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Cover picture: ‘The ‘Standard Kilogram’ - Illustration reproduced by kind permission of Bureau International des Poids et Mesures, Sèvres

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Editorial

"International Conferences

- where do we go from here?"

When, on a cold November day in 2011, I trudged into the IOP Head Office in Portland Place to speak with Science Officer, Claire Copeland, about the possibility of initiating a European conference of the history of physics, it was very hard to peer into the future and foresee all the hurdles we would have to meet and leap. The idea had been floated over coffee - so to speak - sometime earlier by Denis Weaire but now we needed some action! Easier said than done!

Initially the idea was pursued under the aegis of our group, and in January of the following year Chairman Peter Ford penned a letter to the IOP Director of Education, Peter Main. But it became clear very early on that we needed to think more expansively to seek funding to create what we hoped could become a conference series. And so later in that year, after a hearty lunch in the cafeteria of the Cavendish Laboratory - a place rich in the history of physics - Denis, Ted Davis - the new group chairman - and I got down to our first proper Steering Committee meeting, discussing budget, funding sources, possible themes and so on.

European - but why not think international; conferences to cover all aspects of history of physics but with a unifying leading theme; primary funding to be sought from organisations of the host nation, where feasible, were some of the decisions which came out of that meeting.

So, in September 2014 our first international conference was born, its birthplace, Trinity College Cambridge, was as fine a venue as could be imagined, delivered under the tireless chairmanship of Ted Davis. It was a great success with around 100 participants coming from nearly 20 countries throughout the world and was a most satisfying culmination of several years of effort.

And now, another successful conference, this time held in Austria in the picturesque Schloss Pöllau under the chairmanship of Peter Schuster.

Again delegates gathered to hear keynote speeches, talks invited and submitted, inevitably some more gripping than others, interspersed with liberal coffee breaks - I've always been a strong supporter of extended breaks as it seems to me this is when the real synergy takes place.

Now I think we should pause for thought - the reason for my title *'Where do we go from here?'*

There is no doubt that participants gain much from the valuable interaction conferences afford but pondering on these events, questions come to mind.

We may ask what impact do they have on the physics community? Not much I suspect but is it reasonable to expect more? The HoP community is pretty small and its practitioners, though thoroughly committed to their discipline, must consider it limited in its penetration of the world of 'sharp end physics'. Or must they? Much has been written about the application of the history of physics for example in the public perception and understanding of physics (actually, of course, I should say science - but I am considering only physics here).

And perhaps more importantly, in the teaching of physics.

I know a number of my colleagues who not only use hop to facilitate the teaching (and understanding!) of physics but consider it vital and not simply adding a little light relief to an otherwise demanding subject.

As Roger H. Stuewer says in his article ‘History and Physics’

‘...students who receive their only exposure to physics and physicists through physics courses as commonly taught will gain an image of physics and physicists that departs significantly from reality. History of physics, accurately taught, can serve as a powerful corrective force: Through history, students can acquire an understanding of the nature of physics, as practiced by real physicists. Teaching in physics and in history also seem complementary; they are to a great extent mutually exclusive, but both are necessary, I would argue, to give students a full understanding of the nature of physics as an intellectual and human activity.’

So how might future conferences address this question? Might we bring together those brave souls who daily try to reveal the secrets of this remarkably (for the most part anyway!) common-sense way of thinking and those who study its history?

And that prompts another rather thorny question - who are best placed for the task - the historians or the physicists. I should qualify the ‘physicists’ as those who have taken a vigorous interest in the history - the phystorians. This is not the place to delve seriously into that but suffice to say that I believe we should continue to use the conference forum to work on strategies so further progress might usefully be made.

So, I ask my readers to turn your minds to these matters and give me your thoughts, suggestions, ideas on how we may harness the power of such conferences to develop new approaches which could bring the history of physics into centre stage.

Malcolm Cooper
Editor

mcooper@physics.org

Meeting Reports

Chairman's Report

This will be my last Newsletter report as Chairman of the History of Physics Group. My term of office came to an end on 30 September 2016 and Professor Andrew Whitaker will take up the position from 1 October.

This year we have held four meetings. The first, on 17 March, was a well-attended meeting at NPL titled ''The History of Physical Standards'. Thanks to Jim Grozier for organising this very successful event. It was noteworthy for many excellent presentations, including the last talk before his death a few months later by Bryan Kibble, the inventor of the 'Watt Balance', which is designed to measure mass electrically and hence define the kilogram in terms of fundamental constants rather than by the existing standard kept in Paris.

The second meeting of the year was very different in style and content from our normal type. It consisted of live readings of sonnets chronicling the life of William Rowan Hamilton written by Iggy Mc Govern. Thank you to Denis Weaire for suggesting this meeting.

Two further meetings were held in October. On 5 October, a meeting took place at the National Maritime Museum, Greenwich with the title 'Revolution in time; Newtonian physics and its influence on John Harrison's pendulum clocks'. I wish to thank Peter Rowlands for organising this event in conjunction with Rory McEvoy. On 19 October a meeting in collaboration with the Royal Society of Chemistry Historical Group 'H G J Moseley (1887-1916), a lost Nobel Laureate?' was held at Burlington House, London.

In addition, the Group co-sponsored the 'Second International Conference on the History of Physics', held from 5 ‾ 7 September in a historic castle in Pöllau, Austria.

Our Newsletters continue to be popular with members, with most having archival value. I should like to express my thanks to the Editor, Malcolm Cooper, for his dedicated efforts in soliciting articles, book reviews, etc. and for editing and producing all our Newsletters for many years. Long may he continue in this role.

In anticipation of the move of the Institute of Physics headquarters to new premises near Kings Cross railway station, the committee of the History of Physics Group was asked to scutinise the archives of the Institute with a view to identifying any items that might be worthy of display and to provide advice on future storage. A sub-group of the existing committee was formed and spent two days undertaking this task. Several valuable items, for example letters signed by William Thomson (later Lord Kelvin), Oliver Lodge and many other distinguished scientists, were discovered, along with detailed accounts of the merger of the former Physical Society and the Institute of Physics.

We always welcome suggestions from members for topics of future meetings. Please write to the new Chairman M Andrew Whitaker a.whitaker@qub.ac.uk with any ideas you might have.

Professor Edward Davis

Wanted!

Articles, Letters, Queries

- long or short

wanted for *your* Newsletter

Please send to Malcolm Cooper, Editor

email: mcooper@physics.org

(Thanks in anticipation!)

A History of Units from 1791 to 2018, NPL, March 2016

by Sophie Osiecki

It is very rare, if not entirely unprecedented, that a conference on the history of something should purport to cover a period of time that has yet to elapse. Nonetheless, on March 17th, 2016, around sixty delegates gathered at the National Physical Laboratory, Teddington, for a conference on "A History of Units from 1791 to 2018.". This was organised principally by Jim Grozier on behalf of the History of Physics Group and sponsored by the Institute of Physics and British Society for the History of Science. There were eight talks in total, delivered by a highly illustrious lineup of speakers with the following titles (in order of appearance):

Terry Quinn -	*The Metre Convention and the BIPM*
Rory McEvoy -	*Counting the seconds: from notion to precision*
Daniel Mitchell -	*Making Sense of Absolute Measurement: James Clerk Maxwell on Units and Dimensions*
Edward Davis -	*Determination of the ohm and other electrical standards by the Third Lord Rayleigh*
Hasok Chang -	*Kelvin's Absolute Temperature and its Measurement*
Graham Machin -	*Temperature Scales: past, present and future*
Richard Davis -	*The Story of Mass Standards*
Bryan Kibble -	*Mass from energy – a unit for a quantum world*

The rationale behind the paradoxical conference title was explained in Terry Quinn's opening talk that covered the institutional lineage of modern metrology (the science of measurement). The Metre Convention was a treaty signed by 17 nations in 1875 that effected the establishment of three institutions -- CGPM (Conférence générale des poids et mesures), CIPM (Comité international des poids et mesures) and BIPM (Bureau Internationale des Poids et Mesures) -- all of which still exist in more or less the same form as specified in the articles of 1875 (though there have of course been many changes since that time). The BIPM is responsible for the oversight of the definitions and maintenance of a coherent system of units, known formally today as the “Système international d'unités” (SI Units).

On March 19th 1791, Paris, under the commission of the Academie des Sciences, a group of France’s most distinguished scientists of the day (Borda, Lagrange, Laplace, Monge and Condorcet) drafted a report on the reformation of units for the consideration of the French Assembly. They proposed a new “metric” system wherein each unit of measurement would be defined in relation to fixed, permanent features of the natural world. For example, the unit for length, the ‘metre’, was to be defined as a fraction of the Earth’s meridian. This system was “taken from nature”, “in no way arbitrary” and thus “perfect”. As such, owing to its “natural” foundations, it was fully intended that this system would become internationally accepted. In the words of Condorcet, it would be “for all people, for all time”. Needless to say, from their perspective, no metrological reform need ever take place again.

“That was a good idea” said Quinn, commenting on the 1791 Report, “[…] but as you know they couldn’t do it, but we are planning to do it in 2018”. That is, according to Terry, the Bureau Internationale des Poids et Mesures (BIPM) will, in 2018, finally achieve what the French scientists of 1791 had set out to do. Since the 1960’s the BIPM has aimed to redefine the base units; i.e. those of mass, length, time and electric current; in terms of fundamental physical constants and atomic properties of nature. The kilogram, unit mass, is the last of the base units to be defined in terms of a material artefact, the International Prototype Kilogram, that we know to be unstable. By 2018 it is thought that the Watt Balance method (discovered by Bryan Kibble!) will be utilised in order to finally realise the definition of the kilogram in terms of the Planck’s constant.

The second talk was delivered by Rory McEvoy, curator of horology (the study and measurement of time) for the Royal Observatory, who discussed the history of time-keeping, tracing the development of such practices from “notion to precision”. Rory actually went back (albeit briefly) to Ancient time periods, including Ancient Egypt, Babylonia, Sumeria, and China. This was an impressively broad, milestone-presentation of the historical narrative, which brought us up to the current SI unit.

The focus of the two subsequent talks was electrical phenomena. Daniel Mitchell and Edward Davis discussed the contributions of James Clerk Maxwell (1831–1879) and Lord Rayleigh (1842–1919) respectively, to the excruciatingly complex subject of the measurement of electricity in the mid to late nineteenth and into the early twentieth century. Mitchell began by introducing the principal driving force behind the huge efforts made by scientific and commercial institutions during the nineteenth century: the then burgeoning telegraph industry. As such, he briefly gave insights into the tensions that were present between theoreticians and electrical practitioners around this time, which was further explored in Davis’ talk (below). Mitchell’s talk focused predominantly on the theoretical work of Maxwell, concerning dimensional analysis and the corresponding notation he offered for making sense of “absolute” electrical measurement – where ‘absolute’ actually means the reduction of electrical phenomena to the dimensions of mass, length and time. He also explored some of the philosophical questions that emerge from Maxwell’s work for present day philosophers and historians of science, such as what Maxwell and his peers meant by “fundamental” and what their views were on how this abstract notation related to the real world.

Edward Davis (or ‘Ted’ to his friends) told us about the work of the third Lord Rayleigh in relation to the determination of the Ohm standard. During the mid-nineteenth century German scientists had made proposals for the resistance standards, and many electricians used Siemen’s mercury standard (which was simply a certain amount of mercury). For practical purposes this standard was well-suited, however, the British Association for the Advancement of Science (BAAS) were keen to develop an ‘absolute’ unit. Unfortunately, there were many discrepancies when attempting to realise this unit. This meant that there was a problem either theoretically or with the measuring instruments. Maxwell, Stewart and Jenkins had previously developed a method of determination that involved a small magnet suspended from a rotating coil. Rayleigh used his fingers to keep a disc spinning at the same speed, which greatly improved the accuracy of this method. He then went on to use a second method devised by Lorentz, the results of which cohered with the ones made using the first method and were more accurate than those of his peers.

This scientist-centric approach to investigating the history of units was paralleled in Hasok Chang’s talk on “Kelvin’s Absolute Temperature and its Measurement”, which aimed to show how “messy and interesting” the story of Kelvin’s physical and metrological work in relation to temperature really was. This messiness was contingent in part on the fact that there were multiple views on how energy could be understood, and the fact that abstract theorisation and practical measurements were often at odds with one another. Notably there was a contrast between “absolute” in Kelvin’s discussion on temperature and that of electricity (which Kelvin also worked on). Chang argued that Kelvin was able to theoretically define the absolute concept abstractly, but to measure we have to fall back to substances. He concluded that “it is evident that the realisation of a temperature scale is logically a process of successive approximations”.

This talk was complemented by Graham Machin’s prop-laden presentation on the past, present and future of temperature scales. Graham Machin currently works at the NPL and is an expert on the measurement of temperature. He gave an interesting perspective on the development and future of temperature scales. His view on the future of the field very much emphasised the practical usefulness of the present standard, the ISO-90, and emphasised that he did not necessarily think that this would need to be adjusted over the coming years. This was met with resistance from Terry Quinn during question time. Graham’s reply was along the lines of “Who am I to disagree, Terry?”, an exchange that was well received by the audience.

The final talks of the day were delivered by Richard Davis and Bryan Kibble and chaired by Terry Quinn. Davis' talk covered the story of the mass unit mass from the eighteenth century before shedding some light on to the present redefinition of the kilogram. Amusingly this covered the recent media coverage of the International Prototype Kilogram, with one newspaper reporting that it was getting lighter while another claimed it was getting heavier, on the same day no less!

At this point it is worth mentioning an awe-inspiring detail: many of the delegates had played prominent roles in the history of the kilogram. Firstly, Terry was director of the BIPM from 1988--2003, who had employed Richard Davis to work for him. Furthermore, of the two methods being considered for the realisation of the kilogram, x-ray crystallography and the watt balance, the discoverer of the latter was none other than Bryan Kibble, who graced us with an account – the “real story” – of how this came about. Kibble recalled a Eureka moment while walking to and from the library one day, and made many references to “good luck” in the process of his discovery.

Overall, this conference was highly enriching, with varied and valuable perspectives on the historical material. Many thanks to Jim Grozier for organising it!

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We should like to point out that this report was written before the sad and untimely death of Bryan Kibble but that we decided to let it stand. We should like to offer our condolences to his family, friends and colleagues.

We hope to produce an ebook based on this meeting and it would be very fitting to dedicate it to his memory.

Sophie Osiecki *Jim Grozier* *Malcolm Cooper*

History of Physics Group of the Institute of Physics

A Mystic Dream of 4

An unusual mix of poets and physicists met on 14th June at the Institute's head office, Portland Place (but no longer No 76) to hear readings of 'A Mystic Dream of 4' - a sonnet sequence by Iggy McGovern, physicist retired, Trinity College Dublin.

Iggy's sequence contains 64 or 4^3 sonnets on all aspects of the life and work of William Rowan Hamilton, Irish mathematician, physicist, astronomer.

Hamilton's life had its triumphs – in particular the prediction of conical refraction, which brought him the Royal Medal of the Royal Society and his knighthood, and the invention of quaternions. Iggy, himself both physicist and poet, conveys the successes of his scientific life, but also its ironies – Hamilton was the Professor of Astronomy and Royal Astronomer - but with little skill or interest in practical observation.

For much of his life he was close to desperation and even suicide as a result of his continuing obsession with his first love, Catherine Disney. As a young man he had not known she had been promised to the much older Reverend William Barlow, and Iggy shows how he struggled to build a family life of his own.

Hamilton said that the quaternion was a 'curious offspring of a quaternion of parents, say of geometry, algebra, metaphysics and poetry' and these are the four sections of Iggy's sequence, each containing a sonnet on the 'parent' itself, fourteen expressing the ideas of (with two exceptions) relations, friends and associates of Hamilton, with the final sonnet, befitting the fact that this was the period of the Great Famine, being on Death.

The first exception was Eamon de Valera, future Prime Minister and President of Ireland. When imprisoned in Lincoln following the 1916 uprising, he entered a competition to write a poem on 'My best girl' with an offering on 'Quaternia'. The second was Erwin Schrödinger, brought to the longest and happiest spell of his academic career in Dublin by de Valera.

The London meeting was attended both by those primarily interested in physics and also by many affected more by poetry, and it was generally found extremely interesting and enjoyable. Iggy introduced several of his sonnets which were then read by members of the audience. Afterwards he answered a range of questions on Hamilton and the sonnets.

Andrew Whitaker

H. G. J Moseley (1887-1915), a lost Nobel Laureate?

Meeting of Royal Society of Chemistry Historical Group
and Institute of Physics History of Physics Group
19th October 2016

The death of Henry Gwyn Jeffreys Moseley at Gallipoli in August 1915 was more than the loss of an extremely distinguished, though still quite young, scientist, who would certainly have been awarded the Nobel Prize for Physics in the next few years for his demonstration of the significance of the atomic number of an element. Isaac Asimov was to write that:

'In view of what he might still have accomplished … his death might well have been the most costly single death of the War to mankind generally'.

The meeting at the Royal Society of Chemistry in London studied Moseley's upbringing, his all-too-brief scientific career and also the last year of his life in Australia and in the army, but it also examined some of the scientific developments that have taken place on the basis of his work in the century since his death.

The first talk by Clare Hudson, Archivist of Trinity College Oxford, described Moseley's family and early life as well as his education. Both sides of his family were well-off and both had achieved a great deal scientifically. Moseley's mother, Amabel, was herself the daughter of John Gwyn Jeffreys, who had become a Fellow of the Royal Society in 1840 for his work on molluscs, while his father, Henry Nottidge Moseley, was a naturalist and became a Fellow of the Royal Society in 1879 and Linacre Professor of Human and Comparative Anatomy at Oxford in 1881. Moseley himself clearly felt that he had much to live up to, but he may also have been encouraged to make his name fast by the sad knowledge that his father had been taken severely ill in 1887 and died in 1891 at the age of 47.

Moseley's early education was at Summer Fields School, a recognized feeder for Eton College, to which school he moved as a Kings's Scholar, one of a talented and privileged group of boys, in 1900. Maybe better known as providing a niche in (relatively) high society, Eton also offers, for those students who want it, the best of educations, and Moseley was certainly one of these, receiving the prizes for physics and chemistry in 1906, but received the first of two blows when his great Eton friend, Julian Huxley, the future very well-known biologist, was preferred to himself for the prime Oxford scholarship, to Balliol College. Moseley had to be content with the lower-rated scholarship to Trinity College, where he gained a great reputation, but his graduation result in 1910 brought his second blow – instead of the first-class honours he had been expecting, he had to be content with second-class honours, a result he referred to as a ‘fail’.

Despite this result, Ernest Rutherford in Manchester was still prepared to take him on, initially as a demonstrator, later as a research assistant. In the second talk Neil Todd of the Universities of Manchester and Exeter described his work at Manchester where he initially followed Rutherford’s lead in working on radioactive disintegration. However he was always more interested in using X-rays, and initially with Charles Darwin, later on his own, at Manchester and later on his return to Oxford, he performed his crucially important work in which he observed the X-ray spectra from a large number of elements, and expressed the values of the series of X-ray wavelengths produced using simple formulae – Moseley’s Laws. As well as making clear the crucial nature of the atomic number of each element, the method could show for more directly than studies of atomic weight where there were gaps in the Periodic Table and elements remained to be discovered.

At this point the meeting was privileged to be addressed by Mr Cern Işik of the Embassy of the Republic of Turkey, who expressed sadness for the death of Moseley and reminded the audience of the sacrifices made on all sides in the First World War; all joined in the wish that after two such wars, such sacrifices would not be required again.

The next talk was by Elizabeth Bruton of the University of Manchester, who first described Moseley’s visit with his mother to Australia in 1914 to attend the meeting of the British Association for the Advancement of Science. This is said to be the first time he had been seen speaking to a female other than a relative, and Elizabeth was bold enough to make the suggestion, unsupported, as she would admit, by any actual evidence, that, having seen his sister and a close friend get married, he decided that it was time he followed suit, and he set about the process as straightforwardly as every other challenge of his life. If she is correct, sadly the task would not come to fruition.

The war began on August 2nd and on the 27th Moseley commenced his return journey to England, determined to enlist, despite the entreaties of, among others, his mother and Rutherford, the latter stressing that such a man could perform much more useful tasks in wartime, and should be saved for the peace to come. He actually had to ‘pull strings’ to join the Royal

Engineers, and after an attempt to transfer to the Royal Flying Corps, which was unsuccessful because of weight he had put on over the previous few years, he was appointed to be in charge of signals for his unit and sent to Gallipoli.

On his way and on the 27th June 1915 he wrote a soldier's will, in which he left all his money to the Royal Society 'to be applied to the furtherance of experimental research in pathology, physics, physiology or chemistry or other branches of science, but not in pure mathematics, astronomy or any branch of science which merely aims at describing, cataloguing or systematising'. He was killed by a Turkish sniper on 10th August.

The next talk was by Robert Friedman of the University of Oslo on 'Moseley and the politics of Nobel excellence'. He certainly disabused any listeners who imagined that award of the Nobel prize was a result of serious and impartial study of the candidates, rather than based on the diverse political and personal agendas of those making the award. Prizes during the First World War went to those associated with X-ray crystallography and so broadly competing with Moseley – Max von Laue in 1914, William Henry and William Lawrence Bragg in 1915, and, after Moseley's death, Charles Glover Barkla in 1917. In connection with the 1915 award, it was recognised that Moseley's work was important enough for him to be awarded the prize, but ironically it was decided that 'his award could wait'.

The last two talks were on the legacy of Moseley's work. Russell Egdell of Oxford University, gave a general account of the work in the area carried out over the last century, while Justin Wark, also from Oxford, pointed out the coincidence that, as Professor of Physics at Moseley's old college, his work was very much analogous to that of Moseley; while Moseley worked from atom to atom, Wark was following much the same procedure but working with a particular atom and systematically moving to a state of higher ionisation.

Overall the meeting gave a most interesting account of the life and work of this supreme physicist, whose career was to be so drastically and tragically cut short.

Andrew Whitaker

Obituary

It is with great sadness that we announce that Dr Bryan Peter Kibble passed away on Thursday 28 April 2016.*

Dr Kibble worked at the National Physical Laboratory (NPL) from 1967 to 1998 as an experimental physicist, and was made an NPL Individual Merit Fellow in 1985. He was instrumental in reshaping the International System of Units (SI), and is best known for his conception of the watt balance, one of the measurement approaches proposed for the redefinition of the kilogram. Dr Kibble will be dearly missed by the international measurement community, with former colleagues citing his quiet and patient guidance, and praising his problem-solving skills.

Before NPL

Dr Kibble was born in 1938 in Berkshire. He studied Physics at Jesus College, University of Oxford, where he was awarded a DPhil in 1964 for research in atomic spectroscopy. Dr Kibble continued his research as a Postdoctoral Fellow at the University of Windsor in Ontario, Canada, from 1965 to 1967, before joining NPL as a Senior Research Fellow in 1967.

Early impact on the SI units

Early in his NPL career, Dr Kibble successfully measured the high field gyromagnetic ratio of the proton. This measurement, in conjunction with a similar low field measurement, indicated that there was a problem with the existing realisation of the ampere, the SI base unit of electrical current. At the time, the ampere was realised using the current balance, an instrument that was difficult to operate and that had a number of inherent limitations, which Dr Kibble was later to address.

Dr Kibble also worked with Dr Geoffrey Rayner on Coaxial AC Bridges and the calculable capacitor from which the SI definition of the unit of resistance, the ohm, could be established. In 1984, Dr Kibble and Dr Rayner compiled and published their research in the book, Coaxial AC Bridges

The imperfections of the current balance weighed on Dr Kibble's mind and inspired him to conceive a new and improved instrument, the moving coil watt balance, which, together with the calculable capacitor realisation of the ohm, could replace the current balance.

The invention of the watt balance

In the early 1970s, Dr Kibble had an idea for a measurement device that would supersede the current balance. He described his idea to Bob Cutkosky, a highly-respected experimental scientist visiting from the USA's National Institute of Standards and Technology (NIST). Cutkosky's response encouraged Kibble to proceed with the idea and also planted the seeds for similar developments in the USA, which were pursued by his friends Ed Williams and Tom Olsen.

In 1978, the Mark I watt balance was built at NPL with Dr Ian Robinson and Ray Smith. The instrument was used to realise the ampere with greater accuracy than was possible with the current balance, and the results played a major role in setting the 1990 conventional values of the Josephson and von Klitzing constants, used today for electrical measurements throughout the world. In recognition of his work, Dr Kibble was awarded the International Union of Pure and Applied Physics SUNAMCO Senior Scientist Medal in 1992.

Redefining the kilogram

The kilogram, the SI base unit of mass, is the last of the seven SI base units to be defined by a physical object. But in 2018, the kilogram will be redefined in terms of a natural constant, the Planck constant, the quantum of action in quantum physics.

In 1990, a second watt balance was built by Dr Kibble, Ian Robinson and Janet Belliss at NPL. It was designed to operate in a vacuum and was intended to measure the Planck constant with sufficient accuracy to support the redefinition of the kilogram.

The watt balance compares the weight of a one kilogram mass to the electromagnetic force generated by the interaction of a current-carrying coil of wire and a magnetic field. Then, the same coil is moved with a measured velocity in the same field, and generates a measured voltage. The combination of these two parts enables the properties of the coil and magnet to be eliminated from the measurement and allows electrical power and mechanical power to be equated. Using the Josephson and quantum Hall effects, electrical power can be measured in terms of the Planck constant and time, allowing the watt balance to relate mass to the Planck constant and SI units of length and time. By changing the definition of the unit of mass within the SI to fix the value of the Planck constant, the last artefact standard in the SI – the platinum-iridium cylinder kept at the International Bureau of Weights and Measures (BIPM) in Paris - can be replaced and, by the additional fixing of the value of the elementary charge, the electrical units can return to the SI.

In 2014, Dr Kibble and Dr Robinson published new principles for building simple watt balance designs, making the instrument more accessible, and in 2014, Canada's National Research Council used the NPL Mark II watt balance to measure the Planck constant with sufficient accuracy for the redefinition. A fitting tribute to Dr Kibble's visionary work, from 2018 watt balances should be used throughout the world to realise the kilogram definition.

An active retirement

Dr Kibble retired from NPL in 1998, but continued to be active in the field. He worked on the Mark II watt balance and high-frequency standards and bridges at NPL, and became a guest worker at both the Physikalisch-Technische Bundesanstalt (PTB) and BIPM, where he played a key part in eliminating a number of unresolved problems with the measurement of the ac quantum Hall effect.

Dr Kibble continued to be active on various international committees. In 2009, he won the IEEE Joseph F Keithley Award in Instrumentation and Measurement and was invited to write a regular column for IEEE Instrumentation and Measurement Magazine. In 2010, he published a book with Jurgen Schurr and Shakil Awan, Coaxial Electrical Circuits for Interference-Free Measurements

Dr Kibble gave his last talk at NPL on 17 March 2016, describing the invention and development of the watt balance to an audience of current and retired NPL staff, and members of the Institute of Physics and the British Society for the History of Science.

* This obituary is reprinted here by kind permission of NPL.

Feature Articles

Lars William Öholm - Letters from Manchester, 1913

Peter Holmberg
University of Helsinki

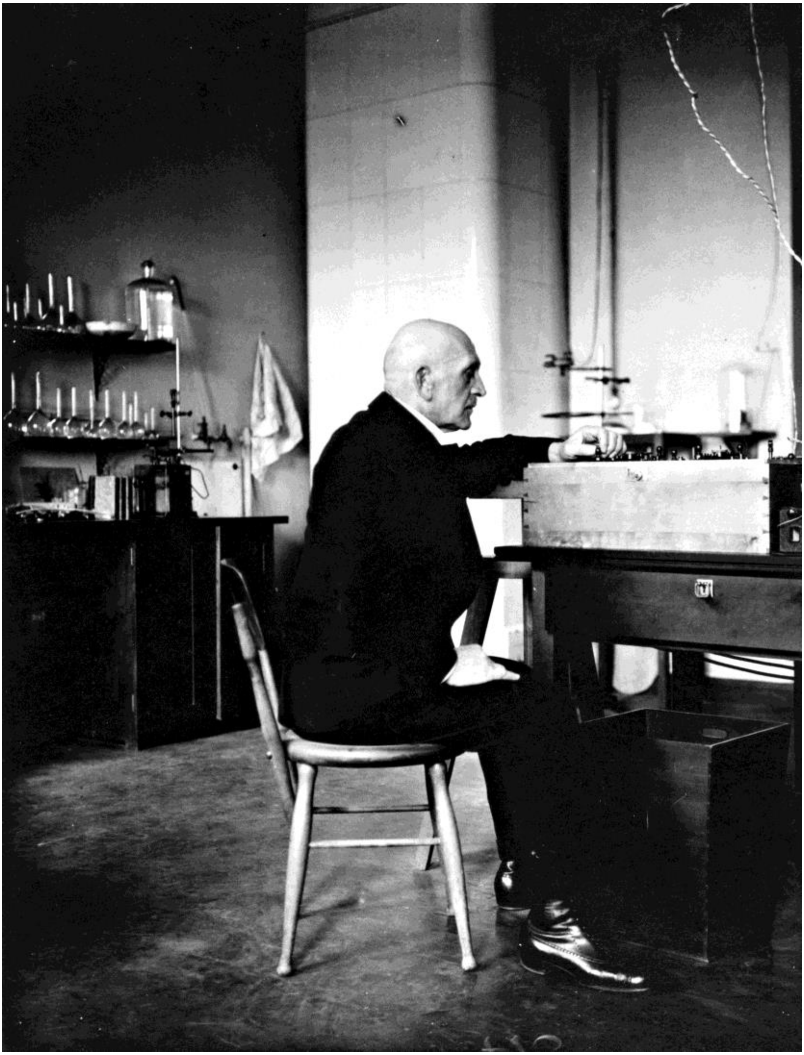
Lars William Öholm was a Finnish scientist, born in 1872 in the Mörskom community, about 40 kilometers northeast from Borgå (fi. Porvoo). He grew up on the family estate owned by his father, Judge Lars Öholm, which later in life Lars William took over and used it as his summer residence.

He went to school in Borgå and graduated from the gymnasium in 1896. He took up studies at the Imperial Alexander's University in Helsinki and completed his master's degree in 1897. In those early days Öholm earned his living as a teacher, but his interest soon turned to research and in 1900-1901 he worked in Svante Arrhenius' laboratory in Stockholm. And it was during these years that Arrhenius and Öholm became lifelong friends.

In Stockholm his main subjects were physics, chemistry and electrochemistry. Returning to Helsinki in 1902, he defended his thesis *"Bidrag till kännedom om hydrodiffusionen särsklidt elektrolyters diffusion" [Some aspects on the hydrodiffusion, especially the hydrodiffusion of electrolytes]* and in 1904 he became an assistant at the Laboratory of Applied Physics. For some years he also held the position as secretary of the physical-mathematical faculty, and learned much about the University statutes and administration.

Öholm was fortunate and due to scholarships he was able to travel to the main laboratories of Europe. He visited Stockholm (meeting Svante Arrhenius, Nobel Prize 1903), Berlin (Walther Nernst, Nobel Prize 1920 and Fritz Haber, Nobel Prize 1918), Leipzig (Luther), Heidelberg, Gottingen, Stuttgart (Erich Müller), Vienna, London (Dewar and William Ramsay, Nobel Prize 1904), Cambridge (J.J. Thomson, Nobel Prize 1906) and Manchester (Sir Ernest Rutherford, Nobel Prize 1908). The list of important persons is long, many of them are Nobel Prize winners and they all invited Öholm to visit their laboratories and discuss ongoing experiments with him.

While in Manchester in 1913 Öholm wrote two letters to Svante Arrhenius. He told Svante about the progress of his work in Rutherford's laboratory and he gave a picture of the town - not a very positive one!



L.W. Öholm in the Laboratory of Physical Chemistry. On the table is a rheostat.

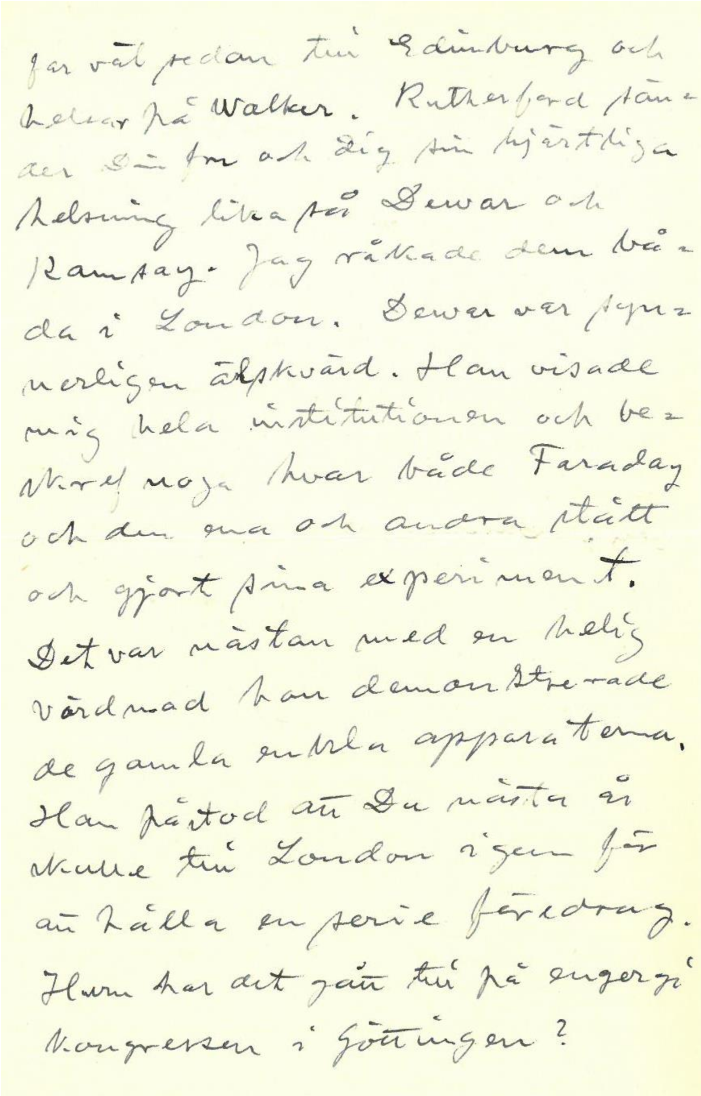
Photo by Matti Taristo, 1920s, Helsinki University Museum

Laboratory of physics
University Oxford street
Manchester.

2/V 1913.

Bästa broder Svante.

Såsom Du ser är jag nu i Manchester och genomgår den radioaktiva mätnings kursen. Här har jag fått ett begrepp om hvad en fabrikstad vill säga. Alltings sotigt och smutsigt. Du kommer väl själf i håg huru det var då Du var här. Till på köpet har här varit tämmeligen dåligt väder. Laboratoriet är ju ej heller vidare tilltalande. Sot och rök har satt sin prägel på allt. Inom denna månad torde jag väl vara färdig med kursen och


far väl sedan till Edinburg och
helsar på Walker. Rutherford sän-
der Din fru och Dig sin hjärtliga
helsning likaså Dewar och
Ramsay. Jag råkade dem bå-
da i London. Dewar var synn-
nerligen älskvärd. Han visade
mig hela institutionen och be-
skref noga hvar både Faraday
och den ena och andra stått
och gjort sina experiment.
Det var nästan med en helig
vördnad han demonstrerade
de gamla enkla apparaterna.
Han påstod att Du nästa år
skulle till London igen för
att hålla en serie föredrag.
Hur har det gått till på energi-
kongressen i Göttingen?

Two pages from the letter to Svante Arrhenius dated Manchester 2nd May, 1913. (Royal Academy of Sciences, Stockholm).

Laboratory of physics
University Oxford street
Manchester

2/V 1913
Dear brother Svante.
As you see I am now in Manchester and attend the course in radioactive measurements. I have also got a picture of what an industrial town means. Everything is sooty and dirty. Perhaps you also remember how it was when you visited here. Moreover, the weather has been quite bad. The laboratory is not very attractive either. Soot and smoke have left footprints everywhere. By the end of this month I suppose I have finished the course and after that I go to Edinburg and meet Walker. Rutherford sends his best regards to your wife and to you. Greetings also from Dewar and Ramsay. I met them in London. Dewar was very friendly. He showed me around in his institution and pointed out where Faraday and others have been standing performing their experiments. It was almost with sacred reverence he demonstrated the old, elementary apparatuses. ... I also spent two days in Cambridge and visited Thomsson [note: Thomsson written with double s] and his lab. It was quite crowded. In the chemical lab there was, on the contrary, lot of space. It was also very large. ... The town is quite interesting. It gives an impression of being in the Middle Ages. I have not met Donnan. He had not arrived in London when I left. He was expected on the last of April or on the first of May.

Yours devoted L.W. Öholm.

*****

Manchester May 21 1913

My dear brother Svante.
Thank you for your letter which I liked very much. I am working long days and I hope I can finish all the laboratory works by the beginning of June. They are about 50 altogether. I have already made some thirty of them. They are quite interesting as they are completely new to me. I will not stay here longer than necessary and I do not start any scientific project now. My intention was to learn the methods of measurements and for that I have had plenty of time. Manchester is not a nice town. Here everything is extremely dirty, the laboratory included. One looks like a chimney sweeper going home in the evening. When I have finished here I go to Edinburgh and meet

Walker, and thereafter to London. I believe I will be back in Stockholm around middle of July. By then I have had enough of bacon and eggs and dirtiness and rainy weather. It has been raining all the time ... I have bought some radioactive apparatuses for Physicum in Helsinki. I begged and talked to Tallqvist and he was ready to spend 500 Finnish [marks] and I could buy a lot, 3 fine electroscopes, one α, one β + γ and one for emanation. I have also actinium for 32. Can we have a few mgr. of radium, then we are well off. But how to get a standard without stealing one, will be hard. ... Yours devoted

L.W.Öholm

Öholm mentioned Tallqvist in his second letter. Hjalmar Tallqvist was Professor of Physics and head of the department in Helsinki. He decided how to use the money given to the department, and Öholm was lucky getting some of that money.

Öholm worked hard in Manchester, but one evening he went to the theatre to see the operetta Merry Widow by Franz Lehár. He was eager to see the performance and make comparisons as he had seen Merry Widow earlier in Helsinki, Stockholm, Copenhagen, Berlin and Vienna. Öholm told Arrhenius that the Manchester performance was a parody. The widow was very chic, but the actors performed like in a circus or variety show (gymnastics, clowns). However, the audience laughed and applauded. They liked the performance.

Earlier, still in Helsinki, Öholm had applied for the position of Professor of Physical Chemistry. In a letter from professor Aschan (Chemistry) in Helsinki, delivered to Öholm in Manchester, Aschan gave the names of the experts considering the merits of the applicants and ranking them. Öholm had to wait for the reports and he was eager to see them. Finally it turned out that Öholm had considerable merit and he was appointed Professor (Physical Chemistry) on February 11, 1915. He retired from this position in the summer 1939 becoming Professor Emeritus. Öholm died in 1944.

Oliver Heaviside – A Tragic Genius

Peter Ford

Formerly of the University of Bath, UK

Oliver Heaviside was born on the 18th May 1850 at Camden Town in North London and was the youngest of four boys. Two people who played an important role in his scientific life, George Fitzgerald and Oliver Lodge, were both born the following year in 1851. Camden Town was a working class area of London with a rather Bohemian atmosphere, which remains to this day. The family was poor and Oliver was unable to receive education after the age of fifteen although he had attended Camden House Grammar School where he came first in natural sciences in 1865. An early bout of scarlet fever left him somewhat deaf and this probably contributed to him becoming introverted and isolated from society. He was a definite loner throughout his life. An uncle was Charles Wheatstone one of the inventors of the telegraph and well known to people of my generation for the Wheatstone bridge used to determine the value of an unknown electrical resistor. Through this connection with Wheatstone, both Oliver and his elder brother Arthur were attracted to work in the telegraph industry. This was a time of the rapid installation of a global telegraph network. In 1866, 150 years ago this year and shortly after the end of the American Civil War, William Thomson, later Lord Kelvin, supervised the laying of the first transatlantic cable from Isambard Kingdom Brunel's gigantic ship the Great Eastern, the only vessel large enough to carry all the cable.

In 1867 Oliver travelled to Newcastle to join his brother Arthur who was working in the telegraph business. The following year he took a job on the Anglo-Danish cable, initially working at Fredericia in Denmark and then back in Newcastle. During this employment he gained experience at working in the most advanced electrical technology of the day. In 1873 he submitted a paper to the journal *The Philosophical Magazine* which gave a comprehensive analysis of the Wheatstone bridge. This drew considerable praise from both William Thomson and James Clerk Maxwell and encouraged Heaviside to devote himself fully to scientific work. The following year he resigned from the cable company and never held a regular job again. He spent the next fifteen years living with his parents in London working on electrical theory. He worked in almost complete isolation and was entirely self-taught. His chief discoveries were in the areas of the Maxwell's field theory and the propagation of telegraphic waves.

James Clerk Maxwell had laid the foundations of our understanding of electromagnetic behaviour in terms of fields and in 1865 he produced his classic electromagnetic field equations. The brilliant and flamboyant American physicist Richard Feynman regarded Maxwell's work as the most important event of the nineteenth century because of its far reaching consequences resulting in huge benefits to mankind. In 1873 Maxwell published his classic book *A Treatise on Electricity and Magnetism*. Despite the importance of his work, Maxwell did not make much effort to promote it. This can be partly explained by him becoming the first Professor of the newly created Cavendish Laboratory in Cambridge in 1871, which must have occupied much of his time and effort. Maxwell died prematurely from cancer in 1879 aged 48 and it was left to others to draw out the full implications of his ideas. These scientists are known as the Maxwellians and the three most prominent members were George Francis Fitzgerald, Oliver Lodge and Oliver Heaviside. It is interesting to speculate the amount of new physics that Maxwell might have discovered if he had lived for another thirty years.

Heaviside was fascinated by Maxwell's work, which he greatly admired, and made an in depth study of his book. It is very difficult to comprehend and master and Heaviside spent several years before he felt able to branch out on his own. He published many articles in the Journal *The Electrician* whose editor C.H.W. Biggs was a strong supporter of Heaviside's work. Eventually Heaviside was able to reduce the twenty or so equations appearing in Maxwell's work to arrive at the four fundamental electromagnetic equations which appear today in all books on electromagnetism. This viewpoint has been disputed by the Cambridge physicist Malcolm Longair who has recently written an article marking the 150th anniversary of Maxwell's discoveries in which he maintains that the four equations are contained within his original 1865 publication*.

The physicist Oliver Lodge, who at the time was Lyon Jones Professor of Physics and Mathematics at University College, Liverpool (the forerunner of Liverpool University), pointed out that implicit in Maxwell's work was the idea of the existence of electromagnetic waves having wavelengths which were very much greater than that of visible light. In the early part of 1888 he succeeded in discovering such waves which he generated by discharging Leyden jars along wires which then glowed in the dark 'with momentary luminosity' at the nodal points. He was able to measure the wavelength of these radio waves and found that they agreed with that predicted by Maxwell's theory. Lodge intended to present his new results at the meeting of the British Association, which was to be held that year in the city of Bath. (The British Association for the Advancement of Science was founded in 1831 as an annual forum for discussing the latest developments in science and the need to communicate this to the general public).

Before the meeting, he headed for a hiking holiday in the Alps. It was while on a train travelling towards the Alps that he read the most recent edition of *Annalen der Physik* and learnt that the German physicist Heinrich Hertz at Karlsruhe University had succeeded in generating electromagnetic waves in air and was able to demonstrate their reflective, refractive and interference behaviour which was similar to that of visible light. Lodge realised that Hertz was likely to get the credit for the discovery of electromagnetic waves as indeed has proved to be the case. However, the contributions of Lodge to the discovery of electromagnetic waves should be more widely appreciated and recognised. He subsequently had a distinguished career in physics making important discoveries in several different areas. In 1900 he was appointed to be the Principal of the new University of Birmingham, the first of the Redbrick Universities.

It says much for the vigour of mid-Victorian Britain that in the space of three consecutive years, between 1864 to 1866, the source of the River Nile was discovered, the Electromagnetic Equations were formulated and the first Transatlantic cable was laid. These world changing achievements took place during a time of grinding poverty, appalling living conditions and huge social inequality throughout much of Britain.

For the development and understanding of the whole area of electromagnetism, the 1888 British Association meeting at Bath was a pivotal event and a turning point in the fortunes of Oliver Heaviside, who up to that time was regarded as an eccentric and maverick scientist. The meeting was attended by Fitzgerald, Lodge and Thomson but not by Heaviside who true to his reclusive nature did not come. The president of the Mathematical and Physical Section was George Fitzgerald from Trinity College, Dublin. In his presidential address he emphasised the importance of the discovery by Hertz of electromagnetic waves in establishing the ideas of Maxwell's electromagnetic field theory. During the meeting several aspects of Maxwell's work were discussed. One of these was the effect of self induction on signals passing along telegraph and telephone lines, a topic which Heaviside had studied extensively.

In 1854 William Thomson had worked out a theory of signal transmission that treated the passage of a pulse of current as an example of simple diffusion. Although it gave a good account of how an initially sharp signal became distorted en route, due to the effects of resistance and capacitance, it was far from complete. Between the years 1874-81,

Heaviside wrote a series of highly mathematical papers in which he modified and extended Thomson's work. In particular he demonstrated the important effects of self induction which could radically alter the behaviour of a pulse of current as it diffused along a wire.

In 1887, working with his brother Arthur, who by that time had become an important engineer in the Post office telegraph system, they submitted for publication a paper on the use of added self induction to improve the transmission of telephone signals. In their joint paper Arthur and Oliver Heaviside discovered that, for a circuit where telephone lines were arranged in parallel, the effect of self induction was to reduce the effect of distortion on the signals passing along the line. Moreover, by loading the lines with an appropriate amount of self induction the effect of distortion could be eliminated. These ideas were in direct conflict with those of Sir William Preece, the head of the Post Office Telegraph engineers, who was adamant that self induction was the great enemy of clear transmission. As the boss of Arthur Heaviside, Preece was able to block the publication of this joint article. In addition, Oliver Heaviside believed that Preece was behind the sudden dismissal of Biggs as editor of *The Electrician* in October 1887 and the resulting cessation of his long running series of articles on many aspects of electromagnetism. As a result, Heaviside retained an antagonism and scorn for Preece which lasted for the rest of his life.

Although he was barred by Preece from publishing in *The Electrician,* at the end of 1887 Heaviside began sending a series of articles to the *Philosophical Magazine* on electromagnetic waves, which were highly praised by Lodge for their insight and mastery of difficult theory. Further warm praise came from Thomson in January 1889 during his presidential address to the Institution of Electrical Engineers during which he highlighted Heaviside's work on the theory of electrical transmission. While remaining very much a loner, Heaviside found himself much more at the centre of things and corresponded extensively both with Lodge and Fitzgerald, as well as Hertz, about his work. Most importantly, in an article of 1893, Heaviside suggested that his idea of improving telephone transmission by loading such lines with inductance might be achieved most effectively by inserting coils at suitable intervals along the line. This indeed proved to be the case and became a huge commercial success. Typically, Heaviside never patented his idea and the resulting huge profits went instead to Michael Pupin, who took out an American patent in 1900. The Bell Telephone Company of America built an extensive telephone network of these loaded lines.

For most people the name of Heaviside is associated with a layer named after him which occurs in the ionosphere. This arose from a throw away remark that he made in an article on telegraphy written for the 1902 edition of the *Encyclopaedia Britannica*. In it Heaviside suggested that the strange ability of radio waves to bend around the earth could be explained by assuming that there was a conducting layer in the upper atmosphere which acted as a guide. This was discovered in 1920 and appropriately named the Heaviside layer. In the musical *Cats*, Sir Andrew Lloyd Webber wrote a song *Journey to the Heaviside Layer*, based on the poems of T.S. Eliot:

Up up up past the Russell Hotel

Up up up to the Heaviside layer

Heaviside lived with his parents in London until 1889, when they all moved to the town of Paignton in the south coast of Devon. He remained there until their deaths. By the last decade of the nineteenth century Heaviside's reputation and status had been considerably enhanced. He was elected a fellow of the Royal Society in 1891 and the following year his collected *Electrical Papers* were published. In 1893 the first of the three volumes of his book *Electromagnetic Theory* (1893-1912) appeared.

The last few years of his life were rather sad and lonely. He became increasingly isolated and eccentric. He published little after 1905 and virtually nothing after the third volume of his *Electromagnetic Theory* appeared in 1912. He had occasional visits from his scientific friends. These included the Cambridge physicist G.F.C. Searle (1864 – 1954), who was the scourge of generations of students in the undergraduate physics laboratory at Cambridge. Searle described Heaviside as ‘a first rate oddity’ but added ‘never, at any time, a mental invalid’. Heaviside died early in 1925, his death being precipitated by a fall from a ladder towards the end of the previous year. His importance as a physicist and mathematician is now fully recognised.

> *I had an interesting e-mail exchange with Malcolm Longair who said that he in no way wished to belittle the efforts of people like Heaviside who did so much to make Maxwell’s equations more transparent. However, he argued that if you make the translation of Maxwell’s notation to modern form, the “standard” form of the equations falls out very naturally. This is elaborated in his recent article:
>
> Longair M. 2015 ‘……a paper…..I hold to be great guns’: a commentary on Maxwell (1865) ‘A dynamical theory of the electromagnetic field’. *Phil. Trans R. Soc*. A**373**: 20140473.
>
> This article was one contribution of 17 to a theme issue ‘Celebrating 350 years of *Philosophical Transactions*: physical sciences papers’.

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A Brief History of Dimensions

by
Jim Grozier

Dimensions are usually dealt with in an extremely rudimentary way in physics textbooks – the sole purpose being to prepare the student for dimensional analysis. This is a shame, because, behind the standard half-a-page treatment, there is a rich history, much of which is in danger of being forgotten altogether, at least by physicists; this despite the fact that, in a sense, it is still going on. Luckily for us, one of the few scholars to have delved into this history in some detail is our own John Roche, a founder member of the History of Physics Group, whose book has been an invaluable resource in my research.

In this article I will survey the history of dimensions from the 17th century to the present, and examine an oft-quoted thesis that the number of dimensions can or should only reduce with time. As a counter-example to this, I will investigate the case of the dimensions of angles.

Early Concepts of Dimension

The concept of dimension, in the sense in which it is used today in dimensional analysis, goes back at least to the 17th century. Descartes, in 1629, extended the familiar concept of spatial dimensions so as to include other quantities such as weight and speed: “length, breadth and depth are not the only dimensions of a body; weight too is a dimension ... speed is a dimension ... and there are countless other instances of this sort” [Roche p189]. For Descartes, a dimension was simply a “measurable property”.

John Wallis, writing in 1684, echoes Descartes’ classification: “besides the three dimensions LBT (Length, Breadth and Thickness) it hath acquired a fourth Dimension ... of Weight ... And if to those four dimensions we super-induce the fifth of Celerity: the Force arising LBTWC, is a Magnitude of five Dimensions” [Wallis p94-95]. For both Descartes and Wallis, dimension is thus a *qualitative* concept only: a way of classifying the various kinds of measurable properties.

In 1822, Joseph Fourier, in his *Analytical Theory of Heat*, added a *quantitative* aspect, assigning, to each term in his equations, an integer representing the dimension of that term *with respect to a given unit*.
Fourier was concerned that equations should not be affected by the choice of a particular system of units. In order for this to be the case, it is necessary for a change in the unit of any one base quantity to bring about a proportionate change in *every* term in the equation; Fourier expresses this by requiring that "every term ... have the same total exponent" with respect to a given quantity [Fourier, Art. 161]. He expands on this as follows: "suppose ... the unit of length to be changed, and its second value to be equal to the first divided by *m*. Any quantity *x* which in the equation ... represents a certain line *ab*, and which, consequently denotes a certain number times the unit of length, becomes *mx*, corresponding to the same length *ab* ... thus the dimension of *x* with respect to the unit of length is 1" [*ibid.*].

The "1" here is the exponent of *m* in the overall expression; had the factor by which *x* changed been m^2 or $1/m$, he would have said the dimension was 2 or −1 respectively, and so on. Notice that Fourier is referring to *length* as a generalised spatial dimension, rather than the distinct *length, breadth* and *depth/thickness* mentioned by Descartes and Wallis. This is because his argument is based around units, and he is clearly assuming that the three spatial dimensions will be measured in the same units.

Two other names which are often associated with the development of dimensional analysis in the 19th century are Gauss and Weber. Both were involved in defining new "absolute" units for electrical and magnetic quantities, which would be defined in terms of the units of the "fundamental" quantities, mass, length and time. This was not just an academic issue: reliable standards of electrical quantities like resistance were needed by telegraph engineers, particularly in the latter part of the century, when problems with undersea cables presented a major challenge.

A system of absolute units is coherent, and dispenses with the so-called "useless coefficients" required to convert, say, from gallons to cubic feet. From the dimensional formula for a particular quantity, one can simply read off the appropriate derived units – e.g. since energy has dimensions ML^2T^{-2}, its unit in the cgs system would be g cm^2 sec^{-2} (later named the erg).

626.] *Table of Dimensions.*

	Symbol.	Dimensions in Electrostatic System.	Dimensions in Electromagnetic System.
Quantity of electricity	e	$[L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-1}]$	$[L^{\frac{1}{2}} M^{\frac{1}{2}}]$.
Line-integral of electromotive intensity . .	E	$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-1}]$	$[L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-2}]$.
Quantity of magnetism / Electrokinetic momentum of a circuit .	m / p	$[L^{\frac{1}{2}} M^{\frac{1}{2}}]$	$[L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-1}]$.
Electric current / Magnetic potential	C / Ω	$[L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-2}]$	$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-1}]$.
Electric displacement / Surface-density	$\mathfrak{D}$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1}]$	$[L^{-\frac{3}{2}} M^{\frac{1}{2}}]$.
Electromotive intensity	$\mathfrak{E}$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1}]$	$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-2}]$.
Magnetic induction	$\mathfrak{B}$	$[L^{-\frac{3}{2}} M^{\frac{1}{2}}]$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1}]$.
Magnetic force	$\mathfrak{H}$	$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-2}]$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1}]$.
Strength of current at a point	$\mathfrak{C}$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-2}]$	$[L^{-\frac{3}{2}} M^{\frac{1}{2}} T^{-1}]$.
Vector potential	$\mathfrak{A}$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}}]$	$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-1}]$.

627.] We have already considered the products of the pairs of these quantities in the order in which they stand. Their ratios are in certain cases of scientific importance. Thus

From ‘Treatise on Electricity and Magnetism’ (1873) James Clerk Maxwell

Maxwell's absolute units and the problem of electromagnetic quantities

The next major contribution to the subject was by James Clerk Maxwell. In his *Treatise on Electricity and Magnetism* (1873) Maxwell introduced dimension as a property of *units* rather than quantities. He spoke of "ascertaining the dimensions of every unit in terms of the three fundamental units" and stated that "When a given unit varies as the n^{th} power of one of these units [i.e. fundamental units] it is said to be of *n dimensions* as regards that unit" [Maxwell (1873) Art. 2]. Maxwell's dimensional formulae consisted of products of powers of quantities M, L and T, which he described as the units of mass, length and time [*ibid*. Art. 3-5]. He provided a table giving the dimensions of all known electrical quantities, many of which featured, somewhat controversially, fractional powers of M, L and T.

James Thomson, however, objected to this talk of "powers of units". In 1878, he wrote that "much of the nomenclature and notation hitherto used is very confusing and unsatisfactory". Thomson was responding, not just to Maxwell, but also to J D Everett, who had proclaimed that the "the unit of acceleration varies directly as the unit of length, and inversely as the square of the unit of time". Thomson argued that we have no right to speak of such things as "the square of the unit of time", since units are not numbers, but entities derived from physical standards, and cannot be multiplied by one another, although they can be added, and hence multiplied by a number.

He suggested an alternative approach which recalled Fourier's method, based on changing the sizes of units. Thomson's *change-ratio,* the factor by which the unit is reduced, was identical to Fourier's factor *m*. Being a pure number, the change-ratio can be subjected to algebraic manipulation, whereas magnitudes and units are not pure numbers and hence cannot be so manipulated. [Thomson (1878) p452].

What Thomson's modification amounts to is redefining the terms appearing in Maxwell's dimensional formulae – M, L and T and products of powers thereof – as change-ratios. Thus, if the unit of mass is reduced by a factor M, that of length by a factor L, and that of time by a factor T, the *numerical value* of an expression representing energy, for example, will increase by a factor $\mathrm{ML}^2\mathrm{T}^{-2}$.

Maxwell's dimensional formulae had two major drawbacks. One was that the formulae he arrived at for the electrical quantities were not unique, and depended on whether one worked in the electrostatic or the electromagnetic system of units. These systems arose, of course, from defining electric charge in such a way that the constant in Coulomb's Law took the value unity, and defining magnetic pole strength in such a way that the constant in the corresponding magnetostatic equation was unity; this was fine as long as electrostatics and magnetostatics were seen as two entirely separate disciplines, but of course it transpired that they were not; one had to choose one or the other of these defining equations, and each then gave rise to a separate system of units and dimensions.

The other problem with Maxwell's formulae was the presence of fractional powers. Clearly, if one defines a unit of charge so that Coulomb's Law takes the form

$$F = \frac{q_1 q_2}{r^2}$$

for the force between two charges q_1, q_2 separated by a distance r, with mass, length and time as the base quantities, if the base units are reduced by the usual factors, the quantity $q_1 q_2$ must increase by a factor ML^3T^{-2}; this means that we are forced to conclude that charge has dimensions $M^{1/2}L^{3/2}T^{-1}$. In the electromagnetic system of units, however, the dimensions of charge are $M^{1/2}L^{1/2}$. And in both systems, almost all electrical quantities turn out to have dimensions which are fractional powers of M, L and T, with few exceptions, among them resistance – which has dimensions $L^{-1}T$ in the electrostatic system and LT^{-1} in the electromagnetic.

These fractional dimensions implied that the corresponding units would also be fractional powers of the base units – but it was by no means clear what this might mean. In contrast, the dimensions of resistance appeared to be the same as those of velocity (or the reciprocal of velocity, depending on which system of units one used). A debate ensued about whether resistance, in the electromagnetic system, could in fact be regarded as a velocity in some sense; experiments were even devised which could read off the value of a resistance in units of velocity, by measuring a real velocity [see e.g. Mitchell pp 23-25]. John Roche quotes William Thomson as announcing to a meeting of electrical engineers in 1883 that "we are going to learn [that] electrical resistance ... is a velocity" [Roche p202]. Daniel Mitchell, however, questions whether Maxwell or Thomson really believed that resistance (in the electromagnetic system of units, or its reciprocal, now known as *conductance*, in the electrostatic) was a velocity, rather than a quantity that was *expressed* as such in the particular experimental conditions of their thought-experiments, concluding that they "never seriously asserted" that it was [Mitchell p26].

The Strong View; Dimensional Analysis

This idea that resistance "is a velocity" – that, in other words, there is something more to two quantities having the same dimensions than simply being measured in the same units – brings us to what Roche has called "the strong view of dimension" and Mitchell refers to as a "physicalist" view: the belief that dimensions reveal the "essential" or "ultimate" nature of a quantity. This recalls the early work of Descartes and Wallis, in which dimensions were regarded as qualitative properties. W.W. Williams was a subscriber to this view; in an 1892 paper, he said that "the dimensional formulae may be taken as representing the physical identities of the various quantities, as indicating, in fact, how our conceptions of their physical nature (in terms, of course, of other and more fundamental conceptions) are formed". He saw this view as "more comprehensive and fundamental" compared with the interpretation of dimension as "merely a change-ratio" [Williams p237].

Percy Bridgman, writing in 1922, had little time for the strong view. He pointed out that "when there are so many kinds of different physical quantities expressed in terms of a few fundamental units, there cannot help being all sorts of accidental relations between them, and without further examination we cannot say whether a dimensional relation is real or accidental" [Bridgman p91]. A "real" dimensional relation between two quantities would, for Bridgman, imply a *physical* relation between them. Against this view, Bridgman argues that "the mere fact that the dimensions of the quantum are those of angular momentum does not justify us in expecting that there is a mechanism to account for the quantum consisting of something or other in rotational motion" [*ibid*]. The quantum – Planck's constant – is usually called the quantum of *action*, since it has the dimensions of [energy] × [time], or ML^2T^{-1} ; but angular momentum is also described as having these dimensions. Another pair of apparently dissimilar quantities which are regarded as having the same dimensions are torque and energy (both ML^2T^{-2}). Interestingly, Williams saw this very dimensional correspondence between distinct quantities – or rather, "the fact that difficulties are felt" in respect of it – as evidence *for* the strong view: if the formulae are to express nothing more than numerical dependence on the fundamental units, “we are not entitled to feel any difficulty in the matter” [Williams p238].

Bridgman’s book, *Dimensional Analysis*, is described by Roche as “the first book devoted entirely to dimensional analysis”. This emerging discipline formalised and built on Fourier’s dictum that every term in an equation should have the same dimension with respect to each of the base quantities, which enabled physicists to narrow down the range of possible formulae for a given quantity, and provided a means of checking formulae for consistency. It proved particularly successful in fluid mechanics.[1]

By the late 19th century, it was established that heat was a form of energy, and hence had the same dimensions (ML^2T^{-2}) so that, with the dimensions of electrical quantities also being expressed in terms of mass, length and time, there was a perception that *all* quantities had mechanical dimensions. This view is sometimes referred to as “mechanical reductionism”. It is well known that Maxwell had a mechanical model to explain electromagnetic phenomena (though whether Maxwell himself saw this as a literal explanation or simply an analogy, is an open question).

However, even at the height of the “mechanical reductionist” era, there were those who regretted the “suppression” of electrical and magnetic dimensions. A. W. Rücker introduced electrical and magnetic constants K and μ, respectively, of unknown dimensions, and produced a table giving dimensions of various electrical and magnetic quantities in terms of M, L, T and either K or μ. He based this on a table which he says Maxwell had given, “in which the dimensions of each of the electrical and magnetic units are given in terms of M,L,T and either electrical quantity or the strength of a magnetic pole” [Rücker p109].[2]

1 Note that the dimensional formula for a given quantity may take different forms depending on what it is being used for. Maxwell’s primary motivation was to “[ascertain] the dimensions of every unit in terms of the three fundamental units” – in other words, to find the appropriate absolute unit for a given quantity. But in Bridgman’s book many of the formulae are given, not just in terms of the base quantities, but sometimes also include derived quantities, such as velocity and force, because the nature of the problem at hand leads us to suspect that these derived quantities will be present in the formula.

2 Rücker does not, however, give a reference for this. Daniel Mitchell explains that Maxwell introduced a magnetic constant in a piece (co-authored with Fleeming Jenkin) entitled On the Elementary Relations Between Electrical Measurements, published in 1865, but removed references to this constant in a later edition published in 1873. [Mitchell p21n].

Williams developed this idea, and found possible dimensions for K and μ which led to "intelligible, natural and connected" dimensions for these quantities, which did not involve fractional powers; note, though, that neither Rücker nor Williams introduced a fourth base quantity – Williams' postulated dimensions for K and μ were still expressed in terms of the mechanical quantities M, L and T.

Dimensions in the 20th Century

The change-ratio concept gives dimensional formulae legitimacy; without it, we would have to *postulate* that dimensions – whatever they are – can be algebraically manipulated. Yet, strangely, explanations of dimensions in terms of change-ratios are very rare in the physics literature of the late 20th century. A typical textbook definition of dimension is that given by Serway & Jewett in *Physics for Scientists & Engineers*: "The word *dimension* has a special meaning in physics. It denotes the physical nature of a quantity" [Serway & Jewett p10]. Most textbooks give qualitative definitions like this one – which calls to mind the strong view of dimension – and omit any reference to change-ratios.

Change-ratios are equally rare in the philosophical literature. Brian Ellis describes dimensions as "the names of particular classes of similar scales for the measurement of quantities" [Ellis p139], which is at least consistent with the change-ratio concept, since the process of changing units is effectively a scaling process. Henry Kyburg's definition is similar: the dimension of a quantity "is to be construed as the set of its magnitudes" [Kyburg p163]. Mario Bunge defines dimensions similarly, as "species" of similar quantities "such as the class L of all length-like quantities" [Bunge p2]. In fact, I have found only one recent book that *does* feature the change-ratio definition – G.I. Barenblatt's *Scaling, self-similarity, and intermediate asymptotics*, published in 1996, and even here, the author does not actually use the term "change-ratio", though his definition is identical to Thomson's. [Barenblatt p32].

The existence of a fourth, electrical, base quantity was formalised by the introduction of the ampère as a base unit by the Conférence Générale des Poids et Mesures (CGPM) in 1948. This four-dimensional "MLTC" quantity space (where C stands for current) gets rid of the problem of fractional powers: all electromagnetic quantities now have dimensions which are products of integer powers of the change-ratios of these four base quantities.

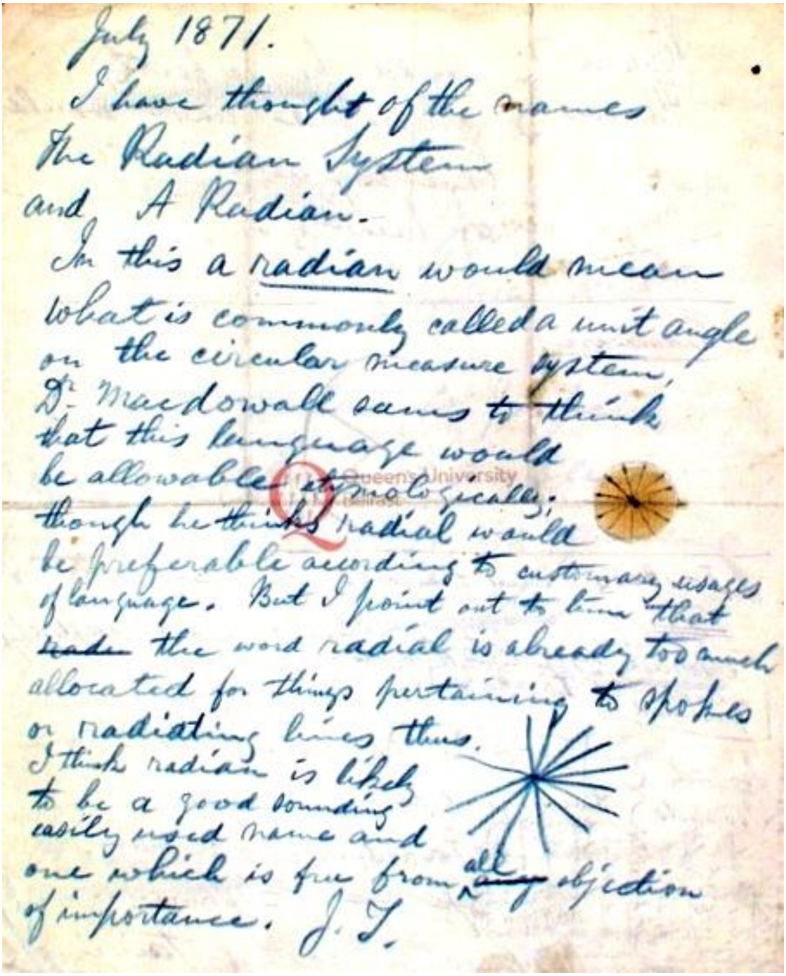
In 1954 the CGPM defined a base unit of thermodynamic temperature, and in 1967 this unit was named the kelvin. Units of "amount of substance" (the mole) and luminous intensity (the candela) followed. This brought the number of base units (and hence, by implication, the number of dimensions) up to 7.

The metrology community appears to have accepted the new base units relatively quickly; for instance, in a 1964 book, R.C. Pankhurst of the National Physical Laboratory included a fourth dimensional constant (which he wrote as $1/\varepsilon$) in electrical equations [Pankhurst p43]. Yet the view that there are really only three fundamental quantities, and hence three dimensions, did not go away; many authors continued to regard all quantities as having mechanical dimensions only. This can be seen in the continued use of formulae for electrical quantities that only make sense in systems of units based on an MLT space, such as that for the fine structure constant, α. Even textbooks have been slow to adopt the new approach: as recently as 1996, a general physics textbook was published which included the statement that "all measurements can be reduced ultimately to the measurement of length, time and mass. Any physical quantity, no matter how complex, can be expressed as an algebraic combination of these three basic quantities" [Fishbane, Gasiorowicz & Thornton, p. 9]. The reluctance to accept a separate electrical dimension may stem partly from the view that the number of dimensions can only diminish with time, as we come to regard quantities, previously seen as distinct, as sufficiently closely connected that one can be defined in terms of the other; and indeed, that they *should* diminish with time, even beyond MLT – perhaps by defining length in terms of time [see *e.g.* Lévy-Leblond] – and even, perhaps, ultimately reaching zero [see *e.g.* Duff et al.]. This drive to constantly simplify our physical laws is of a piece with the mechanical reductionism of the late 19th century.

One question that appears at odds with this narrative is that of the dimensions of angles.

The Dimensions of Angles

Krantz *et al.*, in their monumental work *Foundations of Measurement*, describe angle as "the bastard quantity of dimensional analysis, about which everyone seems a bit uncomfortable" [Krantz et al. p455]. Angle is often described as a dimensionless quantity, yet it is also a measurable, physical



July 1871.
I have thought of the names
The Radian System
and A Radian.
In this a radian would mean
what is commonly called a unit angle
on the circular measure system,
Dr Macdowall seems to think
that this language would
be allowable etymologically
though he thinks radial would
be preferable according to customary usages
of language. But I point out to him that
~~made~~ the word radial is already too much
allocated for things pertaining to spokes
or radiating lines thus.
I think radian is likely
to be a good sounding
easily used name and
one which is free from all ~~any~~ objection
of importance. J. T.

From James Thomson’s notebooks (courtesy Queens University Belfast) quantity like length, which we might feel ought to have dimensions. The origin of this conundrum is of course the widespread use of the radian as an angular measure; an angle in radians is the ratio of subtended arc length to radius, so as a ratio of similar quantities it does indeed appear to be dimensionless. The radian was invented by Euler in 1765, although it was not named until over a century later, when James Thomson came up with the name as a contraction of "radial angle". It has become an invaluable tool for theoretical physicists, because of the simplicity it affords to various formulae, in particular to the Small Angle Approximation, and thence to Taylor series for trigonometrical functions. But it is a strange unit, because it is *theoretical* only; we never measure angles in radians, but only use them in derivations. The SI literature nominates the radian as the unit of angle, but describes it as a "dimensionless derived unit" [Bell p15].

A reductionist narrative might suggest that, before Euler's time, angle *was* considered a dimensioned quantity, and that the demise of an angular dimension was simply a part of the onward progress of science in its quest to simplify natural laws, in the vein of the confluence between work and heat in the mid-19th century; see, for instance, Lévy-Leblond on the "unification" of physical concepts and the corresponding disappearance of fundamental constants. Yet if we look at the earlier writings of Descartes and Wallis previously quoted, we will find no mention of any angular dimension. This is not because they had not considered the question of angle; Wallis, at least, wrote an entire treatise (entitled *Treatise on the Angle of Contact*) in which he included angles in a class of properties that he called "inceptives": "the Angle ... tho it be no whit of Distance, yet is inceptive of Distance, and so soon as ever we be past the Angular Point, the legs are actually Distant" [Wallis p96]. This classification may stem from the perception that the angle exists *at a point* (what Wallis calls the Angular Point) and therefore cannot have dimension in any sense of the word. (Later, though, he does admit that "these Inceptives ... have a magnitude of their own") [*ibid*].

Fourier – writing in his *Analytical Theory of Heat* – stated that "Angles, sines and other trigonometric functions, logarithms or exponents of powers are, according to the principles of analysis, *absolute* numbers which do not change with the unit of length; their dimensions must therefore be taken equal to 0, which is the dimension of all abstract numbers" [Fourier art. 161]. Note that, although this was written some 60 years after Euler's introduction of the radian, he does not seem to be appealing to circular measure here, but rather to the fact that angles appear in mathematical formulae which have no connection with measurable quantities. If angle was traditionally included among geometrical, rather than concrete, properties, this might explain why it has been thought inappropriate to consider it as having dimension; although the same could be said of distance, of course. (However, he may perhaps be confusing angles with *phases* here; I will have more to say about this distinction later on).

K.R.Brownstein, in a 1997 paper, describes the situation regarding the interpretation of the concept of angle as “not very satisfying”, and attempts to resolve this problem. He highlights this problem with examples from elementary mathematics and physics which require, on insertion of numerical values and units, either the deletion or insertion “seemingly at will” of additional units to make the equations give a consistent answer with the right units.

Brownstein starts from several assumptions, including that “each symbol used for a physical quantity must have definite physical dimensions”, and an assumption of unit-invariance: “the actual numerical value as well as the associated units of any symbol need not be specified until one has to evaluate an expression containing that symbol” [Brownstein p606]. He then states that “From our point of view, the concept of “angle” has a meaning which is not tied to any relation of the form $\theta = s/R$” [*ibid*. p607]. Indeed, using Norman Campbell’s theory of measurement, it is easy enough to formulate a procedure for defining and measuring angles which does not require any concept of an arc or a circle. Angle is then not linked to length, but must be regarded as a separate base quantity. Trivially, if we apply the change-ratio A to the unit of angle, the numerical magnitude of any given angle will increase by the same factor A, so that angle can be described as having dimension A.

Brownstein re-writes the equation relating angle to arc length as $s = \square R\theta$, where “□” is a constant with the dimension A^{-1}.[3]

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3 Brownstein explains that the square symbol is a Hebrew letter, chosen as a pun – both linguistic and visual – on his main thesis of “treating angles squarely”. However, since it may not only become confused with the d’Alembertian operator of relativistic quantum mechanics, but – worse still – may be interpreted as the default character which appears in word-processed documents when the software cannot interpret a particular piece of code, we will avoid using it.

He also draws a distinction between "geometric" trigonometric functions which map angles to numbers, and more generalised trigonometric functions which map numbers to numbers, by giving the former a capital letter (e.g. Sin) and the latter a lower-case letter [*ibid*. p609]; these "angle-like" numbers Brownstein defines as *phases* [*ibid*.] Since it is these latter functions that tend to be associated with Taylor series, the complication introduced by the use of Brownstein's constant would not arise here, because we are not dealing with angles.

W.E.Eder endorses this view. He quotes R.D.Stiehler's definition of plane angle as "the divergence between two intersecting straight lines ... The magnitude of the divergence can be expressed in a variety of measurement units, such as radian, grad, degree, minute, or second. The magnitude is *not* a number without units." [Eder (1980) p320]. And Henry Kyburg says of angle: "measured directly, it has its own dimension, of course" [Kyburg p129]. This reminds us that it is only when the "measurement" is of the indirect kind in which an arc length is divided by a radius, that the question of angles being dimensionless arises. J-M. Lévy-Leblond also welcomes Brownstein's constant, although, as an ardent reductionist, he welcomes it only to prophesy its imminent demise in the onward march of scientific progress. [Lévy-Leblond p815].

Brian Ellis, in dealing with a problem associated with the displacement of the sides of a cube of material subjected to a shearing stress, multiplies the angular displacement by "a new scale-dependent constant dependent not only on the choice of stress scale, but also on the choice of angle scale". This constant is clearly on a par with Brownstein's. Ellis points out that such a units-invariant format gives us more information than one restricted to a particular angular scale; the moral of which, he says, is that "we can only get out of our dimensional formulae what we put into them. And, if we choose to express our laws with respect to particular scales, we shall inevitably impoverish our dimensional formulae" [Ellis (1966) pp147-148].

Brownstein recommends the reclassification of angle as a separate (8^{th}) SI base quantity. In fact, the SI system is to be overhauled in 2018 (see Sophie Osiecki's article on page 7 about this) but this will not affect the status of angle. Nevertheless, at a conference in 2015, a former Director of the BIPM said that "this question of angle, how you measure it, and what you call it, is indeed something that is still being discussed in the metrological community".[4]

4 Terry Quinn, The Making of Measurement conference, Cambridge, July 2015.

One interesting consequence of angle being upgraded in this way is that it would remove the degeneracy cited by Bridgman against the strong view, since then such quantities as torque and energy would take distinct dimensions, and this might add further fuel to the debate about the exact status of dimensions.

Conclusion

I hope to have convinced the reader that there is far more to dimensions than is commonly appreciated – particularly by physicists. I have struggled to do justice to the topic in this short article, and would refer anyone who is interested in researching it more thoroughly to the bibliography below – particularly the asterisked works.

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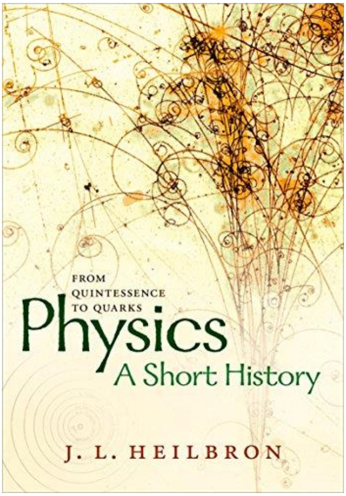
Conference abstract rejected!

It has come to our notice that the following abstract was rejected for a recent conference. Unfortunately the Data Protection Act prevents us from revealing which conference and why, but we understand that it was regarded as too brief.

‘A scrutiny of Lord Rayleigh's personal diaries reveals that he always cut his toenails on a Thursday morning. What influence this had on his scientific work is discussed with reference to the fact that there was lunar eclipse on his 18th birthday. There is some evidence that later in life the 3rd Baron was dismayed at the non-appearance in the ‘Spirits Bottle’ of any messages from his deceased colleague, Sir William Crookes.’

Book Reviews



Physics, a Short History: from Quintessence to Quarks

J L Heilbron

OUP *2015*
ISBN *978-0-19-874685-0*
228pp *Hardback 10.99 GBP*

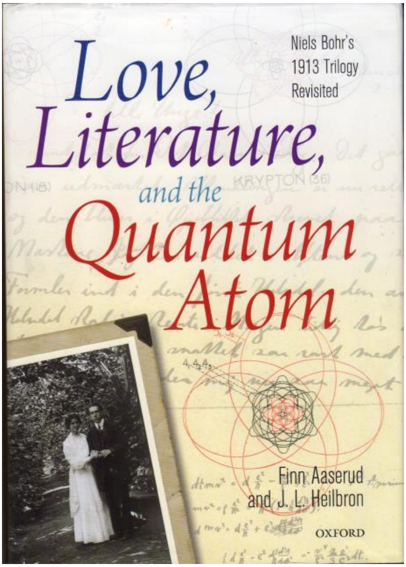
Reviewed by:
Derry W Jones, Applied Sciences University of Bradford

John L Heilbron (b 1934) was educated in history and physics at the University of California, Berkeley, of which he eventually became Vice-chancellor, 1990-94. His post-retirement scientific life has been spent largely at the Museum of the History of Science at Oxford, England, from where he has written books on polymath Galileo (the father of science), Rutherford and (with Finn Aaserud) Niels Bohr (not forgetting his wife). All this seems a pretty good foundation for a short (228 fairly small pages) history of physics, and it shows in a superb but painstaking monograph. *Physics, a Short History* ranges from Plato and Aristotle to the 1954 US Bevatron and a picture (the 26 figures in the book include drawings and photographs) of the route of CERN's Large Hadron Collider. All this is supported by frequent use of Latin (and occasionally of other languages) and the elegant and witty application of English. Heilbron's overall aim is to see how physics or philosophy (between logic and ethics) in Greek times, which he calls *physica*, transforms into modern international physics accomplished by paid physicists who may be called on by governments for advice.

A chapter on Islamic science describes how language difficulties are overcome and mathematics is called on to tackle astronomy as it emerges from astrology. Universities were formed and William of Ockham taught that the fewest causes should be invoked to explain apparent relations between entities. The volume is barely halfway through before Francis Bacon (1561-1626) and Rene Descartes (1596-1650), developing optics, meteors and analytical geometry, are introduced. In the seventeenth century, Newton, as president of the Royal Society, is described as the Napoleon of the Scientific Revolution. There are other delightful expressions such as that of prosaic physicists being 'stampeded' into believing, after the Compton effect, the concept of the proton.

As Physics becomes a recognized discipline and profession, Chapter 5 begins with a drawing made around 1900 of the physical laboratory building (in which I later studied) at Manchester, although the chapter then deals with earlier European physicists. In particular, the researches of Kelvin, JJ Thomson and Rutherford led them to modern classical physics, especially of motion and meteorology. Chapter 6, 'from the old world to the new', with industrial physicists and Ph Ds doubling per decade in each of the 20s and 30s, starts with the earlier work of von Laue, the Braggs and Einstein. World War I encouraged progress (though lamentably not of Henry Moseley) and led to the contributions of Bohr, Born, Heisenberg and Schrodinger and the interpretation of psi squared as a probability. After World War II, despite an emphasis in the UK on radar, there was an increasing concentration of physics progress in the USA (and the USSR) around atomic and nuclear bombs.

The final short Chapter notes that the Royal Society's 19th century review, under Bridgewater's bequest, tried to unite traditional belief with science but that both Einstein and Bohr rejected the notions of a personal deity and an afterlife. There have been several searches since then for extraterrestrial intelligence and a theory of everything. Steven Weinberg's 'the more we know about the universe, the more it is evident that it is pointless and meaningless' doesn't prevent attempts at comprehension. Finally, Heilbron hazards that several theories of everything might emerge 'in the fullness of time'. References for each chapter amount to eight pages in total while background books are given in another five-page list. The 14-page index gives dates for the many physicists and others mentioned.



Love, Literature, and the Quantum Atom

Finn Aaserud and John Heilbron

OUP *2013*
ISBN *978-0-19-968028-3*
296 pp *12.45GBP*

Reviewed by:
Edward A Davis, University of Cambridge

This is in effect two books in one. The first part, written by Finn Aaserud, draws on previously unpublished correspondence between Niels Bohr and his fiancé, Margrethe Nerlund, in addition to letters to his family, in order to assess the influence of his personal relationships on his scientific achievements. The second part, by John Heilbron, describes in detail Bohr's professional life and analyses the progress of this in the light of his literary interests, his interactions with colleagues, and his personal beliefs and ambitions.

The date of publication of the book coincides, not accidentally, with the Bohr trilogy – three papers published in the Philosophical Magazine, all with the title 'On the constitution of Atoms and Molecules' (with the second two having the subtitles 'Systems containing only a single nucleus' and 'Systems containing several nuclei', respectively). The papers are reproduced as facsimiles at the end of the book. They build on the

Rutherford nuclear model of the atom, in which essentially all of an atom's mass is concentrated in a volume tiny compared to the size of the atom – a nucleus containing a positive charge. Bohr's significant development of the model was to postulate and analyse the way in which the compensating negative charge, in the form of individual electrons, is distributed in the volume surrounding this nucleus. The visualization of electrons whizzing around the nucleus like planets around the sun is ingrained in the minds of all schoolchildren and adults alike, as well as being used in countless logos, in spite of the fact that it is an inappropriate and largely incorrect picture.

Neils Bohr (1885-1962) was born and raised in Denmark. His doctoral dissertation on the electron theory of metals was submitted to the University of Copenhagen in 1911 – the year in which Rutherford's classic paper appeared. After a short time at the Cavendish Laboratory in Cambridge - where he appears to have become disillusioned with the lack of interest shown by J J Thomson in his previous work - he joined Rutherford in Manchester for just four months in 1912. It was then that Bohr had his revolutionary insights.

Bohr's answer to the question of why electrons circulating around a nucleus did not radiate energy, owing of their centripetal acceleration, and hence spiral into the nucleus (just as satellites encountering air resistance eventually return to Earth), was simply 'the inadequacy of the classical electrodynamics in describing the behavior of systems of atomic size'. Furthermore he proposed that, unlike orbits in the corresponding gravitational analogy, there were only a limited number of allowed orbits for the electrons. Then in a bold, and to many at the time unjustified, mixture of classical and quantum theories he calculated out the radii of the orbits and postulated that an electron could jump from one orbit to another, losing or gaining energy in units of hv, where h is the Planck constant and v is the frequency of electromagnetic radiation emitted or absorbed. This condition, as he shows in the first paper of the trilogy, is equivalent to the electron in each orbit having an angular momentum quantized in units of $h/2$. An immediate result from this analysis was a formula for the frequencies of the spectral lines of hydrogen in excellent agreement with experimental data.

Although, perhaps surprisingly, no biography of Niels Bohr has yet been written, one of the authors of the present book (JH) coauthored, with T S Kuhn, an article in 1969, which investigated the scientific background for Bohr's atomic model. In the book reviewed here, the same author précises this analysis and then describes in detail the various steps in Bohr's journey into the atom leading to the publication of the trilogy. It is quite a complicated story – difficult to follow by reading the original papers, mainly because they contain several different formulations of his basic tenets and several inconsistencies as Bohr develops his ideas. Any reader of the papers is advised to have Chapter 26 of this book at the ready for detailed explanations!

In the words of John Heilbron, 'Bohr's achievement welded together of pieces of contradictory theories with little justification other than success. The input was broken bits, the output a gem. One thinks of Goethe's little dog, who lived on broken glass and excreted diamonds.'

The title of the book portends the attempt by the authors to make a case for the love between Niels and Margrethe, in addition to their shared passion for literature, as being influential in Bohr's groundbreaking ideas and formulations. There is no doubt that the correspondence between the couple reveals Bohr's innermost thoughts and ambitions, and that he sought Margrethe's support throughout his time in Cambridge and Manchester whilst they were engaged but before they were married.

"*I had completely forgotten*", he wrote from Cambridge, "*how amusing it was for me to read Larmor's book. When I read something that is so good and grand, then I feel such courage and desire to see whether I too could accomplish a little something. Tell me, will you help me?*"

In reply to a similar plea in an earlier letter, Margrethe replied,

"*Dear dear Niels, you ask whether I will care for your work, oh dear Niels, I cannot at all describe to you how much I love you and how much I love your work, and I cannot distinguish you from it and I cannot describe to you how much I long for the future, for being allowed to help you a little sometime, if only I can.*"

Margrethe and Niels, Easter 1912

The exchange of letters on a frequent basis – on occasions Bohr wrote more than one per day – are fascinating to read in that they provide glimpses of Bohr's thoughts during the period leading up to his great insights and subsequent to the publication of the trilogy. They reveal a person who, on arriving in England, lacked self-confidence - perhaps not surprisingly in his early days at the Cavendish Laboratory, struggling with the language and the traditions of Cambridge colleges, and working in the laboratory of the discoverer of the electron, J J Thomson. In Manchester with Rutherford he was more comfortable and his thoughts were beginning to surface. He writes to Margrethe modestly:

"*It may be that it is very silly and that it amounts to nothing at all, as usual, but I believe that I have found out a little bit. What I can do with it, and what can come out of it, I do not know at all. Tell me, my own little darling, whether you are sad because I am so silly that my blood can get feverish over so little and because I am longing so unspeakably for the time you are going to keep account of my thoughts and make it grow for me in case I possess something that has the ability to grow*."

Later, after the publication of the trilogy, and with the encouragement of Margrethe to whom he was now married, Bohr attended the 1913 annual meeting of the British Association for the Advancement of Science held in Birmingham and chaired by Oliver Lodge. Recognition was now forthcoming, as he reported in a letter to his new wife:

‘*Jeans, gave a very beautiful and sympathetic presentation of my theory. I think he is convinced there is at least some reality behind my considerations. Here too (as in Cambridge) I have not been able to talk to him so much, he is very reserved, but I have come to be so fond of him, I think he is such a fine man. You should only know how endearing Lorentz is, and how in the discussion, in the wisest and at the same time most amiable and firm manner, he reproached J J Thomson and the others of the old school. I will meet him this afternoon at Sir Oliver Lodge's; you can be sure I am looking forward to it*..’

Niels could now place himself within the physics community as part of the ‘new school’. In 1920 Bohr founded the Institute of Theoretical Physics at the University of Copenhagen, now known as the Niels Bohr Institute. In 1927 he was a principal participant at the Fifth Solvay Conference where the world's most notable physicists met to discuss the newly formulated quantum theory.

In the Preface, the authors state that the book ‘commemorates the centennial of the creation of the Bohr atom, or, rather, the Bohrs’ atom, since from a psychological perspective it might be said to belong to the work of both of them’. This seems a little overgenerous to Margrethe, although it is clear that the support she gave to her fiancé was incredibly important in those formative years abroad. Later in his life that support grew stronger. One custom derived from Bohr’s time in Cambridge that became the norm when he became Director of his own Institute was the equivalent of J J Thomson’s wife, Rose, serving tea and playing host to students – a practice that Margrethe undertook with aplomb in Copenhagen.

Sir John F.W. Herschel
The Forgotten Philosopher

Eileen Shorland

No picture available

Herschel Family Archive
(2016) 15GBP
ISBN 978-0-9531772-1-9
(For availability see page 58)

Reviewed by:
Peter Ford, Formerly Chair of William Herschel Society, Bath

Bath is a World Heritage City which attracts annually thousands of tourists. They come to view the Roman Baths, the magnificent Georgian Crescents and to walk in the footsteps of Jane Austin. Few visitors realise that the Bath area has been an important region for science, medicine and technology. An important scientist was the astronomer William Herschel, who came to Bath as organist and director of music at the fashionable Octagon Chapel. Whilst here he developed an interest in astronomer and rapidly became an expert maker of telescopes. It was while carrying out a systematic study of the night sky, together with the assistance of his sister Caroline, that in March 1781 he discovered the planet Uranus, the first planet to be found since antiquity. Herschel later moved to Observatory House in Slough, Berkshire to become the King's Astronomer. He married quite late in life to Mary Pitt, a wealthy widow, and they had a son, John Frederick William who was born on March 7th 1792.

This interesting biography of John Herschel was written by Eileen Shorland his great granddaughter. The book has had an interesting gestation. It was written in the late 1960s and the manuscript was deposited in a leather trunk, which had originally come from Observatory House. It was while John Herschel-Shorland, the son of Eileen Shorland, was examining the contents of this trunk, which had come into this possession following the death of his mother in 1980, that he came across the manuscript and this has recently been published by the family.

John grew up in Observatory House under the shadow of the famous 40 foot telescope, a leviathan which could be seen for miles around Slough. Whereas William Herschel had received only a rudimentary education, no expense was spared on that for that for John. He was educated at private schools, which included a brief spell at Eton, as well as having private tutors at home. John Herschel proved to be an outstanding student and at the age of seventeen went up to St John's College, Cambridge having been awarded a Founder's Scholarship. He greatly enjoyed life at Cambridge and made some lifelong friends, which included Charles Babbage and George Peacock. At that time much of the teaching in Cambridge was outdated and archaic. In particular, the teaching of mathematics was in a moribund state due to adhering to Newton's Principia. Herschel, Babbage, Peacock and others rebelled against this teaching and were anxious to introduce new methods, particularly those developed by the great French mathematicians such as Laplace and Lacroix. In this they had some success. Herschel graduated in 1813 as Senior Wrangler with Peacock as Second Wrangler.

Like many young people today, John Herschel was unclear as to what to do after graduation. He was pleased to be elected Lay Fellow of St John's College. In the autumn of 1813, he made the impulsive and somewhat strange decision to study law and registered at Lincoln's Inn, London. While training to become a barrister he still maintained his interests in chemistry and mathematics and a paper that he wrote on *Finite Differences* was warmly praised by Laplace. By 1816, Herschel appeared to be having a pleasant lifestyle, working for the Bar at Lincoln's Inn and frequently visiting Cambridge and his family at Slough. However, his health began to deteriorate and his medical doctor strongly advised him to move away from London where the smoky and polluted atmosphere was playing havoc with his lungs. At the same time his father, who was now seventy eight years old, was also having his own health problems and found the strain of continually observing the heavens was becoming too much. With considerable initial reluctance on his part, John Herschel was encouraged to abandon his career as a barrister and take over the observing work from his father. He undertook a "Review of the Catalogue of Double Stars, discovered by William Herschel and Others". John Herschel rapidly learnt to cast and polish mirrors and make telescopes as well as develop the arts of observing. Slowly he found that he began to enjoy this work and realised that he had found his vocation. However, he was also active in other areas such as being appointed by the Admiralty to the 'Board of Longitude' and maintaining contact and having wide ranging discussions with his friends from Cambridge.

In 1820, a group of enthusiasts, which included John Herschel, founded an astronomical society in London. Sir William Herschel was reluctantly persuaded to become the first president, although he never attended a meeting and died shortly afterwards in 1822. By 1830 it became the Royal Astronomical Society, the oldest astronomical society in the world, and today heading towards its bi-centenary. Around this time a crisis occurred in the famous Royal Society of London, which involved John Herschel and some of his friends. The aged President Joseph Banks, who had been in the post for some forty years, and dominated the Society died in 1820 at the age of eighty three. Finding his replacement was made difficult despite the fact that the most obvious choice was Sir Humphry Davy. However, many people including Herschel were anxious not to choose Davy, who had become overbearingly arrogant, and a group of people hoped that William Wollaston would apply. However, he declined and Davy was duly elected to become the next President remaining there for a further seven years.

John Herschel returned to Slough to continue his astronomical observations having by now taken over completely from his father who had become very old and frail. He was increasingly anxious to acquire a good wife having had earlier disappointments in love, the cure for which was an extended trip on the Continent. It was during one of these trips in 1822 that his father died, John learning about this while he was visiting relatives in Hannover. He now became head of the Herschel household and this created additional work and responsibilities. John Herschel married Margaret Stewart in 1829. She was the elder daughter of the late Dr Alexander Stewart, who had been a Minister at the Cannongate in Edinburgh and a well known theologian and writer on religious matters. His widow, together with her four sons and two daughters, had recently come to live in London. John was attracted by Margaret's good looks, attractive personality and easy going temperament. Despite their age difference, he was thirty six years old and she only eighteen, it was a highly successful and loving relationship and together they had twelve children, nine girls and three boys, several of whom became prominent in their day.

William Herschel had once casually remarked to his son that the work on cataloguing the stars would only be complete if similar observations could also be carried out in the southern hemisphere. He realised that he would never be able to do this himself but the idea remained implanted with his son. His mother died very early in 1832 and John Herschel realised that if he did not go soon to the southern hemisphere he would be too old. His wife, being much younger, was also enthusiastic despite already having three young children. He decided to go without having any government finance since he believed that this would mean that he was under no obligation to anyone. Herschel initially had thought of travelling to Calcutta but realised that the dust there would play havoc with his telescope mirrors. Eventually he decided on the Cape of Good Hope at the southern tip of Africa, which was a much shorter journey. The 20foot telescope in Observatory House was dismantled and carefully packed for transport to South Africa. All told six wagons, each pulled by eight horses, made the journey from Slough to Portsmouth for embarkation on the ship from Portsmouth to the Cape. In those days, sea travel was by sail and therefore highly dependent on the wind conditions. John Herschel, his wife, family, servants and equipment left England in November 1833 and the journey to the Cape of Good Hope took about two months. It was uneventful apart from appalling weather conditions in the Bay of Biscay.

The family lived at a delightful Cape Dutch house called Feldhausen situated several miles from the centre of Cape Town. Here he quickly erected his telescope and began a systematic and meticulous survey of the night sky over a period of several years. During this time he was able to observe the passage of Halley's Comet. Nowadays there is no evidence of his house but the site of his telescope is marked by an obelisk situated in a primary school in the suburbs of Cape Town, next to which I have been photographed. In addition to his astronomical observations, Herschel, and especially his wife Margaret, was able to observe the enormous variety of flowers and plants in the Cape area as well as explore the geology and features of Table Mountain. It is an area of stunning beauty, which I know well. Cape Town was a regular stopping off point for voyages between Europe travelling to India and the Far East and there was a regular flow of ships arriving and departing. The Herschels were prominent in the social and intellectual life of the city. An important visit was that made by Captain Fitzroy and his ship H.M.S. *Beagle* and it was during this that Herschel met Charles Darwin. In addition to all his scientific work, he played an important role in setting up an excellent educational system in the Cape. The family spent just over four eventful and productive years there before returning to England. The return journey was as long and uneventful as the outward one, their ship finally docking in the Thames in May 1838.

An enormous banquet was held in his honour to mark his return. 1838 was the year of the Coronation of Queen Victoria, which he attended in Westminster Abbey. As part of the Coronation celebrations, he was offered a baronetcy which he reluctantly received. Herschel's main wish on his return to England was to be able to settle down quietly at home and write up his work from the time spent in the Cape as well as to have the peace and lack of distractions to be able to pursue his researches in private. Hence he refused the offer of several Professorships as well as to stand for Parliament as a member for the Cambridge Colleges. He was anxious to meet up again with his many friends and also to see some of the technological progress which had been made during his absence. In particular, there had been enormous advances in the development of the railways. He recounts a two day journey by train and stage coach to attend the British Association Meeting in Newcastle in 1838.

Shortly after his return to England, the problem arose as to where he should live. He had an ever burgeoning family increasing by about one new child each year. Observatory House was becoming too small to accommodate them all. After extensive house hunting they decided to move to the quiet village of Hawkhurst in Kent, which went back to the time of William the Conqueror. They settled into a very practical Georgian house called Collingwood which had extensive grounds for Herschel to plant his large collection of bulbs from the Cape and sufficient outbuildings for him to house his equipment. Just before leaving Slough for Collingwood, he carried out some important work in connection with photography. As early as 1819, he had discovered that sodium hyposulphite (hypo) was an effective way of fixing images which had been projected onto light sensitive paper. The 1830s marked the real beginning of the science of photography pioneered by people such as Louis Daguerre in France and William Henry Fox Talbot in England. Herschel collaborated with Fox Talbot in the development of improved fixing agents and coined the terms “positive” and “negative” in connection with photography.

Herschel spent many happy years at Collingwood carrying out research, attending meetings and playing an active role in the scientific community centred on London. An important event took place in November 1850 when he was asked to become the Master of the Mint following in Newton’s footsteps. This post had been held by many of the most prominent scientists of the day and generally involved little work. However, change was now on the way. The current structure and organisation of the Mint had become totally unsatisfactory with counterfeiting and theft being rampant. Herschel was asked to sort out the situation and he threw himself into the work with his customary zeal and effort. Under his leadership, there began a thorough reorganisation of the Mint and things gradually began to improve.

An important event occurred in 1855 with the birth of his final child, a daughter who was named Constance. At that time, John Herschel was sixty three years old while his wife was in her forties but gave birth without trouble. The little girl grew up and became Constance Lubbock and she wrote an important biography of the Herschel family called the *Herschel Chronicle*. By this time, Herschel's health was beginning to deteriorate and soon he was spending many hours sitting at home in a bath-chair. He enjoyed watching his children growing up and his mental capacity was still fine despite looking so old and haggard with a large shock of white hair. This can be seen in a well known photograph by Julia Margaret Cameron, who was a famous portrait photographer at that time. The cover of the book is of an oil painting by Esther Herschel-Shorland based on the photograph by Julia Cameron. Herschel died on the eleventh of May 1871 aged seventy nine years. Many of his close friends had already predeceased him. He was given a very public funeral in Westminster Abbey and his bones lie in the Abbey next to those of Isaac Newton.

This is an excellent biography by Eileen Shorland. It is particularly interesting and important since she had access to the family archives and was writing about her great-grandfather. John Herschel is not so well remembered today but he was a true polymath making important contributions in several areas of science and astronomy. He played a prominent role in the development and running of British science for over fifty years during the nineteenth century. This book should be read by all those interested in the history of science in this country.

This book is privately published and is not available from the usual book outlets. Initially it has a very limited first-edition print-run and is only available from The Herschel Museum of Astronomy:

www.herschelmuseum.org.uk/visiting/contact-us/

The British Society for the History of Science

Founded in 1947, the British Society for the History of Science (BSHS) describes itself as “Britain’s largest learned society devoted to the history of science, technology and medicine”. It is the professional body for historians of science, but also welcomes writers, students, teachers, museum curators and private individuals. The society’s aim is to bring together people with interests in all aspects of the field, and to publicise relevant ideas within the wider research and teaching communities and the media.

The history of science is, of course, a very much wider field than the history of physics; it includes not only the history of other “exact” sciences such as chemistry and biology, but also that of the social sciences, technology, and medicine. Nevertheless the history of physics does feature in its publications and events.

The BSHS publishes a scholarly journal, the *British Journal for the History of Science*, and a “colourful and entertaining” magazine, *Viewpoint*. Recent issues of *Viewpoint* have included an article on the history of CERN by John Krige, and a contribution by blogger Thony Christie on history-of-science myths, such as the idea that there was a centuries-long conflict between science and religion (the Draper-White thesis) or the idea that Einstein did badly at school (which was apparently caused by confusion between the Swiss and German grading systems, which allocated the letters A-F in opposite orders). *Viewpoint* is quite accessible to budding authors: you don’t have to be a practising historian of science to be considered. For example, in 2015 they published a short article I wrote about bubble chamber scanners in the 1960s.

The society holds an annual conference, and also an annual postgraduate conference, which is open to anyone who is doing a postgraduate degree in the field, or has recently completed such a course. It is an ideal opportunity to give a talk to a sympathetic (though not uncritical) audience. The society also offers grants for events such as one-day meetings; indeed, it co-sponsored the History of Physics Group’s meeting on the history of units at NPL in March 2016.

Membership is not restricted to professional historians of science; there is a "student / unemployed / independent scholar / retired" category which costs £24 a year. For this you get the *Journal*, *Viewpoint*, advance invitations to conferences and other meetings at reduced rates, and other benefits. I would encourage History of Physics Group members to join; promoting communication between the two distinct history-of-physics "communities" (historians interested in the history of physics, and physicists interested in the history of physics) can only benefit both. Even if you don't want to join, it is worth visiting the BSHS website (http://www.bshs.org.uk/), which includes a useful News & Events listing

(http://www.bshs.org.uk/category/news).

Jim Grozier.

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Forthcoming Meetings

Meetings on Rutherford's chemists, the history of MRI, the history of particle physics and history of nuclear fusion are being planned - all in collaboration with other groups. Details will follow on the group's website.

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Disclaimer

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

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