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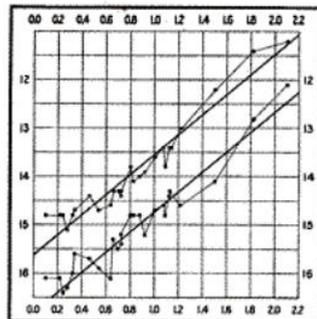


FIG. 2.

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Next Meeting

Belfast on 25th June, is entitled “Belfast Physicists “ and will celebrate the work of Joseph Larmor, Thomas Andrews, John Bell, Ernest Walton, Daniel Bradley and David Bates.

Cover pictures: Small Magellanic Cloud (*ESA/NASA/JPL-Caltech/STScI*)
 Period/Luminosity graphs Pickering/Leavitt 1912 HCO

Editorial

Many years ago I put forward an idea for posters in schools which were to feature some characteristics of physicists and their work but without looking too 'educational'. To make them appealing they were to be expressed purely visually in 'cartoon' form*. The idea was that the poster would grab the attention and lead to questions about the subject in the picture. It was not taken up. The criticisms were that 'there was insufficient physics depicted' (which was true – but deliberate) and that my list of possible candidates gave the impression that most physics was done by dead males. As I had been covering only up to the early 20th century the criticism was fair but certainly not the point I wished to make. What was I to do? I couldn't change how things were. Or could I?

In March of 1961 John F Kennedy signed Executive Order 10925 ushering in the age of 'affirmative action'. This referred to the employment of African Americans but was extended a few years later to include gender. Positive discrimination has attracted much attention and debate over the years and many would argue against it. However, whatever the outcome surely no one would disapprove of the airing of clear cases of negative discrimination and I'm very pleased that we have in this issue an article on Henrietta Leavitt, by Prof. Pangratios Papacosta, who after many years has now been accorded her rightful place in the history of physics.

What we don't have in this issue are the usual reports from our last two meetings - unfortunately no speakers were in a position to provide transcripts. However, the talks by Finn Aaserud and John Heilbron (see page 4 for a report) were largely based on their book about Neils Bohr's work in 1913 entitled 'Love, Literature, and the Quantum Atom' published this year by OUP, and we hope to have a review in the next issue.

Malcolm Cooper

* The only picture which was finished can be seen on the front cover of the August 2009 issue (No 26) of the Newsletter.

Chairman's Report

The first meeting of the History of Physics Group in 2013 was held on 4th April in the Department of Earth Sciences, University of Cambridge, in conjunction with the British Geophysical Association and the Royal Astronomical Society. The title of the one-day meeting was "Rutherford's Geophysicists: Blackett and Bullard". Over 70 participants heard talks by a distinguished array of speakers, including Professor Mary Fowler, granddaughter of Ernest Rutherford, and Professor Sir Arnold Wolfendale.

In July, the History of Physics Group was represented at the International Congress on the History of Science, Technology and Medicine held in Manchester, which hosted about 1,800 delegates.

A one-day meeting "Manchester 1913: Neils Bohr, Harry Moseley and the Origins of the Quantum Atom" was held in Manchester on 19th October. The speakers were Dr Neil Todd, Dr Finn Asserud (Neils Bohr Archive, Copenhagen) and Professor John Heilbron (University of California, Berkeley).

Committee meetings were held at the first and last of these meetings and the AGM took place on 19th October. Two new committee members, Neil Todd and Jim Grozier, were elected during the year.

Two meetings are being arranged for 2014. One, to be held in Belfast on 25th June, is entitled "Belfast Physicists" and will celebrate the work of Joseph Larmor, Thomas Andrews, John Bell, Ernest Walton, Daniel Bradley and David Bates. The second is an "International Conference on the History of Physics" to be held at Trinity College, Cambridge 4-5 Sept 2014. The Steering Committee consists of: Edward Davis (Chair), Malcolm Cooper (Secretary), Denis Weaire, Peter Schuster (EPS), Stephen Elliott (Trinity College) and Peter Main (IOP). There is also a 26-person Advisory/Programme Committee, composed of Historians of Science and Physicists. It is anticipated that this will be the first in a biennial series of conferences with the same title.

There is also the possibility of a tour of Lord Rayleigh's extant laboratories in Terling Essex during 2014.

Suggestions for future meetings or events from members are welcome.

Professor Edward A Davis, Chairman, History of Physics Group

Meeting Report

‘Manchester 1913: Neils Bohr, Harry Moseley and the Origins of the Quantum Atom

Almost 50 people gathered in the Pear lecture theatre, University of Manchester for this fascinating meeting organised by Peter Rowlands and Neil Todd for the History of Physics Group in conjunction with the Manchester Branch on October 19th 2013.

After registration and coffee the group enjoyed a masterly tour of the 1912 buildings by Neil Todd – versed in physics, neurology and science history – stopping at significant points outside and inside the buildings, pausing to show details of the rooms and sites of the equipment devoted to Physics and Electro-Technics in 1912. In particular, he indicated where in the lower ground floor Bohr and Moseley occupied adjacent labs in 1912 and 1013. The group was also treated to an audio recording of Neils Bohr delivering the Rutherford lecture at Imperial College in 1958.

After lunch DR. Todd gave a short talk on the probable interactions of Bohr and Moseley in Manchester. Arthur Schuster had been in charge until 1906; when Rutherford took over in 1907 there was an emphasis on studies in radioactivity. In 1912 Bohr would have undergone the training course of experiments in radioactivity designed by Miss Doris Bailey.

Then followed talks by the two authors of ‘Love, Literature and the Quantum Atom’ – upon which much of the talks were based – Dr. Finn Aaserud and Prof. John L Heilbron. Dr. Aaserud, under the title: ‘**At home while away: the private background of Bohr’s scientific creativity**’ described how Bohr’s private life affected his creativity. He was first with JJ Thomson in Cambridge but was much happier in Rutherford’s Manchester laboratories. Aaserud had been granted access to some 200 letters between Bohr and his fiancé Margrethe, later his wife, and other members of his close and affectionate family. Even on their honeymoon Bohr and his wife first visited Cambridge and Manchester to refine a paper before going up to Loch Lomond.

Prof. Heilbron, of UC Berkeley – though often in the UK – chose as his lecture title ‘**Bohr, Moseley: a fleeting collaboration**’. Although each had close family support, Moseley, the conservative Anglican Englishman, was very different from the free-thinking atheist Bohr and they probably had

little contact in that early summer of 1912. Bohr criticised a paper by CG Darwin (though they became friends later) while Moseley thought von Laue had not understood the x-ray diffraction experiment properly. Heilbron is convinced that, had Moseley survived the battlefields of Gallipoli, he would have shared the 1917 Nobel Prize with Charles Barkla.

After this thoroughly enjoyable day many of the group gathered at Hulme Hall, where Bohr stayed in 1912, for a sumptuous wine reception and celebration dinner. Neil Todd had the novel idea of going round the table and encourage each to speak to a toast of their choosing.

It should be mentioned that accommodation had been arranged at the Manchester Business School which was excellent and favourably priced at around £50 b&b.

Report by Derry Jones and Malcolm Cooper

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## **Wanted!**

**Articles, Letters, Queries**

**- long or short**

**wanted for *your* Newsletter**

**Send to Malcolm Cooper, Editor**

**email: [mjcooper@physics.org](mailto:mjcooper@physics.org)**

## Putting Science back into the History of Science

*Jim Grozier*

The 24<sup>th</sup> International Congress of History of Science, Technology and Medicine (ICHSTM) took place in Manchester in July 2013. It was described as the largest meeting in the history of the field, and featured nearly 1400 papers across 23 parallel tracks, 411 sessions, around 100 social and public events, receptions, walks, tours and excursions, and was attended by 1758 delegates. The History of Physics Group had a presence: several leading members of the group were delegates, and we had a stall at the Monday evening “Learned Societies’ Reception” at which copies of various editions of the Group’s newsletter, as well as flyers for the International Meeting to be held in Cambridge in 2014, were given out.

The opening plenary talk was given by Professor Hasok Chang of Cambridge University, in his capacity as President of the British Society for the History of Science (BSHS) – the main organising body of the Congress. His title, which he described as “deliberately provocative and controversial”, was “Putting Science back into the History of Science”.

Anyone fortunate enough to have attended the 25<sup>th</sup> anniversary meeting of the Group in 2009 will remember the talk given by the Group’s first Chairman, Professor Jack Meadows; if you missed the meeting you may have read Prof Meadows’ article in the March 2010 newsletter (No. 27). In his talk, Meadows gave an overview of how the history of science had changed over the duration of his career, not always for the better. He listed several factors that had changed the course of the subject in the late 20<sup>th</sup> century: concepts from other disciplines, such as sociology and psychology, had been introduced; the new generation of historians of science “often had little background in scientific research”; and the discipline was being ravaged by the “internalist/externalist debate”. As a result of these developments, he said, “physicists and historians of science tended to drift apart”.

Internalist history of science concentrates on the development of the science itself; externalist history prioritises contexts such as political, cultural, social, economic and religious factors. Clearly both viewpoints are needed, but in recent years there has been a marked emphasis on externalist history, to the point that some studies in the history of science often seem to include very little science; and academic departments in the discipline tend to be

associated with humanities faculties rather than with science ones. There is very little “cross-over” between these academic departments and groups such as our own; there are, effectively, two distinct communities.

In his talk in Manchester, Chang echoed Meadows’ lament about the current state of the discipline. He pointed out that “scientific content is just not a preoccupation in this line of work”, and instead “social and cultural history of science is the order of the day”. As well as the factors mentioned by Meadows, Chang blamed an over-reaction to “whiggish history” – the writing of history from a modern standpoint, and its portrayal as simply a linear progression towards present-day science. He quoted the historian and philosopher of science, Nick Jardine, as complaining in 2003 that “all too often recent historians of science have abandoned common sense in their flight from presentism”.

Chang is living proof that studying the internalist history of science need not give rise to whiggism. One of the hallmarks of a whiggish attitude is to ignore or play down those episodes in the history of science that were later regarded as mistaken or wrong; yet he spends much of his time reviving lost scientific ideas such as the phlogiston theory (including recreating historic experiments himself in the laboratory), and showing that along these scientific blind alleys there was much knowledge that has been lost, including some surprisingly “modern-looking” concepts which, given time, might have simply represented alternative routes to what we now regard as the truth. He is a pluralist, and sees no problem in the existence of multiple paths to the truth, or indeed, multiple versions of the truth. We should perhaps forgive him for having come up with such an unfortunate name for this approach, “complementary science” – definitely not to be confused with complementary medicine!

Pointing out that historians of other disciplines, such as art, are expected to know something of their subject-matter, and to engage with it, Chang argued that the role of “science studies” (a blanket term for history, philosophy and sociology of science and other related disciplines) is “to challenge authority of scientists when and where they deserve it” – but such criticism requires a “sound understanding of the content and methods of science”. Against possible charges that it is just not feasible for humanities-based scholars to engage with science, he responds that it is “not impossible to learn the science we need – it’s easier than learning Latin or Chinese”.

(At the Cambridge department of History and Philosophy of Science where Chang is based, there are indeed classes available in Latin and Greek; but sadly not, as yet, in science).

Ever the pluralist, Chang was careful not to disparage other approaches:

*“there are no enemies here, except for those who are in the habit of making enemies, declaring that anything that doesn’t fit into their own narrow-minded view of good scholarship is not history or not real history of science, and therefore not worth pursuing.”*

This drew a shocked reaction from the audience; but, in my experience at any rate, he paints an accurate picture of the mentality of some current practitioners of the discipline, who seem to me to be often inward-looking and tribal, with an overly pugnaeous attitude to those on the outside.

Actions speak louder than words, of course, and Chang is aware of that. As if giving such a provocative talk to such a large gathering, from the authority of his current position in the BSHS, was not action enough, he is now busy with his latest idea – “Coffee with Scientists”, a new series of regular meetings in Cambridge between historians of science and scientists, which will hopefully promote the sort of engagement that he is arguing for, and perhaps even spread to other towns. Our own History of Physics Group can, of course, help to close the gap between the two camps with similar initiatives, and indeed it is already doing just that. Next year will see a golden opportunity for engagement between physicists and academic historians of science, with our own International Conference on the History of Physics, due to be held in Cambridge on 4-5 September. The organisers and advisors for that event include, as well as leading lights of the History of Physics Group, an impressive list of names from the global history of science community, among them a certain Hasok Chang. It promises to be a true meeting of minds from all across the spectrum.

<http://historyofphysics2014.iopconfs.org/home>

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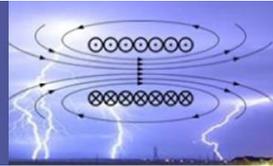
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IOP Institute of Physics

International Conference on the History of Physics

4–5 September 2014, Trinity College Cambridge, Cambridge, UK



Development of a conference – *or how they brought the good news from Lausanne to Cambridge.*

In June 2011 Peter Schuster, Chair EPS History Group, Denis Weaire, former Chair, IOP History Group and James Bennett of the Science Museum, Oxford met in Lausanne and discussed the possibility of launching a new European wide series of conferences on the history of physics. This idea was fired by the fact that there was no substantial international conference on the history of physics, despite the strong growth of professional and popular interest in the subject.

It was strongly felt that such a conference – supported by the IOP and the EPS - would provide a forum for discussions of all periods and sub-disciplines of physics, together with its applications and consequences. It would bring together historians and practicing (or retired) physicists to maximise interaction of these two constituencies.

There the matter lay until late in 2012 this author raised the matter informally with the Groups Coordinator and Ted Davis formally proposed to the Group committee that an approach be made to seek funding from the IOP. Thus it was that a letter from the then Chair of the group, Peter Ford, was sent putting forward the proposal.

The response was not encouraging. It was originally not thought to be a group matter and it was recommended to place the matter before the Science Board. However, just at that moment, quite coincidentally, the Conference Committee was formed and a formal submission – suggesting September 2014 - was considered by them. And they smiled upon it!

76 Portland place was to have been the original venue but by that time Head Office would be deep in the throes of packing up preparing for the Big Move so another venue had to be sought. Trinity College, Cambridge does not normally host external conferences but through the good offices of Fellow Professor Stephen Elliot all doors were open. We have now secured substantial funding from the IOP and also financial support from the EPS and the organisation is well under way.

So, for details please see <http://historyofphysics2014.iopconfs.org/home>

Developing school electron-physics apparatus in the 1960s and 1970s: a personal account*

Dr Dennis S Beard

In 1961, the schools science inspectorate complained to the manufacturers of science teaching apparatus about high cost and poor quality. As Technical Director of the laboratory supplier Griffin and Tatlock, I became secretary of an educational study group within the Scientific Instrument Manufacturers' Association. The eventual outcome was the production of a wide range of apparatus (including a small Van de Graff generator) approved for the Nuffield Science Project. Earlier apparatus to demonstrate in UK schools the 'modern physics' of the electron beam had used high (60-70 kV) cold-cathode spark-coil tubes with consequent radiation hazard. Schools Inspector Wilf Llowarch suggested using a lower-voltage filament electron source. As Strawson [1], formerly of Abingdon School, recounts, this was taken up in the UK by Solus, manufacturers of X-ray tubes and rectifiers for General Radiological's diagnostic and teaching (eg Geissler and Maltese Cross) units. With these, voltages were now a few kV and currents μA rather than mA. The German company, Leybold, made an apparatus that gave a fine demonstration of the circular path of electrons in a magnetic field, rendered luminous in a helium atmosphere, but it was too expensive for schools. The concept of the hot-cathode tube was acquired by Teltron Ltd, set up in 1963 by Derek J Power, an electrical engineer, son of Ernest Power (a founding partner in the Bush-Murphy radio company who then acquired Solus). Teltron planned to develop discharge tubes that would enable school laboratories to demonstrate the production and properties of electrons.

My first two post-graduate decades had been spent in a miscellany of physics-based activities embracing spectroscopy, X-radiography and diffraction, laboratory instrument design (both in a University [2,3] and with a manufacturer), school teaching (setting each pair of practical physics students on a different experiment), and academic patent applications at NRDC (precursor of BTG). Joining Teltron as Technical Director in early 1964, my brief was to collaborate with Power in the design, development and testing of attachments and experiments that would appreciably extend the range. Progress was such that we presented at the Manchester ASE meeting later that year prototype apparatus in a simple glass envelope (like a 1litre long-necked flask) for demonstrating e/m, the triode, Crooke's tube, and negative and positive rays.

Encouraged by former radiologist and enthusiastic Malvern College teacher John Lewis, we followed Power's e/m tube described by Strawson with one to demonstrate electron diffraction and the de Broglie wavelength; a thin layer of graphitized carbon gave a ring display.

Teltron portable tubes and applications developed [1] included:

Gas-filled triode - helium instead of hydrogen as in the German Leybold tube, the 'Faraday cage' of thin tin was improved to eliminate hand-capacitance effects.

Luminescence and Crookes tube; critical-potential tube.

Perrin and Thomson e/m tube – negative sign of electric charge could be demonstrated with an electroscope; should the beam strike the outside of the Faraday cage collector, secondary emission causes the beam to appear negative.

Continuously pumped discharge tube with screens to illustrate negative and positive rays; in a variant of the Maltese cross experiment, with negative instead of positive charge, electrons can be deflected so that the sharp cross is distorted.

Double-beam helium-filled tube; the fine beam could demonstrate the circular path of the electron in a magnetic field.

For crystallography and radiography, an air-cooled low-voltage (30 kV) X-ray tube with copper target was assembled from inexpensive, available components in a spectrometer-like arrangement. The envelope and bubble window (as in the flow sight-glass of an old-time petrol pump) were of borosilicate glass (which would absorb slight off-beam radiation) rather than beryllium or Lindemann glass while a moulded foot ensured mechanical alignment. Use of sealed light-tight dental film (38 by 34 mm flat film or 140 by 10 mm strip for cylindrical powder camera), which was punctured for processing, eliminated the need for a dark room. For detection, there was the Philips MX16P Geiger-Muller tube as used in schools' radioactivity studies. The need for a goniometer was obviated by mounting the crystal, eg alkali halide, directly on the rotation axis. Power completed the careful engineering design work on the Tel-X-ometer and used simple glue-based thin-walled powder sample tubes that were fabricated easily. At the suggestion of Lewis, the film holder/slide carrier was arranged to mount filters for an explanatory optical analogue experiment [2].

The complete portable Tel-X-ometer diffractometer had a simple 2:1 device for coupling detector and crystal rotation, based on one for an early full-size

diffractometer devised with Dr GE Pringle at Leeds University in 1950. Three rotating wheels at 120 degrees on a table top were linked to a flat disc parallel to the table and a central axle with a ledge to hold the crystal. Additionally, there was the potential for absorption edge and atomic number (Moseley's Law), Compton Effect, ionization, and fluorescence experiments on transition-metal foils. Although the set of atomic physics experiments received a Duke of Edinburgh Industrial Design award, its expense made it more appropriate for first-year University labs than schools. The Teltron unit was at the heart of an electron diffraction demonstration apparatus for sixth-form pupils and developed by Drs RL Eades and DG Smith for use by students at UC of North Wales, Bangor. This was filmed by Granada TV in Manchester in the ITV for Schools programme which had an accompanying booklet [4]; the experiments included the Bangor determination of Cp/Cv by Ruchhardt's method, marketed by Lennox in Dublin. [In 2008, Teltron was acquired by the UK arm, 3B (UK) Scientific of Weston-super-Mare, of a German company.]

In 1973, I left Teltron to concentrate on my Ideas for Education company for school physics apparatus. We developed an inexpensive version of the diffusion (or continuous Taylor) cloud chamber, made from a 10 cm diam pvc tube, for observation of alpha-particle tracks (dry ice helped initiate the mist). Instead of the radium paint source of the 1960s and 1970s, Ralph Whitcher devised a thoriated tungsten electrode as source for the weak irregular scans to be identified with a digital camera. More recently (in my 80s), I have manufactured a direct vision spectrograph for schools from readily available slit, grating and lenses; one trusts that cost and quality meet the standards of the 1961 meeting. The 1960s had been an exciting time to be helping to modernize school practical-physics teaching, with enthusiastic teachers who engaged in long evening discussions at teachers' centres.

I thank Prof Derry W Jones for persistently encouraging me to record this story.

References

- [1] RJ Strawson, *Physics Education*, **44** 522-367 (2009)
- [2] DS Beard, EG Cox and Sutton, *J Sci Instrum*, **28**, 246-251 (1951)
- [3] DS Beard, *Physics Education*, **7** 363-367 (1972)
- [4] Experiment: 'A' level Physics Experiments, Granada Television (Autumn, 1975)

Key to our Universe: Henrietta Leavitt's 1908 discovery.

*Prof. Pangratios Papacosta
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Henrietta Leavitt*

Only four years after the 1920 Great Debate in Astronomy between Heber D. Curtis and Harlow Shapley about the size of our galaxy and the possibility of “island universes,” Edwin Hubble showed that Andromeda was not leftover material at the edge of the Milky Way but a galaxy of its own with billions of stars, more than a million light years away. Hubble was meticulous and thorough in his work, but even with such fine qualities, he would have been unable to make this historic discovery if it was not for two powerful tools he had at his disposal. The first one was the 100-inch Hooker telescope at Mount Wilson, at the time the largest telescope in the world. With this large telescope Hubble could see farther and distinguish individual stars including Cepheids, variable stars whose intrinsic brightness change periodically.

* Courtesy of American Association of Variable Star Observers (AAVSO)

The second came from a discovery - that Cepheids can be used as standard candles to measure distances beyond the galaxy- a daunting task at that time. This new technique was based on a 1908 discovery made by Henrietta Leavitt, a little known “computer” at the Harvard College Observatory (HCO).



AAVSO Annual Meeting, 1918. Henrietta Leavitt is 10th from the left. On her right and behind her is Edward Pickering, director of HCO.*

Henrietta Swan Leavitt (1869 – 1921), was a member of the photographic photometry department at the Harvard College Observatory and she made her discovery from photographs of the Small Magellanic Clouds. The director of HCO was Edward Pickering under whose name Henrietta’s discovery was published in 1912¹. She proposed that the longer the period of brightness oscillation of a Cepheid the larger its intrinsic luminosity was. Therefore a Cepheid star whose periodicity is known could be used as a standard candle and measure astronomical distances – even distances beyond our galaxy.

Henrietta’s assignment was merely to catalogue stars. This involved examining thousands of glass plate negatives of the night sky taken by telescopes some as far away as Peru and shipped to the Harvard Observatory. The sheer size of the volume of these glass plates turned Edward Pickering into the unintentional CEO of the first astronomy factory.

* Courtesy The Harvard College Observatory



A group of young computers at Harvard College Observatory around 1890.*

He employed dozens of women to examine these plates and compute the characteristics of the stars and catalog them according to their apparent brightness and spectral characteristics. Because of these computational tasks these women were called computers. Theirs was a tedious and strenuous work (stars appeared as tiny black dots on the glass plate negatives) for which the “computers” were paid ¢25 cents per hour. But Henrietta did more than that just examine and catalog stars. When working with glass plate negatives of stars from the same region in the night sky, photographed many times over for months, she noticed that Cepheid stars have a distinct property. The brighter the Cepheid was the longer its period of oscillation. She made this extremely valuable discovery on her own initiative.

Ejnar Hertzsprung was the first astronomer to recognize the importance of such a discovery and used it to make a crude distance measurement of far away stars within our galaxy. But it was astronomer Harlow Shapley who, after a revised calibration, used Henrietta’s Period-Luminosity Law to measure the distances of globular clusters in the Milky Way galaxy. ²

* Courtesy of The Harvard College Observatory

Edwin Hubble used Shapley's method to measure the distance to the Andromeda nebula by measuring the period of Cepheid stars in the nebula. The powerful telescope and the Leavitt method led to his 1924 discovery that the Andromeda nebula was a galaxy with billions of stars and very far away from us and not, as believed, leftover material at the edge of our galaxy. At that time the Milky Way galaxy was thought to be the entire universe.

In 1929 Edwin Hubble made his second historic discovery. He found that the spectra of galaxies were shifted to the red by different amounts, evidence that galaxies are moving away. This discovery helped a struggling cosmological theory later to be known as the Big Bang. Hubble used the Leavitt method to measure the distance of galaxies and showed that the farther away a galaxy the faster its speed, a relationship that is known as the Hubble law. In his 1936 book *The Realm of the Nebulae*, Hubble refers to the property of Cepheids as "*a new feature of extraordinary significance.*"³ Yet his reference to Henrietta Leavitt in the next paragraph is lukewarm. He acknowledges the use and calibration of the period-luminosity relation first by Hertzsprung and later by Shapley and ends the *Period-Luminosity Relations to Cepheids* section in his book without ever mentioning that he, Hubble, had used Shapley's technique. Instead he writes,

*"Thus, whenever a Cepheid may be found, the period will indicate the absolute luminosity, and the apparent faintness then measures the distance. It was by this method that the first reliable distances of nebulae (galaxies) were determined."*³

The 1924 and 1929 discoveries by Hubble have become the foundations of the new astronomy and the first undisputed evidence that the universe is expanding. Hubble's cold acknowledgement of Henrietta Leavitt is an example of the lack of recognition that Henrietta and many other women scientists were forced to endure, no matter how great their contribution to science was. With the exception of naming a moon crater after her and the sporadic footnote references in some astronomy textbooks, the profession of astronomy has done little to acknowledge or celebrate Henrietta Leavitt's work. No astronomy prize is given in her honor. The period-luminosity relation was only recently renamed as the Leavitt-Periodicity Law, thanks primarily to the lobbying efforts of this author. Her discovery was the key that opened up the cosmos to us, yet no space telescope bears her name and no postage stamp was issued to honor her. It is only through gestures like these that the public at large will recognize the importance of her discovery.

Like many other unsung heroines of science, Henrietta Leavitt reminds us of the injustice with which history has treated women scientists. Few efforts to recognize her work were made in the past, but often such efforts were ineffective because they were expressed only in private, and at times they were confidential. Here are some examples.

In March of 1925 Professor Mittag-Leffler, a member of the Swedish Academy of Sciences, wrote to Henrietta Leavitt the following.

“Honoured Miss Leavitt,

*What my friend and colleague Professor von Zeipel of Uppsala has told me about your admirable discovery of the empirical law touching the connection between magnitude and period-length for the S. Cephei-variables of the Little Magellan’s cloud, has impressed me so deeply that I feel seriously inclined to nominate you to the Nobel prize in physics for 1926, although I must confess that my knowledge of the matter is as yet rather incomplete.”*⁴

Unknown to Professor Mittag was the fact that Henrietta Leavitt died of cancer in 1921. Nobel prizes are not awarded posthumously and we will never know whether Henrietta Leavitt would have received one had she lived longer. Regardless, the thought of nominating her, which interestingly came not from American but foreign scientists, speaks highly of the importance of her discovery. If her work impressed Professor Mittag as early as in 1925 one wonders how much more impressed he would have been after Hubble’s 1929 discovery of an expanding universe and the establishment of the Hubble Law.

In a 1979 article titled *“The rise of astronomy in America”*,⁵ historian of science Stephen Brush lists the most important discoveries in Astronomy from 1800 till 1950, as recognized in the works and references of other historians of science. For the period of 1900 – 1950 he lists ten major discoveries, of which the second listed is the period-luminosity relation with the names of Leavitt, Hertzsprung and Shapley attached to it.

In a recent article titled *Gender and science: Women in American Astronomy, 1859 – 1940*,⁶ authors J. Lankford and R. L. Slavings write,

“Women measured plates and reduced data in the great factory observatories, helping raise American astronomy to world-class status while they themselves were relegated to second-class status.”

The authors suggest that such treatment merely mirrored the values in American culture and the rigid application of gender-specific roles. They quote Maria Mitchell, America's first woman astronomer, who on the subject of gender-specific roles she pointed out some of the advantages that women astronomers had, over their male co-workers.

*"The eye that directs a needle in the delicate meshes of embroidery will equally well bisect a star with the spider web of a micrometer. ... Routine observations....dull as they are, are less dull than the endless repetition of the same pattern in crochet-work."*⁶

We can explain but not justify the double standard treatment that women scientists like Henrietta Leavitt suffered under. Those were difficult times for women who were treated by men as non-equal partners in every field, including the arts, sciences, politics, business and sports. They earned much less than men and they rarely received the recognition they deserved. Henrietta's own personality compounded her obscurity.

Unlike the prevailing flair of antagonism and professional competition amongst male astronomers like Hubble and Shapley, Henrietta Leavitt was a humble, quiet and shy person, not because she was hard of hearing but because of her personality and character. These were shaped by her stern upbringing in a family proud



of its Puritan ancestry and with a clergyman father of national prominence. Henrietta's personal qualities were well captured by Solon I. Bailey of the Harvard Observatory. In Henrietta Leavitt's obituary he wrote.⁷

"Miss Leavitt inherited in a somewhat chastened form the stern virtues of her puritan ancestors. She took life seriously. Her sense of duty, justice and loyalty was strong. For light amusements she appeared to care little. She was a devoted member of her intimate family circle, unselfishly considerate in her friendships, steadfastly loyal to her principles, and deeply conscientious and sincere in her attachment to her religion and church. She had the happy faculty of appreciating all that was worthy and lovable in others, and was possessed of a nature so full of sunshine that to her all of life became beautiful and full of meaning."

Photograph above courtesy of Harvard College Observatory

Despite his many efforts this author has been unable to find any documents that could shed light to the personal life of Henrietta Leavitt. We do not know of any personal diaries, letters or personal relationships. She never married and took care of her mother. The Schlesinger library that houses many records of students who attended Radcliffe College (Harvard's sister institution for women) acknowledges the absence of any personal information on Henrietta in their collection. But her academic records show her to be an excellent student. Her classes included languages (Greek, Latin, French, German, Italian and Spanish) history and philosophy, mathematics, physics and astronomy. Her interests in history, art and culture took her to Europe on few occasions. It is quite possible that because of her stern Christian upbringing, her love of solitude compounded with humility, she may very well have destroyed any such personal writings before she died.

Henrietta Leavitt is a poignant example of a missing part of the history of science, the acknowledgment of the significant contributions to science made by women scientists. But just as much as the role of women scientists is overlooked by history of science, history of science is itself overlooked by science. It is so unfortunate that history of science continues to remain undervalued, underused or completely ignored by scientists and science educators despite the many benefits it can offer.

First and foremost history of science is what drives the preservation efforts of important historical documents, equipment, manuscripts, photos and oral narratives associated with the advancement of science. But more than that, history of science can humanize a dry and mechanical discipline as perceived by the public. Knowing something about the personal lives of scientists and the circumstances of their discoveries, their struggles and at times even their persecutions, can add a warm human drama to science.

History of science can also help in the prevention of dogma. It reminds us of the different explanations given for the same phenomenon at different times in history, and makes us aware that every discovery has a price, dozens of new questions never asked before. All this can invoke a sense of humility and a stern reminder to worship neither a theory nor a scientist.

For all these reasons selected elements of the history and biography of science should be integrated in the teaching of science at all levels from the elementary to the graduate level. This can be accomplished if historians of science, science educators and scientists get together to figure out the best way to provide that experience to our students and the public at large.



The author standing next to the Leavitt family headstone in the Old Cambridge Cemetery in June 2008

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A shorter version of this paper appeared in STATUS for women Astronomers journal in January 2005. Professor Papacosta is the producer of a 50-minute documentary on Henrietta Leavitt, the first of a series on women scientists ignored by history. The documentary will be ready for free distribution to the public in 2013-2014.

Newton's Second Law of Motion and the Concept of Force

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Introduction

Physics students nowadays learn Newton's Second Law in mathematical form, in some variation on the following [e.g. Hudson & Nelson p82]:

$$\vec{F} = \frac{d\vec{p}}{dt} = m\vec{a} \quad (1)$$

The left-hand side here represents the net force on an object and the right-hand side its resultant rate of change of momentum. But is that what Newton actually said?

Here is the relevant extract from the *Principia*:

"The alteration of motion is ever proportional to the motive force impress'd; and is made in the direction of the right line in which that force is impress'd." [Newton, vol.1, p19]

Michel Blay tells us that this "should not be confused with what is now called 'Newton's Law'" (which he writes as an equation similar to (1)) [Blay p226].

A brief inspection indicates that there are at least three ways in which these two statements of the Law differ: (1) Newton stated it in words, not mathematical symbols; (2) he speaks of the "alteration of motion" rather than the "rate of change of momentum", and hence the meaning of the word "force" may also differ; (3) the proportionality referred to by Newton has become an equality – the equation is used nowadays to *define* force, but does not play this rôle in the *Principia*.

In this essay I will attempt to find out exactly what Newton meant by his Second Law and its reference to "force", in the light of his various Definitions, the other Laws, and commentaries that have been made since the publication of the *Principia*. In this investigation I will clearly have one eye on the modern formulation – one cannot "unlearn" such a fundamental component of modern science – but will try to concentrate on the contemporary context, rather than the various glosses that have been added since Newton's time.

Verbal versus mathematical format

Nowadays science – or at least physical science – is considered to be intrinsically mathematical in nature; on opening a physics textbook one expects to be confronted with page after page of equations. However, Newton lived at the dawn of the mathematisation of science (and was partly responsible for it) and at that time the standard way of enunciating a law was in words. Nevertheless, there is no reason why expressing scientific facts in words should substantially affect their meaning. To be sure, it is important that we know the definitions of all the terms used; but this applies equally when a mathematical format is used. There is possibly an additional difficulty with the verbal format, however, in that we are apt to assume that we know the meanings of any very common words that are not specifically defined, yet these meanings may have changed in the intervening 350 years.

A further contrast – which may not be particularly relevant to Newton’s Second Law, but is worth mentioning here – follows from the compactness of the mathematical format as compared with the verbal, and the fact that complicated expressions may be exceptionally difficult for the reader to take in, in the latter format. As an example, here is a sentence from Calandrini’s note on corollary 5 of proposition VI in Book III of the *Principia*:

“The force of the magnet at X will be to the force of the magnet at M, as the sine of the declination of the needle from the magnetic meridian when the magnet is at X divided by the sine of the deviation from the magnet situated at X is to the sine of the declination from the magnetic meridian when the magnet is at M divided by the sine of the deviation from the magnet situated at M”

In mathematical format, that could be expressed as something like this:

$$\frac{F_X}{F_M} = \frac{\sin \alpha \sin \delta}{\sin \beta \sin \gamma}$$

Note that, in any case, the idea of expressing one quantity in terms of another, different, quantity, was also very new. Herivel tells us that “the notion of a proportion between dissimilar quantities was *unthinkable* to both Greek and medieval philosophers” [Herivel p2n]. So we must not expect Newton, whose education had been based on the works of these philosophers, to think in terms of equations such as (1).

Definitions

In the Scholium following Definitions I – VIII, Newton tells us that

“Hitherto I have laid down the definitions of such words as are less known, and explained the sense in which I would have them to be understood in the following discourse. I do not define Time, Space, Place and Motion, as being well known to all” [Newton vol 1, p9]

The rest of the Scholium is concerned only with certain distinctions between aspects of these phenomena, and classifies them into categories such as “absolute, relative, true and apparent”. We may thus be left in some confusion over his use of the term “the alteration of motion”. Fortunately, he *does* define “quantity of motion”, in Definition II, and clearly it is the alteration of this quantity to which he refers in the Law:

“the Quantity of Motion is the measure of the same, arising from the velocity and quantity of matter conjunctly”.

Following this definition Newton gives us further clues as to the meaning of ‘arising from the velocity and quantity of matter conjunctly’: he says that “in a body double in quantity, with equal velocity, the motion is double; with twice the velocity, it is quadruple” [*ibid.* p2]. And furthermore, Definition I tells us that the “quantity of matter” is synonymous with what we today (and indeed, Newton too) would call mass. Hence we can be fairly sure that the word “motion” in the Second Law can be translated unambiguously as “momentum”, the modern word for the product of mass and velocity. Momentum is, of course, a vector quantity, having both magnitude and direction, which explains why the Law is in two parts; the first part deals with the magnitude of the “alteration”, and tells us that it is proportional to the “motive force impress’d” and the second part tells us in what direction this alteration is made.

Definition IV then tells us that by “an impress’d force” Newton means “an action exerted on a body, in order to change its state, either of rest, or of moving uniformly forward in a right line” [*ibid.* p3]. He does not define “action”; and the second half of this definition anticipates the Second Law, but is less precise; thus Definition IV does not really help us much in our present endeavour to understand the meanings of the words, but I will come back to it later on when discussing the use of the Second Law as a defining equation. There is also a note following the definition listing three different

“origins” of impressed forces, namely “from percussion, from pressure, from centripetal force”. Again, I will investigate the implications of these types of force later on.

What I have established so far, then, is that when a force is applied to a body, it alters the momentum by an amount proportional to the magnitude of the force, and in the same direction as that in which the force acts. We might alternatively express this by saying that the force gives the body an additional *component* of momentum whose magnitude is proportional to the magnitude of the force, and whose direction is that in which the force acts. But note that this “force” is really an impulse (see “Force and Impulse” below).

However, in the Second Law, Newton does not speak simply of a *force*, but of a *motive* force. What does he mean by this? Well, the word “motive” appears as one of three “kinds” of centripetal force that are introduced at the end of Definition V, which defines centripetal force, these three kinds being “absolute, accelerative, and motive”. I will come back later on to the concept of a centripetal force and how it relates to the other two “origins” of force, namely percussion and pressure.

The three kinds are then given their own definitions, as follows:

Definition VI: “The absolute quantity of a centripetal force is the measure of the same, proportional to the efficacy of the cause that propagates it from the centre, through the spaces round about”.

Definition VII: “The accelerative quantity of a centripetal force is the measure of the same, proportional to the velocity which it generates in a given time”.

Definition VIII: “The motive quantity of a centripetal force is the measure of the same, proportional to the motion which it generates in a given time”.

These definitions appear to describe, in actual fact, three different *ways of measuring* centripetal forces. It is not easy for a 21st century reader to understand this – especially when Newton often omits the words “quantity of”, and speaks simply of “Motive, Accelerative and Absolute forces” as though these were different *types* of force. It can be illuminating to take the most obvious centripetal force – gravity – and try to identify these three aspects in such a concrete case.

We would say that the force F between masses m_1 and m_2 , separated by a distance r , is given by

$$F = G \frac{m_1 m_2}{r^2} \quad (2),$$

where G is a constant, and I have represented the force by its magnitude only for simplicity.

This is sometimes written as $F = m_2 g$, where $g = G m_1 / r^2$, if we are considering the weights of bodies at the same distance from the centre of the earth.

The “accelerative quantity”, according to Newton’s description following the definition, is a quantity that “at equal distances [from the body of the Earth] ... is the same every where”. This looks very much like g , the acceleration due to gravity; we would not say that it is a *force*, but it is a measure of the strength of the gravitational field, analogous to the electric field strength in an electrostatic field due to a central charge. The “motive quantity” is clearly the product of the accelerative quantity and the mass of the body; so it is mg , or the weight of the body.

In order to explain what he means by “absolute quantity”, Newton uses the analogy of magnetism. This is unfortunate for us, as magnetism is seen very differently nowadays; we would be happier with an electrostatic analogy, but presumably that was not such an obvious choice for Newton. He identifies the “absolute quantity” with the *cause* of the force; this can be understood in the case of a magnet or lodestone, when it is placed near a piece of iron, which is therefore attracted to it. There is no symmetry in this interaction; it is the lodestone that induces magnetism in the iron and hence *causes* it to be attracted to the lodestone. If we were to think of the force between two magnets, or better still, two magnetic *poles*, we would be hard pressed to designate either one of them as the cause; likewise, when we think of the gravitational force between two masses, neither one can be said to be the cause of it. But if one mass is very much larger than the other, it is perhaps permissible to label this larger body as the cause of the force. Hence we look for some property of the larger body to identify with the “absolute quantity” of the gravitational force, and conclude that it must be the mass. Again, mass is not a *force* though, so it is a rather strange concept to modern ears.

Note that, although Newton defines “motive quantity” in terms of centripetal forces, he later adds that the term can equally be used for impulses:

“I likewise call Attractions and Impulses, in the same sense, Accelerative and Motive; and use the words Attraction, Impulse or Propensity of any sort towards a centre, promiscuously and indifferently, one for another” [Newton vol 1 p8].

Unfortunately, Definition VIII, when substituted for “motive force” in the Second Law, produces a circular argument: the alteration of motion is ever proportional to the motive force, which is proportional to the motion which it generates in a given time. So the alteration of motion is proportional to the motion generated in a given time. In fact this is worse than a circular argument – it is inconsistent, since Newton starts off with “alteration of motion” which we translate as “change in momentum”, and ends up relating this to “motion generated in a given time”, or *rate* of change of momentum. The confusion here is because Newton’s use of the term “force” is sometimes identical to the modern sense of it (e.g. in Definition VIII) and sometimes to what we would now call “impulse” (e.g. in the Second Law). We should not be too critical of this apparent inconsistency; Boudri reminds us that these were early days for the concept of force:

“In the history of ideas, it is not unusual to find that very different ideas originate historically from a common concept. Frequently, the common term is then reserved for one of these ideas, while the others acquire new names” [Boudri p34].

The term “force” was not restricted to forces and impulses; Newton also spoke of an “Innate Force of Matter” in Definition III. This “force” is what we would now refer to as inertia; it appears that Newton counterposes it to his concept of “impress’d force”. In fact, according to Richard Westfall, “it remained for the eighteenth century to define the concept of force with adequate rigour” [Westfall p476].

Force and Impulse

A major theme of Cohen’s 1970 paper, *Newton’s Second Law and the Concept of Force in the Principia*, is the distinction between the phenomena known nowadays as (continuous) forces and (short-lived or instantaneous) impulses; Blay goes as far as to say that “the main aim of Newton’s *Principia* is, in my opinion, to answer these questions [about the transition from the discontinuous to the continuous]” [Blay p226].

As Cohen points out, a modern interpretation of Newton's wording in the Second Law suggests that he was talking about impulses here rather than continuous forces, since the nearest modern equivalent to the phrase "the alteration of motion" is the *change in momentum*, which in turn, according to the modern form of the law, is related to the applied *impulse*. Cohen observes that "in the modern formulation of Newtonian dynamics ... the concept of a continuous force \vec{F} is primary and the concept of impulse ($\vec{F} \cdot dt$) is derived from it" [Cohen 1970, p146] but this was not the case in Newton's time. Continuous forces cannot be "seen", whereas impulses can. (Indeed, according to Aristotelian philosophy, we do not need to postulate a force to explain falling bodies, since they are only seeking their "natural place" and so the resultant motion is "natural", other motions being "violent").

Newton's contribution to dynamics was to introduce the concept of a centripetal force (such as gravity) and show that the same laws apply to such forces as to the more everyday kinds of forces, which are impulses – for example, kicking a ball. Cohen tells us that in Newton's time "the very concept of [centripetal] force was a novelty" – in fact, Newton "held it to be his own invention" [*ibid.* p154]. So it is not surprising that he articulated his Second Law in terms of impulses rather than continuous forces.

Cohen does not seem to consider the possibility that, rather than Newton being rather vague in his reference to forces, using the same word for a continuous force and an impulse, the vagueness might rather have been in his use of "the alteration in motion", to mean either the change in momentum or the *rate* of that change. And yet Cohen himself tells us later on, in his Appendix II, that "the modern reader is apt to be confused because Newton does not always make a clear distinction between the *mutatio* (dx) of a fluent quantity (x) and its *celeritas mutationis* (dx/dt)" [*ibid.* p184].

We can, of course, derive an "impulse" formulation of the law from the standard formulation given in (1), by replacing the differentials with finite elements:

$$\vec{F} = \frac{\Delta \vec{p}}{\Delta t} \quad (3),$$

whence

$$\vec{F} \Delta t = \Delta \vec{p} \quad (4).$$

Here the right hand side is clearly the "alteration of motion" associated with the impulse $\vec{F} \Delta t$.

Not all authors are equally fussy about Newton's terminology. For instance, in his book *Newton's Principia for the Common Reader*, Subrahmanyan Chandrasekhar disposes of the Second Law in five lines [Chandrasekhar p23]:

“The statement of the Law is self-explanatory. In current terminology it states:

Force = change in motion
 = change in [mass \times velocity]
 = mass \times change in velocity
 = mass \times acceleration.”

He does not say anything about the dimensional inconsistency between the fourth and fifth lines!

Cohen's argument about whether Newton was referring to impulses or continuous forces springs from his analysis of the First and Second Laws, and particularly from the question whether the First Law is strictly necessary, since it appears to be a special case of the Second Law. (If forces produce accelerations, then an absence of acceleration implies an absence of force; hence a body at rest or in uniform linear motion implies that there are no forces acting on the body). Cohen's interpretation is that the First and Second Laws are about different kinds of force: the First Law being concerned with continuous forces, while the Second relates to impulses. James McGuire, on the other hand, commenting on Cohen's paper, sees the rôles played by the First and Second Laws as providing necessary and sufficient conditions respectively for a change in motion, rather than relating them to particular kinds of force.

The First Law, in Newton's words, is as follows:

“Every body perseveres in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impress'd thereon” [Newton vol 1 p19].

Cohen says that if it is accepted that the Second Law is about impulses, then the First Law is only derivable from it for impulses; but in fact the First Law is true for continuous forces as well, and so it tells us more – hence it is not just a special case of the Second Law. It is tempting – especially for the modern mathematically-trained mind – to claim that the Second Law can

easily be extended to continuous forces, since all that is required is the mathematical operation corresponding to going from (4) to (3) above. But continuous forces imply taking a limit, and the simple act of dividing by Δt is not really permissible.

McGuire's argument for the interdependence of the First and Second Laws rests on his observation that the Second Law gives only a sufficient condition for a change in motion; it "does not prescribe the necessary conditions" [McGuire p186]. He says that these conditions are laid down by the second half of the First Law, which "asserts that only an impressed impulse, and no other cause, can alter the state of a body" – so that both laws are required in order to specify necessary and sufficient conditions for a change of motion. But note that McGuire uses the word *impulse*, despite the fact that the First Law refers to "forces". Presumably he does this so that he can argue that the two Laws both deal with the same phenomena – which is a requirement if they are together to constitute necessary and sufficient conditions for the same thing – something that Cohen would not agree with, since he maintains that they apply to different kinds of force.

Cohen gives other reasons for the First and Second Laws being separate; he says they are modelled on two hypotheses of Huygens, and also makes the observation that "Law 1 embodies a radical departure from traditional physics to the extent that it declares motion ... to be a "state" and not a "process" – in the Aristotelian sense" [Cohen 1970 p146].

One point that neither Cohen nor McGuire seem to have touched on is the question of how Newton could ever have arrived at the Second Law if impulse really was regarded as the primary quantity. Impulse cannot easily be measured, and certainly could not have been measured in Newton's day; nor, indeed, could the force of gravity, until perhaps a century after the publication of the *Principia*, when Cavendish used Michell's torsion balance to measure the gravitational force between two large weights. (Attempts had been made to measure the magnetic force during Newton's time – in fact Newton commented on Hooke's measurements – but these attempts were stymied by the fact that the force between *magnets* is not a centripetal force, and magnetic *poles* cannot be entirely isolated.) So Newton could not have abstracted his Laws from experimental data; but in the case of the force of gravity, he could, and no doubt did, arrive at the Second Law by verifying it – specifically, guessing its format and showing that Kepler's Laws could be deduced from it. This cannot be done for the impulse version of the law – luckily for the human race, there are no

impulses in planetary dynamics (at least, not this side of Jupiter!) This suggests that he arrived initially at a law of continuous forces, and only then adapted it for impulses.

On the other hand, several authors point out that Newton dealt with calculations involving continuous forces by modelling them as sequences of impulses; the trajectories of bodies then appear as polygons rather than smooth curves. In order to arrive at a curve such as an ellipse, it is of course necessary to take the limit as the durations of the impulses tend to zero. But the bulk of the calculation appears to be geometrical in nature. No doubt Newton either was happier using a sequence of impulses, or believed his readers would be. We must also bear in mind that, although Newton invented the calculus, the necessary infrastructure of algebraic representation, so universal nowadays, was at that time in its infancy.

More recently, quantum mechanics has taught us that there is no such thing as “contact”, and that therefore all forces act at a distance; moreover, since we also now understand that the percussive forces between material bodies are electromagnetic in origin, we would now see an impulse as simply a continuous force that acts for a very short time. The ability to film very rapidly occurring events and replay them in slow motion has also made it clear to us that no impulse is instantaneous. Nevertheless, we must try to forget this modern knowledge when considering the development of 17th century dynamics.

The Second Law as a Defining Equation

As stated in the Introduction, Newton’s Second Law is nowadays used to *define* force; the original wording said that “the alteration of motion is ever *proportional* to the motive force impress’d ...” (my emphasis), but in the modern form of the law the proportionality has been replaced by an equality, so that the equation defines force in terms of mass and acceleration, and force thus becomes what Percy Bridgman calls a secondary quantity [Bridgman p20]. Newton’s version of the Second Law, on the other hand, must, if transcribed into mathematical symbols, contain an arbitrary constant k . Cohen is faithful to this in his mathematical formulations; yet he has not entirely removed his 20th century blinkers, for he says that “mass is the measure of the proportionality of forces to accelerations” (and this immediately before an equation in which the constant is clearly “ $k.m$ ”!) [Cohen 1970 p153] among numerous other similar remarks.

But didn’t Newton define a force? Yes, he did, in Definition IV; part of this has already been quoted, the full text being:

“An impress’d force is an action exerted upon a body, in order to change its state, either of rest, or of moving uniformly forward in a right line”

[Newton vol 1, p3].

Thus Newton’s definition of force is inextricably linked to the force’s ability to accelerate a body on which it acts. Boudri, for instance, tells us that “after Newton, mechanics linked forces and motion almost by definition” [Boudri p32]. Almost, but not quite: the modern definition, which says that the force *is* the product of the mass of the body and the resulting acceleration, does not appear in the *Principia*, having been introduced at some later time. The ambiguity – between continuous and impulsive forces – in Newton’s use of the term, which Cohen makes a very thorough study of, survives Newton’s definition (since this can be true of both impulses and continuous forces, although it is couched in a language which suggests impulses) but has disappeared in the modern version. Cohen believes that Definition IV is deliberately vague; he points out that it allowed Newton “to deal with attractions and centripetal forces as if they had the same physical reality as percussion and pressure”; this was important because although Newton “believed personally in the reality” of these forces, he “was surely aware that this would hardly be the case for the majority of his contemporaries” [Cohen 1970 p154].

Some 20th century writers have pointed out the arbitrariness contained in the modern definition, and have suggested alternatives. Indeed, Ipsen goes so far as to say that the modern formulation of the Second Law, $\vec{F} = m\vec{a}$, “can be regarded as being chosen only in defiance of the conventional physical notions”. He suggests instead a constant of proportionality which he writes as $1/g_0$ [Ipsen p32]. Pankhurst also has a constant of proportionality k [Pankhurst p37]; however this constant is not arbitrary (depending on our choice of units) but is tied to an alternative definition of force in terms of gravitation.

Conclusion

I have found a number of differences between the commonly quoted version of Newton’s Second Law, and what he actually said. The main point of interest concerns the use of the word *force* in the 17th century, and the fact that it could apply equally to a continuous force and an impulse, which was not then thought of as simply a force multiplied by a time interval. I have also noted that the law was not, at that time, used to *define* force, as it is now.

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Letters to the Editor

Dear Sir,

Anthony Constable's article "Kaye and Laby — a Centenary" (Newsletter No 30, July 2012) elicited an interesting response from my friend Dr John Warren, veteran campaigner for rigour and accuracy in text-books, examinations and popular presentations of physics. Since John's response has an historical flavour, I thought it might be of interest to readers of the Newsletter.

In the later years of World War II, to be precise during the academic year 1944/45, John was a part-time student at the South-East Essex Technical College, in the final year of his B Sc course. In addition to following the prescribed final year laboratory course he also acted as a demonstrator for less advanced classes. He was therefore ideally placed to observe wartime practical physics at close quarters. He writes: "Despite all the difficulties of wartime, much apparatus and materials were nevertheless available for

educational purposes. However, there were shortages, often in the most basic items.”

This prompted him to pen the following lines, which in due course appeared under the pseudonym “Nil Desperandum” in the College Magazine.

The Experimental Physicist or Absolute L

The science student soon will find
 When he comes to this college,
 That he must do experiments
 To add unto his knowledge.

At first he likes “Elec and Mag”
 But very soon will tire,
 For when the circuit’s half complete,
 He finds there’s no more wire.

He wraps a calorimeter
 In insulating packing,
 But when he goes to find some ice —
 He finds that ice is lacking.

Experiments may sound all right
 Whilst the teacher’s speaking,
 But when he tries to pass some steam —
 He finds the boiler’s leaking.

But still, he has two noble friends
 Who always ‘hold the baby’
 For when at last the lesson ends,
 He looks up “Kaye and Laby”.

Stuart Leadstone
 Banchory, Kincardineshire

Kelvin and the Age of the Earth

Edward A Davis
Universities of Leicester and Cambridge



William Thomson (later Lord Kelvin) was the *bête noir* of the geological community at the end of the nineteenth century for his insistence that the Earth was certainly no older than 400 and probably as young as 20 million years. His estimate, made in 1863, was based on knowledge of the existing radial temperature gradient below the Earth's surface (which he took to be 1

$^{\circ}\text{F}$ per 50 feet) and his calculation of how long it would have taken to establish this gradient during the cooling of an initially hot molten sphere at $7,000^{\circ}\text{F}$ (3870°C) having a cold solid crust at 0°C [1]. The difficulty was that geological studies of sedimentary rocks, and the time needed for living organisms to evolve as suggested by the fossil record, both required a much older Earth.

Nevertheless, Kelvin's lower estimate for the age was supported by another of his calculations - namely one for the age of the Sun, which he put at no greater than 20 million years. He argued that the Earth could hardly be expected to be older than the Sun, except perhaps as a barren planet.

In the early years of the twentieth century, Ernest Rutherford, working with a chemist Frederick Soddy at the University of McGill in Canada, explained the recently discovered phenomenon of radioactivity in terms of the spontaneous transmutation of one element into another. He proposed that it might be possible to use the decay of radioactive elements to date the age of rocks by looking at the amount of helium they contained, since helium nuclei (alpha particles) were emitted at each atomic disintegration. One of his first estimates using this technique yielded 700 million years for the age of a sample of pitch-blende. How was this age to be reconciled with that proposed by Kelvin?

There was another aspect to radioactivity that it was believed could provide the answer to this question, namely the heat associated with radioactive transmutations. Indeed this was proposed by Rutherford - at a well-documented lecture at the Royal Institution in 1904 with Kelvin in the audience - as an extra source of terrestrial heat that could significantly slow down the cooling of the Earth. Rutherford's recollection of that meeting was later expressed in the following account.

"I came into the room, which was half dark, and presently spotted Lord Kelvin in the audience and realized that I was in trouble at the last part of the speech dealing with the age of the earth, where my views conflicted with his. To my relief he fell fast asleep but as I came to the important point, I saw the old bird sit up, open an eye and cock a baleful glance at me! Then sudden inspiration came, and I said 'Lord Kelvin had limited the age of the earth, provided no new source of heat was discovered. That prophetic utterance refers to what we are now considering tonight, radium! Behold!' The old boy beamed at me."

Rutherford, 1904

Later in 1904 Rutherford and Kelvin were invited, along with other guests, to the family seat of the Third Baron Rayleigh for a ‘full English Edwardian weekend’. One can only speculate at the discussions that took place at the gathering but it is likely that the topic of the age of the Earth would have been raised, particularly as Lord Rayleigh’s son, R.J. Strutt, was himself experimenting with the helium method of dating rocks and would almost certainly have sided with Rutherford.

In spite of a growing tide of opinion against Kelvin’s calculations, he never accepted that radioactive heating was a factor that could seriously undermine his estimate for the age of the Earth. *It now appears that he was justified in not doing so.* Stubborn he might have been in the face of geological and biological evidence and the results of radioactive dating, but the main reason for his estimate being a factor of about 200 too short (the Earth is currently considered to be about 4.5 billion years old) turns out to be that the Earth has a highly viscous fluid lower mantle, which transfers heat from the interior outwards by convection rather than by conduction in a uniform Earth as Kelvin had assumed (see [2]). A higher rate of transfer of internal heat keeps the upper mantle (with a depth ~ 660 km below the thin crust) warmer for longer and hence more time is required to attain the present geothermal gradient.

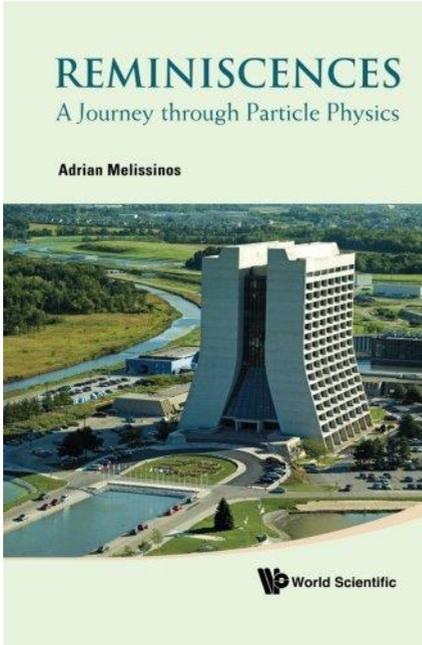
Ironically, Kelvin’s former assistant, John Perry, had challenged Kelvin’s assumption of a low thermal conductivity and showed that if the Earth had a convective lower mantle and a thin crust, it could be as old as 2-3 billion years. Perry tried privately to persuade Kelvin to modify his views. The efforts failed and the discussion became public in 1895 with the publication of papers in Nature [3].

It turned out that many eminent physicists of the day sided with Kelvin, and Perry, not wanting to see his pupil/teacher relationship with Kelvin worsen, made no further attempts to try to convert him. Nevertheless he had at least won the hearts of geologists and biologists by demonstrating that Kelvin’s assumptions could be challenged and thereby freeing them from the obligation of trying to fit their own findings into the restricted timescale that Kelvin had proposed.

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Book Reviews



Reminiscences: a journey through particle physics

Adrian Melissinos

World Scientific 2013
 ISBN 978-981-4405-00-3
 202pp Hardback £26.60

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

Reminiscences is much more a participant's view of the progression of particle physics from 1958 to 2008 than it is about personal remembrances. It is a mainly chronological recollection in twelve chapters of how Melissinos and his collaborators contributed to experiments and instrumental development in high energy physics across half a century. Although he visited CERN earlier and spent sabbatical years on the European Muon Collaboration (1977-78) and the Large Electron Positron Collider (1989-90) projects, Melissinos is probably better known in the USA than in Europe. He is the author of well-regarded and updated physics text books and there was a big Adrianfest in his honour in 1999 at the Dept of Physics and Astrophysics in Rochester, NY, where he spent much of his career.

Melissinis's education (presumably during the Civil War) at the Greek Naval Academy was followed by 1948-1954 at sea before he went to MIT for his PhD, 1955-58, which is where this book begins. There, as assistant to F Bitter (of the high magnetic fields), he was engaged in double optical resonance and specifically involved with the radioactive Hg-197. Leaving atomic physics in 1958, he went to Rochester as a nuclear emulsions post-doc under Mort Kaplan but in 1959 he became an early resident guest user of the Cosmotron proton synchrotron at the Brookhaven National Laboratory, NY. The BNL collaboration continued with muon-proton scattering in the 1960s and early 1970s. Melissinis devotes one chapter to a collaboration, unusual in the Cold War times of the early 1970s, the US-USSR gas-jet experiment, partly at Dubna and partly at the Fermi National Accelerator Laboratory or Fermilab. Into the 1990s, the Rochester group, with its experience in short-pulse lasers, collaborated in photoinjector research at the two-mile long Stanford Linear Accelerator (SLAC) and Fermilab.

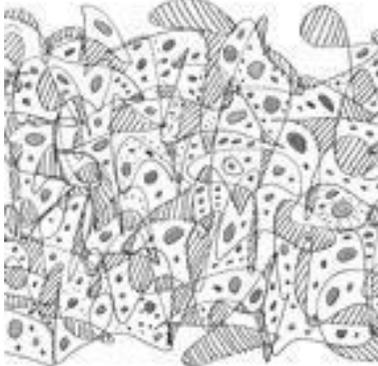
A final chapter The Earth Tides describes Rochester involvement from 2001 to 2009 with the Laser Interferometer (in the Michelson configuration) Gravitational Observatory at the Hanford (Washington State) lab. The predicted effect of the tidal component seems to be much weaker than that observed but apparently the sensitivity of detecting gravitational waves has so improved that future detection is predicted. For the future more generally, Melissinis sees some shift in participation from Universities to staff and guest workers at international centres. He feels that nowadays experimenters and the large numbers needed in accelerator building are such distinct communities that they have different training needs. The extreme cost of reaching higher energies in the laboratory point to greater concentration on cosmic phenomena.

Melissinis includes over 90 detailed figures, showing apparatus, experimental layouts, spectra and graphs, many extracted from papers in the journals, especially *Phys Rev* and *Phys Rev Lett*. Repetition of their legends and pages on pp 175-186 seems unnecessary. Both text and figures contain much experimental detail and are awash with abbreviations of instruments and national or international facilities. A list of these abbreviations and a time line of the author's involvement with them would have been helpful. The small pack of photographs shows mainly research groups; in the book Melissinis names both senior colleagues and research workers, students and post-docs, at each stage. There is a name index separate from the subject index.

Appropriately, the book is dedicated to senior and junior colleagues, many of them at accelerator institutes outside Rochester; however, the actual dates and even locations are sometimes difficult to extract.

Reminiscences is valuable for science historians and physicists interested in a short but detached review of how significant parts of particle-physics research advanced in parallel with instrumental developments, particularly in the USA, from 1960 to the 2000s. It does not attempt a biographical introduction or a reflective ending. Melissonis became a Visiting Professor at Athens in 1996 but his book, confined to science, gives no hint of why a former naval person in Greece should abandon one career in his mid-twenties to emigrate and aim for academic life in America.

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### **A Palette of Particles**

Jeremy Bernstein

a palette of particles  
**JEREMY BERNSTEIN**

Harvard University Press      2013  
 ISBN      9780674072510  
 224pp      Hardback £14.95

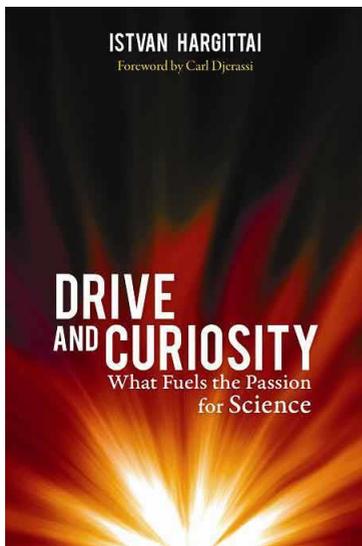
***Reviewed by Dr. Peter Ford***  
***University of Bath***

The unusual title of this book is immediately explained to us in the Introduction: "I have been exposed to the physics of elementary particles for over half a century. These particles now appear to me as colors in a palette that can be used to compose the tableau of the universe. ....".

Within this theme the Contents of the book are divided into three sections. Section I is devoted to Primary Colors. There are separate chapters on The Neutron, The Neutrino and The Electron and the Photon. Section II has the Secondary Colors with separate chapters on The Pion and the Muon, The Antiparticle, Strange Particles and The Quark. Section III is called Pastels and discusses in separate chapters The Higgs Boson, Neutrino Cosmology and Squarks, Tachyons and the Graviton. This is followed by three Appendixes on Accelerators and Detectors, Grand Unification and Neutrino Oscillations.

A lot of interesting information is packed into a very small and compact volume of about two hundred pages, which can easily be slipped into a jacket pocket. The author Jeremy Bernstein is justifiably regarded as an outstanding writer of science for the general reader and has produced many books on a wide variety of subjects. The present book is no exception being clearly and concisely written on a subject area which is of considerable interest. Being published this year, it is able to confirm the likelihood of the existence of the elusive Higgs boson detected by the famous Large Hadron Collider at CERN in Geneva. As he said in the Introduction, Bernstein has been involved in this area of physics for more than fifty years, although part of this time in a somewhat peripheral manner, and he is therefore able to enrich the book with some of his personal reflections. I especially enjoyed his vignettes on Wolfgang Pauli and Sheldon Glashow, both of whom obtained Nobel Prizes in Physics, as well as him using the building housing the particle accelerator the Cosmotron at Brookhaven as a suitable venue to play his trumpet when the machine was not running.

This is an excellent short book. However, almost all of the subject matter has been extensively covered by a large number of other excellent books and articles, which this one in no way supersedes. However, it is a good and easy read which I greatly enjoyed and thoroughly recommend.



**Drive and Curiosity: what fuels the passion**

Istvan Hargittai,

*Prometheus Books* 2011  
 ISBN 978-1-61614-468-5  
 338 pp Hardcover £20

***Reviewed by Emeritus Prof. Derry W Jones  
 Applied Science, University of Bradford\****

There have been a number of attempts to encapsulate the crucial factors that enable a scientist to make a significantly creative rather than a merely incremental discovery. The greater predominance of financial rewards may have led to more emphasis on research aimed at gradual improvements and there is some perception that creativity has decreased by comparison with the mid-20<sup>th</sup> century. Reflecting on the later 20<sup>th</sup> century physics, Nobel prizewinner WA Anderson regrets both the increased tendency towards quantity over quality in publications and also the reduction in support by government and industry of intellectually exciting research. Istvan Hargittai, a respected scientist brought up in Hungary but with wide international experience, has made serious research contributions in symmetry, crystallography and molecular structure. In the past decade or so he has carried out informed relaxed interviews with over 200 Nobel-level scientists and has written short and longer informal biographies. These have led to 14 books (some jointly with Magdolna Hargittai), including six volumes of the *Candid Science* series.

\* First published in RSC History of Chemistry Newsletter

In *Drive and curiosity*, Hargittai has drawn on this experience to select 15 distinguished scientists, mainly chemists or physicists, each of whom represents a character trait that, coupled with drive and curiosity, has led to genuine creativity and discovery in the field of that chapter. Most are well-known and the majority are Nobel prizewinners. One, Dan Shechtman representing *Stubbornness*, was even awarded the 2011 Chemistry Nobel (for a discovery in 1982) during publication. Although many achieved success in the USA and a few in Britain, a good proportion were of German or Eastern European extraction, with the scientist or parents leaving because of anti-Jewish discrimination; three were brought up in Hungary. The least familiar is the Romanian/Hungarian combinatorial chemist Arpad Furka (born 1931). Coming from a humble farm-worker background in troubled political times, Furka had (like many of the examples) much to overcome; but his technique for peptide synthesis is chosen to illustrate the stimulus of *Saving time and labour*.

In the 1940s and 1950s, women in research had an extra handicap to overcome, not least in the USA, in addition to any personal tragedy. Chemist Gertrude Elion (1918-1999) and physicist Rosalyn Yalow (1921-2011), who epitomise the themes of *Personal tragedy* and *Proving oneself* as incentives, each had to surmount poor immigrant backgrounds before encountering academic prejudice. Yalow, who later coped with partial paralysis, began physics research as the only woman in a large engineering faculty. Each woman had a long scientific partnership with a better known male collaborator. Although Solomon Bersohn was nominated before his death in 1972, he could not share the Nobel with Yalow when it was actually awarded in 1977. Elion shared the Nobel in 1988 for drug development and treatment with George Hitchings, previously her senior in a pharmaceutical lab from 1944. Rosalind Franklin (1920-1958), considered in Hargittai's Watson chapter, had an unfriendly relationship with her colleague Wilkins and died before the Nobel was awarded to Crick, Watson and Wilkins in 1962. Georgina Ferry has recently outlined the ideal family, location, and education from school to post-doc (in a developing interdisciplinary field) for young women to achieve scientific distinction in the UK; parental support, single-mindedness and stamina are *sine qua non*.

Despite the above, Hargittai chooses the magnetic resonance imaging (MRI) pioneers Peter Mansfield (born 1933) and Paul Lauterbur (1929-2007) to illustrate the *Overcoming of handicaps* in scientific education. Mansfield trained first as a composer, began his degree at 23 and, through immense drive, graduated Ph D at age 29 while Lauterbur was 33.

Military service was a delaying experience for some but Lauterbur was able to specialize in NMR spectroscopy at a US Army Chemical Center. Active-service experience in Vietnam transformed the aspiration of Craig Venter (not described here) towards medically related research. Lauterbur had great difficulty in getting his ideas for zeugmatography, the reconstruction of two-dimensional images, recognized, funded, published or patented, while Mansfield even had to contend with Raymond Andrew's group competing in the same department. The first hint that NMR could be applied medically came from Raymond Damadian, who did not receive a share of the MRI prize. As one supported in cancer research, I recall being intrigued around 1971 by reprints of his pioneering NMR relaxation-time tumour-detection papers. Damadian took whole-page newspaper advertisements in Britain and the USA protesting that he should share in the Nobel (although he received other awards).

In the double-helix chapter, James Watson (born 1928 into a supportive family) is presented as the 'ignorant' genius, in that his lack of awareness of the limitations of structural chemistry caused him to aim further. His partner, Francis Crick (1916-2004), who spent seven years at the British Admiralty on R and D in magnetism, acoustics and electronics before moving towards biophysics is, I feel, an exemplar of collaboration. At successive stages in his career, Crick engaged in fruitful partnerships and so extended his creativity into old age. The right co-worker can fulfill one of Watson's criteria for success: ensure that ideas are exposed to informed criticism. Another example of this is the 2000 Chemistry Nobel for conducting polymers awarded to Alan Macdiarmid (1927-2007) together with his younger partners, the entrepreneurial physicist Alan Heeger and the polymer chemist Hideki Shirakawa. Hargittai uses their achievement to highlight the *Risk to reputation* that Macdiarmid took in moving mid-career from inorganic to unfamiliar organic polymer chemistry.

Sherwood Rowland (1927-2012) has a chapter headed *Reluctant environmentalist* because his first environment-related research exonerated industry by finding that mercury in ocean-going fish was not a consequence of industrial pollution. After realizing with Mario Molina that ozone was being removed from the atmosphere he suffered many years as a research outcast, despite being confident of meticulous measurements. They shared the Chemistry Nobel with Paul Crutzen in 1995 but Rowland (then aged 68) remained a fairly restrained environmentalist.

He had been prompted to investigate chlorofluorocarbons in the atmosphere when he became aware of the measurements of James Lovelock who had in the 1950s developed GC detectors (See AS Travis, *RSC Hist Gp N/L*, 2012, **62** 18-25).

Hargittai's final theme *The joy of understanding* concerns the unorthodox genius George Gamow, keen on jokes from his early days in Ukraine and Russia. Despite outstanding contributions to nuclear fusion and astrophysics, including the famous Alpher, Bethe (a contrived non-contributing author) and Gamow paper on the Big Bang, Gamow received few prestigious prizes. Best known for his fine semi-popular books, Gamow did the science that entertained him. Incidentally, Fred Hoyle, the steady-state enthusiast who coined the derisive expression Big Bang for what was presumably the ultimate creative event, intended to read Chemistry at Leeds until a scholarship visit lured him to Mathematics at Cambridge

Hargittai does not pretend that there is a common way for such diverse personalities to achieve outstanding science although most Nobel prizewinners seem to cross conventional disciplinary boundaries. (Lauterbur thought all good research was interdisciplinary.) Obviously, scientific achievers do not all fall into one or other of the 15 chapter types, which include competition (Linus Pauling) and beating Nature (Neil Bartlett) but not seeking fame. The quiet biochemist Frederick Sanger (born 1918) said that possession of two Nobel prizes gave him a secure job! Those who have read any of Hargittai's collections of miniature biographies will doubtless select different representatives and, indeed, make different classifications of motivation. Unusually, the printed pages of this book begin with recommendations from six Nobel Laureates and three other distinguished scientists (with six more on the jacket) and the Forward, Preface and Introduction by equally eminent scientists are all favourable. In the light of this praise, one can add only that Hargittai's biographical collections generally contain thoughtful insights into the genesis of discoveries that are worth reading; this is no exception.

## Reference

G Ferry in *The Royal Society and Science in the 20<sup>th</sup> Century*, ed. P Collins, *Notes Rec R Soc*, 2010, **64**, 163-172.