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History of Physics Group

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Newsletter

25 Years

NEWSLETTER No. 1 March 1987

DISCORSI
E
DIMOSTRAZIONI
MATEMATICHE,
intorno à due nuoue scienze
Attenenti alla
MECANICA & i M... VIMENTI ...OC... I;
del S... nor
GALI... GA... I... LI... CE
Filosofo e M... atic... rim... del S...
Gra... ... di ... cana.
Con una Appendice de... o di gr... alcuni Solidi.

IN LEIDA,
Appresso gli Elsevirii. M. D. C. XXXVIII.

The title page of Galileo's Mathematical Discourses and Demonstrations concerning Two New Sciences (Leiden 1638).

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Cover picture: Front cover of the first issue of the Newsletter

Contents

STOP PRESS!

Next meeting will be held in July at Birmingham University (to be confirmed). Further information will be posted on the website.

Editorial

The Rutherford Appleton Laboratory is a well known feature on the landscape of British research establishments and it appears in a number of places in this issue. The work of Rutherford is, of course, very well known but that of Edward Victor Appleton is perhaps a little less familiar.

Born in 1892 in Bradford, Yorkshire, he showed great aptitude in school from the start and in 1903 he won a scholarship and a first class matriculation at the minimum possible age of 16. In 1911 he was awarded an exhibition at St. John's College, Cambridge where, studying under JJ Thomson and Ernest Rutherford, he gained first classes in the natural sciences tripos in 1913, 1914. His experience as signals officer in the First World War stimulated an interest in radio wave propagation problems and on his return in 1919 to Cambridge he turned his attention to the radio frequency properties of ionized gases. Just 5 years later, working with MAF Barnett, he showed that signal fading could be caused by the interference of direct waves and those reflected from an ionized layer in the upper atmosphere, which worked out to be at a height of about 90 km. Just such a layer had been suggested by AE Kennelly and O Heaviside in 1902. Further work resulted in the discovery of another layer at a height of about 230 km – with even greater ionization characteristics. This became known as the Appleton Layer and it was this work which, in 1947, led to the Nobel Prize in Physics for 'his investigations of the physics of the upper atmosphere especially for the discovery of the so-called Appleton layer".

It is interesting to note that Robert Watson Watt, in his crucial wartime work on radar, employed some of Appleton's techniques. Also working on radar, though on microwave antenna design, in 1943 was JD Lawson, who died in 2008, (known for the 'Lawson Criterion' - for a minimum required value for the product of the plasma (electron) density n_e and the "energy confinement time" τ_E. needed for a fusion reactor to reach ignition). Following work on the klystron at AERE he moved to the National Institute for Research in Nuclear Science - later to become the Rutherford Laboratory.

Back in 1924, the Radio Research Station was established, instigating systematic observation of the ionosphere. After absorbing space into its title it later transmuted into the Appleton Laboratory and in 1979 joined forces with the Rutherford Laboratory to become the Rutherford Appleton Laboratory.

For a concise summary of these events see Kate Crennell's review on page 47

Malcolm Cooper

News

Readers will recall from the article by Jeff Hughes on the withdrawal of funding by the Royal Society that the continued existence of the NCUACS was seriously in jeopardy. However, we have just received the good news that Unit will be able to carry on its excellent work although in a different guise.

It is to be restructured and associated with the Science Museum Library and Archives as the Centre for Scientific Archives at the Science Museum and will be based at their site at Wroughton, Swindon. The Centre has been incorporated at Companies House as a Company Limited by Guarantee on 30th November 2009 and has applied to the Charity Commission for charitable status.

Peter Harper, who had been with the NCUACS since 1987 and its director since 1996 has left the unit. His work over the years has been invaluable – in the words of the last report from Bath :

"..he saw the size of the Unit double and its output increase greatly. He enhanced its profile among archival colleagues and was a pioneer in international cooperation. It is difficult adequately to summarize the debt that scientists and their families, researchers and archive colleagues owe to his work"

Dr. Anna K Mayer, who joined the unit in 2006, has also left and has taken up a new post at the University of Glasgow.

The Trustees of the Centre, who form the management board of the Company, are: Anne Barrett – Archivist & Corporate Records Manager, Imperial College London (Chair of the Trustees), Rupert Williams – Head of Library and Archives, the Science Museum, and René Kinzett – Head of Public Affairs, National Council on Archives

The Centre will continue the work of the NCUACS and all the parties involved now see this as a great opportunity to develop the work of the unit for the future of scientific archives, in a close association with a national scientific institution.

Any enquiries to: René Kinzett, Email: rene@nca.org.uk, tel 020 8392 5376

M Cooper

Meeting Report and AGM November 25th 2009

This meeting was a special one in that it celebrated the 25th anniversary of the History of Physics Group and the lectures associated with the event reflected the occasion. We were fortunate that four of the six chairmen of the group were able to attend and they all spoke at the meeting. The first chairman in 1984 was Professor Jack Meadows of the University of Loughborough and he began the proceedings with an interesting and wide ranging talk on studies of the history of physics. Professor Meadows was followed by the second chairman Dr John Roche of Linacre College, Oxford who gave an account of the group since its inception and listed many of the events which have taken place. The list reflected the wide range of topics which have been covered during this twenty five year period. We are delighted that John has recently rejoined the committee and continues to play an active role in the activities of the group. The last lecture before tea was given by Mr Stuart Leadstone from Aberdeen who has been a stalwart member of the group since its beginning. His lecture had the intriguing title of "Night Thoughts of a Classical Pedagogue" and discussed many misunderstandings and confused thinking about physics which occurs among students as well as writers and broadcasters. The lecture included some fascinating examples.

During the tea interval there was an opportunity to examine the entire set of newsletters from number 1 to 26 and other items associated with the group such as a comprehensive list of the officers. These were kindly supplied by Malcolm Cooper our Newsletter Editor who also spoke briefly on the development of the newsletter. Its format, following closely on his predecessor's good work has now been described as a 'quasi journal'. A suggestion made by Professor Derry Jones, (a regular contributor to the newsletter) that the title 'Physics History' should be added to reflect this development. A majority of the meeting agreed that this idea should be adopted.

After the tea interval Professor Denis Weaire of Trinity College, Dublin spoke about George Francis Fitzgerald, an early Professor of Physics at Trinity College, Dublin. Fitzgerald is best remembered in his connection with the "Fitzgerald Contraction" in relativity. However, he was a true polymath and made contributions and generated ideas in many areas of science as well as being an early pioneer of flight. The final lecture was given by the present chairman Dr Peter Ford, formerly of the University of Bath, and was an account of the life and science of our third chairman Professor Brian Pippard, who died in 2008 (see also an obituary in our newsletter number 25 for February 2009).

The lectures were held at the headquarters of the Institute of Physics in Portland Place, London. At the end of the lecture programme there was an interval for drinks and nibbles before the AGM. This gave a good opportunity for delegates to socialise and also look at the various items on display.

Annual General Meeting - Chairman's Report

The chairman of the group pointed to a successful year which included two meetings. The earlier meeting had been in Cambridge and was a celebration of the 400^{th} anniversary of the invention of the telescope by Galileo. An account of this meeting was given in Newsletter No 26 in August 2009. The chairman did point out that the numbers attending the meetings could have been better but appreciates that the membership is very widely scattered around the United Kingdom and overseas and that many are quite elderly. However, he would like to encourage better attendance at future meetings.

The Institute of Physics does have guidelines over the length of time that members should sit on the committee. As a result two members - Peter Rowlands and Chris Green stood down and I would like to express my thanks and appreciation to both of them for their considerable input and enthusiasm over many years. They have both offered to play a useful role in continuing to support the group. They were replaced by Professor Andrew Whittaker of Queens University, Belfast and Dr Vincent Smith of the University of Bristol. Both have been long standing members and we welcome them onto the committee. The remaining committee members were re-elected.

The chairman thanked the tireless efforts of Mr Malcolm Cooper for producing two excellent Newsletters each year as well as acting as secretary for the group. This latter role has now been taken up by Dr John Roche and Malcolm Cooper agreed to become the Treasurer. Kate Crennell drew attention to the continuing problems with the web-site for the group. This has been pointed out to the Institute of Physics Headquarters and I hope that the problems will be speedily resolved. Mr Stuart Richardson has drawn attention to the importance of the study of the history of physics in schools and he has been asked to draw up a document on this for presentation to the officers of the Institute of Physics.

On the conclusion of the AGM the chairman thanked all those for attending the meeting and some ten people retired to a local restaurant for an informal meal.

Dr Peter Ford
Group Chairman

25th Anniversary talks*

The vision of the group, and some memorable meetings 1984-2000

Dr John Roche,
Linacre College, Oxford

As one of the founder members of the History of Physics Group, I was delighted to carry out a brief surveyof its history from 1984 to 2000. In 1982 various members of the Institute of Physics, interested in history of science, discussed setting up a History of Physics Group. This led to a preliminary meeting on the 1 February 1984, hosted by Dr Frank Greenaway of the Royal Institution, who was very helpful in establishing our Group. A steering committee was established with Professor Jack Meadows as Chairman, with myself at Secretary, and, Brian Gee, David Hooper, Professor Nicholas Kurti, Stuart Leadstone and Raj Williamson and committee members. The inaugural meeting of the group was held in1985 at 47 Belgrave Square, then the Headquarters of the Institute of Physics, with the title *Experiments and Instruments: The interaction between experimental skills and instrumental craftsmanship.* The Group has a strong experimental flavour. Council of the IOP ratified the History of Physics Group in December 1987.

The main aims of the group are to secure the written, oral and instrumental record of British Physics for posterity; to foster a greater awareness concerning the history of physics among physicists; to promote the importance of the history of physics in physics teaching; to provide a forum where the different disciplines of physics my interact fruitfully. The group recognises that the special skills of the practicing physicist is indispensable for certain kinds of research in history of physics, especially in the clarification of difficult physics concepts.

The Group ran three meetings each year. The venues were distributed around the country, but mainly in London, Edinburgh, Oxford, Manchester and Liverpool. By 2000, 43 meetings had taken place. One of the most ambitious ventures was a 3-day conference held in Oxford in 2-4 July 1986, on the theme *The History of Physics for the Physicist.* This led to a conference book. Two other conferences also led to books.

* These articles are taken from the author's notes for the meeting – Ed.

The pedagogy of physics has been strongly represented on the Committee by Mr Stuart Leadstone. Perhaps the mot important pedagogical meeting in this period was *The History of Physics Workshop of Physics Teachers* on the 18 March 1989 in 47 Belgrave Square, London.

We arranged various joint ventures with other bodies and societies. A particularly successful meeting was held in July 1990 *Aether to Relativity*. It was arranged by Peter Rowlands, a member of the committee, and held in the Chadwick Physics Laboratory and held jointly with the Liverpool Society for the History of Science and Technology, at University of Liverpool.

Many meetings arose as the result of anniversaries, or as reminiscences of senior physicists, as in 1999 *'Recollections of early years as a physicist in Poland'*, by Professor Joseph Rotblat, at 76 Portland place, London.

Dr Alan Morton of the Science Museum, London was invited to join the Committee in 1988, and became Hon secretary in 1991 and this gave rise to a fruitful liaison with the Science Museum.

One of the considerable successes of the Group has been its Newsletter, which links member of the Group around the world. Being editor is one of the most an arduous tasks of a Group. The first editor was Mr David Hooper of Chester which very committed to photography, and this is shown in the historical photographs which grace his editions of the Newsletter. Mr Bob Joyce of Dudley continued this tradition, as did Miss Lucy Hudson who is the last editor of my period.

Membership of the History of Physics Group has been important to me professionally and has given me enduring friendships. I believe the Group has also, in a small way, supported the profession, but my ambition has been for physicists to recognize the importance of the history of physics in clarifying concepts in present-day physics.

Physics History – Individuals and Institutions

Jack Meadows

Why was the History of Physics Group formed when it was? I would start the story back in the 1960s (though I am probably biased - I became Head of the Astronomy and History of Science Departments at Leicester University in that decade, which gave me a grandstand view of developments). We all know that the 1960s were heady days for physicists, not least with the expansion of space exploration. It is less well-known that they were also heady days for the history of science. The subject expanded rapidly in universities - partly in response to the 'two cultures' debate - leading to a much larger research community. It might be thought that an expanding interest in both physics and history of science could only be good for the history of physics. Unfortunately, it did not work out quite like that. There were problems both with the physicists and with the historians of science.

Prior to the Second World War, most scientists knew something of the history of their subject. After the war, the rapid growth in number of researchers, especially in physics, and the increased competitive pressures meant there was less time and less motivation for thinking of history. By the 1960s, the older generation of researchers was claiming with sorrow that their younger peers knew only the very recent history of their subject. But the problems were not all one way. Developments in the history of science were making it less easy for scientists to assimilate the historical research produced. One reason was the introduction of concepts from other disciplines, such as sociology and psychology. Another was the fact that the new generation of historians of science often had little background in scientific research. A third was that the 1960s was a peak time for what was known as the internalist/externalist debate. Physicists tended to be interested in internal factors - how concepts such as 'energy' came to be defined, who influenced whom in the development of a particular theory or experiment, and so on. Historians of science were now inclined to concentrate more on external factors - such as the extent to which the 17th/18th century concern with astronomy was due to the needs of navigation, or whether the development of relativity theory was related to the zeitgeist of the period. The overall result was that physicists and historians of science tended to drift apart in terms of their interests.

Young researchers may have little time for the history of their subject, but things can change as they get older. Their own careers now form a part of that history. So they may wish to understand better how the activities in which they have been involved fit together when seen retrospectively; or they may wish to know more about events and people they see as important. Sooner or later, most physicists find it necessary to make some mention of history - as almost any issue of *Physics World* illustrates. I advised the IoP on their book publishing programme in the 1970s and 1980s. It was evident that a number of physicists were interested in writing on historical aspects of the subject. Their work was intended mainly for their peers, rather than for historians. What also became evident was that an audience existed in the physics community for books of this sort. My involvement in the establishment of the History of Physics Group therefore stemmed from the belief that there existed both physicists who, for a variety of reasons, wished to explore the history of their subject, and a wider audience interested in hearing about the results of their studies. The continued existence of our Group suggests that this belief has proved reasonably accurate.

But this is not the end of the story. History of science has itself developed since the 1960s. One change is the increased interest in recent science, and in the people involved in its development. This requires attention to the type of 'internalist' questions where physicists can provide insight. Another factor is the increasing role of the institution in the history of physics. The traditional view of science emphasizes the significance of the individual - Newton, Faraday, Maxwell, etc. - for the development of the subject. This perspective is difficult to maintain in an era of big science. A body such as the Rutherford Appleton Laboratory, may have many advances pioneered by individual employees, but the significant developments in historical terms are tied up with the institution itself, along with its major instruments. The study of institutional history is something currently of interest both to physicists concerned with history and to historians concerned with physics. Consequently, there is a greater mutuality of interest between these two groups nowadays. In addition, transfer of knowledge and ideas between the two has become easier. Physicists and historians of science have traditionally used different publishing outlets. The transition to electronic publishing is now allowing better access to each other's work, which can only be advantageous for the study of physics history.

Night Thoughts of a Classical Pedagogue[1]

Stuart Leadstone
Aberdeen

Introduction

This talk is prompted by an unashamed yearning for things past, complemented by a dissatisfaction with things present, but, hopefully, leavened by a touch of humour here and there. Whether the humour is *aqueous* or *vitreous* I shall leave the audience to decide.

Picture the scene. It is a Tuesday afternoon in the 1950's at an educational establishment in the north-west of England. For reasons which were not clear at the time, but which are now transparently obvious, this educational establishment was called a *Grammar School.*[2] It is a dull autumn afternoon and in one of the laboratories there are young gentlemen dressed in blazers and flannels crouched over the benches and peering through the gloom at and through various optical components. This is strange optics however because there are no light sources in evidence. There are, however, a number of large pins stuck in corks, some of which are left undisturbed whilst others are being moved around. For some peculiar reason the young gentlemen are all afflicted with a strange side-to-side head movement which they execute at random intervals. What are they doing? They are students of an archaic subject called *physics* and are in their first term in an esoteric brotherhood called the *lower sixth*, having just embarked on a 2-year course at *Advanced Level* of the *General Certificate of Education.* The scene described above is a regular weekly practical class. This Advanced Level course has begun, you will notice, with *Light* – not Mechanics as may be thought more appropriate – the reason being, I think, that the theoretical basis[3] of a large number of experiments could be rapidly acquired, and hence the weekly laboratory work could quickly get under way.

What was the aim of this laboratory work (which, of course, would not be allowed nowadays because of the sharp-pointed pins)? It was to measure the focal lengths and radii of curvature of a variety of mirrors and lenses in a bewildering number of different ways, most of which involved image location by the *method of no parallax.* Political correctness requires us today to regard all this as dull and boring. The young students of the lower sixth were, however, extremely fortunate. First of all they were under the guidance of a teacher well qualified in physics[4], and who, moreover, was allowed to devote the greater part of his educational endeavour to the subject. In the regular exposure to practical work they were learning something of great value - the necessity for extreme care in the setting up of experiments and for taking great pains in order to achieve reliable results. Also, as the relevant theory sank in, they were becoming aware of the main thrust of physics, namely the explanation of a wide range of phenomena in terms of a few basic assumptions, developed and expressed through the medium of a language – now dead – called *mathematics*.

Furthermore, by being required to write up every experiment with title, labelled diagram, method, theory, readings, analysis and conclusion, they were receiving a rigorous training in the clear presentation of scientific work. My own school practical books contain full write-ups of 60 experiments performed during the A Level course of 1953-55, which averages at 10 per term. At a similar stage of one typical modern course in physics the students are required to write up formally just one experiment from each main course section, i.e. on average, just one per term. This requirement by the course designers is there for two reasons: (1) to make sure that the students do *some* practical work and (2) to make sure that the teachers do some marking! For mark them they must, according to a scheme designed by the devil himself. This involves such extremes of devotion and thoroughness as checking the plotting of every point on every graph of every student. In an attempt to introduce some "oomph" into a class practical on the verification of Boyle's Law (sorry, I should have said "on the investigation of the relationship between the pressure and the volume of a fixed mass of gas at constant temperature") I decided to use a gas syringe coupled to a pressure sensor and hand-held data-logger. This enabled each pair of students to acquire fairly quickly 20 pairs of values of pressure and volume. Volume being the notional "independent variable", and having an eye to the outcome of the experiment (leaked by numerous text-book authors) the whole class of 20 plotted pressure against 1/volume. The alert amongst you will realise that the values to be plotted were unlikely to be simple integers. My pedagogic enthusiasm thus landed me with the task of checking the plotting of 400 graphical points, the coordinates of which were numerically "awkward". This I did with assiduous care, and as a consequence made an important educational discovery: not one of the students managed to plot all 20 points correctly! Does this mean that they all failed this formal assignment[5], and hence the entire course? Not a bit of it; the rules of modern education allow the student to go on re-submitting until he or she finally gets it right. The examining bodies will never lay down **how many** re-submissions a student should be allowed, this being left to the teacher's discretion. The teacher, however, is scared of being sacked if his or her pass rate drops. Thus we have the guarantee of "standards of excellence", a beautiful illustration of the universal law that "**all educational initiatives are doomed to succeed**".

Why do I tell you all this? Because during the course of my 4-decade teaching career I have seen the abandonment of rigour, numeracy, literacy[6] and honesty. One could summarise the transition as going from profundity to triviality. Enough of this gloom: let us turn to those with real knowledge and expertise in our subject: examiners, authors and broadcasters.

First, examiners.

I have recently being giving maths tuition to a young man at a notable tertiary institution, a university no less. He brought along a copy of a Final Exam Resit Paper, which involved amongst other things applications of calculus. One of the questions was as follows:

A force F varies with time t according to the expression $F = e^{0.5t} \sin 0.07t$.

Calculate the work done during the time interval from $t = 0$ *to* $t = 5$.

This of course reveals a total confusion in the mind of the examiner between ***impulse*** and ***work done***. Should this bother us? Well, bearing in mind that this paper was the last opportunity for some students to gain the course qualification, I should say that it is an extremely serious matter. I have no doubt that marks were given for answering an incorrect question incorrectly.

At this point I must share with you a technique which I devised for combatting escalating blood pressure brought on by exposure to the slings and arrows of modern educational practice. If you are, or have been, a fellow-labourer in the educational vineyard, I am sure you will agree that it is important to give vent to one's feelings but without inflicting injury on other persons. I therefore hit on the idea of having a rubber stamp made that I could slam down on a student's work if something outraged or angered me to excess. The first of these was simply the word UNITS! In modern physics teaching, the requirement to know formal definitions has virtually disappeared and it is not surprising that omission and carelessness with units are commonplace. Finding it necessary repeatedly to write the word UNITS! on students' scripts becomes very trying. Hence the use of the rubber stamp. In my simple way I hoped that the appearance of this, say, 15 times in a student's test paper would make an impression on the student.

For the above exam question however we need something else; something to encapsulate the extreme degree of pain and frustration experienced on reading such a question. For the appropriate acronym I am indebted to Tim Watson, formerly of The King's School, Worcester, who confided in me that he used to write the word WAGOT! against anything so wrong as to be outrageous. So I decided that this would be my second rubber stamp. Meaning of WAGOT! ? WAILING AND GNASHING OF TEETH ! The exam question cited definitely gets a WAGOT!

I recall another exam question, this time from an end-of-course physics paper for the much lauded International Baccalaureate. The candidates were given a situation involving a body on an inclined plane. The various forces acting on the body at a particular instant of time were given. The candidates were asked in one part of the question to state the direction of motion of the body. This shows that the examiner was a pure Aristotelian! This type of mistake in an important examination is serious, especially for the good students who understand the subject, because they realise that they are being asked a question which is strictly unanswerable.

Second, authors.

In a much lauded award-winning popular science book from the year 2002 the author describes the traditional fields of physics before moving on to more exotic areas. With reference to Maxwell's theoretical treatment of electromagnetism he writes that:

> *"Maxwell's equations . . . describe the connection between an electric field **E** and a magnetic field **B**, where **E** is a vector function that for each point and each moment of time gives an electric current*[7] *(a vector) at that point . . . "*

At this juncture I am tempted to do some audience research and ask how many of you find this statement (a) true, (b) false, (c) obscure. However, time is limited, so if you have strong views on this please tell me during the refreshments[8]. Suffice it to say that the relationship of this statement to the two formal definitions of electric field strength used by physicists (namely the force per unit charge or the potential gradient) is by no means obvious.

A further quotation from the same book relates to Planck's quantum hypothesis. Regarding this he states that:

"He (Planck) posited that energy comes in discrete 'clumps', which he called quanta. The number of quanta carried by an electromagnetic wave is proportional to the frequency of the wave. The higher the frequency, the more quanta."

What has been missed here is the fundamental correlation between frequency and quantum **energy** on the one hand, and intensity and **number** of quanta on the other, though the author correctly states these when discussing the photoelectric effect a page or so later.

A second example from popular science writing is from an acclaimed 2005 book on the hidden dimensions of the universe. In her treatment of the origins of quantum mechanics the author states:

"Bohr decided that electrons couldn't move in just any old orbit: an electron's orbit had to have a radius fit a formula he proposed. He found these orbits by making a lucky and ingenious guess."

At this point I imagine that most of us would expect some reference to the quantisation of angular momentum. However what follows is:

"He decided that electrons must act as if they were waves, which meant that they oscillated up and down as they circulated about the nucleus."

Bearing in mind that Bohr's paper on the spectrum of atomic hydrogen appeared in 1913, eleven years before de Broglie's paper on the wave nature of the electron, this account is seriously flawed historically, quite apart from any deficiencies in the physics. I suspect that there is a view around, certainly in the publishing world, that this sort of error is unimportant because the book is only intended for the lay person. Let me call Peter Guthrie Tait to the witness box. Invited to speak to the British Association in Glasgow in 1876, Tait chose not to present the latest or deepest thoughts of an eminent Natural Philosopher, but rather to speak on a fundamental concept of classical physics, namely ***force***[9]. He began his address thus:

It was not to be expected that I could, at short notice, produce a Lecture which should commend itself to the Association by its novelty or originality. But in Science there are things of greater value even than these - namely, ***definiteness*** *and* ***accuracy.*** *In fact, without them there could not be any science except the very peculiar smattering which I usually (but I hope erroneously) called 'popular'. It is vain to expect that more than the elements of science can ever be made, in the true sense of the word, popular; but it is the people's right to demand of their teachers that the information given them shall be at least definite and accurate so far as it goes. And as I think that a teacher of science cannot do a greater wrong to his audience than to mystify or confuse them about fundamental principles, so I conceive that wherever there appears to be such confusion, it is the duty of a scientific man to endeavour by all means in his power to remove it.*

The contrast between this and the spirit of our age could not be more marked.

Third, the broadcasters.

Surely here, where the speaker must be aware that a large number of people are receiving the information simultaneously, we can expect attention to accuracy and clarity. Anyone who has listened to some of the scientific topics tackled by Melvyn Bragg in the Radio 4 programme "In Our Time" will, like me, have been dumfounded by the errors perpetrated by his "experts". Leaving aside small details such as an implied inverse-square law between bar magnets, and an impression given that Bode's Law follows from Newtonian gravitational theory, I will focus on a programme earlier this year (May 2009) which dealt with the physics of the vacuum. With reference to the Michelson-Morley experiment, one expert was asked why the experimenters turned the apparatus through 90°. She replied, after a thoughtful pause: "It's always good to do things twice." Another expert, asked by Bragg to give an example of what he meant by "the conservation of energy" said "when heat is turned into power". As a response this fails on three counts: (1) it does not explain the significance of the word "conservation"; (2) power and energy have different physical dimensions so cannot possibly be transformed one into the other; (3) "heat" has no more right than "work" to be regarded as a form of energy[10], of which more later.

Perhaps we should turn to Radio 3 for serious science. In a recent (22-26 June 2009) series of programmes on "Strange Encounters - Key Moments in Science History" one talk was entitled "The spark of genius that made broadcasting possible". The speaker, a recent biographer of Maxwell, described the experimental discovery of electromagnetic waves by Heinrich Hertz. I quote:

'The young man (Hertz) imagines the presence of his old mentor Helmholtz at his shoulder. "You've not finished yet! Sparks aren't enough on their own. You need to prove that it's ***waves*** *that the primary circuit is sending to the detector. You know how to do that, don't you?" Indeed he does. If you reflect any travelling wave back directly towards its source, the forward and reflected waves combine to make a standing wave which appears to oscillate in the same place, like a violin string. There will then be peaks, where the oscillation is greatest, and troughs, where there is no oscillation at all'.*

End of quotation.

This is the moment to reveal to you my third rubber stamp. NIMU! NOT IN MY UNIVERSE[11]. In case you have missed the point, the speaker has completely confused the peaks and troughs in a travelling wave with antinodes and nodes in a standing wave. Let me make clear my point here. All of us, myself included, can easily make a slip like this. But (a) we would probably spot it in checking over the text and (b) responsible production of radio programmes would provide independent checking and proof-reading. In the context of the public engagement of science these are clearly no longer features of the policy of the BBC, for whom Tait's "definiteness and accuracy" have been sacrificed to pace, gimmickry, and something called the "wow factor". I shall return to wow factors in a moment but first let me refer back to my querying of "heat" as a form or type of energy.

The problem here stems from what I shall call "unfinished business" on the part of the physics community. The term "heat" is used in common **and** specialist jargon as both a **type** of energy, and a **process** by means of which energy is transferred from one system to another. This has embedded a confusion into the subject which could be eliminated by a rigorous adherence to agreed technical terms. This is not a solitary example. My short list of terms which are misleading for various reasons is as follows:

free fall – a body in "free fall" may be rising, falling or neither. It is also not free ! (Incidentally, neither are "free electrons" free!)

electromotive force is not a force

binding energy is actually defined in terms of **unbinding** (i.e. dismantling) of a bound system. For a bound system, the energy involved in its formation from its constituents has already been liberated. What sense does it make therefore to speak of "the energy locked up in the nucleus" or "energy-rich molecules" ?

dark energy – since energy and mass are equivalent it follows that the presence of additional energy should add to self-gravitation and hence slow down the expansion of the universe rather than speed it up

acceleration – in common parlance, and in much elementary teaching, this is taken to mean "the rate of increase of speed". This is so far removed from the physicist's use of the term, namely "the rate of change of velocity" that it is a matter of extreme urgency to restrict the term acceleration to its common (scalar) usage and to introduce a new technical term for vector acceleration which has no apparent connotation regarding the change in the magnitude of velocity. The need for such a term is clear if we draw up a table of the fundamental physical quantities of kinematics:

Scalars	*Vectors*
distance	displacement
speed	velocity
acceleration	?

My candidate for the missing term, which I owe to several classicists of my acquaintance, is **epitachysis**[12]. Etymologically this is the Greek equivalent of the Latin derivation "acceleration". Those in favour are invited to sign a petition for presentation to the most important person in the country – Peter Mandelson!

To whom shall we entrust the "unfinished business" to which I have alluded? There is undoubtedly a roll here for the History of Physics Group. I have long thought that an essential step in clarifying understanding is to look back to the origins of the fundamental concepts of physics, concepts with which we are all too familiar. This indeed was one of the original aims of our group, as envisaged by our founder, John Roche, and I pay homage here not only to John's vision, but to his own following up of this in practice, as his recent papers on mass and momentum testify[13].

Some of you may think that all this emphasis on error, inaccuracy and lack of clarity is mere pedagogic fussiness. In my defence I would point out that university academics are not uncritical of the teaching to which their undergraduates have been subjected at school, and in case you are thinking that I am raising a storm in a tea-cup let me quote from a source much more authoritative than myself, namely Robert Lindsay and Henry Margenau's "Foundations of Physics"[14]. Under the heading "Need for precise definition of concepts" they write:

One of the great difficulties associated with the framing of hypotheses for any theory lies in the definition of the symbolic concepts which are to be used in the hypotheses. It goes without saying that the more precisely this definition is carried out the more definite will be the results of the theory, and the more valuable it will be as a description of phenomena. It would hardly be necessary to stress this matter at all were it not for the fact that in many physics texts - popular and professional - definitions are cited which are quite meaningless or positively misleading.

I would like finally to return to "wow factors", though I would prefer to call them "encounters engendering deep emotion". Three of these which I have experienced in my time as a student and a teacher of physics are, respectively, a **theoretical device**, an **instrument of measurement**, and a **demonstration of a physical phenomenon**.

First, the theoretical device. In the study of geometrical optics I was brought up on the "real-is-positive" sign convention, and this I have also consistently taught at school level, despite the scorn shown for it in university circles. What impressed me about this when first I met it was the sheer ingenuity of using the **sign** of a physical quantity to convey something about the **physical nature** of the entity to which the quantity related. Thus, if you did not anticipate the sign of the answer, the mathematics would deliver to you the nature of the image or the optical component, as the case may be. It also shed light on that most mysterious of ideas - the virtual object. Furthermore it yielded just **one** formula for conjugate object and image distances for **both** types of curved mirror and **all six** types of thin lens. Fabulous!

Second, the measuring instrument.

This is the Nicholson hydrometer[15], as illustrated right. Given only a set of calibrated masses, and the mass of the hydrometer itself, this elegant instrument enables one to determine the relative density of both a solid and a liquid. In the dark days to which I referred at the beginning of this talk it was customary to expose young minds to problems where one really had to grapple with detailed application of physical principles, such as errors in barometric readings caused by expansion of both the mercury and the metallic scale, and, as here, in the use of Archimedes' Principle. It was a true eureka moment when I realised that I could not only reproduce the theory of the Nicholson hydrometer, but that I understood it!

Nicholson hydrometer

Third, a demonstration of surface tension, which still fascinates me. I place a Petri dish (Julius Richard Petri: 1852-1921) on the overhead projector and pour in some clean water. I take a spiral of fine copper wire and float it on the water - a miracle! I place a drop of soap solution in the centre of the spiral and behold, it spins! Another miracle! How can anybody not be fascinated by physics! I would respectfully suggest to the Institute of Physics and other such bodies that the effective demonstration of physical phenomena is much more likely to get young people interested in physics than premature introduction to quantum mechanics and modern cosmology.

I have been a member of the History of Physics Group since its inception in 1984. In searching through my archives I came across an overhead transparency which I had prepared for the History of Physics Workshop which we held in the former Institute premises in 1989. It summarises succinctly my reasons both for joining the group and for continuing my membership.

The PRESENT is UNBEARABLE
The FUTURE is UNTHINKABLE
Which leaves.....THE PAST

Night thoughts indeed!

Notes and References

1. The title of this talk pays homage to Russell McCormmach's excellent novel *Night Thoughts of a Classical Physicist* (Penguin Books, 1983). The central character of the novel, Professor Victor Jakob, though fictitious, is a composite of real physicists, and the novel is concerned with his thoughts and memories as he contemplates retirement in 1918 at the age of 70. In particular he regrets the passing of classical physics, with its clear deterministic picture of physical reality, and its inherent values of restraint and balance. At the end of the book the author has added a section on "Sources", which includes an extended bibliography which is a rich vein of information for the historian of physics.

2. The school referred to is Altrincham Grammar School in Cheshire, due to celebrate its centenary in 2012.

3. This "theoretical basis" required mastery of algebra and trigonometry, and knowledge of radian measure and small angle approximations.

Derivations included the far-from- trivial case of refraction at a single spherical interface between two different media, and it was assumed at the outset that students would learn and know the derivations.

4. Harry Lodge (1915-1995), inevitably known as "Oliver" to his students. Among the many things in his teaching for which I am grateful is the historical introduction which he gave to each topic. This laid the foundation for my eventual fascination with the history of physics.

5. In the era described by this talk there were no "teacher-marked assignments" as part of the overall examination process. The practical work was properly examined during a three-hour practical exam in which candidates were required to perform and write up two experiments. The current practice of using teachers as surrogate examiners is a chore for all, and a festering sore for the conscientious. It is also seriously flawed because of the impossibility of ensuring a uniform standard of assessment.

6. I recall one A-level exam question from the 1960's which said: "State the phenomena which may occur when light is incident on an interface between two transparent media." Most present-day students would not be able to answer this properly because they would not appreciate that "phenomena" is plural noun.

7. Apart from any other points with which to take issue, *electric current* is a scalar, not a vector, whilst *electric current density* is a vector. Curious, when you think about it!

8. Or write to me c/o The Physics Department, Fraser Noble Building, University of Aberdeen, Aberdeen AB24 3UE.

9. Tait P, 1876. "Force": Lecture XIV in *Recent Advances in Physical Sciences* (2nd ed). London: Macmillan & Co.

10. For the whole of my teaching career I have to confess to joining the throng and to classifying heat as a form of energy. There is however a genuine issue here. In thermodynamics "heat transferred" and "work done" are two processes by means of which the internal energy of a system may be changed. Why do we never include "work done" in the list of energy types?

Denis Weaire is keen to hear responses to this issue so please communicate with the *Editor of the Newsletter* if you have strong views.

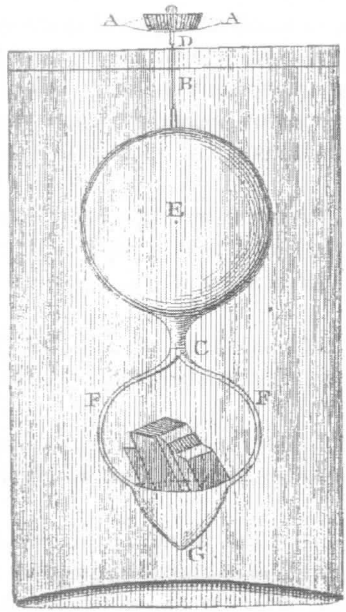
11. All three rubber stamps are still in my possession, though gathering dust as I am now retired. If there is a young pedagogue out there who would like to take them over for active use, I shall be happy to donate them. (See 8 above for contact address.)

12. Private communication from Dr Tom Pearce of the University of Aberdeen, and discussions with Mr Alastair Lumsden of Banchory Academy.

13. Roche J *What is mass?* Eur. J. Phys. 26 (2005) 225-242
Roche J *What is momentum?* Eur. J. Phys. 27 (2006) 1019-1036

14. Lindsay R B & Margenau H *Foundations of Physics* Dover Publications Inc. (New York 1957).

15. This instrument was a development of Daniel Gabriel Fahrenheit's hydrometer by William Nicholson (1753-1815). Nicholson incorporated a scale pan at the lower end of the instrument in addition to the one at the top of the stem. This enabled the relative density of a solid to be found as well as that of a liquid. The Fahrenheit/Nicholson type of hydrometer is a "constant volume" instrument (i.e. constant volume of liquid displaced) as distinct from the "common hydrometer", which is a "constant mass" instrument. Nicholson wrote an account of his hydrometer which was published in *Memoirs of the Manchester Literary and Philosophical Society* (Vol 2, 1785), from which the illustration (right) is taken.



Nicholson Hydrometer
Courtesy of the Manchester Literary and Philosophical Society

400 Years of the Telescope:

Part 2 – The second two centuries

Dr John S. Reid

Hon Curator of the Natural Philosophy Collection of Historical Scientific Instruments, Department of Physics, University of Aberdeen

Introduction

The first part of this article (Newsletter issue No. 26, August 2009) described the first 200 years of the evolution of the telescope, from its introduction as a commercial instrument by Dutch artificers in the early 1600s to its apparently sophisticated development by the beginning of the 19th century. As a historic scientific instrument curator I am almost in awe of the productions of Jesse Ramsden, Edward Troughton and other leading lights of two centuries ago. Immense thought went into the design of their instruments, manual skill that is hardly surpassed today went into their production and the results were elegant. The instruments generally served the science of the times well. In truth, however, their refractors seldom exceeded 10 cm in diameter, and reflectors of the day were hardly ever a few times this figure. To make an academic analogy, if today's telescopes are considered first-class Honours productions, two centuries ago the discipline was still in the primary school.

William Herschel had indicated what secondary school was about, how large reflectors were the way forward for deep space astronomy but his example didn't get taken up by national observatories of the day. I mentioned previously how the English makers lost their leadership at the beginning of the nineteenth century to makers in continental Europe. It was the Swiss Pierre Louis Guinand who discovered a method of making large flint glass disks of quality, the pre-requisite for large achromatic objectives. The baton for leading-edge instruments passed to French makers such as Lerebours and Cauchoix, building on a revival of instrument making in that country that had begun in the late 18th century. In Germany Reichenbach, Fraunhofer, Repsold, Merz and others laid the basis for an optical industry that is still strong in the country to this day. Guinand worked with Fraunhofer at Munich for almost a decade and Fraunhofer's very good 24 cm telescope for the Tartu observatory in 1824 was the largest refractor in the world for many years.

The leviathan of Parsonstown

One person who did appreciate William Herschel's lesson was William Parsons, 3rd Earl of Rosse, amateur astronomer *extraordinaire* with wealth as great as his passion for astronomy. Over a four year period in the 1840s, the Earl designed and had built by far the largest telescope in the world (Fig. 10), with speculum reflectors of 72 inches (1.83 m) in diameter and a tube 58 ft (17.7 m) long.

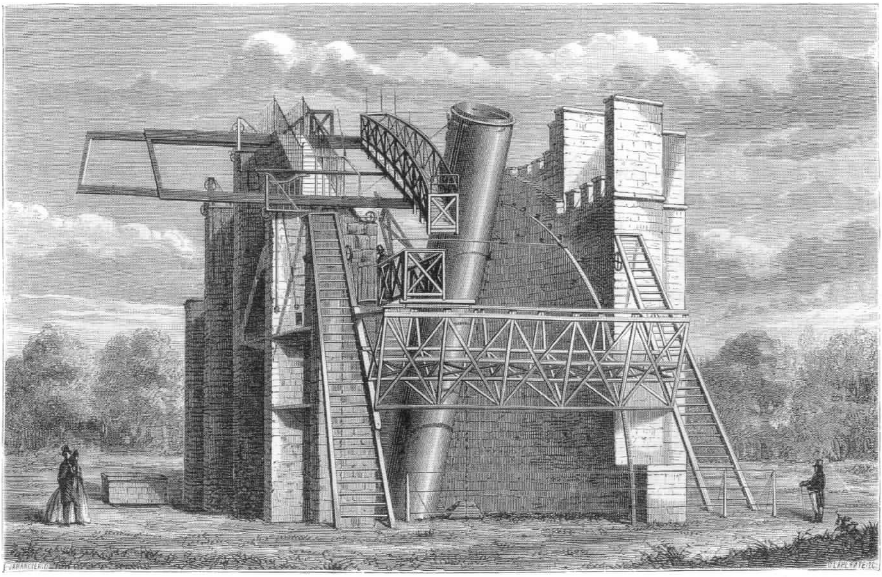


Fig. 10. Lord Rosse's great telescope at Parsonstown

I say 'reflectors', for the Earl cast two successful ones, so that he could have one re-polished to remove the natural tarnishing while the other was in use. He also hired professional astronomers to use the instrument to its full potential. William Parsons was an 'amateur' only in the sense that he didn't draw a salary from astronomy but in practice carried out astronomy to the highest professional standards. Aberdeenshire's Lord Lyndsay, Earl of Crawford, FRS, PRAS, etc. was another 19th century example in the same mould, whose activities have had a lasting impact on Scottish astronomy, but that is a digression. When William Parsons died in 1867 his son, who became the 4th Earl, continued the development of the instrument and the observing program lasted until 1878. The telescope was not exceeded in size until the 100 inch Mt Wilson telescope of 1917.

The public love astronomy

Telescopes didn't just develop over the centuries like an oak tree grows from an acorn. Telescopes developed because what they showed interested, indeed fascinated, many people, lords to laymen alike. The street telescope in figure 11 is from a book in my library published in 1847.

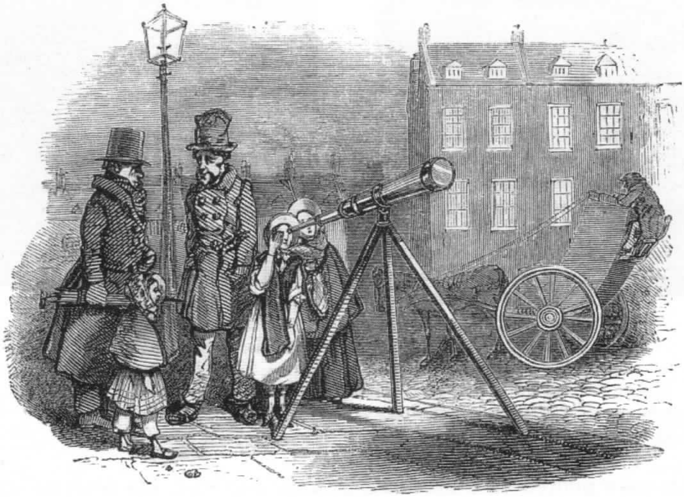
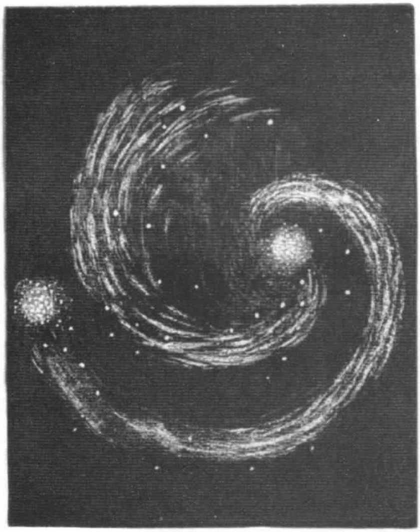


Fig. 11. Street telescope introducing astronomy to the public in the 1840s

A similar picture could be drawn today, not from a city street but at least from a city park or darker site. Astronomy has introduced a great many youngsters to a career in science and has fostered a scientific interest in many adults whose careers have little connection with science

Fig. 12 are images from Lord Rosse's great telescope, which showed to people for the first time the nature of spiral galaxies (the whirlpool nebula, M51, shown here) and that 'nebulosities' in the sky were not all the same by any means. Some could be resolved into stars, others like the ring nebula (M57) or dumbbell nebula (M27) could not. Few people could look through great telescopes but many people could look at the drawings of what was seen and read popularizations of their interpretation.



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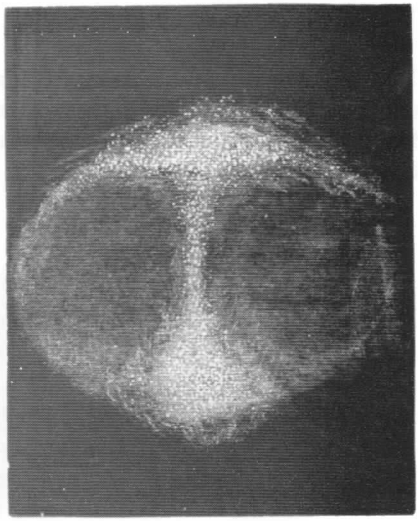

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Fig. 12. Images from Lord Rosse's telescope published in *'Orbs of Heaven: a popular exposition of the great discoveries and theories of modern astronomy,'* O. M. Mitchell, [Routledge, Warne and Routledge, London, 1869].

The originals were printed on a blue background. (a) M51, the whirlpool nebula, (b) M27, the dumbbell nebula.

Fig. 13. James Nasmyth at his home 'Patricroft' with the 20" reflector completely made by himself, circa 1840.

Fig. 13 shows the telescope of James Nasmyth, retired industrialist, made around 1840. He went for a reflector not unlike today's Dobsonian but because of its size (a 20 inch mirror) he routed the optical path through the mounting axis. This has become known as the Nasmyth configuration; a slight modification is the Coudé optics, now common in large observatory telescopes. Nasmyth made the instrument himself from first to last, including casting the speculum and polishing it. Even in my youth, grinding one's own glass mirror was the norm for an amateur. All that has changed in the last few decades.

Nasmyth's example was exceptional. More typical of amateur telescopes were portable refractors on a platform that could be wheeled out onto the patio or balcony. Figure 14 illustrates a design very similar to an example in our collection. The only 'cutting edge' aspect of such telescopes is making good ones cheap enough for the mass market but their hidden importance is that without a healthy amateur base astronomy would wither. The fact is that astronomers cannot themselves afford the instruments in professional use today and need patronage or public money. Ultimately, research instruments rest on an enormous base of public interest in astronomy; they have done so for a long time. Astronomers forget the public at their peril. Fortunately they do not.

Fig. 14. Typical mid 19th century amateur telescope for patio use.

Mid 19th century observatories

With hindsight, large reflectors were the key to deep space astronomy but without the handmaidens of spectroscopy and astrophotography, progress was necessarily restricted. Spectroscopy was in its infancy mid-century and photography through the telescope limited to the Sun and Moon. The Paris observatory commissioned a substantial Newtonian under a large shed on rails that could be rolled away but it was clearly unwieldy. In fact, equatorially mounted refractors were the workhorses for most observatories, mounted under an opening dome. Fig. 15 shows the RGO's 'Great Equatorial' instrument in about 1860, with Airy's innovative and mechanically sound mounting. This telescope had an objective glass of 32 cm by Merz of Munich and the remaining optics by Troughton & Simms of London.

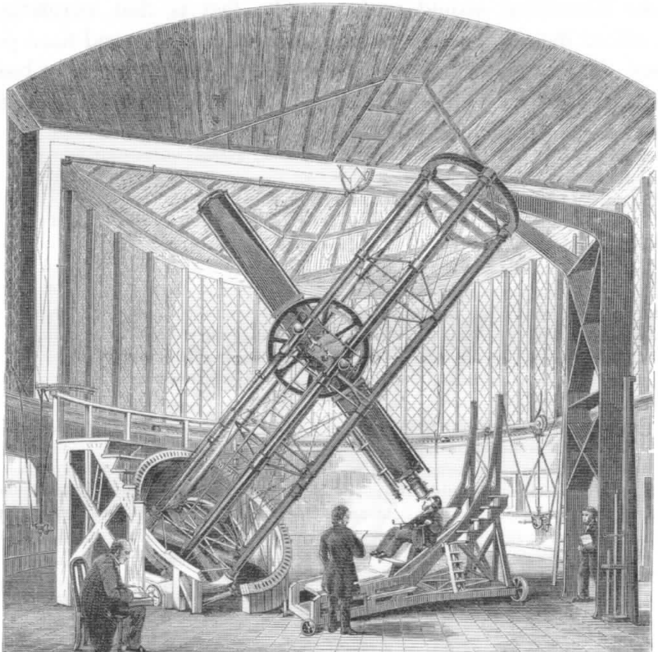


Fig. 15. The 'Great Equatorial' of the Royal Observatory, Greenwich, circa 1860.

Transit telescopes

The Transit telescope is another concept of the 17th century, the first one being devised by Roemer in Copenhagen in 1689, but Transits weren't much in evidence until the 19th century when their development and use blossomed. The transit instrument is an astrometric device, equipped with the highest precision scales available to measure the angle of tilt of the telescope (Fig. 16).

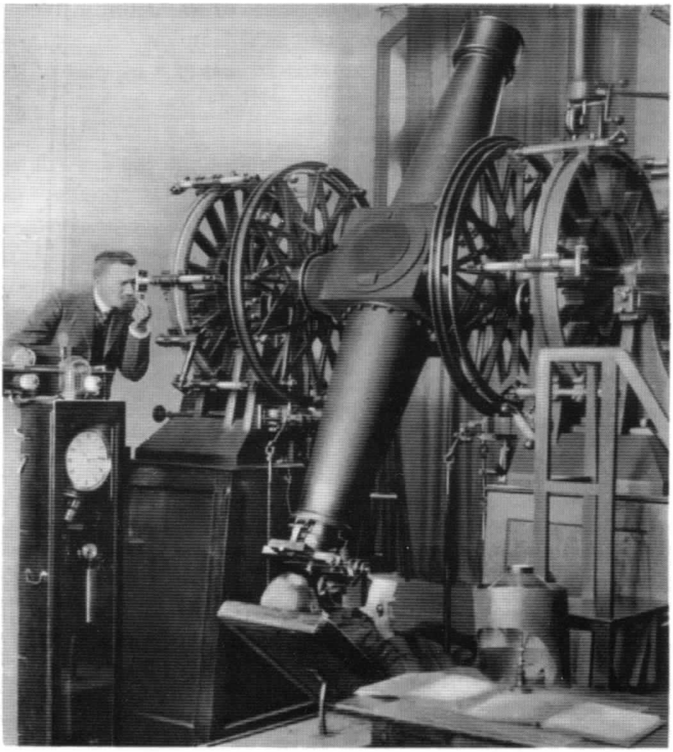


Fig.16. Example of the Paris observatory's transit telescope showing stable meridian mounting, precision scales with multiple read-outs, observatory clock and twin observers, one in the reclining chair and the other at the scale microscopes.

A specially designed instrument with a visible cross-wire system is set up on a horizontal axis exactly on the meridian so that it can swing in one vertical plane. The very precise scales measure the altitude of a star and using the observatory clock and cross-wires the exact time of transit across the meridian can be determined to enable the right ascension, namely the celestial longitude, to be found with precision. The mechanics of a transit instrument are even more important than its optics. National time standards were determined by stellar transits. It was in the 19th century (1884 to be precise) that the Greenwich meridian became the standard meridian for world time and the cartographic zero of longitude for the world. It is the vertical plane of the centre of the Greenwich transit instrument that defines this meridian, as it happens one set up in 1850 by George Airy, the Astronomer Royal, that was 19 feet (5.8 m) east of the meridian used by his predecessors.

For a transit instrument, the scales and the mounting are the key. It was the 19th century astronomer and mathematician Friedrich Bessel who said '*every instrument must be twice made, once by the artist and again by the observer*', meaning that it is up to the observer to detect the defects of the instrument and make allowances that reduce their effects to a minimum. Usually much more time is spent making allowance for defects and temporary influences than is spent in observation.

Grinding

Without precision optics, a telescope is next to worthless. It is hard to over-estimate the importance of accurately forming the mirror and lenses, particularly the objective. Amateurs may have ground their lenses and specula by hand but some professionals developed lens grinding machinery in the 17th century. Machinery may help but the process of accurately figuring a lens or mirror required a lot of detailed technique, experience, feel and the application during the manufacturing process of such quantitative tests as were known about. Machinery did not replace the need for highly skilled artisans, who would guard their procedures as 'trade secrets'. Nonetheless, by the mid-19th century volume production was aided by machines such as the steam-engine driven lens grinder shown in Fig. 17.

Exit speculum reflectors, enter better eyepieces

Sir John Herschel, William Herschel's astronomer son, described the reflecting telescope as '*that eminently British instrument*' and at the time that was a fair comment. However, from the middle of the 19th century, the development of reflecting telescopes became largely a non-British affair.

The great physicist Leon Foucault showed how effective the silvered glass mirror was as a reflector and Steinheil was quick to produce these commercially using a new mercury-free process for depositing silver, developed by the chemist Justus von Liebig.

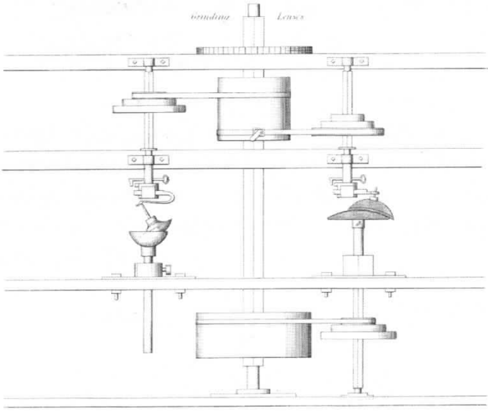


Fig. 17. Lens grinding machinery illustrated in the Encyclopaedia Metropolitana, plate LXXXIV, 1845

In 1858 Foucault also devised the 'Foucault test' for the true sphericity of a mirror that allowed mirrors to be polished to higher tolerances than before. Testing objective mirrors before they are installed is a serious issue, as the Hubble Space Telescope disaster showed. Without it, the phenomenal performance of telescopes now taken for granted wouldn't be achieved. The more modern Hartmann test was developed around 1900 by the astronomer Johannes Hartmann, the results of which enable the various lens aberrations of the objective to be quantified.

The 19th century also saw a much needed development of eyepieces. Jesse Ramsden had introduced a variant of the 2-lens Huygens eyepiece in the last quarter of the 18th century that allowed cross-wires or a micrometer scale held just in front of the first eyepiece lens to be seen in focus simultaneously with the objects viewed. The Ramsden eyepiece was developed into a 3-lens version with improved achromaticity and a wider field of view by Kellner in mid 19th century. This was soon followed by the very successful 4-lens Plössl eyepiece using two achromatic doublets and in 1880 by the orthoscopic 4-lens eyepiece (corrected for distortion) introduced by Ernst Abbe. See Fig. 18. There were further developments of course in the 20th century, notably the popular wide-field Erfle design of around 1920, before eyepieces rather disappeared altogether from professional instruments much later in the century!

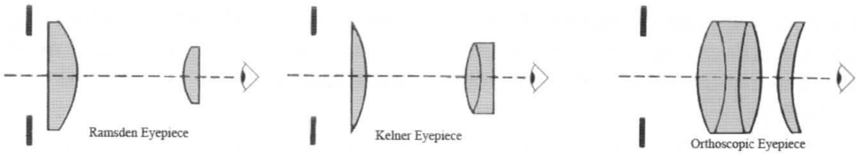


Fig. 18. Evolution of eyepieces with visible cross-hairs or scales. Diagrams adapted from Wikipedia Commons.

The ingredients for the great 20th century telescopes were coming together: larger optics, designed with the benefit of detailed theory, ground by machine and tested for tolerance; improved eyepieces; appropriate and complex mechanical design integrated with an understanding of the properties of materials; precision manufacture, that was developing right through the 19th century and one new consideration - location away from centres of population.

Lick

Lick was the first permanently occupied mountain-top observatory, erected on Mt Hamilton in California. In 1890 it had the largest refractor in the world, with a 36 inch (91cm) objective. You can see some of its mechanical sophistication in Fig.19

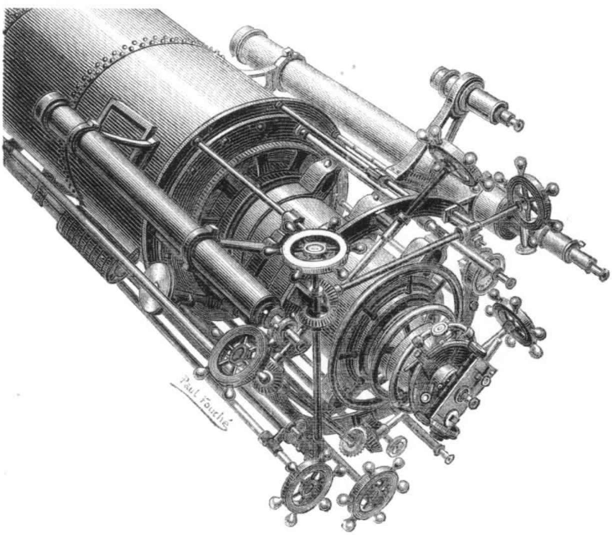


Fig. 19 The observer's end of the Lick equatorial, showing the complexity reached in such instruments by the end of the 19th century.

Thomas Grubb based in Dublin, later his son Howard and subsequently the firm Grubb Parsons produced some of the world's great telescopes during their existence from 1830 to 1985. The Lick was amongst them.

It wasn't just the problem of making large lenses of bubble-free, stria-free glass that made the Lick one of the last of the large refractors. Lenses must necessarily be held around their rim only, whereas mirrors can be supported across their area. It comes back to a point alluded to early in part 1, namely that the shape of the objective needs to be maintained to an accuracy of a few tens of nanometres whatever the orientation of the instrument. It is mechanics that dictated that large 20^{th} century telescopes would be reflectors and not refractors.

There was one further ingredient in the 20^{th} century story that took some half-century to evolve from concept to practicality, namely astrophotography.

Astrophotography

Fizeau and Foucault took the first astrophotograph, a daguerreotype of the Sun, in 1845. For about three decades astrophotography concentrated on the Sun, mainly because of the slow speed of photographic emulsions. Several important aspects of the Sun, such as the nature of its prominences, were solved with solar photography.

Astrophotography showing star fields didn't really get going until the 1880s. David Gill in South Africa, one of the keen exponents of astrophotography, found that surprisingly good pictures could be taken by piggy-backing the camera of the day onto his telescope. He and Admiral Mouchez, Director of the Paris Observatory, were key in organizing the first ever big multi-observatory, multi-national, astronomical project: a photographic survey known as the *Carte du Ciel,* a catalogue (never fully completed) that would contain over 10 million stars down to 14^{th} magnitude using specially designed astrographic telescopes. Fig. 20 illustrates that they were double instruments, one of which carried the photographic plate and the other allowed the observer to monitor the tracking and make fine corrections to keep the field of view rock steady for perhaps 45 minutes of exposure. Howard Grubb made the instruments for about half the 20 participating observatories. Almost all the others were made by Paul & Prosper Henry of Paris. The project begun in earnest in 1887 and lasted decades.

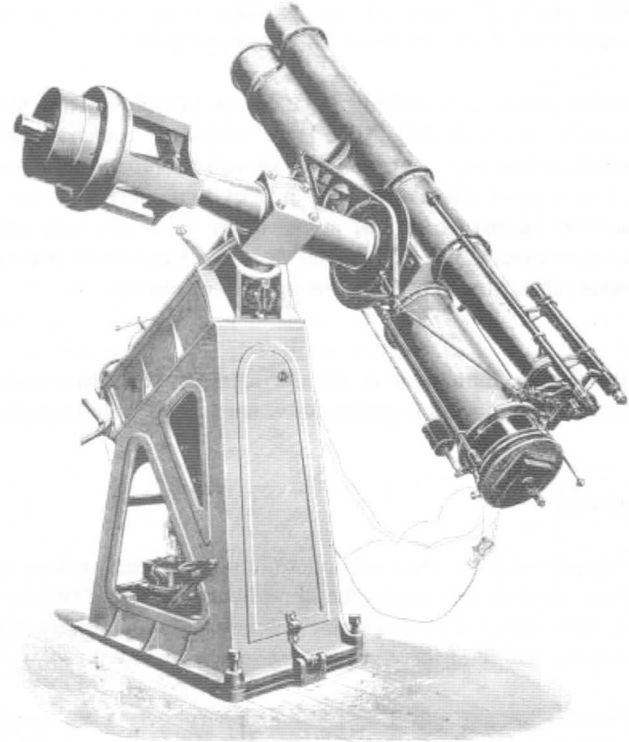


Fig. 20. The astrographic telescope with doubled optics, one set for the camera and a second for visual monitoring.

Some 20th century highlights

Commercial instrument makers at the beginning of the 19th century were want to describe their products as 'perfected', the implication being that you weren't going to purchase ones much better or, at best, only minor improvements were likely to be made in the future. How wrong they were in respect of telescopes. Early 19th century instruments were small, frequently mechanically flimsy, had no sidereal drive and the opticians had no concept of diffraction limited resolution. Much was improved over the 19th century but far more was to come in the 20th century. We would see *telescopes up to 10 m diameter; the best accessible sites in the world utilized; new optical configurations exploiting new materials, segmented mirrors, active optics and adaptive optics.*

Stellar spectrographs would come of age and other ancillary techniques such as polarimetry and time resolved imaging would be developed almost from scratch; computer control would be used for positioning, mirror shape adjustment, electronic imaging and image processing; telescopes would be launched into space and multi-national ventures would be the norm in professional astronomy.

The Hooker Telescope

From 1917 to 1948, the Hooker telescope on Mount Wilson was the world's largest telescope with a 100 inch (2.5 m) diameter mirror. It has had many highlights in almost a century of operation. One of them was the work of Hubble, Slipher and others that made clear in the 1920s the expansion of the universe. The telescope was awarded world landmark status by the American Society of Mechanical Engineers and Fig. 21 highlights very clearly that mechanics are no less important for a telescope than its optics.

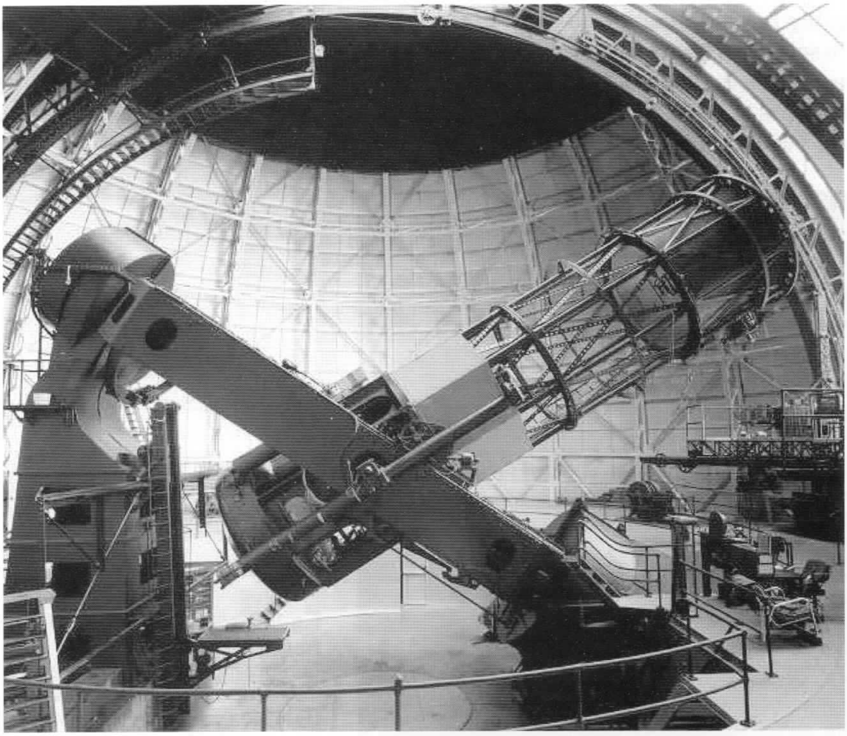


Fig 21. The 100 inch Hooker telescope on Mount Wilson.

Mount Palomar 200 inch Hale telescope

Conceived and planned in the late 1920s, the successor for the mantle of 'world's largest' optical telescope took 20 years to see first light. It was the first telescope where an observer could operate a camera at the prime focus - no easy job sitting exposed to severe cold at the top of a mountain in an open cage. Apparently electrically heated trousers of the kind worn by aviators in open-cockpit planes were the order of the night.

For more than a century, equatorial mounts had been used in all large telescopes and the Hale telescope was no exception. In this design there is a fixed bearing aligned exactly North-South and tilted up parallel to the Earth's rotation axis. Only one rotation of the telescope is needed to follow the movement of a star. In various observatories there are symmetric and asymmetric mounts, with no one preferred design. For the Hale telescope with its mirror of 200 inches (just over 5 m) in diameter the weight of the yoke and telescope is 530 tonnes. Making a tilted bearing for that weight with a low enough friction that smooth motion to a fraction of a second of arc rotation can be obtained was a real challenge. Looking ahead in time, modern control techniques have allowed the new generation of large telescopes to be alt/az mounted, with the vertical weight being mainly taken by one large bearing in a horizontal plane. Modern telescopes look different.

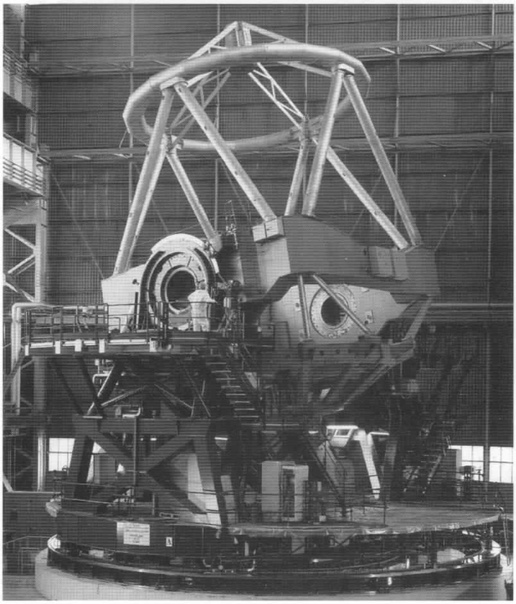


Fig. 22. One of the group of four 8.2 m telescopes composing the Very Large Telescope in Chile run by ESO (European Southern Observatory). A technician is on the platform near picture centre.

The VLT

The Very Large Telescope consists of four inter-connected 8.2 m dishes of which one is shown in Fig. 22. The collecting area of one 8.2 m dish is about a million times greater than that of a dark adapted eyeball. We have two eyeballs but the VLT has four dishes; moreover, they can point independently in different directions. The VLT is the flagship instrument of the European Southern Observatory, located in the Atacama Desert of northern Chile. The size of the instrument is indicated by the technician near the centre of the picture.

The best sites

Long gone are the days when London, Paris, Bonn, Vienna, St Petersburg were suitable for an astronomical observatory. Even the next generation of sites like the Pic-du-Midi and Mount Hamilton are not the best in the world. The very best sites, with minimal cloud cover, steady seeing and minimal atmospheric interference are high and dry. The energetic and perceptive Scottish astronomer Piazzi Smyth appreciated this in the 1850s, proving his point by taking an 18 cm equatorial to the well-named Alta Vista site in Tenerife, 3250 m above sea level. There are comparatively few excellent sites in the world that are feasibly accessible and they have each been accumulating a suite of telescopes, not least because of the capital cost of installing services and facilities in otherwise uninhabited places. For example, the top of Mauna Kea in Hawaii houses an international array of at least 10 telescopes, including the two 10 m diameter Keck instruments.

New Optics

What has made improved optical configurations possible has been the ability to make aspheric components to phenomenal accuracy. Surfaces accurate to 1/20th of the wavelength of light are expected nowadays, i.e. to a tolerance of about 25 nanometres. As an alternative to the Gregorian system mentioned in part 1, some 10 years later Cassegrain proposed a variant with a convex secondary mirror instead of a concave mirror. The main effect of this is to produce a shorter instrument, which one would have thought better but Cassegrain's idea was strongly criticised by Newton as a deeply flawed concept and dumped into obscurity as a result. In fact it was Newton's criticism that was flawed but the damage had been done. Cassegrain has been rescued from obscurity and most large modern telescopes are based to some extent on Cassegrain's basic design.

The Hale telescope is a true Cassegrain design, with parabolic primary and, if used, hyperbolic secondaries. Theory tells us that a parabolic mirror produces an ideal image for an on-axis object at infinity, which is true. What happens off-axis? The Hale telescope has a primary of focal length 660 inches (about 17 m). The field of good definition at the primary focus, where most photography is done, is about 1 cm. In reality a corrector lens is usually added in front of the image to enlarge this area. Nonetheless, big telescopes of traditional design typically have an extremely small field of sharp view. 1 cm over 17 m is about 2 minutes of arc, less than a grain of salt held out at arm's length. You would never buy a pair of binoculars that showed only 10 times this field of view sharply. Several 20th century telescope designs give a much bigger field of view.

The Ritchey-Chrétien configuration uses two hyperbolic mirrors to eliminate low-order coma that otherwise stretches out the images of off-axis objects. The design is completely achromatic but does have other residual aberrations. It is also very sensitive to misalignment of components but many of the large professional telescopes use it. The Hubble Space Telescope is one example; others are the VLT telescopes and the twin 8 m Gemini instruments.

Bernhard Schmidt was an astronomical mirror maker of great repute. In 1926 he was persuaded to join in the work of the Bergedorf Observatory near Hamburg and spent much time thinking about how to improve the off-axis performance of telescopes. One source says "*It is on record that he spent much time at this period roaming the woods around Bergedorf and talking to himself. Fortunately for optics, no-one attempted to interfere with these activities.*"

The basic Schmidt telescope has the corrector lens at the centre of curvature of the primary mirror and a photographic plate at the prime focus. There is no secondary mirror. Professional observatories use this design. The Schmidt-Cassegrain is popular with amateur astronomers. It uses a thin aspheric corrector plate just in front of the secondary mirror to almost correct for spherical aberration and reduce other aberrations. The mirrors are spherical. Fig. 23 shows the general layout. The concept is not unlike using a pair of spectacles to correct defects in our own vision, in the sense that the supplementary lens reduces the point image spread at the detector. The corrector plate gives the Schmidt telescope a field of view that can be at least 5 degrees wide, even wider than a typical pair of binoculars, ideal for photographic survey work, galactic imaging, etc. There are some notable Schmidt telescopes, the largest of which is 2 m in diameter.

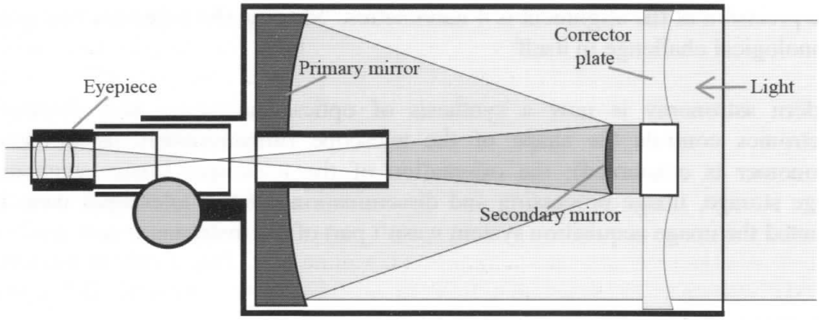


Fig. 23. Schematic diagram, not to scale, of the Schmidt-Cassegrain configuration popular with amateurs and local astronomical societies.

New techniques

Accompanying new optical designs in the 20th century have been new fabrication techniques. These included vacuum coated reflecting surfaces and, comparatively recently, the use of low expansion ceramic instead of glass as a substrate.

Single mirrors have been made up to about 8 m in diameter. To cut down on weight, larger mirrors have a rib structure. Even so, the 200 inch Hale mirror weighs about 13 tons. Stand it up on its edge and it will deform into an ellipsoidal shape or something more complicated in detail; turn it round through 90° and the ellipsoid changes. Active correction for natural distortions as the telescope is moved is a necessity and this was done in the Hale telescope by a system of struts and moving weights behind the ribs that compensated for the changing strain induced by the mirror's own weight. In today's large telescopes, the connected sections are moved by computer-controlled actuators and the arrangement is known as *active optics*.

An example of how sensitive mirrors are to mounting was given a century earlier by Lord Rosse's speculum reflector. It was 6 feet in diameter, 6 inches thick, 4 tons of material about as rigid as wrought iron. Yet Rosse reported that the strong pressure of a man's hand at the rear was enough to noticeably distort a stellar image.

The largest mirrors are truly modular. For example, the 10 m Keck mirrors are physically made in a mosaic of 36 separate 1.8 m segments of a single parabolic surface, kept in place by an electronic alignment and control system.

The precision of the alignment is 4 nanometres. Shaping the segments was quite a technological challenge in itself.

Modern astronomy is now a synthesis of optics, mechanics and electronics. Electronics controls the shape of the telescope (unconsciously as far as the astronomer is concerned); the orientation of the telescope; image acquisition; image storage, image processing and dissemination. When telescopes were first invented the image acquisition system wasn't part of the instrument; now it is.

Adaptive optics

'De-shimmering images', 'taking out the twinkle', 'straightening starlight', call it what you like but adaptive optics is arguably the single most important innovation in telescope design in the last few decades. Stellar images in large telescopes don't twinkle like they do in our eyes because the large telescope samples many turbulent cells in the atmosphere, which smoothes out the twinkles but not the spread of light over the image field. Our eye samples just one turbulent cell at any height.

Adaptive optics is a system of compensating for the effects of atmospheric turbulence. Within the telescope, image forming light is reflected from a nominally plane mirror that is composed of many slightly deformable elements (typically 200 – 2000). These elements can be rapidly changed by small amounts, just a few microns, but enough to provide the opposite misdirection to that introduced by the turbulent atmosphere. The system uses a reference star (or an equivalent stimulated emission in the upper atmosphere generated by a laser beam) to monitor the twinkle and hence control the whole image. It works best for longer wavelength images, IR in particular, and for telescopes with a narrow field of view where the light is essentially traversing the same part of the atmosphere in the same direction across the field of view. Adaptive optics was initiated by the military in the late 70s but astronomers have been taking it forward over the last 3 decades to the extent that ground-based telescopes are producing pictures of comparable resolution to those from the Hubble Space Telescope. Indeed, the expense of working in space and the effectiveness of adaptive optics have swung the balance back in favour of large ground-based instruments. The twin 8.4 m mirrors of the Large Binocular Telescope in Arizona and the 10.4 m Gran Telescopio Canarias are two examples that have seen first light at the end of our 400 year survey period. Much larger telescopes are planned for the future.

Stellar spectroscopy

I would hazard a guess that spectroscopy has contributed more to astronomical knowledge than imaging, though of course it's not a question of one or the other. Two of the world's largest new telescopes (the effective 9.2 m Hobby-Eberly in Texas and the 11 m SALT in South Africa) have been built specially for spectroscopy. I can only plead by way of an apology for giving spectroscopy one short section that I'm treating the spectroscope as an attachment to a telescope. My one example citing how spectroscopy has evolved is taken from the Anglo-Australian Schmidt telescope, which has been one of the most productive telescopes anywhere. Spectroscopy used to be done on this telescope with an objective prism, 1.2 m in diameter and prism angle 44 minutes of arc, that was placed in front of the corrector plate. Each image was spread out into a low resolution spectrum on the image plate but a spectrum that could be contaminated by other images or background that it covered.

Fig. 24. Example of late 20th century automated spectrographic pick-up where light fibres placed over selected images in the focal surface transport the image forming light to a remote spectrograph for digital spectral analysis and recording. Courtesy AAO.

The modern development is highlighted in Fig. 24. A robotic arm places fibre-optic pick-ups at places on the image surface where spectra are wanted. The stellar or galactic image light is then conducted away to a fixed medium resolution spectrograph whose output is picked up by CCD and displayed on a computer screen. By the end of the 20th century, this system had 200 fibres and the resulting 'image' in the spectrograph displayed 200 clean spectra. A larger system for the Anglo-Australian Schmidt was developed this century and a decade later, new builds elsewhere are aiming for more than a thousand simultaneously collected spectra.

The Hubble Space Telescope

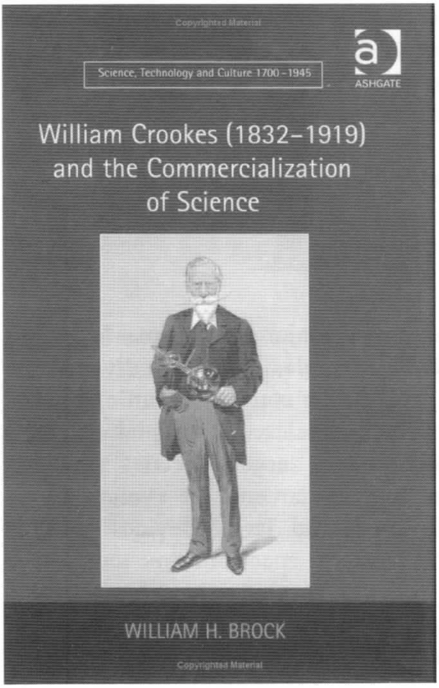
No coverage of 400 years of the telescope should omit a mention of the Hubble Space Telescope, simply known as the HST, a joint NASA and ESA mission. Ask the 'man or woman in the street' to name a famous telescope and my guess is that the Hubble Space Telescope would top the list or at least gain instant recognition by many when mentioned. Few professionals would argue with the verdict that it has been one of the most important observatories in the history of astronomy.

Space-based telescopes moved from the realm of dreams to possibility with the development of rocket power in World War II. Lyman Spitzer (after whom the Spitzer IR space telescope is named) is credited with pushing the concept from the 1940s. Small space telescopes were launched in the 1960s. After a not unfamiliar on-off-on funding scenario for the development of a multi-purpose large instrument, polishing the Hubble main mirror was begun in May 1979. One of the advantages of working in space is that the telescope would cover some of the UV as well as the visible and the near IR. The shorter wavelength of the UV required a mirror polished to an accuracy of 10 nm. For various reasons, including the Challenger disaster, the HST wasn't finally launched into a nearly circular low-earth orbit until April 1990. The Hubble telescope remains the only astronaut serviced space telescope. Had it not been so, then few people would now have heard of it because an error in testing the main mirror resulted in it having a point spread function of about 10 times its design intent, making it no better than a terrestrial telescope. It's an interesting story of how a successful corrective optics module was installed at the first servicing mission in 1993 at the expense of one of the instruments but that subsequently replacement instrument parts included their own corrective optics and the 1993 corrective module has now been taken off. A replacement instrument, a UV spectrograph, has been slotted into the vacant space. The 4th and final servicing mission in May 2009 has given the telescope another 5 years of planned life. The HST teams have been exemplary in releasing images and explanatory context to the public, so much so that had it not been for public support around the world, the 4th servicing mission would most likely not have gone ahead. The public have paid, but they feel they have got their money's worth and there is a lesson here for all of big science.

In fine

This two-part article has attempted to cover major developments of the astronomical telescope, illustrating trends by examples. Undoubtedly, another author would have varied the emphasis and chosen some different examples. There are a lot to choose from. A modern research telescope is a platform for a raft of associated instruments whose performance largely dictates the work done by the telescope. My overview has scarcely covered the evolution over the centuries of ancillary devices both small and large, devices such as eyepiece micrometers, photometers, polarimeters, spectrometers and image recording devices, and specialisms such as zenith sectors, solar telescopes and interferometry. I've ignored the expansion of the concept to other electromagnetic wavelengths, from γ and X-rays down to radio waves, a spectral range that includes three of NASA's four Great Observatories, the 4th being the HST. I have not looked at 21st century developments that are certain to bring larger telescopes, even more impressive survey instruments, imaging of extra-solar planets (here with us already as dots, at least) and, I would bet, telescopes on the Moon (is there anywhere better for a large robotic instrument?). Nevertheless, in 400 years to date, the telescope has evolved from an apparently simple device of two lenses in pasteboard tubes that with a bit of skill one could make at home to one of the most sophisticated, precise and technologically challenging devices on the planet, and indeed off it. The International Year of Astronomy has been a good time to look back on this evolution and marvel at what has been achieved. Our understanding of the Universe has been transformed by this one instrument.

Book Reviews



William Crookes (1832-1919) and the Commercialization of Science

William H Brock

Ashgate publishing, Aldershot, 2008

ISBN *978-0754663225*

586pp *Hardback £65*

Reviewed by Prof. Denis Weaire
Trinity College Dublin

By the end of his long career William Crookes had accumulated an impressive list of honours: the Order of Merit, a knighthood, Presidency of the Royal Society and other learned and professional societies, membership of foreign academies…

He did not attain the level of these high distinctions by the conventional route, which might have begun with Wranglership at Cambridge and a prestigious chair. Rather he began with a practical training in chemistry, and set out to apply it to the new technology of photography. In addition he became and editor and proprietor of magazines.

One pictures him at home in his laboratory by day and in his study by night, losing no time in his struggle to succeed in that enterprising Victorian age, which gave full measure of reward and despair to success and failure respectively.

He never forsook his early devotion to journalism - it often placed him at the centre of speculation and controversy. Eventually his writing expanded into to authorship. While most of his books were collated or translated (from German) rather than fully original works, they played an important role in the international transfer of new knowledge and technique.

As photography developed he might well have stuck with it and made a comfortable living. Instead he moved on opportunistically into an extraordinary variety of fields in which a fortune or a reputation might be made, *en route* to the top.

In all of these he combined entrepreneurship, assiduous experimentation, instrument design, speculative enquiry and public commentary ~~to~~ great effect, spanning chemistry, physics and engineering.

All this has been painstakingly recounted by William Brock. He is not the first to do so. Fournier d'Albe, who in many ways resembled Crookes himself, was commissioned as his biographer. His *Life of Sir William Crookes* is as hagiographic as might be expected from a respectful contemporary, and a more perceptive modern treatment is very welcome.

Crookes is usually remembered for the discovery of Thallium, the invention of the radiometer and the demonstration of cathode rays with the Crookes' tube. While these are only a few of his achievements, they are appropriate enough as a sample. Each required long hours of patient development on the laboratory, each caused a) public sensation, each gave scope for his rhetoric and his profit. That his little radiometer (a miniature version of which he wore in his lapel) is still sold to children is also an appropriate memorial to his commercial ambitions.

Across this remarkable career lay two shadows. Firstly, he never won an academic appointment in his early years, and eventually gave up applying for them; perhaps this was no bad thing in the end, but no doubt he resented it. Secondly, his speculative spirit and interest in the mysterious forces that drove his radiometer lead him into the trap of spiritualism.

He was not alone in falling for the charms of beguiling young ladies in darkened parlours but he was particularly vociferous in their defence. In the end he renounced but he never denounced spiritualism.

Here perhaps Brock gives rather to much rein to a fair-minded statement of the case both for and against spiritualism (or at least its Victorian manifestations). Surely it was (at best!) a sad and silly delusion, and one should make no bones about it today?

As for the (real) science, he recounts the complex and confused processes of scientific discovery in considerable detail, and there are times one wishes that he would cut to the chase by stating the physics behind the phenomena. His method makes for suspense, but some readers may feel the need for a little more education, along the way.

He makes many references to the difficult personality of his subject, without trying to get underneath it, as is fashionable today. Crookes was egotistical and somewhat exploitative (but, dare we venture, *who is not* among leading scientists?). His competitive urge often led him into antagonistic behaviour, and his editorials made this public. The resulting feuds usually blew over eventually.

In trying to understand this, one is drawn to the frank remarks of Oliver Lodge in the Foreword to the original biography:

Crookes was not a learned man in the professional sense.

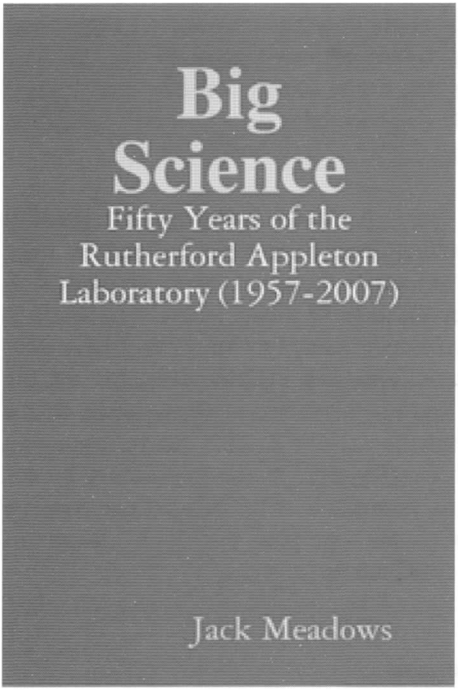
His personality was not specially impressive. In his presence one did not feel the worshipful enthusiasm which some of the great men of science have aroused.

And later he says...

... he was even accused of being practically insane on one side of his brain, while sane enough on the other...

Could it be that Crookes suffered from what today is called a "bipolar" disorder", causing mood swings from aggressive self-assertion to depression? Perhaps that is a common concomitant of genius?

Perhaps Brock fails to "get under the skin" of his elusive subject, but in all other respects his work is a great success, deserving of a prominent place on the shelf of major biographies of the great figures of Victorian science.



Big Science. 50 years of the Rutherford Appleton Laboratory (1957 - 2007)

Jack Meadows

Lulu, USA *2009*

ISBN *978-1-4092-5562-8*

126pp *Softback, £9.95*

Reviewed by Kate Crennell

The Rutherford Appleton Laboratory is a service organisation funded by the UK government to construct and maintain large scale scientific facilities for use primarily by UK academic scientists.

In 2009 it is in the parish of Chilton, Oxfordshire, on the flat land in the North Wessex area of Outstanding Natural Beauty, just North of the Ridgway National Trail. The area has been in constant agricultural use since Roman times, mainly grazing sheep and training racehorses. Other ancient tracks cross the area, the Icknield way runs to the North, the old Hungerford Road on the West was used by William of Orange on his way to accept the throne of England in 1688. When the Laboratory was first set up in 1957 it was in the County of Berkshire, with the re-organisation of County Boundaries in 1974 it found itself in Oxfordshire. These changes are merely some of the management problems with which the Laboratory has had to cope during the last 50 years including changes in laws concerning waste water and archaeology. In 2009 before new buildings are constructed excavations have to be carefully carried out to avoid damage to any archaeological relics, similar to those thought to be of a Roman and his dog found during construction of a Car Park in about 2004. Water running off the roof of a new building has to be provided with soakaways within the site.

This book covers the period 1957 to 2007. It has a four page introduction followed by 6 chapters, a list of Directors/Chief Executives and a four page (double column) index. The chapters describe the main areas of science in which the Laboratory has worked over the last 50 years: *Chasing particles, Neutrons and photons, Atmosphere and Earth, Looking Outwards, Computing at Chilton* and *An evolving organisation*. This last chapter describes all the changes in the name of the laboratory and all the different departments of the UK Government responsible for its funding since 1957. This can be confusing for the reader so the author decided to usually refer to the Laboratory by the name 'Chilton' the site where it was built, regardless of the official name in use at that time. There are no diagrams or photographs, due to a change of management, see 'Notes on this publication' below.

The Beginning, Funding and Management

During World War II the part of the site in the parish of Harwell was an RAF airfield, shortly afterwards it became the Atomic Energy Research Establishment (AERE) concerned with research into both weapons and reactors for peaceful purposes. Tight security was maintained around the site to protect the national weapons development, but it was also important to study the fundamental properties of particles and materials being used in reactor construction. About 1956 it was realised that a Research Laboratory was needed for academic nuclear research outside the wire fence of the AERE. The Chilton site was chosen because, being near to AERE, it could use some of their existing facilities, including medical care, building heating and payment of salaries. Employees were not Civil Servants but had similar grades and salaries and were managed by a different Government agency, the DSIR (see table below) which already managed Research Councils for Agriculture (ARC) and medicine (MRC), a third one was added, SRC, the Science Research Council, with three Boards, Nuclear Physics, Astronomy Space and Radio, Science and Technology, which covered the changing areas of Science on the Chilton site.

Changing Science, what was Chilton used for?

Originally the research was on elementary particles, later it was found neutrons could be used to study the structure of materials and that they were able to show the positions of hydrogen atoms in proteins and viruses for biological research so the Chilton accelerator was modified. Other research was done on muons and neutrinos. Research expanded into other scientific areas: the study and use of large lasers, astronomy and space science. Technology and computing services were needed to construct equipment and analyse data; separate service departments were needed for these.

This book is not intended as an academic work for historians or physicists, it is mainly about the management and problems of running such a Laboratory. Personally I find it frustrating because there are few details of the science or the people doing it. For example he states that 19 truck loads were needed to deliver the Atlas computer in 1964, but not what was innovative about it compared with other computers at that time. Computer input/output is not mentioned, nor the work of Atlas computing staff in supporting research. On page 50 he describes the Ariel satellites but does not mention the software written by Atlas staff for Ariel data collection. On page 80 he mentions the Nautical Almanac and management problems of moving it to Chilton, but not the fact that Chilton staff had written software to output the Nautical Almanac some years before on a high resolution device, the FR80 microfilm recorder with many uses but apparently missed by this author. This probably gave rise to the idea that the computing production work for the Nautical Almanac could be done in Chilton.

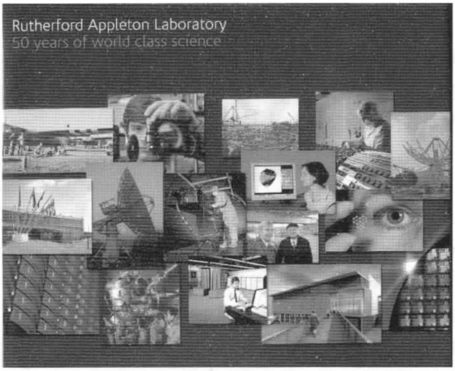
In 1981, as more work was done for engineering, the name was changed to SERC (see table below). At the same time the Government decide to try to save funds by moving the Appleton Laboratory, (founded in the 1920s to study radio propagation at Ditton Park, Slough, near Heathrow Airport) to the Chilton Site.

In 2006 it was decided to separate the large facilities funding from that for science in the Universities so the Chilton site, together with large telescopes and international collaborations is funded by the STFC. The international collaborations fund CERN, in Switzerland for nuclear science, ESA, the European Space Agency and others not mentioned in this book, and are paid in the currency of the country where the headquarters of the organisation are sited, thus currency fluctuations affect the amount of UK funds required.

Jack Meadows, the author of this book, is a member of the Information Science Department at Loughborough University. Toward the end of 2005 he was commissioned to write a history of the Rutherford Appleton Laboratory for its 50th anniversary in 2007. The text was written and the book was in the proof stage when the UK Government decided on yet another change of management; so the laboratory became the responsibility of the Science and Technology Facilities Council who decided their remit was to support the scientific facilities for future research and declined to fund work on history books, so there are no drawings or photographs in the book as these were to have been provided by Chilton before the change in management. (I am surprised that someone from an Information Science Department did not have access to word processing software to produce a simple table such as that I have made as an appendix.) Administration papers for Government Organisations are supposed to be sent to the National Archives in Kew, so Laboratory management takes no interest in history or archives of any sort. The copyright of the text of this book reverted to the author, who decided to publish it himself using an 'on-demand' publisher. It can be ordered either direct from the publisher via their website at www.lulu.com or via www.Amazon.com. I had trouble finding it at Amazon.co.uk.

Date	Name	UK Government Management
1956	NIRNS National Institute for Research in Nuclear Science	AEA Atomic Energy Authority
1957 - 1969	RHEL Rutherford High Energy Laboratory	DSIR Department of Scientific and Industrial Research
1969 - 1979	Rutherford Laboratory name changed to reflect other sciences and amalgamation of the Atlas Centre	SRC Science Research Council Boards ARC, MRC, SRC
1979 - 1981	Rutherford and Appleton Laboratories amalgamation of Appleton Laboratory	
1981 - 1994	Rutherford Appleton Laboratory	SERC
1994 - 1995	Daresbury and Rutherford Appleton Laboratories	Science and Engineering Research Council
1995 - 2006	Central Laboratory of the Research Councills. CLRC Rutherford Appleton Laboratory	CCLRC Council for the Central Laboratory of the Research Councils
2006 - 2007	Rutherford Appleton Laboratory	STFC Science and Technology Facilities Council

Appendix: Time Line of Laboratory Names and Management



Rutherford Appleton Laboratory: 50 years of world class science

Editor: Karen Lynn Whitaker

Science and Technology Facilities Council 2007

ISBN 978-0-95566-16-3-1

p126, hardback

Reviewed by Kate Crennell

This is a 'coffee table' book published as a glossy record of some of the laboratory work and given to scientists invited to a celebratory day in 2007; and to other interested scientists. It is mostly colour photographs although the earlier ones are black and white. The eldest is a photograph of Sir John Cockcroft cutting the first sod in 1957, watched by an admiring crowd of men all wearing suits with white shirts and ties, very different from today's informal jeans and tee shirts. Other photographs are of visiting politicians, Lord Hailsham in 1957 at the Laboratory opening, and Margaret Thatcher in 1972 when Minister of state for Education and Science. Many photographs show scientific equipment under construction or in use; they are mostly taken within the last 15 years, not spread evenly over the 50 years of operation, with the exception of those on pages 124, 125 showing the evolving landscape from the air between 1957 and 2006. You have no idea how much the equipment costs, how many people were involved, how long it took to construct or how much electricity it uses. For those who worked at the laboratory, there is some amusement in looking for photographs of former colleagues to see how they have aged.

There are six sections, corresponding roughly to the Laboratory Divisions: Particle physics, Technology, Computing, ISIS, Lasers and Space Science with a foreword by Andrew Taylor, head of ISIS. Each section is complete in itself and begins with a useful summary page printed in white text on a grey background, facing a coloured image relevant to the work of the section. Within each section photographs are ordered chronologically. There is little technical detail or results of experiments just a few highly coloured images with a small amount of explanatory text as a figure caption.

Comments on book production:
This book is not intended as an academic work, there is no contents page, no index or bibliography. The authors are anonymous, although an editor can be found on the glossy paper dust cover which carries an ISBN number.

Summary:
Neither of these books gives a good idea of the exciting science done at the Rutherford Appleton Laboratory over the last 50 years nor gives any suggestions for further reading, either books or websites. The first one is only text and describes thoroughly mainly administration and management problems; the second has a few photographs taken in the late 1950s but the rest are from the last 15 years. I know there were many interesting photographs before then because I computerised the first index to them. For example a view of the old Drawing Office could have been usefully compared with today's CAD systems used in Technology Department.

Some Websites associated with the Rutherford Appleton Laboratory in 2009

These are most easily found by starting from the Science and Technology Facilities Home Page at: **www.scitech.ac.uk** and then click on '*Research facilities*' which should take you to a page with several links to *science at the Chilton site*:

The Central Laser Facility at: **www.clf.rl.ac.uk**

ISIS Pulsed Neutron & Muon Source at: **www.isis.rl.ac.uk**

Molecular Spectroscopy Facility at: **www.msf.rl.ac.uk**

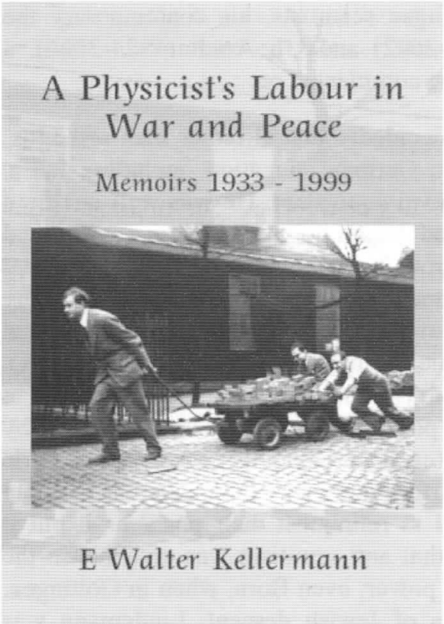
Diamond Light Source at: **www.diamond.ac.uk**

CERN - European Organization for Nuclear Research at:
public.web.cern.ch/public/

European Space Agency, the ESA Portal is at:
www.esa.int/esaCP/index.html

Institut Laue Langevin in Grenoble is at: **www.ill.fr**

Refugee scientists: Kellerman, colleagues and contemporaries



A Physicist's Labour in War and Peace: a memoir 1933-1999

E Walter Kellerman

Stamford House Publishers, 2004

ISBN 1-904985-09-2

242pp

Personal X-ray reflections

Uli Arndt

Athena Press, London, 2006

ISBN 978-1844016945

200pp

Reviewed by Prof. Derry W Jones
Chemical and Forensic Sciences, University of Bradford

In a mountainous country one can often gain the best vista of the highest peaks from a vantage point on a somewhat lower nearby summit. Similarly, a talented and thoughtful scientist who has interacted with some of the most distinguished scientists can often provide an interesting perspective of those at or near the Nobel level. In the 1930s, Walter Kellerman (born 1915) heard lectures from Walther Nernst in Berlin, and, in Vienna, studied under H Thirring and Hermann Mark ('brilliant lectures, crowded out by chemistry and physics students') and carried out semi-conductor research with Karl Przibram. In the UK he was on the research or academic staff of Max Born in Edinburgh, Patrick Blackett (with colleagues including C C Butler, L Janossy, ACB Lovell and GD Rochester) in Manchester, and John G Wilson in Leeds. I should declare an interest in that Kellerman demonstrated to our year at Manchester and we had lectures from his boss and colleagues; further in two spells at

Leeds, I researched in the adjacent department and so interacted with several of Kellerman's colleagues. Some comparisons can be made between the careers of Kellerman and those of two other refugee scientists, his contemporary the Nobel prizewinner Max Perutz (1914-2002) and Uli Arndt (1924-2006), a decade younger, whose biography will be commented on.

Although his father had been a rabbi, decidedly liberal rather than orthodox, Kellerman received a broad humanistic German education, including Latin, Greek, mathematics and physics to O-level equivalent. He was well qualified to enter Berlin University in 1933 and began to attend Nernst's classical physics lectures until ejected on racial grounds by the university's new political officer, a uniformed SS man. Kellerman admired the attempts made by established UK scientists (including Neville Mott) through the International (later World) Student Service, Academic Assistance Council (now CAR A), and Jewish Refugees' Committee, to advise and support refugee scholars in Britain at a time of paucity of academic posts. The AAC was set up in 1933 by William Beveridge and Lionel Robbins (each subsequently famed for a historic Report), with the support of Lord Rutherford and many other scientists to help 'men of science who are being obliged to relinquish their posts' as it put it euphemistically. Kellerman also notes that anti-semitism was endemic in the Weimar Republic before Hitler came to power; even Born, when in Göttingen, could not appoint 'too many' assistants of Jewish descent. Lindemann was particularly successful in enlivening Oxford physics by transplanting refugee low-temperature experts £fom Breslau, as is well described in chapter 9 of Jack Morrell's Science at Oxford 1914-1939. Kellerman (whose widowed mother had only a small pension) had aspirations already in 1933 for Cambridge but his Uncle Julius, in temporary safety in Saarbrucken, could offer sufficient support only for Vienna. As pro-Anschluss demonstrations increased, Kellerman left Vienna, with a Dr Phil obtained (by researching after only three semesters as a student) in the shortest time allowed, in September 1937. He applied through the I(W)SS to work with Max Born in Cambridge on theoretical physics, a field recognized more slowly in the U K than on the continent (Dirac and R H Fowler took chairs in mathematics). Thus he scored a near miss on Cambridge as Born, aged 54, had recently left for a chair at Edinburgh.

In Edinburgh, Born was Tait Professor of Natural Philosophy, Head of Department of Applied Mathematics, later with '(Mathematical Physics)' added, and housed in a basement. His Nobel came late, in 1954, but Kellerman found him generous in acknowledging the contributions of others.

He found ET Whittaker, Professor of Mathematics, an outstanding, influential and caring person; the reason for some tutorial teaching arranged for Kellerman to supplement his meagre finances was disguised as 'to improve Kellerman's English'. There were visits from physicists as eminent as Paul Ewald ('an upstanding man, neither Jewish nor communist, who preferred to sacrifice status rather than acquiesce to unsavoury ideas'), HA Kramers, Sommerfeld and Planck. In Whittaker's small seminar room, a large kettle on a roaring fire was timed to boil after 50 min and prompt the lecturer to engage in congenial informal discussion. Kellerman's immediate research colleague was the Marxist (though not then active) Klaus Fuchs, son of a pastor, who had been helped by the AAC and is pictured as a capable mathematician with an ostentatious disdain for dress conventions and convinced of the superiority of German over British education. Probably Kellerman's greatest individual research achievement in solid-state physics arising because he was not too overawed by Born to detect a mistake in a widely accepted article by the Master, was in the calculation of the lattice frequencies of the phonon spectrum of a real crystal, published under a single author in Trans Roy Soc and nearly awarded a DSc.

Max Perutz was born in Vienna to a Czech father and a Viennese mother in an affluent household with five servants and a place in the country. Although of Jewish origin, the family was non-practising such that at age six Max was baptized a Catholic, from which he later lapsed. Despite his father's preference for him to read law, Perutz studied chemistry from 1932 at Vienna University. He left in 1936 (not, at this time, as a refugee), the year before Kellerman, with the intention of researching for his Vienna degree under the flamboyant physicist J D Bernal at the Cavendish, perhaps the world's centre of physics. This transfer to Cambridge, when Perutz had neither a degree from Vienna (unlike Kellerman) nor any knowledge of crystallography, was achieved thanks to generous financial help from his father and some fortunate chance contacts. He only became formally a refugee when the Germans entered Austria in March, 1938; thus he did not receive help from the Society for the Protection of Science and Learning (as the AAC had become) but he was a supporter. Perutz, a patient, persistent, but ultimately passionate scientist (Crick even used the word plodder), published many autobiographical essays. Georgina Ferry's fine biography Max Perutz and the secret of life (Chatto and Windus, 2007) was reviewed in I 0 P Hist of Phys N/L No 23, 65-68 (Jan 2008).

The talented and perceptive physicist Uli Arndt (1924-2006) was born in Berlin, a decade later than Kellerman and Perutz, of mixed German, Russian and Dutch parentage. Through industrial connections (the father was employed with weighing machine manufacturers Schenck in Darmstadt from 1930), the comfortably off family was able to leave Germany in 1936, the same year that Perutz entered the Cavendish, with most of their possessions. Consequently, Arndt's largely classical education was partly (to O-level equivalent) in Berlin and Darmstadt, early in the Nazi period, and partly in England; his first science lessons came after his School Cert. Neither the father, Ernst, on work 'of national importance' at Averys for the aviation industry, nor young Uli (an enemy alien at 16 in 1940) was interned. Arndt was thus fortunate to be one of rather few people going straight from school to Cambridge and taking a wartime degree in physics. He recalls lectures on quantum mechanics and relativity by Dirac and Eddington, barely aware of their shrinking audiences as the term progressed, and lecture demonstrations by mature academics: Alex Wood whirling round on a piano stool to illustrate angular momentum and G F C Searle (aged 80 in 1944) handing out lengthy practical scripts containing deliberate mathematical mistakes. Adjudged in his 1944 interview with C P Snow to be too much of a risk for radar research, Arndt was again lucky to be able to go to the Cavendish for a project, sponsored by the Electrical Research Association, under Henry Lipson (already met and liked during the Part II year) on alloy crystallography.

Incidentally, the ERA (where I worked in the summer of 1947) sponsored much of the early research of Fröhlich (1905-1991), who had escaped from Germany and Russia via Vienna in 1935. In Vienna, he had briefly assisted the polymer scientist Hermann Mark, who had returned to Austria in1932 because of the regime developing in Germany; Mark had taught Kellerman and Perutz and contributed to Perutz's acceptance by Bernal. After the Anschluss in 1938, Mark emigrated from Austria via Switzerland to Canada and thence to the USA; in1940, he tried to induce Perutz to join Brooklyn Polytechnic when released from internment. In 1942-44, Bernal, Mark and Perutz were all highly involved with the secret Anglo-American-Canadian Habbakuk bergship floating airfield project.

Arndt spent the 1950s and beyond, mainly engaged on X -ray instrumental design and development (often in collaboration with other laboratories), at the Royal Institution, under the headship of E N Da C Andrade and subsequently of W L Bragg. He took a 1957-58 sabbatical at Madison University, Wisconsin.

In 1963, Perutz, who had been collaborating with Arndt, attracted him to the Cambridge M R C Laboratory of Molecular Biology, where the company included Sydney Brenner, Francis Crick and John Kendrew. Arndt's expertise in crystallographic-instrument conception, design and construction - 'finding an elegant solution tomorrow to yesterday's problem' -led to interaction with several instrument manufacturers and with X-ray and neutron scientists at A ERE, Harwell, the Daresbury Synchrotron Lab, the Brookhaven National Laboratory, and the ILL, Grenoble, where he spent a year. He observed more Equality and Fraternity among British than French laboratory staff, compared French professionalism with the occasionally more effective British amateurism, and overall found that he preferred working in small rather than large institutes. Arndt contrived to avoid most international committee service with Nobelist Sanger's ploy 'I'm not very good at that sort of thing'

Both Kellerman and Perutz have written about their relatively brief but harrowing internment experiences, first in a housing estate camp at Huyton and in Douglas boarding houses in the Isle of Man, and then sailing in the troopship Ettrick to Canada. On passage, they knew of the fate of internees in the torpedoed Andorra Star. Kellerman mentions that Arndt's elder brother Heinz (later an economics professor) was largely responsible for drafting a protesting document about conditions to a camp commandant. Herbert Fröhlich noted the irony that he had avoided camps in Russia and Germany only to be interned by the British. In the Sherbrooke camp was Klaus Fuchs, a colleague of Kellerman in Edinburgh, but a political rather than a Jewish refugee, who never forgave the British for their treatment of genuine refugees. Perutz recalls Fuchs as an austere aloof character who gave lucid theoretical physics lectures in the camp. At least Kellerman did hear of the acceptance of his second Royal Society paper while in the camp, though his proof corrections were lost. Also, those few months taught him about democracy and leadership in active politics and how to present cases and achieve a compromise between people of strong independent views, e g between Jewish and political refugees (with some non-genuine refugees also in the camp). Eleanor Rathbone, M P, is commended for her untiring advocacy of the refugees' case. Arndt attributed his facility in scientific discussions to becoming secretary of the college debating society in the 1940s.

On the return to the U K of the priority refugees in early 1941, Fuchs was assigned to secret atomic work, first with Peierls (another refugee physicist) in England and then in the US. He was later revealed as a spy for Russia; apparently, when Fröhlich turned down a position in Theoretical Physics at

Harwell, Fuchs was appointed instead. Kellerman took a temporary lectureship in physics at the then University College of (much bombed) Southampton, which had some first-class academics on its staff, he notes, including N K Adam. To aid the war effort, Southampton, like longer established universities, had expanded its classes for teachers, scientists, electronic engineers, etc (in two-year courses for the London General Degree), plus six-month short courses for RAF cadets, so that there was no opportunity for research. Fire-fighting and fire-watching (with opportunities for animated discussions with academic colleagues) figure in the later wartime recollections of both Kellerman and Arndt, the latter finding the fire brigade remunerative in Emmanuel College. Co-ordinating Civil Defence at U C Southampton was the congenial spectroscopist Harry G Howell, with whom Kellerman shared an office; he later became head of physics at Bradford Institute of Technology and Director of the British Plasterboard Research Association.

As the War came to its end, Kellerman's thoughts (partly influenced by the popular lectures he had been called upon to give following the dropping of the atomic bombs) turned to research in cosmic rays. Prompted by summer-vacation research in Manchester with Blackett's deputy, the likable Hungarian refugee L Janossy, he set up a small-scale cosmic-ray telescope experiment - counters separated by lead layers - to measure the energy spectrum of emerging muons. Janossy was so impressed by these Southampton results and Kellerman was sufficiently keen to join Blackett's Manchester laboratory that he accepted a mere DSIR research assistantship in 1946. Kellerman's research on the nuclear-active particles - hadrons - contained in extensive cosmic-ray air showers was stimulated by the photographic emulsion results of C F Powell - 'warm-hearted, sympathetic, and an attentive listener' - who became a friend. A more extensive array, 50m by 50m, of linked counters was relatively inexpensive in 1949 (with 500 valves) because of the availability still of war-surplus electronics. Blackett is greatly admired for his energetic and passionate approach to physics (and politics) and his brilliant scientific intuition. George D Rochester is remembered warmly as a great experimentalist, kind and modest, but J G Wilson who, under Blackett, oversaw the cosmic ray team, seemed prejudiced against theoretical speculations and was unaware of Kellerman's considerable teaching experience. Incidentally, around this time, the polymath Michael Polanyi, asked Dr Peter Plesch (later Prof at Keele) to enquire whether intense cosmic ray showers, detected in Blackett's laboratories, coincided with unusually rapid polymerizations observed in the Chemistry Department. (Polanyi had qualified as a medical doctor in Hungary and developed X-ray analysis of fibres in Germany before escaping to the chair of physical chemistry at Manchester.)

Unfortunately, perhaps, within a few years of Kellerman joining the staff at Leeds, Wilson left Manchester and soon became head of physics at Leeds. Having introduced cosmic-ray research to Leeds, Kellerman persuaded Blackett in 1956 of the need for a very large scale (10km by 10km) shower experiment in Britain and convinced the community that it should be Leeds-based. Kellerman helped build up the Havrah Park experiment so that it was giving results in the 1960s and closed only in 1987. In the 1970s, what ultimately turned out to be a spurious energy peak (attributed three years later to a computer fault from burnt-out wires) led to Kellerman's tentative announcement of a the discovery of a new particle.

Kellerman's concern for national science policy was triggered by attendance at a wartime British Association conference at the R I essentially on post-war science. As AScW secretary at Southampton, he dealt with the tax inspectorate while still formally an enemy alien. He was soon involved in a science policy committee whose members included future Nobelists John Kendrew and R L M Synge. Their views were published in 1946 in a remarkably comprehensive Penguin Book (I have a copy) Science and the Nation (by A Sc W members but with no names listed apart from Blackett, the President, who wrote the introduction). Kellerman also contributed to an analogous AUT report on post-war universities. Science policy activity continued at Manchester through the A Sc W and W E A; surprisingly, convinced socialists Bernal and Blackett were found to support the existing UGC funding system rather than something akin to a Ministry for Universities. In retirement, Kellerman continued into the 1980s and 1990s to participate in the Fabian Committee for the Arts, Science and the Environment and the Labour Party Group for Finance and Industry (L F I G). These tried to alert people to the parlous state of UK science, to campaign for improvement, and to press for, e g, a Ministry for Science, foresight panels, and a broader school curriculum at sixth-form level. A book U K Science Policy (ed Maurice Goldsmith, Longman, 1984), with introduction by Hermann Bondi, emerged from the L FIG, followed by Kellerman's on Science and Technology in France (Longman, 1988).

Arndt, Perutz , and Kellerman were happily married to women, French, German, and English, accomplished mostly in areas other than science. Arndt pays affectionate tribute to his late wife, a linguist and an Anglican. As well as his wife and children, Perutz had to assist his previously affluent parents who had arrived in Britain with little. Kellerman gives what amounts to a cv of the later career of his wife, who had excelled at skating and the piano before being a French Resistance worker and, in the U K, a language teacher and administrator. Arndt reveals more about family life than Perutz (both were experienced skiers) and much more than Kellerman.

By definition, an autobiographical memoir, or 'an account of interactions with science, scientists and politics', selects material from a single viewpoint. Kellerman was clearly disappointed that research time was wasted chasing a spurious particle, that other groups failed to take up his large ionization calorimeter design, and that late professional recognition did not lead to academic promotion. However he not only made substantial contributions to physics, notably the crystal phonon spectra and founding the Havrah Park cosmic-ray experiment, but also made many shrewd contributions to discussions about future U K science policy and education

Kellerman expended some of his energy at Leeds in supporting the Liberal and Reform Jewish synagogue, thus being refused contact with orthodox family members in Israel. All three physicists were enthusiastic (although non-cricket-playing) anglophiles before and after naturalization; gentle in character, they would subscribe to Perutz's book title Science is not a quiet life. Arndt's enjoyable, good-humoured anecdotal memoir contains less detailed physics than Kellerman's and reveals more about domestic affairs, recalling sailing, skiing, travelling and remembered books.

In view of the many thumb-nail sketches of well-known physicists, it is a pity that neither Arndt's nor Kellerman's book contains an index or formal footnotes; a date summary of the author's career would have been helpful. Kellerman includes a generally useful glossary of terms, although the entry for Legendre polynomials, for example, 'polynomials satisfying Legendre's equation' may be of limited value to the lay reader. The contribution to science of refugee scholars is well recognised, even if remembered chiefly through Nobel prizewinners like Perutz. However the point is made equally by the careers of physicists somewhat below this level as illustrated by the stories of the dryly humorous Arndt (conscious of being at the right place at the right time) and the more serious Kellerman, who, despite some regrets, enjoyed a fulfilling scientific life. Both Arndt and Kellerman have compiled engaging and enjoyable memoirs which are recommended.

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