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Cover picture: Late 17th Century French or Italian microscope.
Courtesy: Museum of Hessen Kassel

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Editorial

Physicists, Historians and Newspaper Reporters

Who are the better purveyors of fine history of physics writing – historically inclined physicists or physics oriented historians? This is an argument which still smoulders unabated between those who stand on the side of the physicist, with their intimate and hard earned knowledge of their discipline and those who place a high premium on the equally specialist but very different practitioners in the art and methodology of the history of science.

There has long been a need for a resolution to, or at least some progress in, this thorny question and I am pleased to report that the foundations have been laid for just that. The matter is adroitly addressed by a new section of the 'Annalen der Physik' published by Wiley-VCH with the well chosen title 'Then and Now'. The idea was initiated by Dieter Hoffmann and Christian Joas who have written an excellent editorial on how they see the problem and its solution. This can be read at: http://onlinelibrary.wiley.com/doi/10.1002/andp.201100709/pdf

So what of newspaper reporters? There is at least one other group writing about the history of physics – that of the professional science writer many of whom come from a background in journalism or broadcasting. But we may look in the other direction – the reporting of scientific issues to the general public. This is, of course, a vast area of interest but it leads me to the recent meeting co-sponsored by our group and the Manchester & District Branch: 'Rutherford and the Nuclear Atom'

One might think that 2011 should have been a more appropriate year but the organisers thinking in broader terms carried the audience a couple of decades forward from 1911 to greater enlightenment of Chadwick's discovery of the neutron. The lectures included a look at the buildings used by Rutherford, a most interesting account of hitherto unknown talent behind the experimental equipment used by Chadwick and a look at how Rutherford's discoveries were presented in the popular press. Star Trek's Captain Kirk might well have said they boldly went where no conference organisers have been before – and went very successfully indeed!

Malcolm Cooper

Meeting reports

'Centenary Celebration of Rutherford and the Nuclear Atom' held March 31st , University of Manchester – co-sponsored by the History of Physics Group and Manchester & District Branch, IOP.

As I commented in my editorial, this meeting, attended by some 40 people, proved to be a most fascinating and interesting day with a fresh approach to the menu of talks on offer.

After a welcome by Peter Rowlands, we were privileged to be taken on a 'tour' which included a visit to the Rutherford Building. I say privileged because access is severely restricted on account of the level of contamination which still exists as a hang over from the days of rather cavalier handling of radioactive materials. We saw the staircase where it is thought that Marsden and Geiger excitedly gave Rutherford the results of the wide angle scattering experiment but we were – quite properly – denied access to the still 'hot' basement.

Following an excellent lunch came the first lecture entitled *'The 1912 extension to the physical laboratories'* by Neil Todd. The extra space was necessary to accommodate the huge increase in the level of research under Rutherford and its consequent demands on laboratory space. It was quite unusual to hear details of such practicalities but it was most interesting to have the opportunity to appreciate some of the problems of administration in those exciting times.

Group A outside the Rutherford Building

From the unusual to the very unusual the next lecture, *'Rutherford's Resonance'* by Brian Cathcart, gave us a rare insight into the way the press received and promulgated results of scientific research in the early part of the 20th C, taking as examples the Rutherford atom of 1911 and nuclear fission by Cockcroft and Walton in 1932.

The last lecture, *'The apparatus used for the discovery of the neutron'*, given by Geoffrey Constable, delighted the audience by presenting new evidence about who actually did design and build the apparatus for Chadwick's discovery of the neutron – some surprises were in store.

All in all this was a most enjoyable and illuminating meeting and the organisers are to be congratulated on their vision to bring new ideas to the conference. MJ Cooper

NB All these lectures will be published in a special issue due out later in the year - Editor

100 Years of Cosmic Rays

The 2nd International conference of the series 'The Roots of Physics in Europe' Pöllau Castle, Austria, May 4th/5th 2012

'A Grand Two Day Out'

This 2 day conference, celebrating the discovery of so called 'cosmic rays' by Victor Franz Hess in 1912, attracted over 90 participants who heard lectures from some 20 speakers – mostly from Europe, but some from the USA, Mexico and Russia. The lectures were very varied – many focussing on national contributions to cosmic ray research from Poland, Russia, Germany, Mexico, UK and, of course, Austria. Others considered different aspects for example Helge Kragh's Cosmic Rays and early physical cosmology and J M Sanchez Ron's (right) 'Physics and Spanish Politics' – the latter having particular interest to our UK members as it dealt with the work of Arturo Duperier who was 'exiled' in England during WWII and a colleague of PMS Blackett.

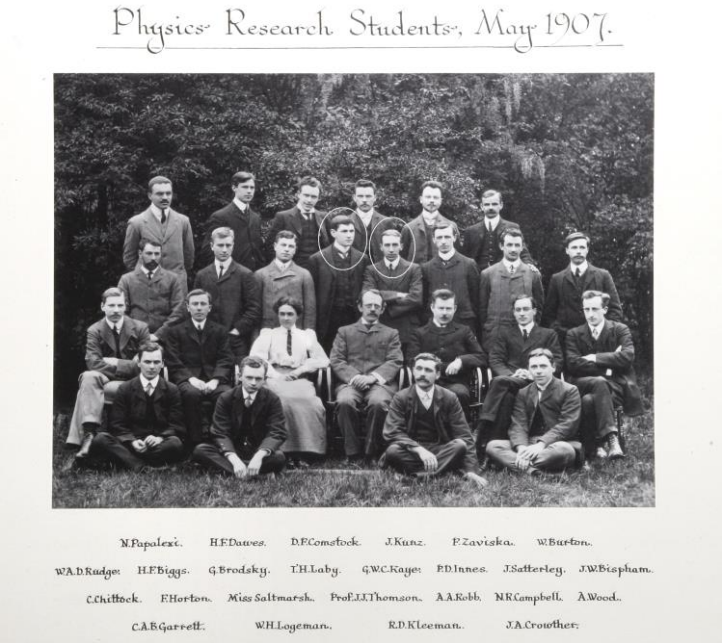
The UK was represented by Sir Arnold Wolfendale who revelled in giving an 'off the cuff' run down on 'British contributions to early cosmic ray research', and not passing up the opportunity of a little fun afforded by the presence of a Minister of State.

As is customary with these occasions the conference was accompanied by much pomp and a good deal of circumstance. There were addresses given by Dr. PM Schuster - the President of the VF Hess Society, Emeritus Professor Dr Harmut Kahlert - the Rector of Graz University, Johann Schirnehofer - the Mayor of Pöllau, and Prof. Dr. Karlheinz Tocheterle, the Austrian Federal Minister for Science and Research. The audience was treated to two musical intermezzi by the brass and woodwind ensembles of Pöllau, and there was an opportunity to purchase a first day cover of the Hess commemorative postage stamp issue. The whole extra curricular activities were topped off with a hot air balloon inflation in the castle courtyard by Austrian balloonist, Josef Starkbaum.

The conference was a great success and congratulations must go to its indefatigable organiser Peter Schuster.

Kaye & Laby - A Centenary

Dr Anthony Constable
Ealing, London

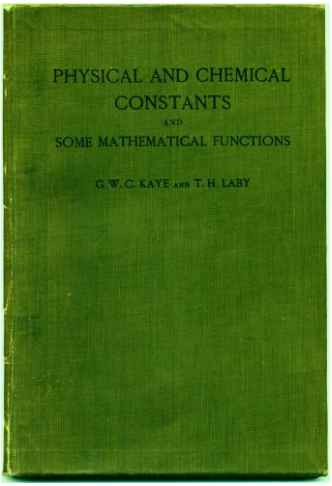


The physicist's most widely used reference book over the last 100 years must certainly be, *Tables of Physical and Chemical Constants and some Mathematical Functions by G. W. C. Kaye and T. H. Laby.*

This book, first published by Longman Green in October 1911, has recently achieved its centenary. It ran through numerous editions and reprints until the 16th and final edition made its appearance in 1995, the edition that has now become the online version we are able freely to access on the NPL web pages.

The original title of the book soon became unnecessary as the two author's names slipped so easily and rhythmically off the tongue to became the perfectly accepted title 'Kaye and Laby'. This was evidently recognised by the publishers as the following images show how the words KAYE & LABY shifted from its original position into that of a full title.

Above photograph reproduced by kind permission of the Cavendish Laboratory



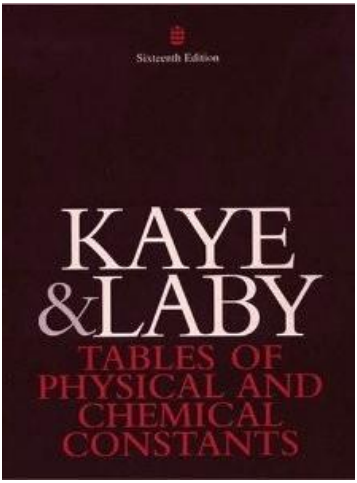


Fig. 1 Kaye & Laby, 1st and 16th editions

Despite our long familiarity with the names, KAYE & LABY, there is not a widespread knowledge of the men themselves: *George William Clarkeson Kaye* and *Thomas Howell Laby*.

In the fullness of time, both Kaye and Laby became Fellows of the Royal Society and I am grateful to that illustrious society for permitting access to the detailed biographies that were prepared for their obituaries (1).

George William Clarkeson Kaye

George Kaye first studied at Huddersfield Technical College and then briefly at Liverpool under Oliver Lodge. He then moved to the Royal College of Science, London, to study physics under Professors H. L. Callendar and John Perry. After graduating he stayed on there for a further year as demonstrator before moving on to Trinity College Cambridge in 1905 to become one of J. J. Thomson's research students in atomic physics and later his research assistant.

Fig 2

By 1911 he had added to his qualifications a Cambridge B.A. and a London D.Sc. At the Cavendish Lab he worked on the penetrating powers of X-Rays and the relationship between total X-ray emission and atomic weight and also demonstrated the existence of characteristic X-rays of elements (2), a precursor to the famous Henry Moseley experiments in 1913. After leaving Cambridge in 1911 Kaye went to work at the NPL in the metrology department and, after 1920, was in charge of radium testing there. He continued to retain a strong interest in X-rays and wrote one of the early books on the subject in 1914 entitled *'X RAYS'* (3). He briefly served in the Royal Engineers in WWI and after the war became a leading light on the subject of X-ray protection in the hospital environment. After 1922 he also took a great interest in acoustics, particularly architectural acoustics, which eventually led to the NPL's famous acoustic laboratory in 1933. He was the examiner in medical physics for the Universities of London and Glasgow. He was also a member of the Röntgen Society, the forerunner of the British Institute of Radiology, and became its president in 1917. Kaye became chairman of the International X-ray and Radium Protection Commission of the Fifth International Congress of Radiology held in Chicago in 1937. He continued to work at the NPL until his death in 1941 and his final position there in the 1930s was Superintendent of the Physics Department.

Thomas Howell Laby

(Fig 4)

Before going to Cambridge, Thomas Laby, who had never matriculated, worked as a demonstrator in the Chemistry School at the University of Sydney. His work on changes of weight in chemical reactions came to the attention of Lord Rayleigh, the President of the Royal Society, who strongly recommended that he be nominated for a special award (the '1851' award) to study in England where he first went to work with J.H.Poynting in Birmingham. Poynting recommended that Laby should go to the Cavendish Lab to work under J. J. Thomson. He arrived there in 1905 as an 'Advanced Student' - a device which allowed promising 'mature' students to work for a Cambridge BA without participation in the standard undergraduate process. Rutherford himself had done that 10 years earlier.

After two years Laby was granted a Cambridge B.A. by research on the basis of two theses, *"On the ionization produced by alpha particles"* and *"The supersaturation and nuclear condensation of organic vapours"* both of which were communicated to the Royal Society by J.J.Thomson (4) & (5). He then applied for the post of Physics Professor at Victoria University, Wellington, New Zealand and was appointed in 1909. In 1915, he became Professor of Natural Philosophy at the University of Melbourne where he remained until a year before his death in 1946.

When these two young research students arrived in Cambridge in their mid-20s they set about their assigned tasks with much attention to detail and found the constant need to accumulate good values of the known physical constants from many different sources, in some cases directly from recent research papers and with much of the atomic physics data being then generated at the Cavendish Lab itself. Their systematic collection of data became known to and used by others working at the Cavendish Lab. They were encouraged by colleague G. A. Carse to begin thinking about publishing their data in the form of a book and they set about the task with encouragement and suggestions from G. F. C. Searle, the Cavendish Lab lecturer, demonstrator and author who is well known for his numerous contributions to the design of laboratory apparatus.

Fig. 4 The old Cavendish Laboratory, Free School Lane, Cambridge

By the time their famous book appeared in October 1911, Kaye was working at the NPL and Laby was professor of physics at Victoria University College, Wellington, New Zealand. These days it is difficult to imagine how two authors separated by 12,000 miles could undertake such a project without a massive interchange of emails etc. However, they had already planned everything while they were together in Cambridge and appear to have divided the remaining tasks between themselves before Laby sailed to New Zealand. Laby remained in charge of Chemical data while Kaye handled all the physical data at the National Physical Laboratory.

The well organised scheme of the book can be seen by glancing at the contents:

	PAGES	pp
GENERAL PHYSICS, ASTRONOMY, ETC	**1 - 43**	**43**
HEAT	**44 - 66**	**23**
SOUND	**67 – 68**	**2**
LIGHT	**69 - 80**	**12**
ELECTRICITY	**81 - 88**	**8**
MAGNETISM	**89 - 93**	**4**
RADIOACTIVITY AND GASEOUS IONIZATION	**94 - 108**	**15**
CHEMISTRY	**109-128**	**20**
MATHEMATICAL TABLES	**129-147**	**19**
INDEX	**148-153**	**6**

The comprehensive introductory section on general physics includes unit definitions, dimensions, conversion factors as well as a wide range of basic physical data. There are separate sections for the classical subdivisions of physics and this is one of the earliest books to contain a good section (15 pages) of data on radioactivity from a wide variety of sources as well as from their research colleagues at the Cavendish laboratory.

I was fortunate enough to find a first edition of Kaye and Laby in a local second hand book shop about fifteen years ago and, as a keen collector of old scientific instruments, have found it quite invaluable when checking electrical and other data in accordance with definitions and units appropriate to the period 1890-1910 when instruments were designed and made by or for such well known men as Kelvin, Ayrton & Mather, Robert W. Paul and others.

The book's layout is extremely clear with words and tables contained in neatly bordered pages with certain key words picked out in bold type and numerous references to primary sources as shown in the adjoining illustration of the table of densities on page 22 (Fig.5).

DENSITY OF WATER

In grams per millilitre.* Pure air-free water under 1 atmos. Temps. on const.-vol. H. scale. Water has a **maximum density** at 3°·98 (Chappuis, 1897; Thiesen, Scheel and Diesselhorst; De Coppet, 1903). The temp. (t_m) of maximum density at different pressures (p), measured in atmos., is given by $t_m = 3·98 - ·0225(p - 1)$.

The **specific volume** is the reciprocal of the density. For reciprocals, see p. 136. (See Chappuis, *Trav. et Mém. Bur. Intl.*, **13**, 1907; and Scheel, L.B.M.)

For density of ice see p. 21; of steam, p. 26. [* 1 litre = 1000·027 c.cs.]

Density of water at **−10°** = ·99815; at **−5°** = ·99930.

Temp.	0	2	4	6	8	10	12	14	16	18
0° C.	·99987	·99997	1·00000	·99997	·99988	·99973	·99953	·99927	·99897	·99862
20	·99823	·99780	·99732	·99681	·99626	·99567	·99505	·99440	·99371	·9930
40	·9922	·9915	·9907	·9898	·9890	·9881	·9872	·9862	·9853	·9843
60	·9832	·9822	·9811	·9801	·9789	·9778	·9767	·9755	·9743	·9731
80	·9718	·9706	·9693	·9680	·9667	·9653	·9640	·9626	·9612	·9598
100	·9584	—	—	—	—	·951	—	—	—	—

Density at **150°** = ·917; at **200°** = ·863; at **250°** = ·79; at **300°** = ·70.

DENSITY OF MERCURY

In grams per c.c. Hydrogen scale of temp. For reciprocals, see p. 136. (See Chappuis, *Trav. et Mém. Bur. Intl.*, **13**, 1907; and Scheel, 1905, L.B.M.)

Temp.	0	2	4	6	8	10	12	14	16	18
	13	13	13	13	13	13	13	13	13	13
−20° C.	·6450	·6400	·6351	·6301	·6251	·6202	·6152	·6103	·6053	·6004
0	·5955	·5905	·5856	·5806	·5757	·5708	·5659	·5609	·5560	·5511
20	·5462	·5413	·5364	·5315	·5266	·5217	·5168	·5119	·5070	·5022
40	·4973	·4924	·4875	·4826	·4778	·4729	·4680	·4632	·4583	·4534
60	·4486	·4437	·4389	·4340	·4292	·4243	·4195	·4146	·4098	·4050
80	·4001	·3953	·3904	·3856	·3808	·3759	·3711	·3663	·3615	·3566
	0	**20**	**40**	**60**	**80**	**100**	**120**	**140**	**160**	**180**
100	13·3518	13·304	13·257	13·209	13·162	13·115	13·068	13·021	12·974	12·927
300	12·881	12·834	12·787	12·740	—	—	—	—	—	—

DENSITY OF ETHYL ALCOHOL, C_2H_5OH . Aq

In grams per c.c. % indicates grams of C_2H_5OH in 100 grams of aqueous solution. Hydrogen scale of temp. (Calculated by E. W. Morley from Mendeléeff's Observations, *Jour. Am. Chem. Soc.*, Oct. 1904.)

At 17° C.

%	0	1	2	3	4	5	6	7	8	9
0	·9988	·9969	·9951	·9933	·9916	·9899	·9884	·9869	·9854	·9840
10	·9826	·9813	·9800	·9787	·9775	·9762	·9750	·9737	·9725	·9713
20	·9700	·9687	·9674	·9661	·9647	·9633	·9619	·9604	·9589	·9573
30	·9557	·9540	·9524	·9506	·9489	·9470	·9452	·9433	·9414	·9394
40	·9375	·9354	·9334	·9313	·9292	·9271	·9250	·9228	·9207	·9185
50	·9163	·9140	·9118	·9096	·9073	·9051	·9028	·9005	·8982	·8959
60	·8936	·8913	·8890	·8867	·8843	·8820	·8797	·8773	·8749	·8726
70	·8702	·8678	·8655	·8631	·8607	·8582	·8558	·8534	·8510	·8485
80	·8461	·8436	·8411	·8386	·8361	·8336	·8310	·8285	·8259	·8232
90	·8206	·8179	·8152	·8124	·8096	·8068	·8039	·8010	·7980	·7950
100	·7919	—	—	—	—	—	—	—	—	—

For other temperatures, interpolate from the above and the following:—

At 22° C.

0%, ·9978; **10**%, ·9813; **20**%, ·9678; **30**%, ·9526; **40**%, ·9338; **50**%, ·9122; **60**%, ·8895; **70**%, ·8660; **80**%, ·8417; **90**%, ·8162; **100**%, ·7876.

Fig. 5

This table of densities of water and alcohol is, for most practical purposes, just as useful today as when the book first appeared in 1911. Kaye and Laby occasionally slip in extra data such as, in this case, the density of water at -10°C and -5°C to extend the range a little.

Not all the original tables survived as the book passed through its sixteen editions and some disappeared in later editions as they became less useful, unnecessary or simply unacceptable as primary data. For example, the table of sparking potentials on page 93 (Fig. 6) was very relevant in 1911 when engineers were installing X-ray equipment in hospitals and often used the spark length to estimate the potential of the high voltage generators then in use such as Ruhmkorff Coils and Wimshurst Machines.

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SPARKING POTENTIALS

SPARKING POTENTIALS

The sparking voltages given below are those which will break down non-ionized air at atmospheric pressure and room temperature. The electrodes are equal smooth polished metal balls of various diameters. Russell (*Phil. Mag.*, 1906) gives the dielectric strength of air at atmospheric pressures as between 38,000 and 39,000 volts for either direct or alternating potentials.

(See J. J. Thomson, "Conduction of Electricity through Gases.")

Spark gap.	Diameter of balls in cms.				Spark gap.	Diameter of balls in cms.			
	0·5	1·0	2·0	5·0		0 5	1·0	2·0	5·0
cm.	volts. × 10^3	volts. × 10^3	volts. × 10^3	volts. × 10^3	cm.	volts. × 10^3	volts. × 10^3	volts. × 10^3	volts. × 10^3
0·1	4·8	4·8	4·7	—	**0·9**	19·6	25·6	28·6	30·1
0·2	8·4	8·4	8·1	—	**1·0**	20·2	26·7	30·8	32·7
0·3	11·3	11·4	11·4	—	**1·5**	22	31·6	39	46
0·4	13·8	14·4	14 5	—	**2·0**	23	36	47	58
0·5	15·7	17·3	17·5	18·4	**3·0**	24	42	57	77
0·6	17·2	19·9	20·4	21·6	**4·0**	25	45	64	92
0·7	18·3	22·0	23·2	24·6	**5·0**	26	47	69	105
0·8	19·0	24·1	26·0	27·4					

Fig. 6

Kay's book 'X RAYS' (2) contains a somewhat more complete version…. from 1 to 220 kV AC and from 5 to 190 kV DC. Such a table is still of use to collectors of Ruhmkorff coils, Wimshurst machines and electrostatic voltmeters. The 1958 edition of K & L includes a rather wry little note on sparking potentials which shows all the usual caution of a well formed committee and refers the reader to the BS (1939) specification while providing no data whatsoever.

The radioactivity data in the 1911 edition is completely in tune with the times and puts us in direct contact with front line early 20th century research. There are 15 pages of data much of it coming right out of the work of the Cavendish Lab and strongly associated with the names of J.J.Thompson and Earnest Rutherford. Most of it is easily recognizable today but some modern readers may wonder what the chemical symbol **Io** refers to. It means **Ionium** - a name given to a decay product of U-238 initially thought to be a new element but which later turned out to be an isotope of Thorium i.e. the radionuclide Th-230. The term "isotope" made its first appearance in 1913/14 with the work of Frederick Soddy, much too late for the first edition of K & L.

There is a small section on page 104 (Fig. 7) that is quite intriguing for historians of science. It deals with the consequences of the work of Rutherford, Soddy and Wilson who in 1903 had suggested the heat liberated by radioactive changes was the source of energy within the earth that Kelvin had been unaware of when he had claimed the earth to be only 24 million years old. The well known story of how Rutherford brought the heating effect of buried radium to the attention of 'the old bird' while delivering a lecture at the Royal Institution in 1904 is frequently quoted. However, the contribution of radium was by no means the only thing Kelvin neglected: had he not neglected convection, as claimed by John Perry, the age of the earth would have been much closer to the billions of years the geologists required, as discussed by Philip England (6).

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$3{\cdot}4 \times 10^{-7}$ gm. Ra is in equilibrium with 1 gm. U (Rutherford and Boltwood, *A.J.S.*, 1906). $7{\cdot}3 \times 10^{6}$ gms. U equal in activity 1 gm. of Ra + its products to RaC. *i.e.* Ra just over 30 days old (corrected by Boltwood, *A.J.S.*, 1908).

RADIUM AND THORIUM IN ROCKS

Rutherford and Soddy (*P.M.*, May, 1903) and W. E. Wilson (*Nature*, July, 1903) suggested that the heat liberated by radioactive changes is one of the sources of the Earth's heat. Thus the distribution of radium and thorium in the Earth's crust is of geophysical importance. Loss of heat from the Earth's surface = temperature gradient × thermal conductivity of crust × area of Earth's surface = $(1/3200) \times {\cdot}004 \times 5{\cdot}1 \times 10^{18} = 6 \times 10^{12}$ calories per sec. Now, elementary radium in radioactive equilibrium (*i.e.* whole U family) gives out 6×10^{-2} cal./sec. gm. (Rutherford §), and therefore $1{\cdot}1 \times 10^{14}$ grms. of radium, or $10^{14}/10^{27} = 10^{-13}$ gm. per c.c., throughout the Earth's volume would maintain it at a steady temperature. Thorium contributes 5×10^{-9} cal. /sec. gm. The **total heating effect** in calories per gram of rock per hour is for the lava indicated below by *, 30×10^{-10}; and for the rock indicated by †, $2{\cdot}9 \times 10^{-10}$; for average igneous rock, 11×10^{-10}.

(See Strutt, *Proc. Roy. Soc.*, 1906–7; Joly, "Radioactivity and Geology," 1909.)

Fig. 7

Why did Kaye and Laby compile these tables? It appears that, although a lot of data was available at the time, it was distributed throughout numerous sources. It might also be suggested that this was the ideal time to publish a book of constants that contained the wealth of new data on radioactivity. I made an attempt to locate the sources that would have been available to Kaye and Laby and their colleagues in the early years of the 20th century. A library search did not yield much but I happened to find an old book of Mathematical and Physical Tables compiled by two lecturers at Manchester Municipal Technical College, James Wrapson and W. W. Haldane-Gee in 1898. This book simply contained the sort of data required by students taking technical courses and the only reason I decided it was worth buying from a second hand book shop was that it was signed by the original owner, W.H.Bragg ! However, at the back of the book, quite unexpectedly there was a complete list of references. This contained 29 sources of data, some of which were quite trivial but the list included the one source that, in their introduction, K & L acknowledged their indebtedness to, Physikalisch-Chemische Tabellen by Landolt and Börnstein 1883. Another important source on the list was the Smithsonian Physical Tables by Gray 1884 and its double bordered page layout is very similar to the one used by K & L. The Smithsonian Physical Tables is a very comprehensive collection of data but, even the 1910 edition did not contain a section equivalent to the Radioactivity & Gaseous Ionisation section of Kaye & Laby.

Kaye and Laby presented most of their material in uncomplicated metric units in contrast to the rag bag that was available one or two decades earlier and still in use in 1911 in many quarters. Only a few years earlier, the units used by physicists often sounded as if they had just slipped out of the farmyard. In 1888 Oliver Lodge quoted a cloud/earth electrical capacitance as 2/3 of a furlong (sic) while naval handbooks were still using *jars* as their unit of capacitance and indeed continued to do so for many years after that. A *jar* is 1000 cm (c.g.s. esu of capacitance) = 1.111 nF. The horse power itself (550 fp/s = 746 watts) indeed did just slip out of the farmyard and even today it somehow doesn't want to go away entirely. Kaye and Laby collected a comprehensive set of units in the well organised c.g.s. system and quoted electrical units in accordance with the 1908 International Conference on Electrical Units and Standards with practical definitions directly referable to their c.g.s. values. We are reminded on page 3 that the use of metric weights and measures was legalised in the United Kingdom in 1897. Now there's a centenary that almost went unnoticed!

The first edition of Kaye and Laby contained 153 pages and was followed by a second edition in 1916 and a third in 1918 with no essential change in the number of pages. Subsequent editions followed every few years and the two authors

remained actively engaged in new editions until the ninth edition in 1941 by which time it had expanded to 181 pages. Kaye died in 1941 and Laby died in 1946. The 10th edition came out in 1948 edited by a formal committee. Each subsequent edition became larger and by 1995 the 16th edition had 611 pages.

The accompanying diagram (Fig. 8) shows how the early years saw small increases in size prior to WWII but a rapid growth after about 1959. I have only been unable to unearth details of the ten editions shown by the points on the graph and those accompanied by boxed dates are simply the editions I own.

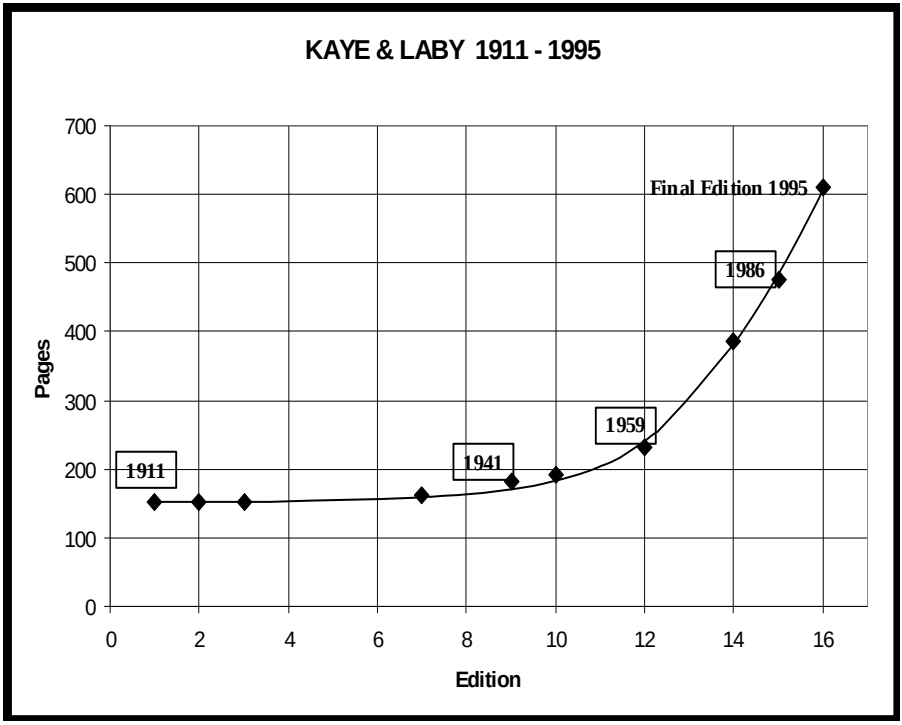

Fig. 8 Graph showing how Kaye & Laby grew rapidly after the 10th edition

By the time the 16th edition was 10 years old in 2005 it was digitised and went online with the support of the IOP and NPL and is now kept up to date by an editorial board and a long list of contributors. It can be freely accessed through their website http://www.kayelaby.npl.co.uk or simply by typing into your browser the two names Kaye and Laby.

Kaye and Laby has been in constant use by educators and researchers for 100 years and there is no reason to doubt that it will continue in this role for the next 100 years.

The author wrote his first article on this topic in 1997 for the Scientific Instrument Society (7). In 1998 a second article on Kaye & Laby was written by Dr Douglas Ambrose (8), a long standing NPL contributor and member of the Kaye & Laby advisory committee. This paper was based on a talk to the IOP History Group on 25th April 1998 and was the basis of a second talk at The Royal Society on 22nd January 2006. Dr Ambrose's article has become a permanent feature of the NPL web site.

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The Nature of Progress in Science -
The differing approaches of Polanyi and Kuhn to paradigm changes

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In Kuhn's book *The Structure of Scientific Revolutions* the author expressed uncertainty about how progress in science can be characterised across what he described as a paradigm-change because of the incommensurability involved. A similar, much less recognised, account of the resolution of conflicting paradigms by Polanyi agreed with many aspects of Kuhn's, but lacked the latter's uncertainties. A comparison of the two approaches concludes that Polanyi's greater consideration of experimental contributions to the resolution of paradigm-change is responsible, in contrast to Kuhn's requirement of intellectual persuasion,. The role of experimental developments within progress in science is discussed in these contexts. A distinction is also proposed between paradigm-changes which are well-termed *revolutions* and others better described as *major developments.*

1. Introduction

Thomas Kuhn (1962), in his seminal book *The Structure of Scientific Revolutions*, initiated a vigorous discussion of scientific revolutions within the field of the philosophy of science. However a slightly earlier discussion of the same type of topic by Michael Polanyi (1958), given in Chapter 5 of his wide-ranging book *Personal Knowledge*, appears not to have received systematic discussion by Kuhn or by the philosophical community, except by those few who are aware of Polanyi's ideas in other contexts (*Tradition and Discovery*, XXXIII No. 2,.2006-2007). Kuhn and Polanyi have similar views on many aspects of the subject but differ in others. This in itself is not surprising as Kuhn's intellectual background to philosophy was primarily as a historian of science, while Polanyi was a distinguished experimental physical chemist and Fellow of the Royal Society before he turned to philosophy. The object of this paper is to compare and contrast their views with particular reference to their approaches to paradigm-change and Kuhn's uncertainties about the nature of progress in science. Kuhn's valedictory book *The Road since Structure*, (2000), and a detailed sympathetic but also critical analysis of Kuhn's work entitled *Kuhn* by Sharrock and Read (2002) are the principal additional texts referred to in the paper.

It is convenient to summarise here the terms used in discussing scientific revolutions. Those used most generally in the field were initiated by Kuhn. They include**:** *paradigm*, a combination of theoretical concepts and related experimental techniques which for a period is dominant in leading to rapid progress within a scientific field**:** *scientific revolution*, a change from one paradigm to another**:**

normal science, the relatively rapid progress in research which is made within the scope of a well established paradigm**:** and *incommensurability*, the relationship between rival paradigms where, as is often the case, they differ in their basic premises. Paradigm-change is used by Kuhn as an alternative term for scientific revolution (see Section **5** below). Polanyi's analogous terms for scientific revolution and paradigm are *scientific upheaval* and *interpretive framework* respectively.

2. Two approaches to scientific revolutions

2.1 The Polanyi formulation

Polanyi's views are described first as they have historical priority. In general terms he early, in *Science, Faith and Society* (1946), put strong emphasis on the importance of traditions *within* science, as is also reflected in the title of *Tradition & Discovery* as the journal of the Polanyi Society. A successful tradition normally consists of a combination of theoretical and related experimental procedures which, as an *interpretive framework*, has enabled rapid progress to be made within a wide research field. However no tradition can be all-embracing, and after a period some experimental findings (by then well-authenticated, although initially considered as doubtful anomalies) cannot be accounted for under the existing theoretical understandings* (Polanyi, 1958, p.138). A radically new theory is then required and usually has to be based on different premises. Under such circumstances it is very difficult for the 'traditionalists' and the 'radicals' to agree, i.e. they tend to 'talk past' each other and a scientific controversy results (Polanyi, 1958 pp.150-160). The new theory necessarily finds strong initial opposition and, even if successful, can take a long time to become generally accepted. Polanyi, who described such an event as a *scientific upheaval*, saw the need for demonstrations to, and even 'conversion' of, the traditionalists to a new outlook before it could be widely adopted. He gave four examples in each of which empirical findings based on experimental procedures were shown to be very effective for this purpose.
His examples of strong disagreements (Polanyi, 1958, pp.152-158) which were resolved in this way included (a) Galileo's observations of the phases of Venus and the moons of Jupiter as supportive of Copernicus's heliocentric theory,

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* There are occasional examples where the necessity for paradigm change has principally a theoretical basis. The best and most important example is the development of Einstein's special theory of relativity which came about by his perceived necessity to find a theory which incorporated the best features of both Newtonian mechanics and Clerk Maxwell's theory of electromagnetism**:** the achievement of this transformed physics.

(b) astronomical observations versus a metaphysical suggestion by Hegel to account for the distances of the planets from the Sun, (c) whether chemical formulae should be meaningfully represented in 3-dimensions, as was later directly shown to be correct by X-ray crystallography**,** and (d) the isolation of 'dead' enzymes from yeast that settled a long-standing controversy about whether living cells were necessary for the process of fermentation.

Although Polanyi did not explicitly prescribe a general procedure for evaluating the merit of the new interpretive framework, by his chosen examples he effectively advocated the experimental method conventionally used by scientists to evaluate any new scientific theory. This, often known as the hypothetico-deductive procedure, in this context is that the radical new theory, which has been conceived to account for the original 'anomaly', must also rationally lead to other new types of expectation which should be explored experimentally (or sought-after in the form of previously unappreciated observations). Once a strong justification for the new theory has been established by these means, attention then turns to the equally important necessity of showing that the understanding of most of the phenomena successfully accounted for by the previous paradigm can be reformulated in the context of the new theoretical ideas. Finally the new theory and its related experimental procedures form the basis of a replacement interpretive framework that will now dominate research for a period in the future. At this point the more enterprising members of that community embrace with enthusiasm the promising new paradigm which, based on different premises, gives a different direction to the main scientific trajectory within the research field. Although there cannot be a *logical* relationship between the new and old interpretive frameworks the transition is nevertheless *rationally* justified on empirical experimental evidence

When the change represents a major development of an earlier one (see Section **5** below) some of the older types of problems, previously unaddressed, may still usefully be explored under the older framework, but fewer scientists continue to work in that less advanced area.

2.2 The Kuhn formulation

Kuhn's lengthier, seemingly independent account of the intellectual difficulties involved in such changes is in many ways closely similar to Polanyi's (Kuhn, 1962, Section XII). He confined his discussion to longer-term controversies and in such circumstances he proposed the term *paradigm* for what Polanyi had termed interpretive framework. In the following discussion Kuhn's now more widely-used nomenclature will be used. In comparison with Polanyi, Kuhn gained more

attention to the overall theme through his use of a more distinctive nomenclature contained in his separate book. His approach to the problem of paradigm-change was however mainly expressed in intellectual terms. Kuhn considered that it was as if the antagonists belonged to different language-communities and, because of the different premises involved, he also claimed that the radical change to the new theory could only be accepted by a traditionalist through 'persuasion'. He appeared to consider that this could be brought about within the scientific community by considering the general merits and promise of the new theory. Because of incommensurability - the term that he introduced to denote the competing paradigms with different premises - he considered that it required a 'conversion', perhaps in the form of a Gestalt experience, for the traditionalist to accept the new paradigm. This is the type of mental event that he, as a historian, had doubtless attempted to achieve in order to understand rejected paradigms of the past. It was pointed out to Kuhn by sociologists of science that his emphasis on intellectual 'persuasion' seemed to imply the possibilities of irrationality occurring within scientific progress through the activities of politically-powerful individual scientists (Latour, 1988**;** Collins and Pinch;1996)[†].

Kuhn strongly defended himself against the possibility of irrationality (1970,Section 5 of the Postscript, and 2000, p.157), pointing out that shared criteria such as 'scope, accuracy, simplicity, fruitfulness etc.' could be deployed during comparisons between the rival paradigms, but he only very briefly mentioned empirical evidence for the competing radical theory (2000, p.119).

[†]A number of sociologists subsequently have emphasised this possibility and claimed that science could lose rationality and objectivity by this means. Some conclusions from related investigations by the sociologists triggered the strong disagreements between them and the few scientists who became aware of their work (Gross and Levitt 1994,, and Weinberg, 2001) and these became referred to as the Science Wars (Segerstrale, *Beyond the Science Wars*, 2000 and generated a substantial literature. Few working scientists are aware of these issues and they do not subsequently seem to have led to uncertainty about the reliability of consensual scientific conclusions. Such conclusions can be classified as *science-in-outcome.* Before a scientific consensus has been reached there do, of course, remain substantial uncertainties while alternative ideas and experimental results are being assessed. This stage of research, suggested classified as *science-in-process*, has been given more attention as a result of discussions associated with the Science Wars. (It is interesting to speculate that if Polanyi's formulation, based on empirical evidence, had been better known than Kuhn's, rather than the reverse, perhaps the sociologists would not have acquired such motivation and the Science Wars might not have taken place!).

He gave the impression that, because of incommensurability, the two communities may never truly be reconciled. He pointed out (as earlier had Polanyi) that the replacement of one paradigm by another can lead to a great changes in research programmes, both in theoretical ideas and in experimental techniques - in conceptual terms to a 'different world' within the relevant field of science. Kuhn (1962), and later in more detail (2000, p. 243 and following pages) expressed strong uncertainty about how to characterize the scientific progress that is made across a paradigm-change, rather than within a paradigm.. He agreed that progress undoubtedly also occurs across a paradigm-change in the form of increased problem-solving power, but he considered that the change in direction required might 'undermine the authority of science' (1962, Postscript, and 2000 p.157) and that the idea of a convergence towards scientific 'truth' has to be abandoned. This view led Kuhn to an open-ended Darwinian model of science which does not involve a specifically predefined goal, only a general quest for progress (1962, Section XIII, particularly pp.171,173). Sharrock and Read are particularly critical of Kuhn's views in this respect (2002, Chapter 5).

3. A comparison of the Polanyi and Kuhn formulations

Polanyi's formulation was given towards the end of his life and only once, during a conference, was he recorded as indirectly commenting on Kuhn's work (*Tradition and Discovery*, p.10). Kuhn never seems to have given a specific assessment of Polanyi's contribution to the subject but, in an interview towards the end of his life, he did admit that, as he was in the process of completing *SSR* for publication, he did not read the recently published *PK* until after his own book was completed, (Kuhn, 2000, pp.296-7). Nevertheless it can be seen that the two gave closely similar accounts of the difficulties that arise in comparisons between rival paradigms based on different premises. Under these circumstances one paradigm cannot be compared against the other on a *logical* basis and even the interpretation of the same experimental observations are likely to differ by those with mental allegiance to the different paradigms. Both Polanyi and Kuhn shared the view that the necessity for a change in paradigm is usually signalled by persistent experimental findings which cannot be accounted for under the already existing paradigm. They both were clear that a paradigm-change would often involve a major reorientation in research objectives - moving into what Kuhn described as (conceptually) a 'new world'.

The principal difference between them was the emphases given within their views about how paradigm-change could convincingly be established within the research community. Kuhn's approach, as a philosopher, was based on finding a general intellectual consensus for a change involving persuasion. Polanyi, familiar with

experimental work, by his examples clearly preferred the use of the standard hypothetico-deductive procedures, routinely used by scientists, involving interplay between theory and experiment to assess the validity of an hypothesis. The latter method can provide more *specifically focussed* evidence for the adoption (or otherwise) of the new theoretical concept. The possibility of incorporating a long-term personal bias in future scientific thinking, a possibility proposed by the sociologists and which concerned Popper and Lakatos (Lakatos and Musgrave, eds,.1970), can essentially be eliminated by this through empirical investigations.

4. The roles of paradigms and paradigm-changes within scientific progress

Kuhn considered that clear scientific progress occurs through work within a paradigm. But he was doubtful how progress towards a consistent final goal or 'truth', can be maintained through a paradigm change which necessarily involves a change in premises and hence research objectives. However Kuhn's expectation that consistency in description ('truth' in his words) about the natural world has to be maintained during progress does not take into account the experimental capacity in modern science for making quite new discoveries. Progress in science is much more than making more explicit what is already known in outline. It involves incorporating new ideas and concepts during continued progress.

Polanyi's general point of view (1946,1958, and as shared by the majority of scientists) is that there is a complex natural world which layer-by-layer, piece-by-piece, we gradually learn to recognise and understand**.** The general objective of our study always remains the same (to account for Nature in her entirety) but the inherent richness and detailed content of that world becomes more and more in evidence with the passage of time i.e. the nature of the ultimate goal is only gradually recognised during the process itself.

This scientists' open-ended model might give the impression that science does not have a firm foundation. A more realistic metaphor for scientific progress than a building with insecure foundations would be a tree which, as it grows, the branches extend greater and greater above ground, and at the same time the roots are strengthened and extended below ground.

The necessity for an open-ended approach led Kuhn to adopt a natural-selection or Darwinian model for scientific progress. However that model can be specifically criticised as implying that separate branches of science should historically evolve independently of each other, i.e. without 'cross-fertilisation.. In fact the originally-separate sciences of physics, chemistry and biology become increasingly strongly interconnected with time. For example the quantum theory has in the 20th century fundamentally connected together chemistry and physics by accounting for the empirical reactivities between molecules, discovered empirically by laboratory

chemists, in terms of those calculated-from-first-principles for individual molecules by quantum theory. Also the molecules of chemistry play similar roles throughout the different sciences. Kuhn's model is only appropriate in the sense of its adoption of 'the survival of the fittest' principle as applied to competing hypotheses.

The details of a sequence of discoveries (see Section 7), are unimportant to a philosopher**:** but the principle that experimental work has as important a role as theory in stimulating the actual progress of science needs to be understood and emphasised. The chicken-and-egg type of relationship between theory and experiment, i. e. that the great majority of problems that have become reasonably well understood in theoretical terms have required the evidence from experimentation for verification is of ultimate importance in science (a notable exception is Einstein's development of the theory of relativity). In general, to again adopt Polanyi's viewpoint, imagination - moderated by judgement - is of the greatest importance in both the theoretical and experimental aspects of scientific progress. Logic, so valued by the philosophers, is important but is not continuously applicable throughout progress in science as new discoveries are made.

5. Two types of paradigm-change

Kuhn uses the term paradigm-change as synonymous with revolution**;** this usage is now common. However the term revolution seems more appropriately applied to paradigm-changes where in practice the old one is *rejected* by scientists once the significance of the new one has become generally appreciated **-** the new paradigm *replaces* the old one. Examples are the rejection of Aristotle's point of view of physical science once the implications of Copernicus's heliocentric theory was developed by Galileo and Newton; the rejection of Priestley's phlogiston theory of combustion in favour of Lavoisier's account; the rejection of the caloric theory of heat in favour of kinetic-molecular theory; or Darwin's theory of evolution of the species in favour of the Biblical account.

In other cases the new paradigm can better be considered as a *major development* of the previous one (an even more advanced form of the original). In *SSR* Kuhn advanced much argument to claim that the profound scientific transition from Newtonian physics to Einstein's theory of relativity, which certainly qualifies as a major paradigm-change, is in fact also a revolution because he claimed that relativity *rejected* Newtonian physics (Kuhn, 1962, pp.98-102). Kuhn himself acknowledged that this was a minority view but he also argued that Newtonian physics was falsified by Einstein because its presumption of universal applicability was no longer seen as tenable. However, most theories are later qualified in their

applicability, and virtually all physicists consider the Newton to Einstein transition to be a profound major development - an extension and generalization - of classical physics. Albert Einstein (1927) himself considered that he was building on Newton's contribution.

Quantum theory, which contracts to classical Newtonian behaviour at the limit of closely-spaced energy levels, can also be best described as a major development of Newtonian physics.

6. The roles of the subject matter, and of the scientific community, in generating scientific progress

Kuhn's concerns about 'new worlds' in science also led him to ask once again why science is uniquely capable of rapid and cumulative advances in knowledge (1962. p.160). It seems now to be agreed by the philosophers that no general scientific *method* can be found, as had earlier been hoped. What is important, as we have seen in Section 3, is the common hypothetico-deductive *procedure,* followed by all the sciences, with its alternation of theory and experiment. This interchange of ideas *and* action is the vital advantage of the natural sciences because they are concerned with the material world were interrogative experiments can be carried out.*. It is a great advantage that many natural processes change either slowly or are repetitive, hence providing multiple opportunities for experimental investigations. These have the unique capability of *generating* focussed evidence in support of, or against, a hypothesis. In most other evidence-based disciplines, such as history or archaeology, the evidence that can be discovered, e.g. documents or shards, is necessarily limited in extent. In a more literal sense, such evidence can only be found rather than generated.

Another great advantage of science is the strongly coherent nature of its community, both between the individual disciplines and historically with respect to past findings. Polanyi and later Kuhn express similar views on the important role of these factors. (Polanyi, 1958, pp. 217-219**:** Kuhn, 1962, pp. 164-170). Within the overall community usually several research groups, often international in coverage, naturally work with different approaches on interesting problems.

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* Even in astronomy, where, except for the use of space probes within the Solar System, one is limited to 'observation' of the heavens, a similar situation applies through an ability to do so with different frequency region of the electromagnetic spectrum - radiofrequency, infrared, visible, ultraviolet, X-rays etc - with each such technique giving different types of information.

They interact at conferences and by e-mail and are said to collectively form an 'invisible college'. These groups have a healthy rivalry in that they have related aims but each wants to be the first to make an important breakthrough. However, ultimately, their shared interests make it easier to reach an informal consensus than in many other fields of study, often with the important help of mutually appreciated experimental evidence**.** They freely criticise their rivals' work in progress but will eventually agree about common findings. Even during major upheavals, when strong differences of view can occur for a decade or more, in the end a consensus emerges that is accepted by the great majority of scientists within that particular field. Kuhn and Polanyi both note that major changes in the conceptual structures of science, such as are required in paradigm-changes, are usually achieved by younger members of the community who have been less exposed to, and therefore have less allegiance to, the existing paradigm.

7. A note on major developments made within a paradigm, i.e. within 'normal science'

Kuhn had characterised work within a paradigm as 'normal' science, mostly involving 'puzzle solving'. This suggests rather routine work in comparison with paradigm change where the change of premise necessarily denotes new science. However it is always possible to do especially imaginative work using well-established methods. For example the famous story of the structure of DNA which was completed by Crick and Watson depended on the imaginative use, by Rosalind Franklin and others, of the standard experimental technique of X-ray crystallography. It was the imaginative choice of research topic that led to a major advance, in this case to a revitalisation of genetics. Furthermore during the extended lifetime of a paradigm there can be very substantial connected developments involving additions, not changes, to the original premises, e.g. by the discovery of new entities or phenomena, Consider the historical development of knowledge about atomic structure. J.J.Thomson discovered the negatively charged electron as a constituent of atoms and thereby showed, contrary to the classical views of Democritus, that atoms can be subdivided; the word atom previously implied indestructibility. Ernest Rutherford, next showed that the positive charges within the atom are concentrated in the heavy nucleus and then this is made up of combinations of protons and neutrons. These in turn led to the whole subject of nuclear physics. Work within a paradigm as well as paradigm-change can also contribute greatly to progress in science.

Alongside these two methods, account has to be taken of the importance of serendipity in science. Two examples of this are the chance discoveries of radioactivity (which Rutherford used to develop the whole science of nuclear

physics), and of X-rays (which led ultimately to quantitative 3-dimensional molecular structure determination in chemistry and biology). Such occurrences, which arise by chance in the process of concentrating on other problems provide good evidence for the 'given' nature of the world that scientists investigate.

8. Conclusions

The principal difference between the discussions of 'scientific revolutions' in Kuhn's well-known book, *The Structure of Scientific Revolutions* and Polanyi's less well known account in *Personal Knowledge* is that the latter gives more emphasis on the use of experimental evidence to resolve choices between rival paradigms. Kuhn's alternative use of intellectual persuasion for this purpose led to the concern of the sociologists about the possibility of irrationality entering into science and hence to the so-called' Science Wars'. Polanyi's approach uses imagination to form promising hypotheses to account for the original experimental anomalies, and then experimental methods to provide a rational and empirically-based preference for accepting the rival paradigm..

Two roles of experimentation help to ensure progress in science, (1) its use, together with the hypothetico-deductive procedure, to assess the status of a theory and (2) the role, often empowered by advances in experimental techniques, of discovering of quite new phenomena. Additionally serendipity, the discovery of unexpected phenomena when searching for something else, illustrates the 'reality' of the world explored by scientists.

It is suggested that Kuhn's strongly expressed concerns at the major transformations of research programmes which often follow paradigm-changes (his 'new worlds') were because they were seen by him to involve violations of an assumed continuity in the progress of science. Instead they can be understood as the results of the radical changes in research findings which in practice occur usually at enhanced levels of experimental techniques. Paradigm-changes themselves are here interpreted as the *occasions* of particularly marked progress in research understandings.

It is also proposed that some paradigm-changes are better denoted as *major developments* rather than as *revolutions*. The changes from Newtonian physics to relativity or to quantum theory, are given as examples of *major developments* :the Aristotle to Newton, Priestley to Lavoisier and the Bible to Darwin transformations, where the new understanding clearly *rejects* their predecessors, are given as examples better described as *revolutions*. The latter were of such importance as to define for the first time the major disciplines of physics, chemistry and biology.

Acknowledgement

I wish to thank Dr. Rupert Read for a number of very stimulating discussions on the themes of this paper.

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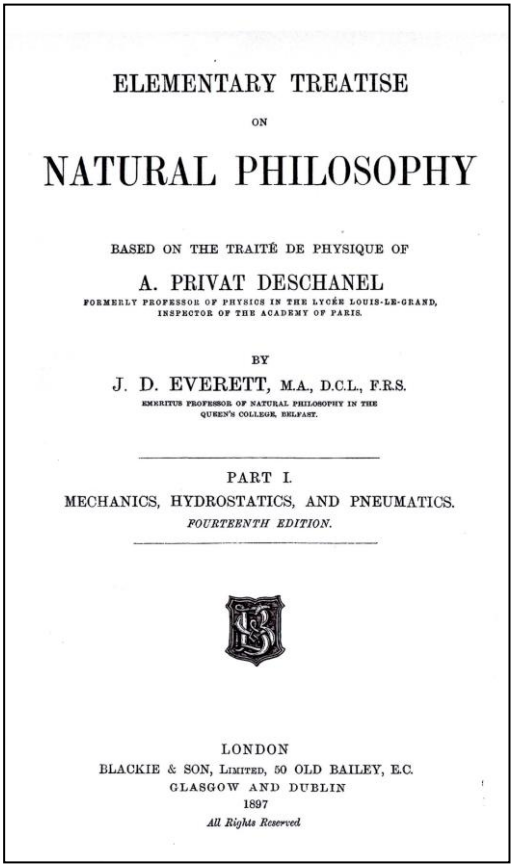
What's in a Symbol?

Stuart Leadstone

I recall a time in the late 1960's when, in my teaching career, I had reached what I considered to be the peak of my teaching of 'O' Level Physics. After several weeks of carefully "guided" practical work using dynamics trolleys, ticker-timers and paper tape, I and the class finally reached the summit of the peak which we were currently scaling, namely the expression of Newton's 2nd Law of Motion in the form $F = ma$. A look of what I thought was enlightenment came over the face of a somewhat laid-back but intelligent pupil. Alas, it was not enlightenment but a form of *déjà vu*. "Oh", he said, "that's $P = mf$!" He had just made the connection between the voyage of discovery carefully navigated by me in the Physics laboratory with what he had been told in his Applied Maths class.

This set me thinking. I myself was brought up on the notation $P = mf$ in both Physics and Applied Maths. I had vaguely pondered the peculiar choice of f for acceleration, but had never been sufficiently aroused to adopt the penetration of the mystery as a cause. By the time I had become a Physics teacher, all the newer physics text-books used $F = ma$, and the question as to why f was favoured over a by earlier authors(1) went firmly onto the back burner – until, that is, one day quite recently when I was browsing in a second-hand bookshop in London. To the average bookseller, "science" means Natural History, and an enquiry about "Natural Philosophy" once resulted in my being shown a shelf of books on "Moral Philosophy". Frequently I find myself looking in vain for anything pertaining to physical or mathematical science. On the occasion referred to, however, as I was about to leave the premises after another fruitless search, the bookseller said, "Oh, there's something that might interest you – three volumes on Natural Philosophy by a French author." (See illustration.) I excitedly lifted the three slender volumes down from the shelf, flicked through the pages and, to my delight, found that it was not only an English translation, but liberally sprinkled with the most exquisitely executed diagrams and drawings. These alone made the books irresistible and I bought all three volumes(2).



ELEMENTARY TREATISE

ON

NATURAL PHILOSOPHY

BASED ON THE TRAITÉ DE PHYSIQUE OF

A. PRIVAT DESCHANEL

FORMERLY PROFESSOR OF PHYSICS IN THE LYCÉE LOUIS-LE-GRAND, INSPECTOR OF THE ACADEMY OF PARIS.

BY

J. D. EVERETT, M.A., D.C.L., F.R.S.

EMERITUS PROFESSOR OF NATURAL PHILOSOPHY IN THE QUEEN'S COLLEGE, BELFAST.

PART I.

MECHANICS, HYDROSTATICS, AND PNEUMATICS.

FOURTEENTH EDITION.

LONDON

BLACKIE & SON, LIMITED, 50 OLD BAILEY, E.C.

GLASGOW AND DUBLIN

1897

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Some time later I was back home preparing a talk on "Basic Kinematics" and decided to take a look at Deschanel's approach to *dynamics*, which, in the older terminology, was divided into *kinematics* and *kinetics*. In his chapter on "First Principles of Kinetics" he writes:

"It is convenient to distinguish between the *intensity* of a force and the *magnitude* or *amount* of a force. The intensity of a force is measured by the change of velocity which the force produces during the unit of time; and can be computed from knowing the motion of the body acted on, without knowing anything as to its mass."

And later, in his chapter on "Laws of Falling Bodies", he further remarks:

The amount of the force of gravity upon a mass of m grammes is mg dynes. The intensity of this force is g dynes per gramme. The intensity of a force, in dynes per gramme of the body acted on, is always equal to the change of velocity which the force produces per second, this change being expressed in centimetres per second. In other words the intensity of a force is equal to the acceleration which it produces."

So there you have it: the relationship of the **intensity** f of a force of **amount** F is $f = F/m$, instantly recognisable as a to the devotees of $F=ma$! The principle of notation being followed here seems to me to be essentially that of denoting "specific" quantities by lower case symbols and "total" quantities by upper case symbols[3].

A familiar example of this practice is *c* for "specific heat capacity of a substance" and *C* for "heat capacity of a body", the former being independent of the mass of the body and the latter not. This leaves only one question to be answered: why do the *f* - practitioners write $P=mf$ and not $F=mf$?

Perhaps readers of this newsletter would care to comment.*

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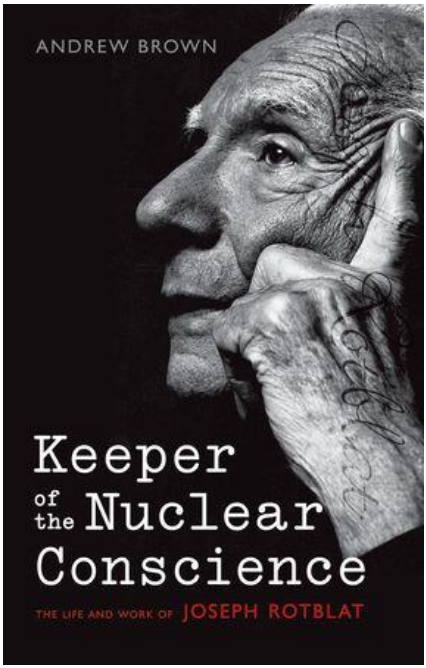
*Please submit comments to the editor.

Book Reviews



Keeper of the Nuclear Conscience:

The Life and Work of Joseph Rotblat

Andrew Brown

Oxford University Press 2012
ISBN 978-0-19-958658-5
347pp Hardback £20

Reviewed by Emeritus Professor Derry W Jones
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Like many science Nobel prizewinners, Joseph Rotblat, who shared the Nobel Peace Prize in 1995, had to overcome considerable handicaps in his early life. An additional tragedy was that, by a combination of circumstances, he had to leave his young Jewish wife in Warsaw in 1939 when he came to England to learn about Chadwick's cyclotron. With the couple's separation enforced by World War II, Rotblat could only presume, years later, her murder in the camps during the early 1940s. In his laboratory work in Warsaw, Liverpool and Los Alamos on neutron scattering, Rotblat contributed significantly to the atomic bomb project, regarding it as a precaution against the acquisition of the bomb by the Axis Powers. Knowledge that the bomb was no longer needed as a deterrent to the Germans led to his withdrawal from the Manhattan Project at the end of 1944 and ultimately to his devotion of the biggest part of his life to trying to free the world from nuclear weapons. He helped set up and ceaselessly supported the activities of the

influential Pugwash group of distinguished scientists. In the earlier years of the Cold War, with both superpowers possessing large arsenals of nuclear weapons, such attitudes provoked strong opposition from the British authorities, although Rotblat became a naturalized British citizen in 1946. He became sufficiently respected to be awarded the CBE in 1965 (during the Wilson Government, but recommended by MacMillan) in recognition of his contribution to East-West conciliation, and was knighted KCMG in 1998.

Andrew Brown, Rotblat's biographer, is a London-trained physician who practiced as a radiation oncologist in New Hampshire for 20 years (and may return to clinical practice). As a science historian, he has written the biography [1] of James Chadwick, discoverer of the neutron and Rotblat's boss and friend at Liverpool and Los Alamos, and has told the immense story [2] of J Desmond Bernal, a distinguished 20th century scientist who also concerned himself with political and international affairs. Indeed, the middle third of *Keeper of the Nuclear Conscience* is a painstaking history of the disarmament negotiations of the Cold War, throughout which Rotblat and Pugwash (see later) made unobtrusive 'back door' contributions. Despite the mysterious loss (doubted by some) of a trunk of papers as he returned on a train to New York in 1944, there is an enormous archive of later papers at Churchill College, Cambridge, to which Brown has had full access. For this, he spent a term at Cambridge, and he also recorded interviews with 16 of Rotblat's intimates and colleagues. Brown includes 14 photographs and there is a good 18-page index. Altogether, 58 pages are devoted to Notes, separated for each of 15 more-or-less chronological chapters, plus a bibliography of sources. The latter include the 2006 collection of articles about Rotblat edited by Attwood and Rowlands but Brown does not seem to make any explicit comment on *War and Peace* [3].

Rotblat (JR) was born on 4 November, 1908, the fifth of seven children to an orthodox prosperous paper merchant Zelman (importing newsprint from Finland), in Warsaw, a city about one-third Jewish and administratively (if not culturally) in a Russian province. Anti-semitism had been driving less affluent Jews westward for some time. With the outbreak of World War I in 1914, Zelman's business collapsed (his transport horses were commandeered) and the family soon became destitute and bullied in the food queues during the German occupation. As the wars continued after 1918, JR's education was limited to that from a rabbi (Zelman's ambition, not shared by JR, was for JR to become a rabbi). An armistice with the Soviets did not come until Oct 1920, when JR was nearly 12. He studied elementary electrical engineering at a Jewish technical school and graduated with an electrician's diploma in 1923. After several years working as an electrician by day and reading physics at night, he managed in 1929 to pass for entry to the Free

University of Poland; this had evening lectures so that students could work during the day. Ludvik Wertenstein, the Dean, Director of Warsaw's Radiological Lab, and JR's mentor, had spent two years with Rutherford but also had wider cultural interests than physics. Graduating in 1932, JR was appointed to an assistantship, and joined the unpaid radiological research staff, studied for his doctorate and later became Assistant Director. After the explanatory report of uranium nuclear fission by Frisch and Meitner, JR guessed that free neutrons would be released. Almost simultaneously with Joliot and colleagues, JR detected these; the findings from Paris and Warsaw each appeared in *Nature* (1939).

JR had met the young Polish Language student Tola Gryn in 1930 and they married in 1935. When Wertenstein arranged a small stipend for JR to spend a year from April, 1939, in Chadwick's Liverpool laboratory to familiarize himself with the cyclotron nearing completion, it was agreed that she should stay in Warsaw. Although Chadwick soon offered an Oliver Lodge Fellowship so that JR could ask Tola to initiate passport/visa applications for the UK, she was recuperating from an appendix operation, then fairly serious. JR had gone back to Warsaw in August to discuss with Wertenstein his paper on the feasibility of a fission bomb and the morality of involvement with its creation. Returning *via* Berlin, he reached Liverpool on 31st August, the day before Germany invaded Poland and Warsaw was bombed. During spring 1940, attempts were made to extract Tola from Poland to England via neutral countries but these efforts were thwarted successively as Germany invaded Denmark and Belgium and Italy invaded France.

Peter Rowlands has described JR's periods at Liverpool, 1939-1943 and 1945-1949 [4] and, in particular, the Liverpool cyclotrons [5] (JR had intended to construct one in Warsaw). Even before the 37-inch cyclotron was operational, JR impressed Chadwick by completing within a month research and a paper for *Nature* on the half-life of radium C' (Po-214). On arrival, JR had been struck by the poor state (no AC supply) of the teaching labs but his lectures soon included reference to nuclear fission and chain reactions, despite the secrecy of his research. There was no careless talk within the Department and JR was unaware of the existence of the Maud Committee of nuclear physicists of which Chadwick was a member. Soon the 'enemy alien' Frisch was directed from Birmingham to join JR at Liverpool. Despite nightly bombing, JR and Frisch measured inelastic neutron scattering and attempted the separation of U-235. In autumn, 1941, copies of the Maud Report on uranium for bomb and power (some of it vetted by JR) went to the USA and the cyclotron work at Liverpool came under the DSIR as Tube Alloys. By mid-1943, Anglo-American collaboration in the Manhattan Project had been agreed. Chadwick and Frisch left for the USA in November, followed by JR, unusually allowed to retain his Polish citizenship, in February, 1944.

Some measure of the esteem in which JR was held is that the General in charge of the whole Manhattan army operation, Leslie Groves, met him on arrival in Washington. In the US, JR was staggered first by the plenty of food and goods in the shops and then, on arrival in Los Alamos, by the limitless technical resources available to a dazzling array of well-paid physicists in beautiful surroundings at 7000 ft remote from the war. Aside from the U-235 bomb, for which the crucial need was isotope separation, research centred on the design of a plutonium bomb and (at Teller's insistence) a fusion or H-bomb. Brown records that it was JR's recollection (not accepted by all historians) that, over dinner with the Chadwicks, he heard from Groves that the ultimate value of the atom bomb was to subdue the Russians. JR, invited by Oppenheimer to meetings of the Co-ordinating Council, soon transferred from investigating gamma-radiation effects on enriched uranium to fast-neutron irradiation of fission products. Bohr and JR discussed their fears about a nuclear post-war world and Bohr recommended that British and American statesmen should tell the Russians about the bomb before using it (presumably on Japan). JR received some accidental radiation exposure, probably in an experiment that revealed xenon poisoning of piles. He was also dispirited by rumours of exterminations in Europe (with no word of Tola or his family), news of the uprising and massacre in Warsaw, and fears of a forthcoming nuclear arms race. Oddly, there had been flying lessons on Sundays in Santa Fe; according to an informant, JR had a notion of joining the RAF and parachuting to inform the Russians (denied by JR). With Chadwick's confirmation of intelligence that the Germans had abandoned development of an atomic bomb, JR asked to be released and to return to the UK. If his wooden trunk of photographs, books and papers *was* extracted from the train in December, 1944, ie before Hiroshima, this would presumably be on the instructions of de Silva, the Los Alamos security chief, as there had been some suspicion that JR was a spy.

Arriving back in Liverpool, the 'Polish cyclone' set about revitalizing physics research and teaching. In 1946, Chadwick, still in the US, proposed that JR, now a Lecturer (he became acting joint head in 1948), should design a synchrocyclotron. By 1947, JR was negotiating to use part of the site cleared for the original ambitious Lutyens cathedral for the proposed 1600 ton 156 inch instrument, although it was several years before it was commissioned in the new Nuclear Physics Research Laboratory. As a continuing member of the British Tube Alloys team, JR had, already in February, 1945, advised its head, Wallace Akers, on the need to set up a civilian Atomic Energy Research Establishment. Although JR had confirmation that Tola had been taken to a death camp in1942, his other relatives remarkably survived. With Chadwick's help and as now a British citizen, JR was able to engineer visas for Britain (they had earlier hoped for Palestine), though he then had to support several people for some years on a small salary. To advance

public education in civil and warlike applications of atomic energy, JR had the idea of an atomic train that began in November, 1947, and visited many towns. While still directing nuclear physics, JR was by 1948 moving towards medical physics and published a classic paper with a radiologist, Dr George Ansell. He also resolved to devote his life to campaigning against nuclear weapons.

Although Liverpool wanted him to stay, JR took up the appointment in January, 1950, as Professor of Medical Physics at the Medical College of St Bart's Hospital and chief physicist to the hospital. Reluctant approval of his appointment by the College dean was the precursor to many tense college/university interdisciplinary arguments later in his tenure. Within two years, he was conducting research in several fields, including neutrons and electrons in tissue, metabolism of living organisms and diagnosis of breast cancer. In the autumn of 1956, JR was cheered by the application to research for a PhD in radiobiology of a young attractive vivacious physician, Patricia Lindop, who was to play a significant role in his life and in that of Pugwash, which began in 1957. In contrast to JR's misfortunes, Patricia came from a comfortably-off family, was a scholar at a good school, had been bright enough to enter male-dominated Bart's in 1948 and take a first in an intercalated BSc, and had a suitable (Malvern and Cambridge) steady boy friend. Already investigating ageing in human patients and test animals, she began with JR to study age in the development of radiation injury. They were also a couple in social life but did not marry, perhaps because Tola's fate was too harrowing or JR's devotion to disarmament too great. Instead she married the boy friend, GPR 'Mick' Esdale, and had three children, but she continued research and teaching at Bart's, attended conferences with JR, and very strongly supported him throughout her active life. Mick Esdale organized the 1962 Pugwash meetings. Brown concludes that JR was heartbroken when Patricia, now a Reader, suffered a severe brain haemorrhage in 1981 and could no longer speak; however, JR soon took her, in a wheel chair, to conferences again.

The middle third of *Keeper of the nuclear conscience* closely reflects the title. It recalls the attempts, especially from the 1950s to the 1980s to minimize the possibility of nuclear war between East and West, from the (even earlier) Baruch Plan to treaties on intercontinental and intermediate missiles. Few may recall even their names or initials (PTBT, SALT, INF, START, etc) but the negotiations involved scientists, and especially physicists, in the preliminary confidence-building stages to an extent that now seems (perhaps regrettably) improbable. Thus, for example, in 1959, US President Eisenhower asked his advisor, the Harvard and Manhattan chemist George Kistiakowsky, to work on a nuclear test ban treaty 'consistent with our national security'; in the early 1960s, the 'conceptual exchanges' with Pugwash were valued by Khruschev; in 1981, Max Perutz was in a Pontifical delegation to Prime minister Thatcher about a drift towards a first

nuclear strike policy; and in 1988, Gorbachev referred to the 'formidable force' of Pugwash in a welcoming message for the 38th meeting, held in the USSR. JR played a persistent pacifying role in the preliminary approaches to almost all these Cold War arms limitation agreements, generally in the context of Pugwash.

In 1955, with the world under threat of nuclear war, JR and nine Nobelists were signatories to the (Bertrand) Russell-Einstein (although Einstein had just died) Manifesto for the renunciation of nuclear weapons; but in1957 the USA and the USSR released much radioactivity into the atmosphere from many nuclear tests. With physicist Cecil Powell, JR canvassed eminent scientists and secured finance for an international off-the-record conference in 1957, on the risks from weapons of mass destruction, from an affluent supporter, Cyrus Eaton; he insisted that it was held in his birthplace, the hamlet of Pugwash on the northern edge of Nova Scotia. JR and Powell were the only British delegates and there were three from Japan. Americans included Szilard, Weisskopf, Doty (chairman of the Federation of American Scientists) and Rabinowitch (editor of the Bulletin of Atomic Scientists). All were invited as individuals, beholden to no-one, but presumably the Russian scientists had clearance from the Central Committee (Topchiev's briefings were covered in *Pravda*). On biological hazards, JR discounted both governmental minimization of the perceived risks and exaggeration by some critics but emphasized that the real menace came not from tests but from use of nuclear weapons in war. Although Pugwash was envisaged as a discreet one-off gathering, such was the thoughtful discussion "without exchange of clichés", that the meeting decided that there should be future Pugwash (the somewhat deprecatory name was retained) Conferences on Science and World Affairs (COSWA). With no budget, JR became secretary-general, helped by Patricia and the physics secretary, until 1973, by which time Patricia was assistant secretary-general. From these beginnings arose many international conferences and national Pugwash groups, with JR continuing to be involved in all (he made nearly 200 visits), ultimately agreeing, in1988 on his 80th birthday, to be 'temporary' President. He was succeeded only in1996 by the retiring President of the Royal Society. In early 1958, Canon Collins and JR's friend Bertrand Russell set up the CND but JR soon left the executive committee to concentrate on Bart's (recently rather neglected) and Pugwash. He liked to quote Russell's aphorism that opinions now accepted were once thought eccentric.

What did JR and Pugwash's informal network of scientists achieve? Its back-channel communications (and by the 1990s JR had more effective contact with the government in Moscow than in London) contributed to the prevention of the Cold War erupting into a hot one. The possession of large nuclear arsenals for mutually assured destruction (MAD) was not a *necessary* condition for deterrence. Brown gives the salient features of many Pugwash symposia and annual conferences,

including one at the Royal Society in 1988. The Nobel Peace prize was shared between Pugwash and JR, for his attempts to stop the manufacture and spread of nuclear weapons, in 1995, the same year that he became FRS. By then, Pugwash had no longer retained its presence in international affairs. Brown quotes JR as believing that peace can best be promoted in three ways: 1 Campaign for the elimination of weapons of mass destruction and biological weapons – since not all leaders are rational, nuclear weapons and human fallibility may lead to nuclear exchange; 2 Recognise that technical understanding by scientists brings a responsibility to warn politicians of the dangers; 3 Accept that verification and responsible whistle-blowing can help inject honesty into public affairs. JR edited *Physics in Medicine and Biology* from its inception in 1960 to 1972, was involved with two dozen books, and received many honours and decorations.

In his nineties, JR's tireless energy (admired, together with his integrity, by Gorbachev) was such that he co-authored *War no more: eliminating conflict in the nuclear age*; this noted that availability of weapons and potential users were the only *necessary* conditions for war. Until his retirement from the chair in 1976, JR continued to make serious contributions to medical physics, especially in the effect of radiation on human tissue. In his idealism, JR may have been naïve, 'reaching for the impossible' (the title of Brown's penultimate chapter) but one can hardly be ashamed of encouraging the ethical application of science. Robert Neild, a Pugwash veteran and first Director of the Swedish SIPRI Institute, who admits to being 'fairly bossy', said JR had the qualities of a great Russian general: charm, stamina, ruthlessness in a good cause, and a tendency to megalomania. Within a few months of suffering a stroke (while working late at the Pugwash office) at the end of 2003, JR visited Denver, to address thousands of teenagers, and then attended the 2004 Pugwash in South Korea. During his final illness in 2005, he was visited by Patricia, unable to speak, in a wheelchair.

Brown has written a fine biography of a man who had been a distinguished nuclear physicist and became an effective radiological physicist as well as an obsessive seeker after peace.

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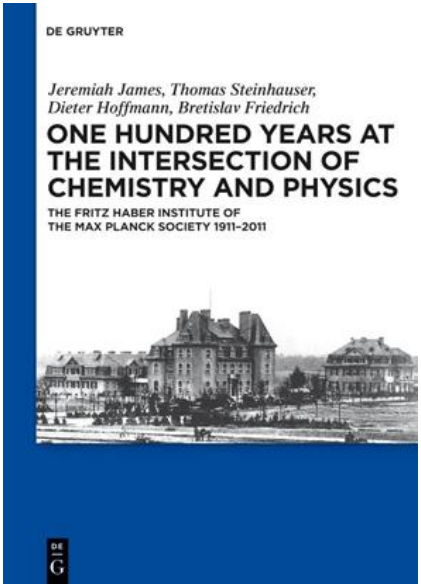

Bretislav Friedrich, Jeremiah James and **Thomas Steinhauser,**
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Dieter Hoffmann,
Max Planck Institute for the History of Science, Berlin.

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ISBN *978-3110239539*
310pp *Hardback £53*

Reviewed by Professor Norman Sheppard
School of Chemistry, University of East Anglia,

The Max Planck Gesellschaft (Max Planck Society) (MPG) of Germany is a self-governing organisation, very well financially supported by the German government, regional administrations, donations and earned licence fees. It has today numerous Institutes that conduct research in many important fields within the physical and biological sciences. These were earlier (until the end of World War II) named the Kaiser Wilhelm Institutes. The Fritz Haber Institute, (FHI) which is the subject of this book, is one of the most prestigious of those concerned with the physical sciences. Fritz Haber was a famous physical chemist who worked on heterogeneous catalysis and discovered the Haber Process for synthesising ammonia from hydrogen and nitrogen (the latter available from the air). He thereby made available an inexhaustible source for manufacturing nitrogenous fertilisers and greatly increasing the yield of agriculture and the capacity of planet Earth to feed human beings. For this he was awarded the Nobel Prize for Chemistry in 1918.

The Haber Process also led to the manufacture of nitrates, which can be used as fertilisers or as explosives: additionally Haber's expertise used in the development of poison gas during World War1 (WWI) has meant that he has always been a controversial figure within and without scientific circles. His position in this

respect resembles that of the team of scientists in the Los Alamos Laboratories in the USA which developed the atomic bomb during WWII.

The Fritz Haber Institute of today was founded as the Kaiser Wilhelm Institute (KWI) for Physical Chemistry and Electrochemistry in 1911 (together with one other KWI institute) with Haber as its first director. Up until the mid1930s, its 'Golden Years', many famous scientists served on its staff, four of whom became Nobel laureates - Haber himself, von Laue, Franck and Wieland. von Laue in particular discovered the diffraction of X-rays by crystals which was further developed by the W.H and W.L. Bragg so that for the first time the structures and dimensions of molecules could be determined. Other famous scientists who worked in the Institute included Polanyi, Freundlich, Wigner, and Bonhoeffer. The research themes within the Institute covered many of the most active topics in physical chemistry including colloid chemistry, atomic structure, reaction kinetics, spectroscopy and quantum physics.

However when Adolph Hitler came to power the staff of the Institute was required to be purged of Jewish or Jewish-related personnel, including Haber himself and two of his Departmental Heads, Polanyi and Freundlich. Altogether nearly 30 of the Institute staff of all categories were expelled in 1933 and had to find careers elsewhere in Europe and (mostly) in the USA .Other German laboratories were treated in the same way. Overall Hitler's racial policies did great long-term damage to German science and culture and much enhanced it in other countries, particularly the USA and UK. England was particularly fortunate to persuade Michael Polanyi to take up an offered chair in physical chemistry at the University of Manchester. He was elected to the Royal Society a few years later. Haber himself was for a period a visitor to the Cambridge Chemistry Department but died soon after in 1934 in while travelling to Palestine. The Institute itself was in effect taken over by the Prussian Ministry of Culture. During WWII and its workshops were used for wartime purposes.

Following WWII there was much confused discussion between the Allied occupying authorities and senior figures in the former KWI until it was finally agreed that the Institutes could continue under the revised name of Max Planck Institutes within the MPG. The specific name of Fritz Haber Institute was chosen for the former KWI of Physical Chemistry and Electrochemistry. During the following decades the standard of research rapidly grew back to the high level of the 1930s, and has included major developments in electron microscopy and in surface science which led Ernst Ruska and Gerhard Ertl respectively to being awarded Nobel Prizes. Other major research fields include those of electrochemistry (Heinz Gerischer) and catalyst-design (Hans-Joachim Freund).

This very welcome book has been written by a Centennial Group of four authors which included working scientists with historical interests, together with professional historians of science. It adds much fascinating detail to the bare outline of the 100 year history of the FHI that has been described above. These relate to scientific achievements, with many photographs of research groups and individuals, and also biographies of the principal scientists. Very interesting accounts are given of the origins of the original KWIs, the fate of the Institute during the Nazi period, and its resurrection after WWII as the Fritz Haber Institute of the post-war Max Planck Gesellschaft. The book ends with Lists of selected References relating to the Institute itself, its Directors, the Kaiser Wilhelm/Max Planck Societies and of some prestigious scientific publications over the years that originate from the FHI. It has an index of personal names but would have been considerably improved by the addition of a general index.

The Centennial Group of four members is to be congratulated on a very fine achievement in their recording of the Institute's activities and circumstances during its first 100 years of activity. The book is written in excellent English, the present-day prevalent language of science, thereby making possible a wide readership. It is highly recommended and is of particular value to physical chemists/chemical physicists with historical interests and constitutes an invaluable source of information for historians of science working in this and related areas.

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The following comments are not strictly part of the review but I consider them worth including – especially in view of the recent interest shown by David Willetts MP, the Minister of State for Universities and Science in case studies of successful innovations from fundamental research, over the last 30 years - Editor

Your reviewer has had personal contact with the Institute, having served recently for about six years as a member of the Institute's international Advisory Board. During that period two positions as director changed as a result of retirements and it was interesting to see the high calibre of the new directors appointed in the subject areas that the Institute, in consultation with the MPG, had in mind. Such persons appointed are fortunate to be welcomed with the offer of resources comparable to those that might be expected by a newly-appointed Oxford or Cambridge professor in the UK. Bearing in mind that the independent MPG have at present about 80 Institutes, nearly all devoted to basic or fundamental scientific problems (the FHI is a larger example), and that in addition the country has the usual proportion of university-based laboratories, one forms an impression of the great strength-in-depth of present-day German science. This contrasts with the position of several other European countries where most fundamental research is still carried out within the universities themselves, supplemented by access to a few national or European Institutes that are sources of neutrons, synchrotron radiation etc. Particularly in the UK a considerable amount of forward-looking industrial research has, with the agreement of the Government, moved onto university campuses in recent decades. This has been accompanied by Research Council support being more concentrated on work related to industry to the partial detriment of fundamental research of the type so strongly pursued in the German MPG Institutes.
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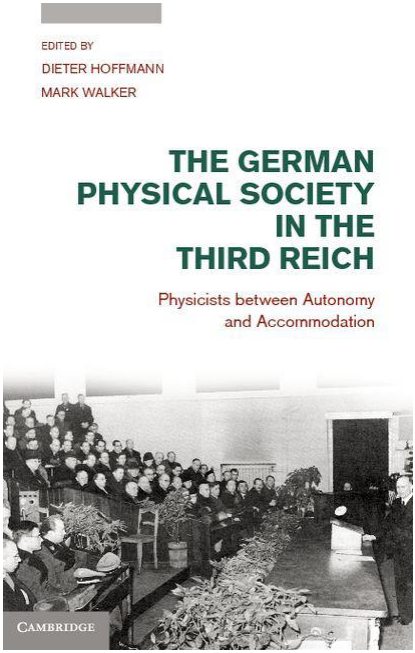
Book previews



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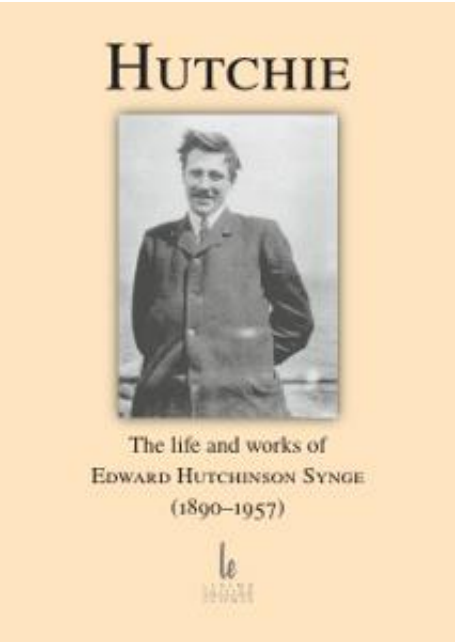
ISBN 978-1107006843
482pp Hardback £55

This is a history of one of the oldest and most important scientific societies, the German Physical Society, during the Nazi regime and immediate postwar period. When Hitler was appointed chancellor of Germany in 1933, the Physical Society included prominent Jewish scientists as members, including Fritz Haber and Albert Einstein. As Jewish scientists lost their jobs and emigrated, the Society gradually lost members. In 1938, under pressure from the Nazi Ministry of Science, Education, and Culture, the Society forced out the last of its Jewish colleagues. This action was just the most prominent example of the tension between accommodation and autonomy that characterized the challenges facing physicists in the society. They strove to retain as much autonomy as possible, but tried to achieve this by accommodating themselves to Nazi policies, which culminated in the campaign by the Society's president to place physics in the service of the war effort.



Hutchie – The life and works of Edward Hutchinson Synge

Editors:
Donegan, Weaire & Florides

Living Edition *2012*
ISBN *978-3-901585-17-3*
154pp *Hardback*

A fascinating and innovative book charting the life of this scientist who is virtually unknown outside his field but who, among other things, contributed so much to advancing the theory, design and applications of optics – especially near field microscopy and telescopic interferometry. Born in 1890 into the prodigiously creative Synge family he worked essentially alone with little formal training and academic background.

This book takes a fresh approach to historiography and includes a relatively brief yet essential account of his life –a life which ended tragically through mental ill health when he was only 67 years old.

Following this biography, after an appendix of his correspondence with Einstein, chapter 2 presents a facsimile of almost all of his published papers – rightly disseminating his works to a wider audience:
A definition of Simultaneity and the Aether
A method for extending microscopic resolution (two papers)
A method of investigation the higher atmosphere
A modification of Michaelson's beam interferometer
A design for a very large telescope
Interference methods and stellar parallax
A note on twinkling
An application of piezo electricity to microscopy

Optica

An exhibition of microscopes and telescopes at the Orangerie in Kassel, Germany. September 2011 until March 2012.

This exhibition displayed some of the fine collection of Karl I (1654 – 1730) Prince (or Count) of Hesse-Kassel (figure 1) and his successors. In part, the idea was to recreate the 'visual arts room', called 'Optica', that Karl had set-up in 1696 in his house. The original murals made reference to the observations possible with the instruments – it was meant to be an 'immersive experience' of sorts where the whole was greater than the sum of the parts.

Figure 1. Karl I (1654 – 1730) Prince of Hesse-Kassel.

The instruments were not just for courtly amusement, but used to research and teaching. The exhibition was the culmination of many years' research, including the unexplored cultural history and significance of the collection, and the understanding of the role of the room as a place to gather 'curious objects' of art and science. A separate room for optical devices was still unusual for the early 18th Century. This modern 'optica' aimed to make accessible this unified way of thinking about the micro- and macro-cosmos. For those already with an interest and some knowledge of the history of science this came across well-enough, but the link may not have been sufficiently explicit for a more casual visitor – at least that would be so for a UK audience. The curators clearly had to work within the constraints of having the originals in display cases and limitations in budget for creating reproductions for hands-on use. Even if viewed solely as a set of beautifully crafted objects and a bit of science thrown in, it was successful.

Like many in his position, Karl took to astronomy and was able to indulge his interests by purchasing high quality instruments from the best makers. It was later that he then started collecting microscopes and observing the small-scale. This was not just a casual interest of Karl's – he was buying instruments from all across Europe, including England. It is not clear how much observing Karl did himself,

but he certainly assisted by providing the infrastructure. When expelled from France in 1685, Karl allowed 4000 Huguenots to settle in Kassel, he also stimulated the metal-industry and was interested in archaeology. Eventually, Karl built the 'Orangerie' as a summer residence, which is now the Museum of Astronomy and Technology. (see www.museum-kassel.de)

Optica consisted of about 40 items in a side gallery. The quality of the craftsmanship was astounding and the instruments were very well preserved. Most of the captions and descriptions were in both English and German. Though I didn't get a feel for how many would normally be on display individually, it seems that some of the exhibits were normally housed the different museums in Kassel. Of the telescopes, there were refractors made by Giuseppe Campani (Rome, 1650), Simeon Mesnard (Paris,1700), John Marshal (London, 1709), Edward Scarlett (London, 1727), Heinrich Ludwig Muth (Kassel, 1730), and several others made by local (but unknown) craftsmen. There were simple and screw-barrel microscopes by Johann van Musschenbroek (Leiden, 1700), Nicolaas Hartsoeker (Amsterdam, c.1700), and Edmund Culpeper (London, 1700). Compound microscopes included instruments by Johann Christoph Sturm (Nuremberg, late C17th), Giuseppe Campani (Rome, 1700), and John Marshal (London, 1710).

The most beautifully decorated was by an unknown French or Italian maker (see front cover). There were a few other optical instruments too, including a large combination burning lens (figure 4) by Ehrenfried Walther von Tschirnhaus (Keslingswald, 1697) and a concave mirrors by Francois Vilette (Lyon, 1698). There were three magic lanterns by Giuseppe Campani (Rome, 1700), a blue and white lantern by an unknown maker working in Kassel in 1702 (figure 5), and fabulously crafted device by Johann Philipp Treffler (Augsburg, 1698). The original slides, many of which were on display, were mostly caricatures (figure 6).

In addition to the beautifully preserved instruments the exhibition had illustrations, computer animations, and hands-on replicas. The computer animations were of the view through late 17th Century microscopes and telescopes. Although it was amazing what was observable with these instruments, I think the exhibition could have provided a stronger link to modern microscopy and astronomy demonstrating the hugely improved image quality now available. Of the replicas, the most interesting was the early 18th century blue and white magic lantern. The exhibition catalogue is more of a book and includes colour illustrations of basic geometric optics and the principles of operation of different configurations of microscopes and telescopes. It is superbly produced, but only available in German.

Figure 4. Lenses by Walther Ehrenfried von Tschirnhaus.

Figure 5. Late 17th century magic lantern made in Kassel by an unknown craftsman.

Both photographs by courtesy of the museum of Hessen Kassel.

Kassel is a modest-sized city of about 200,000 inhabitants in central Germany. It is best known for where the Brothers Grimm wrote their fairy tales and a monument to Hercules above the city. Since 1955 the Documenta, an international exhibition of modern and contemporary art, has been held every five years in Kassel. There is the usual array of country houses and Schloss nearby too. It was extensively rebuilt in the 1950s, but retains some interesting older buildings, including the Town Hall, hospital, the main park, and the Orangerie. The Orangerie has a very pleasant setting in the park and houses the museum, a planetarium, and a hands-on centre for astronomy. The museum has a collection of other scientific instruments: vacuum pumps, electrostatics, surveying equipment, astronomical clocks, and glassware. Sadly I didn't have enough time to explore this part, however judging by the visitors it seems to be successful.

Colin Axon

The Manchester Museum of Science and Industry (MOSI) is a vast group of buildings occupying the old Liverpool Road railway station and houses a diverse array of exhibits from air, road and rail transport, Textile Machinery to the commercial and domestic development of energy distribution via gas and electricity.

It holds collections from many local firms including some well known names such as GEC and Ferranti – both involved in heavy electrical machinery and fine electronics.

But the section which was the reason for my visit was the somewhat cryptic 'Manchester Science'. It comprises four galleries off a large concourse, one each for John Dalton, James Prescott Joule, Ernest Rutherford and Bernard Lovell.

Outside each gallery, by way of 'setting the scene' was an intriguing display using 'Pepper's Ghost' technique overlaying actors on model sets – the one seen here is of that of Geiger and Marsden passing on their famous surprising results to Rutherford.

Each gallery was a mix of wall displays, charts, photographs etc. with a central part devoted to working models and 'hands on experiments'. The competing AV presentations both in the concourse and dedicated galleries were distracting – something I hope the new Director will address.

It was a well thought out scheme and generally successful (judging by the responses I got from visitors) and, of course it was good to see the history of physics especially the contributions of these particular physicists, given such a high profile in the city where they worked.

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Scientists and Technologies involved in the above

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Next Group meeting

The next group meeting will be held at the Bath Royal Literary and Scientific Institution on 8th November 2012. It will be a half day session from 2-5pm entitled:

'The Braggs and their Legacy'.

There will be two subject based lectures – one on 'Chemistry and Crystallography' and another on molecular biology. The third talk will be on JD Bernal by John Finney.

The meeting is being organised by Peter Ford.

The group AGM will be held on the same day (probably before the lectures but details are yet to be arranged.

Disclaimer

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