This EPS programme is not meant to designate a residence or birthplace of noteworthy physicists and so complements the numerous blue plaque schemes in many countries.



Essen and Parry with the world's first caesium atomic clock, developed at NPL in 1955.

The NPL was recognised as the birthplace of atomic timekeeping, where in the 1950s, the world's first atomic caesium clock was constructed which revolutionised global timekeeping and made modern communications and location services possible. The Blackett Laboratory was recognised as a place of much ground breaking experimental and theoretical physics research including the design of the hydrogen bubble chamber by Butler and Goldsack, and the unification of the weak and electromagnetic forces by Abdus Salam.

There are now over 40 historic sites in 17 different countries, including the Observatory of Tycho Brahe in Sweden, The Neils Bohr Institute in Denmark and the Goldfish (Fermi) Fountain in Rome.



Professor Tom Kibble, whose pioneering work helped in the discovery of the Higgs boson, at the unveiling of the Blackett Laboratory plaque

The European Physical Society encompasses 42 Member Societies (including the Institute of Physics) and 37 Associate Member Institutions such as CERN, GSI and ESA. It also has 3500 Individual Members and works to promote the interests of physicists and physics in Europe and Internationally. The aim of this Historic Sites Programme is to provide and promote a local awareness of our scientific cultural heritage. The EPS works with nominators to obtain local authorisations for siting plaques and in organizing the commemorative ceremonies. Further information, including a nomination form, can be found on the EPS website <http://www.eps.org>. It is to be hoped that more EPS Historic Sites can be declared in The UK and Ireland in the near future – there is no shortage of appropriate locations!

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