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Contents

Editorial		2	
Obituaries			
	Jeff Hughes	<i>by James Sumner</i>	3
	Peter Holmberg	<i>by Karl Grandin</i>	7
Meeting Reports			
	Magnetic Resonance Imaging and its History		9
	Open Meeting: “From Newton to the Free Electron Laser”		13
	International HoP conference, San Sebastian, Spain		15
News & Comment			17
Features			
	Open Meeting - From Newton to the Free Electron Laser: The paradigm shift of physics-religion-unbelief relationship from Renaissance to the 21 st century	<i>by Elisabetta Canetta</i>	21
	Psychical and Colour research between Lord Rayleigh’s “Naturalism” and “Dualism”	<i>by Gregory Bridgman,</i>	30
	Robert Pohl interview	<i>by Colin Hempstead</i>	48
	A partial history of cosmic ray research in the UK	<i>by Alan Watson</i>	59
Committee and contacts			67

Editorial

A question of memory?

"It's good to have a past". Someone said that to me recently meaning that one could feel closer, be closer, to friends - in the present - with whom one has shared memories of the past. Or to put it in more colloquial terms 'do you remember when we...' This joint recollection often brings a greater appreciation of those events which occurred long ago. And this recall is frequently accompanied by the observation of how things have changed over the years.

Well, you may say that's a fairly common experience, but I wonder - do we dismiss such experiences without giving enough weight to this enrichment? Do we set these memories in context?

Let us consider: how often do things change because of recalling, assessing; do they improve? Do we build on past successes? Of course I am talking here about personal memories, but could this be extended to the memories of others? Yes, I think so - to a limited extent but what if they are not just memories but the uncovering of the memories, events, thoughts and work of others - does this not also bring us closer to those who had them? Perhaps, if they are set out in detail, formalised accounts of experience - even of ideas and experimentation. Again, the understanding of these accounts is very dependent on the context in which they were cast.

All very interesting but it would surely be much more significant if one could change, improve and build on those accounts. Of course that would require an intimate knowledge of the subject which would be greatly enriched by an intimate appreciation of the context - its background and development over the years.

So, yes, it's good to have a past.

Malcolm Cooper
Editor

In memoriam

Jeff Hughes, 1965-2018



With the death of Jeff Hughes, at the age of just 52, the international history of science and technology community has lost a much-loved friend and colleague and one of its finest research pathfinders and mentors.

Initially trained in chemistry at Oxford, Jeff was recruited early into the field, completing his PhD at the Department of History and Philosophy of Science at Cambridge in 1993. This focused on what he called the “radioactivists” – the early radioactivity researchers whose ideas and experimental practices shaped the emergence of atomic physics as a discipline – and particularly the community which formed around Ernest Rutherford, at Manchester and then Cambridge, and its relations with researchers in Vienna and elsewhere. Reflecting the priorities of his own research group, Jeff moved beyond simple descriptive history to consider broader points about how science works, addressing controversy over the replicability of the Rutherford group’s experiments, the suppression of uncertainties over the validity of scintillation experiments during the search for alternative methods, and the practical role of selective narratives in securing community consensus.

This project defined an approach that was reflected in many of Jeff's later publications. His biographical interest in the mass spectrograph pioneer Francis Aston, for instance, inspired an examination of the role of Nobel Prizes in setting the scientific agenda: the award of prizes to Aston and Frederick Soddy, he found, was an important step in stabilising and promoting the validity of the isotope concept. Another article challenged nostalgic visions of a pre-1940s university physics based on benchtop-scale experiments and thrifty, "sealing wax and string" resourcefulness: progress relied significantly on industrial mass production for electrical valves and other essential parts, and connections with commercial suppliers such as Metropolitan-Vickers, seeded during Rutherford's time in Manchester, remained crucial following his move to Cambridge.

Jeff's own principal relocation, however, was the reverse of Rutherford's. In 1993 he took up a lectureship at the University of Manchester's Centre for the History of Science, Technology and Medicine (CHSTM), established a few years previously under the direction of Professor John Pickstone. As the first long-term member of staff working primarily outside the history of medicine and the biosciences, Jeff was instrumental in shaping CHSTM's now-familiar profile as a genuinely integrated cross-disciplinary group with a strong critical mass in twentieth-century studies. This he achieved through his own ongoing research, which established him as a mainstay of the international history of science community; through undergraduate teaching, focusing particularly on nuclear culture; and, above all, as a PhD supervisor and mentor. He was, at times, lead supervisor to as many as half the doctoral students in the Centre: several of these developed projects close to his own interests, but others pursued a much wider variety of topics, from crystallographic computation to airships to electricity consumption in the home.

Another priority that shaped Jeff's career was accessible communication. His best-known work, *The Manhattan Project: Big Science and the Atom Bomb*, appeared in 2002 as part of Icon Books' Revolutions in Science series, established as a forum for academic historians to produce affordable and readable accounts of "scientific history as it really happened" for general audiences. Typically, Jeff's narrative blended a history of the atomic bomb project itself with a broader analysis of the "big science" phenomenon, using the insights of his Rutherford-era research to argue that the Manhattan Project did not mark a fundamental watershed, but accelerated existing trends. The book was well received by its intended audience but has also found a niche as an undergraduate set text accessible

to science-focused students; several researchers now working in the history of science or science studies have mentioned it as their first or favourite introduction to the field.

The majority of Jeff's original research, however, concerned British science, albeit often in the context of its international networks. His engagement with the "radioactivists" continued throughout his career, but was joined by a growing interest in the role of the Royal Society as a *de facto* national science academy and its relationship to government, chiefly in the years after 1945. Both themes addressed the interrelation of science, the State, and warfare (whether hot or cold), but the latter allowed Jeff to pursue his fascination with the mechanics of officialdom and the power and perceptions of the British establishment. I particularly recall his delight at discovering a classic "smoking gun" document in the archives, showing that traditionalist Fellows of the Royal Society opposed plans for a grand showpiece Science Centre to be constructed on London's South Bank because of the distance this would take them from their favoured haunt and strategic base, the Athenaeum Club. The same fascination appears to have had a memorable effect in Jeff's account of the 1955 Strath Report, in which the bureaucracy of civil contingencies planning collided uncomfortably with the prospect of a thermonuclear Armageddon.

Other publications dealt with remarkable and unexpected stories uncovered in the course of his research: the recollections of Samuel Kay, Rutherford's chief lab technician in Manchester; Francis Aston's familiarity with the "occult chemistry" promoted by the theosophist Annie Besant; the influence of politically engaged science journalists in shaping national policy (the seed of Jeff's teaching in science communication studies, now a major focus for CHSTM's postgraduate profile); the production history of Ewan MacColl's extraordinary, absurdist 1946 agitprop stage drama *Uranium 235*.

Likeable and approachable by nature, Jeff saw academia as an inalienably social activity, and made many friends throughout the international community. A mainstay of the departmental social life that clustered around the Eagle pub in Cambridge, he imported something of the same culture to Manchester, most notably through a monthly reading group, hosted in the convivial setting of the Ducie Arms, attended by staff, graduate students, and international visitors. At conferences he would sit up until all hours, reminiscing with old colleagues or planning the future with new ones: one such session is attested to have concluded with a trip to a Rusholme curry-house as the sun rose around four in the morning.

Jeff's reputation as the life and soul of the party tended to conceal his equally valuable capacity as a skilled and efficient organiser (perhaps fittingly, given the close attention to the doings of "invisible administrators" in his Royal Society research). His career-long involvement with the British Society for the History of Science included terms as Secretary and, from 2008 to 2010, as one of the youngest Presidents of recent times. His greatest administrative achievement was undoubtedly the 24th International Congress of History of Science, Technology and Medicine in 2013, which brought over 1700 delegates to Manchester from around the world for a week of academic and social events, tours and excursions, and remains the largest event in the history of the field. The connections this established led to a new role as Treasurer to the historical division of the International Union of History and Philosophy of Science and Technology in the final years before illness led Jeff to cut down the scale of his commitments.

For all that Jeff was a highly social academic, academic culture did not dominate his life: he was part of a close-knit family and a devoted member of live folk music communities in Manchester and beyond (a passion that was supplemented in his later years by an equal enthusiasm for jazz). Following his cancer diagnosis, and consequent early retirement, Jeff focused increasingly on spending time in the company of those closest to him, with frequent visits to the Northumberland coast, another passion. Yet he remained active in the research community to the last, finalising in September a chapter on the connections between Cambridge physics and the interwar radio ham enthusiast community which called back to his earliest work and developed its themes with reference to current scholarship.

Jeff is survived by his wife Natalie, a large extended family, and three generations of appreciative colleagues, students and friends.

James Sumner

Centre for the History of Science, Technology and Medicine, University of Manchester.

In memoriam**Peter Holmberg (1938–2018)**

Professor Peter Holmberg from Helsinki passed away after a short illness in late May 2018 a month away from his 80th birthday. In his final days he could delight in seeing his book in Swedish on the arctic explorer, A.E. Nordenskiöld's polar expeditions 1858–1883 (Nordenskiöld of world fame for the first North East Passage with the Vega ship in 1878–1879). This happened 475 year on the day after another book was delivered to another author's death bed – Copernicus'. Of course, *De revolutionibus orbium coelestium* is something other than Holmberg's final book, but Holmberg managed a much more extensive production of closer to 500 published texts than Copernicus managed.

Holmberg came from the Swedish speaking minority in Finland, but he spoke Finnish as well. He started his career in nuclear physics and wrote his dissertation on *Investigations of excited nuclear states* in 1967, which he defended at Helsinki University. He then became assistant professor at the recently established University in Uleåborg, but in 1974 he took up a position as head physicist and as assistant professor at Helsinki University Hospital. Later he became full professor in physics at Helsinki University's Medical faculty.

He was a member of the Finnish Society of Sciences and Letters.

Besides his many scientific papers in nuclear and medical physics he also wrote text books for high schools and university students. He also wrote a lot of articles for the daily press in Finland. Early on he developed an interest in the history of physics and in 1992 he published *The History of Physics in Finland 1828–1918*. When the European Physical Society established a group for the History of Physics in the late 1980s Holmberg got involved early on and worked in that group until his death, attending the recurring conferences giving papers most often with historical examples from his great knowledge of the development of physics in Finland. Those of us who had the privilege to know him will miss his genuine friendliness and his positive impact.

He is missed by his wife, Merike, and two grown children from a previous marriage, and many friends in Finland and Europe.

Karl Grandin, chair EPS HoP group.

I should like to add a few words to those of Karl.

I first met Peter in 2005 at a meeting in Dublin. He struck me as a kind, gentle and thoughtful man and those first impressions have proved over the years to be well founded. He was an avid reader of our newsletter and rarely failed to contact me to say how much he appreciated receiving each issue and how interested he was to keep abreast of the group's activities.

He was a good friend and will be much missed by all those who knew him.

- Malcolm Cooper

Meeting reports

Magnetic Resonance Imaging and its history

The IOP History of Physics Group joined forces with the Medical Physics Group and the BRSG: Magnetic Resonance Group at the University of Nottingham on Wednesday the 18th of April 2018 to explore the early history of MRI, celebrate the work of Prof Sir Peter Mansfield FRS, Nobel prize winner in the field, and look to the future of magnetic resonance imaging.

Programme:

10.30 Coffee

10.55 Introduction

11.00 NMR in the UK – the first 15 Years

David Gadian, University College London

11.50 Early MRI research at the University of Aberdeen

Glyn Johnson, University of East Anglia

12.40 Lunch

1.40 The Mansfield years

Peter Morris, University of Nottingham

2.30 Outcomes of faster MRI

Robert Turner, Max Planck Institute for Human Cognitive and Brain Sciences, Leipzig

3.20 Tea

3.40 Quantitative NMR

Sue Francis, University of Nottingham

4.30 The modern MRI scanner: parallel everything, and more with less

Paul Glover, University of Nottingham

5.20 End

It's no mystery that the conference should have been in Nottingham. Although Queen Mary University of London was Sir Peter's alma mater, he spent much of his professional career at the University of Nottingham. Peter died in February 2017, but his legacy lives on at the university in the form of the Sir Peter Mansfield Imaging Centre along with his colleagues and PhD students, some of whom are still at the university leading contemporary research.

The conference took the participants on a journey from its NMR (nuclear magnetic resonance) beginnings in the UK through the key stages of research that was required to get this technology to the clinical MRI (magnetic resonance imaging) systems that we take for granted today. David Gadian (University College London), began his story at the Clarendon Laboratory in Oxford. Early NMR experimentation involved making custom equipment and hand winding magnets. The skill of those at the Clarendon to wind good magnets was demonstrated later on when Martin Wood spun out Oxford Instruments, a company still famous for making high field precision magnetic instrumentation. David suggested that the ability to create good magnets with high magnetic field (for the time) and the discovery of magic angle spinning were the key enabling technologies for NMR which led to the technique being adopted by more physicists and, ultimately, chemists. Commercial spectrometers became available in the early 1950s, resulting in even wider adoption by the growing NMR chemist community. It was noted that other groups were also getting more and more involved, including the National Physical Laboratory and other universities such as the University of Aberdeen and the University of Nottingham.

Glyn Johnson (University of East Anglia) described the development that took place at the University of Aberdeen in the 1970s and 1980s. He noted that in the early '70s it was not obvious to anyone how to form an image with an MRI spectrometer. However, in 1973 and 1974 the first 1D and 2D images had been acquired with people starting to attempt to use Fourier techniques to perform image reconstruction. This was largely gated by computing power at the time. In 1974 a restricted group of people saw the first proton density image of a mouse. This was the first evidence of "full body" imaging with NMR. In 1977 the Medical Research Council gave John Mallard from the University of Aberdeen £30k to make a full body human scanner. After three years of hard work, overcoming a mountain of technological challenges, the first ever clinically-useful full-body MRI scan was performed on the 28th of August 1980. The scanner had poor field homogeneity and so pulse sequences had to be created to produce clean images. The Aberdeen team therefore had the first full body scanner in the world and also the first working pulse sequence.

Peter Morris (University of Nottingham) described the 'Mansfield Years' at the university. In the 1970s Peter was trying to modify an NMR spectrometer to gather spatial information from a sample positioned in a

0.35 Tesla 3 inch gap magnet. He had been trying to develop a line scanning technique for some years by the time they imaged PhD student Andrew Maudsley's finger in 1976. From this seminal achievement imaging human tissue, came Mansfield's 0.1 Tesla body scanner. This was real physics and real engineering: magnets had to be shimmed by hand and scan subjects had to crawl into the magnet wearing additional coil belts. Finally, once in place, the subject would be completely trapped by additional coils slotted in though the bore of the main magnetic coils. The audience were treated to a video of Peter crawling into the bore in a suit and tie for the TV show Tomorrow's World in the 1970s. At the time, it was not certain whether the radio frequency field would be strong enough to give the test subject a heart attack, but they carried on anyway. 2 – 3 years later nuclear magnetic resonance imaging was considered absolutely safe, and indeed much safer than ionising radiation imaging methods. Already pathological targets were being sighted, the first being breast cancer. At that time it was noted that it took 10s of minutes to process an image. As more advanced and ultimately commercial imaging systems were produced, Peter Mansfield and his team continued to perform basic research. He retired in 1994, a year after being knighted and won the Nobel prize for his work in 2003. Peter saw magnet strengths at the University of Nottingham's MRI research lab go from 0.1T to 7T over the course of his career.

A short debate broke out in the meeting trying to debunk the (possible) urban myth that NMR imaging was changed to MRI for the sake of not scaring the patients. However, there were a number in the audience who were aware of a turf war, especially in the US, between radiologists and those working in nuclear medicine. Each side wanting NMR imaging for their own. The story remains that radiology won and changed the name to MRI.

Robert Turner (Max Planck Institute for Human Cognitive and Brain Sciences, Leipzig) spoke about the period from the 1980s to the present day. The first scanners produced purely anatomical images. There was very little functional or physiological information acquired. They could be used for repetitive motion such as the motion of the heart of lungs to some degree through gating and progressive acquisition, but they cannot be used to image anything irregular such as heart defects, gastrointestinal imaging or the brain. Technology was advancing rapidly with faster computing power, faster switching and high field gradients becoming available. This facilitated and encouraged the development of the fast pulse sequences

which have become the trademark of modern MRI, building on the early work of echo-planar imaging (EPI) developed by Peter Mansfield and others. By the late 80s and early 90s much more complicated sequences were common with techniques like diffusion weighted imaging becoming standard practice in stroke evaluation and functional imaging becoming possible using deoxyhaemoglobin as a 'natural' contrast agent.

In the second part of the day Sue Francis (University of Nottingham) presented the state of the art in quantitative MRI techniques, facilitated by the advent of clinical scanners now available with fields up to 10.5T with extremely high signal to noise ratio and spatial resolution including: phase contrast MRI which has enabled quantitative flow analysis through arteries and the heart; magnetic resonance elastography (MRE) which has been shown to be able to detect tissue disease such as cirrhosis and fibrosis. Sue reported that MR is also starting to be used as a technique to detect disease biomarkers enabling the characterisation, prognosis prediction and treatment planning. It is potentially much more effective and thorough than biopsy for the localised detection of disease which can easily be missed by biopsy. The audience were very much left with the impression that we are about to enter an age of quantitative MRI that could replace some expensive, inefficient diagnostic techniques.

The day was completed by Paul Glover (University of Nottingham) taking the audience through the technology of MR from analogue images, through the first digital capture technology that required racks of analogue electronics and yet more racks of minicomputers with kilobytes of RAM to modern MR scanners which have digital (optical) output from the coils. In 40 years the electronics has shrunk orders of magnitude and computing power has expanded even more orders of magnitude, with modern imaging processing taking place on graphics processing units (GPUs) with 1000s of individual processing cores. The future is even more fantastic with the possibility of using contemporary machine learning techniques to self-optimize multi-channel pulse sequences to get the best SNR uniformity throughout the scanner at the highest field strengths.

Phil Marsden (Chair of the Medical Physics Group Committee)

Open Meeting: “From Newton to the Free Electron Laser”,
Institute of Physics, 27 September 2018

The first seeds of this meeting – where talks would be *contributed* by individuals on a topic of their choosing, rather than *invited* by the organisers on a pre-set topic – were sown at a committee meeting back in June 2015. As it was a new format that the Group had not tried before – at least in living memory – it was slow to catch on, and it was another three years before plans got seriously under way, with a call for abstracts going out to Group members and various university history-of-science departments. This was done in recognition of the fact that the history of physics is a field populated not only by physicists, but also by historians who may not necessarily be members of the IOP.

The final programme featured seven contributed talks and two invited talks. Of the former, four were given by Group members, the other three coming from historians of science working in university departments. Isobel Falconer, of St Andrew’s University, and Richard Staley, of Cambridge, were invited to give the “keynote” talks. Attendance at the meeting was about 35.



L to R: Cormac O’Raifeartaigh, Thomas Sefton, Richard Staley, Paul Ranford, Isobel Falconer, Joseph Martin, Gregory Bridgman, Elaine Seddon, Alan Watson, Elisabetta Canetta

Since a meeting needs a title, and especially a title that will intrigue and stimulate the potential audience, we thought that just “Open Meeting” was a little plain. Hence we borrowed some words from two of the contributed

talks which appeared to constitute the end-points of the historical period under consideration – which extended from the 17th to the 21st century – and appended the subtitle “From Newton to the Free Electron Laser”.

As always with our meetings, we asked the speakers to provide newsletter articles on the subjects of their talks. Two speakers – Cormac O’Raifeartaigh of Waterford Institute of Technology, and Isobel Falconer – had recently published papers on their topics, and have kindly provided links to them rather than writing duplicate articles (see pg 49). We received articles from the other seven, and in addition, Colin Hempstead, who had intended to give a talk but had to cancel at the last moment, has contributed an article, as has Thomas Sefton, who submitted a poster to the meeting.

The meeting was enjoyed by the audience, and also by the speakers (who were also, of course, in the audience). A typical response from one of the contributors was: *“I really enjoyed the meeting yesterday. A great set of people and a different perspective than the one I normally take.”*

I hope you enjoy reading the articles. There was a feeling among those present that this sort of meeting ought to be repeated, possibly on a regular basis, so watch out for announcements on our website and in future newsletters.

I would like to personally thank Marcia Reais, Claire Garland and Heather Blackhall of the IOP for their assistance in making this meeting a success.

Jim Grozier.

Programme:

Symmetries: On physical and aesthetic argument in the development of relativity
Richard Staley

Maxwell and Cavendish’s null method for the inverse square law of electrostatics
Isobel Falconer

The Origins and Development of Free-Electron Lasers in the UK Elaine Seddon
Psychical and optical research between Lord Rayleigh’s naturalism and dualism
Gregory Bridgman

When Condensed Matter Physics became King Joseph D. Martin

A Partial History of Cosmic Ray Research in the UK Alan Watson

The paradigm shift of physics-religion-unbelief relationship from Renaissance to the 21st century Elisabetta Canetta

Newton’s 1st Law – a history Paul Ranford

Interrogating the legend of Einstein’s ‘biggest blunder’

Poster Lord Kelvin’s compass cards Thomas Sefton

3rd International History of Physics ConferenceSan Sebastian, Spain, 17th - 20th October 2018

A personal report by MJ Cooper

This conference, third in the series, following the first in Cambridge and the second in Pöllau, Austria, was held in the Palacio Miramar (above), San Sebastian - Donostia, Spain.

The conference was billed as being joint between the 3rd International Conference (supported by the Donostia International Physics Centre, EPS, IOP, EPJ, Echophysics and the Basque Government) and the 4th Early Careers Conference (ECC) supported by the AIP.

The 37 talks, presented in two fine salons, were arranged in two parallel sessions. This had the advantage of offering a larger number of talks than would otherwise have been possible in the time allowed but with the disadvantage of inevitable clashes of interest - I myself missed at least 4 talks which I would very much liked to have attended.

The presentations were usefully arranged under the headings:

Histories of Theory Change	National Identities
Problems in 18 th C Physics	Forgotten Names in Physics
Mathematical Methods	Forgotten Projects in Physics
Quantum Physics	Scientific Institutions

and 3 plenary sessions entitled:

‘Physics and Gender’, ‘PhysicsEstoire’* prize speech by Jim Bennett
‘History of the EPS’

* This is a prize awarded annually for a ‘lifetime’s contribution to the history of physics’ and is sponsored by the EPS and Echophysics.

The main conference ran from 3.30 pm on Thursday 18th through to the Saturday lunchtime. In the afternoon the whole party was bussed to the town of Bergara where, in 1783 tungsten (or wolfram as it was then known) was first isolated by the Elhuyar brothers.



Commemorative plaque unveiled in Bergara

The site has been declared an ‘Historic Site’ by the EPS and received its official opening by its President, Rüdiger Voss. Also present were the mayor of Bergara, the Deputy of Gipuzkoa, Urkullu Olano and Professor P.M. Etxenike who gave a very interesting talk on the discovery and role of tungsten in the modern world. The evening was rounded off with copious amounts of tapas and free flowing local wine.

The conference was most enjoyable but for me somewhat marred by what I felt a bias of talks toward historians rather than physicists and the ECC being a closed meeting to which other members of the conference were denied access. This seemed to miss an opportunity to engage early career historians with delegates to the main conference and vice versa

Nevertheless it was good to see the series continuing in good health.

News

Group committee creates new post of Publicity Officer

Publicity officer writes:

The History of Physics committee has considered that it would be useful to have a renewed focus on publicity for the various events that the group arranges. It will also be useful to highlight other related conferences related to the history of physics which are being organised by other organisations. We are at an early stage of developing the History of Physics Group publicity. Specific ideas which are being considered include more events on the Institute of Physics HoP group calendar, setting up a Twitter feed to highlight events, and working with other organisation with similar interests. We are talking with the Institution of Engineering and Technology which also in particular celebrates the work of Michael Faraday.

By collating a number of events from different organisations we hope to develop a richer understanding of the work that is being explored in the history of physics. This should enable group members to participate in a wide range of activities. In addition, we expect that there will be an interesting cross-fertilisation of ideas from different points of view which could well lead onto conferences or seminars in their own right. It will also be good to encourage people in other organisations or with other prime interests to explore more of the fascinating story of the exploration of physics over the last few hundred years.

Julian Keeley

julian.keeley@gmail.com

4th International Conference on the History of Physics

The 4th International Conference on the History of Physics is planned to be held at Trinity College Dublin, 17-19 June 2020, following successful meetings at Cambridge, Pöllau, Austria and San Sebastian, Spain.

This is a conference that aims to bring together physicists and professional historians of science. Members of the History of Physics Group Committee have played a crucial role in initiating and ensuring the continuation of the series.

Denis Weaire (TCD) will be chairman of the Local Organising Committee. Edward Davis (Cambridge) chairs the International Advisory Committee which is overseeing the series of conferences and Malcolm Cooper is its secretary

The conferences services of the Institute of Physics will undertake the organisation and will have established a website for it by early 2019.

Any advice or queries from members of the History of Physics Group will be welcome.

Denis Weaire FRS
Emeritus Professor
School of Physics
Trinity College Dublin
Dublin, Ireland

Disclaimer

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

Comment

History, Physics and the History of Physics

by Jim Grozier

It was a great honour to be elected Secretary of the History of Physics Group earlier this year; but it was also a challenge. The history of physics is an interdisciplinary field, which means that it straddles the somewhat arbitrary boundary between the intellectual disciplines of History and Physics. To make sense of the history of science, one really needs to be proficient in *both* history and science – but these subjects tend to be taught in different departments, and generally to different people, who must choose, rather early in life, whether they are going to study “science” or “the arts”. While these categories are somewhat less clearly defined nowadays than they were a few decades ago, they are still separated by a fairly impenetrable barrier. Our discipline suffers from that segregation.

As a result, there are two distinct “history-of-science” communities: one consisting of people who have specialised in science, and the other of people who have studied history. And each of these communities tends to carve out its own intellectual territory; those from a science background concentrate on the science itself, while those from a history background are generally more interested in the *contexts* in which science is practised – that is, in such things as the cultural, political and sociological factors that influence scientists. These communities are sometimes referred to as “internalist” and “externalist” respectively; sometimes (rather unkindly) their members are described as “amateur” and “professional” historians of science, despite the fact that the level of scholarship evident in the former group can be just as high as in the latter.

Often what emerges from this polarisation is somewhat of a caricature of the history of science, in which either the science itself is at best sketchy, and at worst absent (or just plain wrong) or we get oversimplified accounts seen from the perspective of the present, which ignore external factors and portray scientific progress as a linear progression to success, ignoring all the dead ends and blind alleys along the way. Of course, what is needed is *both* these approaches. We need scientists who know history, and we need historians who have studied science. If such scholars are in short supply (and generally they are) we need historians and scientists to collaborate; but they rarely do.

So that is the challenge. The History of Physics Group, being a specialist group of the Institute of Physics, is made up overwhelmingly of physicists – it is unlikely that anyone who has studied history, or even the history of science, would be motivated to join the Institute. So we, as physicists interested in history, need to reach out to the historians who are interested in physics.

The International Conference on the History of Physics, first held in Cambridge in 2014, has succeeded in bringing together members of the two communities; our own Open Meeting in September 2018, which has given rise to several articles in this issue of the newsletter, also contributed to that process in a small way. I hope we can continue to chip away at this intellectual “iron curtain”, for the benefit of all.

Future meetings?

Your committee was discussing topics and locations for future Group meetings, when we were told that our group was one of the largest Groups of the IOP. We agreed that we knew very little about them,

so

this note is to ask you to tell us where you live, what topics in physics you find most interesting, are there any famous scientists commemorated in your area, perhaps a statue, a memorial, a museum

or

even a road name. I am making a list of locations and hope to produce a leaflet of tourist sites for Physicists.

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The paradigm shift of physics-religion-unbelief relationship from
Renaissance to the 21st century

Dr Elisabetta Canetta
Faculty of Sport, Health and Applied Science
St Mary's University, Twickenham, London

Abstract

The physics-religion-unbelief relationship is probably one of the most controversial and ever-changing interactions from both historical and social perspectives. The public perception of this relationship has shifted from Renaissance time to our modern days. This paradigm shift is underpinned by the dramatic change in the socio-historical roles that physics, religion and unbelief had in the past and have now. In this paper, the perception that physicists have of what physics, religion and unbelief are and how they fit in our highly technological society will be compared to the perception that scientists had in the Renaissance era and during the Enlightenment.

Rational interpretation of religion: A historical account

Religion is a term which, at first, can make some people feel uneasy and uncomfortable. Perhaps, this is due to the way they perceive religion. The Oxford Dictionary defines religion as “the belief in and worship of a superhuman controlling power, especially a personal God or gods” [1]. An etymology of the noun “religion” was proposed by the early Christian author Lactantius (c.a. 240 – 320 AD) who understood religion as “the bond of piety that binds to God” [2]. Interestingly, religion is different from faith, as the latter concerns the sense of human finitude and dependence upon a supernatural and transcendent force. The antonym of religion is unbelief, which according to the Oxford Dictionary is a “lack of religious belief; an absence of faith” [1].

Despite the apparent lack of relationship between religion and rationalism, a rational interpretation of religion has developed and flourished for more than 2000 years. The first account of a rational approach to religion can be found in Plato’s masterpiece *Timaeus* (c.a. 360 BC) [3] where an astonishingly detailed account of the creation of the Universe and explanation of the visible natural order can be found. The *Timaeus* is also considered to be the first treatise of natural theology as Plato attributed the harmony and order observed in nature to the work and intervention of a supreme and divine being. An even more rational and scientific

interpretation of religion was given by Plato's student, Aristotle (384 – 322 BC). Aristotle's approach was much less "mystical" and more "realistic" than that of Plato. He interpreted the visible and material world as real and as the primary source of knowledge. It is because of that that Aristotle can be considered as the first "scientist" and as the founder of a so-called "moderate realism". Aristotle's philosophical and scientific ideas for unravelling the first principles and causes of the visible world, provided the prolific soil in which Christian philosophy put down its own roots and flourished until the end of the Middle Ages (5th – 15th century).

The Medieval period was followed by the Renaissance era (14th to 17th century) which thrived in Europe and saw the birth of physics as the science trying to answer not only the "how" (typical of scientific inquiry) but also the "why" (characteristic of philosophical and theological investigation) of natural phenomena. Physics and astronomy were, therefore, still deeply rooted in metaphysics and theology. The influence of the Catholic Church on almost every aspect of society, including science, was so strong that even the most sound physics theories were bound to be perceived as blasphemous if they were not abiding by Biblical principles. However, Renaissance was also the time when unbelief began to grow due to an increase of the autonomy of reason, compared with faith, for the search of truth. During this period of transition from a religion-centred to a reason-centred approach to truth, great minds like René Descartes (1596 – 1650) and Baruch Spinoza (1632 – 1677) tried to reconcile science and religion and did so by using a rationalistic approach.

The "Age of Enlightenment" (18th century) saw the beginning of a religion-free physics and the birth of rationalism. The universe was perceived as governed by natural laws totally disconnected from Biblical principles. This is when physics, religion and unbelief became three separate and autonomous spheres of thought. It was also the period when the fast-paced growing philosophical atheism worked toward superseding religion and explaining natural phenomena using only empirical tools of investigation. This brings us to our modern society (19th – 21st century) in which physics is perceived as the science trying to answer the "how" of natural phenomena but not necessarily the "why". Technology is heavily dominating the science arena and unbelief seems somehow superseding religious belief in the way scientists interpret physical phenomena and try to unravel their most subtle and complex intricacies. In particular, the 19th – 20th centuries witnessed an increase in the science-religion schism, in particular due to the work of the so-called "New Atheists" (i.e. Sam Harris, Richard Dawkins, Daniel Dennett, and Christopher Hitchens) who make use

of the natural sciences to disprove the existence of God and of any theistic belief [4]. The divergence between science and religion is continuing in the 21st century. Despite that, history shows us that the quest of physicists for the key which could unlock the real meaning of nature and ultimately find answers to fundamental questions, such as “where do we come from?”, “why are we here?”, “why nature works the way it does?”, has never stopped. In the 20th and 21st century, physicists of the calibre of Fr. Dr. Georges Lemaître, Rev. Dr. John Polkinghorne, Albert Einstein, Freeman Dyson, Richard Feynman, Paul Davies – just to cite a few – have explored and are still exploring the relationship between physics and religious belief.

Physics, religion and unbelief during the Renaissance era

The modern approach to physics was born during the Renaissance era; a formidable cultural period that saw a renewed interest in the fundamental values of Ancient Greek natural philosophy. It is during the Renaissance that the first *Scientific Revolution* occurred, which paved the way to modern science as we know it.

At the heart of Renaissance there was “humanism” [5] whose primary goal was to make knowledge available to everybody. The most notable product of this approach to learning and teaching was the “seven liberal arts” education. This educational system was divided into two parts: *Trivium* (i.e. grammar, rhetoric, logic) and *Quadrivium* (i.e. arithmetic, geometry, astronomy, music) and aimed to create “free thinkers” capable of using scientific and humanities knowledge to pursue the truth and unravel the secrets of the world. This was the period when the “transcendental” nature of mathematics and physics were rediscovered. The transcendental nature of mathematics was a Pythagorean concept that formed the foundation of “geometry”; Plato interpreted geometry as the language in which the metaphysical world was made manifest and known to humanity on the material plane [3].

One of the main supporters of geometry as a sacred language was the German mathematicians and astronomer Johannes Kepler (1571 – 1630). Kepler is best known for his three laws of planetary motion at which he arrived because of his desire to unravel the secrets of the Universe; that desire was kindled by his profound religious beliefs. As he famously wrote to his mentor and teacher Michael Mästlin “*I wanted to become a theologian. For a long time I was restless. Now, however, behold how through my effort God is being celebrated in astronomy*” (letter dated 03rd October 1595). Kepler published his first two laws of planetary motion in 1609 in his book *Astronomia Nova* (New Astronomy) [6]:

Ergo ellipsis est Planetæ iter (Thus, an ellipse is the path of the planet)

Demonstratio, quod orbita Martis, ... , fiat perfecta ellipsis
(Proof that Mars orbit,..., is a perfect ellipse)

Kepler published his third law in 1619 in his book *Harmonices Mundi* (Harmonies of the World) [7]:

- *Sed res est certissima exactissimaque quod proportio qua est inter binorum quorumcunque Planetarum tempora periodica, sit præcise sesquialtera proportionis mediarum distantiarum, ...* " (But it is absolutely certain and exact that the *proportion between the periodic times of any two planets is precisely the sesquialternate proportion* [i.e., the ratio of 3:2] of their mean distances, ... ")

Kepler was convinced "that the geometrical things have provided the Creator with the model for decorating the whole world" [7].

The French mathematician, scientist and philosopher René Descartes (1596 – 1650) also used geometry to describe natural phenomena (e.g. law of optical refraction) and he developed analytical geometry. Similar to Kepler, Descartes was deeply religious and developed a philosophical-scientific framework that not only was not in conflict with the Church teachings, but also allowed the Church to accept science without reservations. His goal was to build a "new science" upon the foundation already laid down by Galileo's scientific work. Hence, this new science was constructed on metaphysical grounds and its aim was to prove the existence of God. To achieve that, Descartes developed a scientific methodology based on doubt rather than on faith [8]. His approach was to question everything and to mistrust his physical senses. He, therefore, relied solely on his mind and intellectual abilities to understand the natural phenomena under study as the only reliable source of information and truth. It is interesting to notice that the branch of physics that Descartes focused his attention on was mechanics. Starting from what was known about mechanics, he developed a new mechanical theory of a universe described by deterministic laws. The universe conceived by Descartes was made up of only matter whose single pieces were pushing one another. Hence, the only energy present in the universe was, according to Descartes, mechanical in nature as it was the energy produced by the motion of matter.

Enlightenment: The Bridge between Renaissance and modern science

Renaissance was undoubtedly a period of great advancement in art, architecture, philosophy, science and mathematics that saw the flourishing of important cultural centres in the main European courts. However, because of its roots in Christian doctrine and more specifically in Roman Catholic teachings Renaissance culture did not allow free thinking to have the manoeuvring space it required for developing further new ideas and for seeking the truth. The most striking instance of such a restriction was Galileo's trial [9].

This period of restriction ended with the onset of the *Age of Enlightenment* whose precursor was the Scientific Revolution of the 17th century. The latter was a period of profound cultural transformation when people began to develop a fervent desire to know how nature worked, to understand the laws that were governing natural phenomena. Key figures of the Scientific Revolution were Johannes Kepler, René Descartes, Francis Bacon (1561 – 1626), Galileo Galilei (1564 – 1642), and Isaac Newton (1642 – 1727). The term Age of Enlightenment was coined by the German philosopher Immanuel Kant (1724 – 1804) to describe the process of freeing one's mind, to achieve "*liberation from superstition*" as Kant explains in his essay "What is Enlightenment?" (1784) [10]. In this famous essay Kant tells us that

"Enlightenment is man's emergence from his self-imposed immaturity. Immaturity is the inability to use one's understanding without guidance from another. This immaturity is self-imposed when its cause lies not in lack of understanding, but in lack of resolve and courage to use it without guidance from another. Sapere Aude! [dare to know] 'Have courage to use your own understanding!'—that is the motto of enlightenment" [10].

The Age of Enlightenment produced free thinkers who could expand human knowledge at an unprecedented rate.

It is during the Enlightenment that scholars used "empiricism", i.e. "*the theory that all knowledge is based on experience derived from the senses*" [1] to understand and explain natural phenomena. Looking at the universe through the lenses of empiricism, brought scientists to see the universe as moved by a soulless machine without God's hand behind every unexplained natural event. Interestingly, not all scientists agreed with the Godless approach of empiricism and some of them (notably Newton) felt that God had to be brought into the picture if they wanted to understand and explain events that, otherwise, would have been left unexplained. Scholars like Newton were called "deists". The differences between how empiricism and

deism explained natural phenomena led not only to the schism between science and religion but also to the diversification of science into separate disciplines (i.e., biology, physics, chemistry, and mathematics). This also marked the end of polymaths (i.e. scholars with a wide and vast knowledge) and the flourishing of secularism (i.e. separation of religion and statecraft).

19th century: The quest for the unification of natural forces

The 19th century saw an increase in the development of a unification theory, of a “theory of everything”. This quest for strenuously pursued by two British physicists, Michael Faraday (1791 – 1867) and James Clerk Maxwell (1831 – 1879).

Faraday was a brilliant experimentalist who believed in a “unifying principle”, i.e. in the fact that *“all the forces of nature are mutually dependent, having one common origin, or rather being different manifestations of one fundamental power”* [11]. This belief brought him to the experimental discovery in 1831 of the so-called Faraday electromagnetic induction [12], which proved that electric and magnetic forces work together because they are different expressions of the same force. Faraday was very passionate about science as well as about what natural mysteries scientific investigation, carried out during his life, was about to unlock; he famously said *“... I cannot doubt that a glorious discovery in natural knowledge, and the wisdom and power of God in the creation, is awaiting our age, and that we may not only hope to see it, but even be honoured to help in obtaining the victory over present ignorance and future knowledge”* [13].

Despite being an outstanding experimental scientist, Faraday was a poor mathematician and could not express in mathematical language the interconnection between electric and magnetic force. This feat was achieved by the Scottish mathematician and physicist James C. Maxwell in 1864 when he formulated twenty equations, which he called the *“General Equations of the Electromagnetic Field”* [14] and that subsequently became the Maxwell’s equations [15]. As Maxwell stated in his 1864 paper: *“The agreement of the results seems to show that light and magnetism are affections of the same substance, and that light is an electromagnetic disturbance propagated through the field according to electromagnetic laws”* [Maxwell 1864]. Besides being an impressive physicist, Maxwell was also deeply religious and in his inaugural lecture as Professor at the Marischal College and University in Aberdeen on 03rd November 1856 he

said that “*As physical science advances we see more and more that the laws of nature are not mere arbitrary and unconnected decisions of Omnipotence, but they are essential parts of one universal system in which infinite Power serves only to reveal unsearchable Wisdom and eternal Truth*” [16].

Most famously, Maxwell had Psalm 111, v.2 inscribed on the doors of the Cavendish Laboratory at Cambridge: “*Magna Opera Domini exquisite in omnes voluntates ejus*” (Great are the works of the LORD , studied by all who delight in them).

20th and 21st century physicists on the physics-religion-unbelief relationship

The 20th century gave us some of the most prominent advocates of the positive and synergic relationship between physics and faith. Physicists of the calibre of Albert Einstein (1879 – 1955), Fr Dr George Lemaître (1894 – 1966), Rev Dr John Polkinghorne (1930) and Paul Davies (1946) wrote extensively on the subject. In particular, Einstein had his personal views about religion and he believed in what he called “cosmic religion” where God’s presence was evident in the order and rationality of nature and the universe in all its aspects and expressions. According to Einstein “*cosmic religious feeling is the strongest and noblest motive for scientific research*”. Einstein had a deep feeling of awe in front of nature and the universe and he believed that “*strenuous intellectual work and the study of God’s Nature are the angels that will lead me through all the troubles of this life with consolation, strength, and uncompromising rigor*” (letter to Pauline Winteler, 1897).

The fundamental scientific experience discloses something new. The fascination of the fundamental scientific experience is not caused by novelty as such, but rather by something which takes place at a deeper level of the mind every time a new insight enters it. Werner Heisenberg clearly expressed this when he said that

“[...] one is almost scared by the simplicity and harmony of those connections which nature suddenly spreads out in front of you and for which you were not really prepared” and “[...] when one stumbles these very simple, great connections which are finally fixed into an axiomatic system the whole thing appears in a different light. Then our inner eye is suddenly opened to a connection which has always been there – also without us – and which is quite obviously not created by man” [17].

A recent survey carried out in 2015 by Prof Elaine Howard Ecklund, founding director of Rice University's Religion and Public Life Program and the Herbert S. Autrey Chair in Social Sciences, clearly showed that although

“there is a popular 'warfare' framing between science and religion” it is undeniable that “science is a global endeavour [...] and as long as science is global, then we need to recognize that the borders between science and religion are more permeable than most people think” [18].

Conclusions

We currently live in a high-tech and AI (Artificial Intelligence) dominated society in which the ever-expanding technology is pushing aside religion. Despite that, many scientists are interested in the synergic relationship between science and religion and in exploring once again the complex interconnectivity between the laws of nature and their inner harmony.

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Psychical and Colour research between Lord Rayleigh's "Naturalism"
and "Dualism"

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Lord Rayleigh's contributions to physics need no introduction. It is also generally recognized, though less celebrated, that Lord Rayleigh had an interest in what has been called both "spiritualism" and "psychical research", two distinct but related notions distinguished by how the phenomenon under investigation was conceived¹. Recent historical accounts have tended to treat Rayleigh's involvement in this area separately from his conventional scientific research². However, assuming 19th century psychical research to have been categorically separate from other scientific domains ignores recent literature on the experimental links between psychical research and physical science³. This assumption also ignores recent literature discussing the extent to which experimental psychology was shaped in opposition to the type of "mind" that many participants in psychical research believed themselves to be studying⁴. "Mind", in this sense, means a self-directed, conscious agent, whose governing systems and mental states are categorically irreducible to physical systems. Whether the target of experimental psychology was such a mind was, and remains, a point of heated contention⁵.

With respect to Rayleigh in particular, the assumption that his psychical research was divorced from his other scientific work overlooks Rayleigh's own stated opinion on the matter. It also fails to recognize how Rayleigh's legacy was viewed by some of his contemporaries. For instance, in an obituary notice for Lord Rayleigh, the esteemed physicist Joseph Larmor, wrote *"The same caution as he showed in connection with psychical*

¹ Lindsay (1970) and Strutt (1924) both note Rayleigh's psychical research. Noakes (2004: 23) implies a difference between John Tyndall's "psychical research" and "spiritualism".

² For instance, Lindsay (1970)

³ Noakes (2004, 2005, 2014)

⁴ Noakes (2014); Sommer (2012, 2013)

⁵ On this point William Barrett (1919: 11) contended that "The insistence of the demand for some explanation of these [mental] phenomena which we find within us, is only a special case of that "continuous pressure of the causal instinct" which characterizes our reason; and it is because of the difficulty of finding any adequate explanation of them in in known and familiar causes, that science distrusts the existence of the phenomena themselves". On one side of the modern debate is Swineburn (2013) and Tse (2013), and on the other is Dennett (1993) and Singh (2016).

research seems to have affected his physical work, in inhibiting discussion of problems depending on disconnected and shifting hypotheses”⁶. What could Larmor have meant by this? One way to approach the question is to ask how Rayleigh’s psychical research affected his physical science. However, this could result in an answer that is too blunt. I think the more subtle approach is to ask where psychical research and physical science intersected such that a particular approach may have proved useful in both domains.

Rayleigh’s non-“naturalistic” naturalism and the problem of causality between mind and matter

Rayleigh has also been grouped by recent scholarship among a community of conservative and religious British scientists whose particular theological commitments framed their view on the qualities of the universe they believed themselves to be studying⁷. The theology of this camp is believed by some to have conflicted with the presupposed models of nature presumed by scientists, and popularizers of science, Like John Tyndall and Thomas Huxley, who advocated for a particular scientific worldview defined in large part by the presumption of a particular kind of natural causality⁸.

The work of Barton and Kim has cast doubt on the extent to which Tyndall’s view of nature should be classed alongside that of Huxley⁹. However, it remains likely that both figures presumed science to proceed from the assumption that nature is a closed system and believed this to be the appropriate worldview for any scientist or scientifically minded person. This type of view has, often pejoratively, been called “naturalism” and is associated with the equally pejorative label “materialism”¹⁰. However, as Armstrong’s account of the “Parson Naturalist” tradition reveals, “naturalism” might also be a terminology appropriate for religious Anglican scientists who believed their practice to reveal fixed and objectively real abstract truths of God’s creation¹¹. Parson naturalists essentially believed that science cannot access truths outside nature, but that what it can access runs in accordance with fixed laws written by God, which are accessible through a rational human mind that is also of divine origin.

⁶ Larmor (1919, 431)

⁷ Bowler 2001; Opitz 2006

⁸ Bowler (2001: 87-88)

⁹ Barton (1987); Kim (1995)

¹⁰ Smith (2015, 5)

¹¹ Armstrong (2000: 4, 89)

Rayleigh may be said to have shared these views insofar as he believed “*The higher mysteries of being...require other weapons than those of calculation and experiment*”, and that he did not consider “*the materialist view possible*”¹².

Rayleigh further hinted at the views of a “naturalist”, in the religious sense, with a set of biblical psalms that he chose as a way to encapsulate the ethos of his *Collected Works*. These included “*thou hast ordered in measure number and weight*” and “the works of the Lord are great, sought out of all them that have pleasure therein”.

If Rayleigh did hold such theological commitments, one may assume they were consistent with, or even explained, his willingness to entertain the possibility that psychical research revealed “spiritual” phenomena, which is to say, phenomena caused by disembodied free-willed agents intervening in nature and disrupting law-bound regularities. However, while Rayleigh’s religious commitments held that God gave humans free-willed rational minds, this belief also entailed the capacity and imperative of such minds to deduce the *regular* laws of nature that God inscribed in the cosmos. In this respect, both secular and religious “naturalists” shared the conviction that science could only proceed upon the assumption of real, objective, and regular natural laws. Perhaps their key difference then was that religious naturalists proposed an explicit metaphysical frame with which to accommodate the implicit assumption made by secular naturalists that humans have a rational intellect which allows them to access physical truths of reality beyond their senses. Religious scientists like Maxwell and Rayleigh accounted for this capacity by considering it a God-given attribute intended to permit the revelation of extrasensory, abstract truths of God’s creation¹³. However, while being a convenient way to understand how humans can access abstract physical truths, this metaphysical construal of “mind” clashed with any effort to integrate causal interactions between disembodied free-willed minds and physical systems within a single theory of nature. What could it mean in a universe of both fixed law, and subjective free-willed mind, that these two categorically distinct forces might causally interact?¹⁴ If the subjective mind is caused to enter states by physical laws, in what sense can it be called “free-willed” or even “mind”? However, if the laws of the physical universe can be causally acted on by disembodied mind, how can such an event be studied by a physical science

¹² Strutt (1924 361)

¹³ Bowler (2001); Stanley (2014)

¹⁴

whose theoretical and predictive functions presume fixed, regular, natural law?¹⁵

Psychical research and Rayleigh's epistemic crisis of evidence

In contrast to current assumptions that Rayleigh's psychical research was isolated from his other work, Rayleigh's own letters, addresses, and honest attention to the question indicated that he believed it to be of fundamental importance to his early science. Rayleigh wrote to his mother that "I rather expect to be converted", and in a letter to Henry Sidgwick Rayleigh stated "*I am amazed at the little interest most people take in the question. A decision of the existence of mind independent of ordinary matter must be far more important than any scientific discovery could be, or rather would be the most important possible scientific discovery*".¹⁶ It is evident in his correspondence with Sidgwick that Rayleigh realized that psychical research had metaphysical implications that could overturn how physical science approaches the causal structure of nature. In this respect, Rayleigh may be compared with his colleagues in psychical research including William Barret and Oliver Lodge, who both expressed similar sentiments¹⁷. Rayleigh's son also recognized that his father initially believed psychical research to be of genuine scientific interest, and might allow for a clearer understanding of nature. He insists that "at the beginning of these psychical investigations, he did not doubt that they would soon lead him to a positive or a definitely negative conclusion", and notes that "he inclined to expect a positive one"¹⁸.

As mentioned earlier, Rayleigh's religious view of science likely influenced his initial sympathy for the project of psychical research and may have led him to entertain the possibility that the "psychical" domain of research truly was "spiritual" in nature. However, Rayleigh's style of religious "naturalism" also presumed fixed natural law in the physical domain. This was reflected in Rayleigh's scientific practice, which entailed many of the same causal assumptions about nature made by scientists like Tyndall. As such, Rayleigh struggled with integrating his understanding of what classifies as legitimate physical theory with his laboratory work in

¹⁵ Simons (2011) presents this as a problem that has sustained itself throughout the history of science.

¹⁶ Strutt (1924: 66 - 67)

¹⁷ Oliver Lodge (1920 [1908]: 174) claimed "Telepathy opens a new chapter in science, and is of an importance that cannot be exaggerated". William Barrett (1919: 12) argued that psychical research will lead to "a change of thought" that will extend "over the educated world".

¹⁸ Strutt (1924: 68)

psychical research, leading to an epistemic crisis. In his seances Rayleigh constantly changed the experimental conditions to isolate the free-willed “causes” in question in the same way that he tried to isolate invisible physical causes by increasing the “precision” with which he studied their effects, an approach which paid with his discovery of Argon¹⁹. He was particularly concerned about the risk of his own subjective experience being faulty and made various efforts to “improve the conditions” of his seances²⁰. In his work on “spirit writing” Rayleigh placed a pencil in a sealed box so as to “*give opportunity for evidence that would be independent of close watching*”²¹. This reveals that Rayleigh’s efforts to bring the evidence he gathered from his psychical research to the point of being scientifically admissible included removing his own subjective perception where possible. However, as Oliver Lodge pointed out, subjective events were both the target of psychical research *and* the only possible kind of evidence that could be gathered to indicate that the non-physical component of the events ever occurred and what their characteristics were²². As such, subjectivity could not be removed from psychical experiments or the scientific evidence gathered therein. This resulted in an awkward paradox. The only class of evidence that could be enlisted to disprove mechanistic physical causality would be inadmissible by the epistemic criteria implicit in the physical mode of causality that the evidence was required to disprove. This was not a conventional problem relating to the “quality” of the evidence in question, because the epistemic concern pertained to the standard of quality itself.

Rayleigh seems to have been initially unclear on the metaphysical nature of this problem, writing in 1874 that in experiments with a medium named Ms. Jencken he simply could not “get anything quite conclusive”²³. However, by 1919 he considers why this may have been the case. In his presidential address to the Society for Psychical Research, Rayleigh announced a dire problem in the basic assumptions from which psychical research proceeds, whereby

¹⁹ Lindsay (1970: 25)

²⁰ Rayleigh (1919a: 647)

²¹ Rayleigh (1919a: 647)

²² Lodge (1920 [1908]: 172) argues that “direct psychical connection” would be “difficult to prove directly, in a way convincing to those who approach the subject without previous study, or with prejudices against it; because in the proof, or to produce any recordable impression, a bodily organ - such as brain or muscle - *must* be used”

²³ Strutt (1924: 66)

*“if heavy tables in a dining room can leave the floor, how is it that in the laboratory our balances can be trusted to deal with a tenth of a milligram?”*²⁴. This statement reveals an anxiety about the implications psychical research held for the possibility of experimental precision, an epistemic value which came to mark late 19th century laboratory practice²⁵. However, Rayleigh’s statement also reveals a concern about what kind of scientific evidence could ever exist that would disprove the regular physical model of causality which gives physical scientific evidence for every other area of science any meaning. In this same address, Rayleigh admits that the key problem in psychical research was the *“quality rather than quantity”* of the evidence, and that this was a problem he seemed unable to identify or experimentally correct²⁶. This left Rayleigh eternally agnostic toward any amount of “evidence” in favour of free-willed intelligences intervening in regular nature. As Rayleigh’s son notes with regard to spiritual phenomena, *“He would not have committed himself to a definitive conclusion on the strength of any amount of evidence”*²⁷. While Rayleigh’s view of science and nature may have led him to initially entertain evidence for “spiritual” phenomena, it did not equip him with the epistemic standards by which to judge when such evidence should be scientifically admissible. Rayleigh concludes his final address to the Society for Psychical Research by lamenting *“some of those who know me best think that I ought to be more convinced than I am. Perhaps they are right”*²⁸.

Rayleigh’s Colour theory and the epistemic problem of causality between mind and matter

After his psychical experiments at his Terling estate, Rayleigh encountered another phenomenon which seemed to causally connect the physical and mental domains, which requires human instruments to reproduce, and which relies on subjective testimony as evidence. “Colour” research had been more or less recognized since Young’s Bakerian address to be in some sense a question of “vision”, but this did not make a less fascinating area for scientists drawn to difficult metaphysical questions²⁹. Rayleigh noted the possibility of vision serving as a scientifically accessible causal link between seemingly distinct “worlds”, making it similar to how he conceived

²⁴ Rayleigh (1919a: 644)

²⁵ Agar (2012: 15)

²⁶ Rayleigh (1919a: 652)

²⁷ Strutt (1924: 363)

²⁸ Rayleigh (1919a: 653)

²⁹ Crone (1999: 88) argues that with Young’s address the science of “colour” became the science of “colour vision”

of “psychical” phenomena. In his presidential address to the British Association in 1884, he contends “The predominance of the sense of sight as the medium of communication with the outer world brings with it dependence upon the science of optics”³⁰. The “worlds” to which Rayleigh is referring are the “inner” domain of qualitative visual experience, and the “outer” system of physical causation involving light and matter. Explaining how both “worlds” causally interacted, included speculating on the extent to which they might both be subject to the same natural laws, which introduced similar metaphysical quandaries to those of psychical research.

Rayleigh performed his first research into “the theory of colour perception” during the 1860’s and 70’s, employing the experimental techniques of James Maxwell and Hermann von Helmholtz, which focused on quantifying the principles of colour “mixtures”³¹. By this time the models for predicting colour mixture, the mapping of colour space, the anatomical systems of the human eye, and the extra-ocular systems for processing colour vision, had yet to be distributed between the disciplines with which we conventionally associate them in the 21st century³². Thus, while Rayleigh presumed there were fixed laws of colour mixture, hence his efforts to define them, it was unclear in 1871 quite what the laws of “colour” mixture were laws *of*. What category of system did they govern, what was the causal framework of that system, and what scientific practice should be authoritative in defining it? Were the laws of colour physical laws, physiological laws, or mental laws? Should all these systems be subject to the same class of law?

Young, Maxwell and Helmholtz’s influential model of trichromatic colour vision had ended most challenges to Newton’s ambiguous theory of light and colour, by uniting a trichromatic model of colour perception with a continuous, non-trichromatic Newtonian light spectrum³³. However, this hybrid trichromatic solution was contested in the late 19th century by Ewald Hering’s opponent process model which, among other things, generally tried to physiologize visual colour systems which Helmholtz’s trichromatic theory classed as “psychological” in nature³⁴. In one sense Newton had moved colour from matter to light, but where colour was phenomenological, the categories of its various causal operations remained contested. Debates raged in 19th century Germany over which colour phenomena were

³⁰ Rayleigh (1884: 336)

³¹ Rayleigh (1871: 79, 83)

³² Johnston (2001) argues colour was contested between disciplines until, and during, the American Committee on Colorimetry in 1922

³³ Mollon (2003)

³⁴ Turner (2013 [1994]: 4)

psychological or physiological, which is to say, to what extent “colour” should be studied as a system of mind or matter. In Britain, this question remained problematic for Maxwell well after his most pivotal contributions to the “Young-Helmholtz” model, indicating the extent to which the issue had not been resolved by this theory. Maxwell’s beliefs about the possibility of ever integrating the human mind and its colour qualia within a physical model of nature are revealed when he categorically separated these classes of phenomena in a lecture outlining the state of colour vision to the Royal Institution in 1871. Here Maxwell argues that any potential theory of colour-vision “cannot compare” the mathematically correlated events of “light falling” on the eye and “the sensation of colour”, because “these two things; they belong to opposite categories” and “the whole of metaphysics lies like a great gulf between them”³⁵.

Rayleigh’s first work on colour theory, published the same year as Maxwell’s address, was largely a continuation and commentary on Maxwell, a point that Rayleigh recognized himself³⁶. Rayleigh’s initial experiments in the 1860’s and 70’s involved additive mixture with light as well as subtractive mixture with spinning colored disks. In the latter case, the only evidence for the “mix” and its resultant colour was Rayleigh’s own subjective mental experience. Here is where we see methodological overlaps between Rayleigh’s early psychical and colour research. Rayleigh’s paper used his private mental states to theorize about the principles of both physical light and human anatomy, assigning a high epistemic value to a class of evidence that caused him much trouble in his psychical research. Rayleigh noted that his tabulations of the “colour equations” taken from his spinning disk experiments were not identical to those of Maxwell, and that this was likely because of differences in sunlight on the days of experimentation, or resulted from differences in the faculties of colour discrimination between the two scientists. Rayleigh stressed, however, that he was in no way suggesting that the tabulation differences resulted from any difference in skill toward “conducting the experiments” between himself and Maxwell³⁷. At the end of the paper, when he takes stock of what his experiments might imply about the principles of colour relationships, Rayleigh discusses the epistemic problems of inferring objective rules of colour mixture with evidence from subjective phenomenological colour experience. Rayleigh targets the subjectively derived laws of colour mixture proposed by “painters”, who Rayleigh

³⁵ Maxwell (1871: 276)

³⁶ Rayleigh (1871: 80)

³⁷ Rayleigh (1871: 81)

contends “*have worked themselves into the belief that it is only necessary to look at the colours in order to recognize the compound nature of green*” to which he responds “*my own prejudice would be on the other side*”³⁸.

In his 1871 paper, Rayleigh was firmly opposed to yellow being considered a “primary” colour, because it conflicted with his preferred trichromatic model of colour vision, which rested on the proposition of a physiological system consisting of three primary sensations approximate to the colours red, green and blue³⁹. The notion that yellow was a primary colour obstructed this system, but disproving the idea proved difficult because, like other primary colours, yellow cannot be phenomenologically split into its component colours. Rayleigh states that “*For everyone, I imagine, sees in purple a resemblance to its components red and blue, and can trace the primary colours in a mixture of green and blue*”, but that for yellow, “*we are unable to analyze the compound sensation*”⁴⁰.

Rayleigh’s concerns about allowing the qualitative principles of yellow derived from introspection to supplement physical theories of light and colour revealed a deeper problem. If colour experience is a property of mind, how could its causal principles be subject to those of physical light? Worse still, how can subjective testimony of mental colour experience be relied on to reveal objective laws of “colour” insofar as they may be conceived of as physical laws? This assumption wouldn’t be a problem if mental activity was presumed to be subordinated to physical laws, but if that were true, then mind and self-caused free will could not exist. How could the human intellect be rationally conscious of itself and of nature without such a mind? How could science proceed without such an intellect? As Rayleigh mused to his son “suppose I discover a new mathematical theorem. Has it never been apprehended by any mind before? To me that is inconceivable”⁴¹.

Concerns about the epistemic legitimacy of mental introspection and its ontological implications were unlikely to have been the only epistemic anxiety informing Rayleigh’s reaction to “painters”. The relative skill between artists and scientists in manipulating colour remained an open question until the turn of the 20th century, with the experimental psychologist and student of Wilhelm Wundt, August Kirschmann, arguing that artists had the upper hand and even that experience in certain sciences

³⁸ Rayleigh (1871: 85)

³⁹ Ibid.

⁴⁰ Ibid.

⁴¹ Strutt (1924: 361)

may diminish the accuracy of colour perception⁴². Could a superior capacity to predictably manipulate colour not be taken to indicate superior knowledge of the principles governing colour? What could it mean then for scientists attempting to standardize the physical laws of colour, and indeed those relying on assumptions about those laws, if artists remained the better equipped to manipulate the phenomena? In this respect, Rayleigh's anxiety about the appropriate role of artists in determining colour principles concerned the question of skill, and as such, was particularly relevant in the mid 19th century scientific context. During this period the ever increasingly sought after epistemic value of precision, which accompanied the rise of laboratories, extended to the skills of the experimenter as much as to numeric precision and well-calibrated material instruments⁴³. In the case of Rayleigh's first work on colour, he was the experimenter but also used his own colour perception as an instrument. As such, Rayleigh's personal capacities were doubly relevant to the question of precision in his early efforts to begin standardizing a British scientific convention around "the theory of colour perception"⁴⁴. Thus, rather than a peripheral concern, the epistemic problems of mind, matter, and perception were central not only to Rayleigh's efforts to define the principles of colour mixture, but his methodological approach to experimenting with colour more generally.

Separating mind and matter in colour science?

All Rayleigh could conclude from his efforts in 1871 to describe the principles of colour mixture was that "*there is much still obscure in the mutual relations of the colours*"⁴⁵. However, he ends with the argument of a pragmatist by contending that "*difficulties such as these should make us all the more determined to build our theories of colour on the solid ground that normal vision is threefold*"⁴⁶. In his later publications, Rayleigh shows less desire to defend a single causal theory of colour, consistent across physical and mental domains. In a paper in 1881, in which he returns to the work he began in 1871, Rayleigh abandons all questions of colour primaries and introspection into colour phenomenology. He instead focuses his discussion on his development of a new instrument, which he hoped would make Maxwell's quantified colour experiments more portable. Rayleigh's 1881

⁴² Staubermann (2003: 759)

⁴³ Gooday (1990); Olesko (1995)

⁴⁴ Rayleigh (1871: 79) saw his work as an initial step in "bringing the theory of colour perception...into the textbooks" of English science. He believed Helmholtz had accomplished this in Germany.

⁴⁵ Rayleigh (1871: 85)

⁴⁶ Ibid.

paper also discusses attempts to replicate and explain different forms of colour blindness and continues his earlier efforts to use chemicals and light to remove the dastardly yellow rays which plagued his efforts a decade prior to theorize about “colour perception”⁴⁷.

The experiments that Rayleigh published in 1881 coincided with his time heading the Cavendish laboratory, whose students he found useful in his new round of colour experiments. Rayleigh believed “it is advisable always to take simultaneous observations from some practiced individual, whose vision may be treated as a standard”. His desire to set “standards”, and work with “practiced” subjects alongside increasingly complex hardware, reveals striking similarities to Wilhelm Wundt’s efforts to establish conventions of precision in his experimental psychology at Leipzig in the 1880’s. Wundt’s efforts also involved increasingly sophisticated and portable hardware and avoided experiments on “inexperienced persons” in order to standardize access to “the generalized mind”⁴⁸.

Rayleigh’s experimental practice in 1881 supports Benschop and Draaisma’s case that not all the new forms of precision which came to mark 19th century science “went into the hardware” but also into the experimenter’s practices in managing themselves and their human instruments⁴⁹. It is remarkable that this research done at the Cavendish Laboratory, which is conventionally understood as an institution of physical science, appeared to borrow so much from German experimental psychology. It is possible that the vector between Germany and Britain in this case was Rayleigh’s familiarity and deep respect for German science and Hermann von Helmholtz’s work on colour vision in particular⁵⁰. However, Rayleigh was also aware of Gustav Fechner’s expression of Ernst Weber’s logarithmic psychophysical law, an instantiation of which became known as “Weber Contrast” which Rayleigh applied to his work on photography⁵¹. Fechner’s work also had an influence on Wundt⁵².

⁴⁷ Rayleigh’s (1881: 542) first stated objective is to “seek an absorbing agent capable of removing the yellow of the spectrum”

⁴⁸ Benschop et al. (2000: 19)

⁴⁹ Benschop et al. (2000: 1)

⁵⁰ Rayleigh (1871: 85) spoke favourably of Helmholtz from early in his career. He advanced German language classes in British schools to keep up to date with “annual reports and abstracts issued principally in Germany” (Rayleigh 1884: 351, 353)

⁵¹ For more on Fechner’s law see Staley (2018). For “Weber Contrast” see Kingdom et al. (2009: 270). Rayleigh (1911: 67) references this law directly and takes it to “represent the facts fairly well”

⁵² Rieber (2013: 35)

In his last paper on colour, in which he discusses the colours produced by the microscopic anatomical structure of certain animals, Rayleigh states “*it is singular that some of the most striking and beautiful optical phenomena should be still matters of controversy*” and ends by saying “*it must be confessed much still remains to be affected towards a complete demonstration of the origin of these colours*”⁵³. This seems a far cry from the confident colour theorist we saw in 1871. Since that time Rayleigh appears to have developed some doubt regarding the progress science had achieved toward establishing a single law of colour theory that would apply across physical and mental categories at all times. However, this is not the only way in which Rayleigh’s colour research changed after his first experimental encounters with psychical and colour phenomena.

Rayleigh’s papers on colour after 1871 split colour theory between colour vision and light. The laws he expected of “colour” become subordinated to one of those two domains of theory and practice. Rayleigh’s increasing research on “defects in colour vision” discuss “colour” as a shorthand for a physiological process⁵⁴. For the purposes of this research, Rayleigh quantified colour categories, and avoided relying on private introspection of colour relationships as evidence for colour principles at the physical level. Meanwhile, in Rayleigh’s physical science, he ceased to study “colour” as such except as a practical question related to improving the instruments for his true focus, which was the physical properties of light and matter that may be said to *result* in colour. Insofar as subjective colour experience remained an *instrument* for studying light in physical science, Rayleigh’s tireless efforts to refine his instruments increasingly became synonymous with efforts to remove subjective colour entirely⁵⁵.

After his earliest psychical and colour research, Rayleigh experimented with technologies that isolated physical processes from the subjective mental experience of those processes. In 1871, immediately after his first work with colour disks, Rayleigh innovated with printing technologies in order to diminish the cost of replicating diffraction gratings, and thus

⁵³ Rayleigh (1919b: 584, 596)

⁵⁴ Rayleigh (1890a: 380)

⁵⁵ Rayleigh (1890b: 385) argues that the “want of achromatism was prejudicial to the appearance of the image”. Interestingly, he notes that this did not necessarily diminish experimental “accuracy”, as long as the scientist performing the experiment had “experience in observation”. Presumably the presence of colour led to diminished accuracy if the experimenter was not experienced in understanding the epistemic problems introduced by phenomenological colour.

increase their use in studying the physical principles of light⁵⁶. Diffraction gratings granted access to achromatic interference fringes, which allow more information about physical light to become independent of subjective colour experience. In 1896, Rayleigh describes an interference refractometer, which would permit the determination of the refractive indices of gas and liquid, again without relying on colour experience⁵⁷. Where colour still remained inescapable as a tool for researching the principles of light, Rayleigh spearheaded the use of photographic plates to capture states of light directly, again bypassing the scientist's own colour experience⁵⁸. A similar trend can be seen in his work on sound, another science which had historically relied on perception. In the 1880's Rayleigh used "sensitive flames" to research sound waves independently of the experience of hearing⁵⁹. Sensitive flames had previously been used in psychical research in the hope of revealing other forces beyond the human senses⁶⁰.

Conclusion

One way to understand how Rayleigh's scientific practice, and attitudes toward science, changed throughout his career is to recognize that, as an Anglican who believed in free-willed consciousness, but also in an entirely law-bound and regular physical universe, Rayleigh was in a difficult situation when trying to scientifically explain psychical and colour phenomena, which both causally linked mind and matter. Bearing this interpretation in mind, it is worth returning to Larmor's statement that Rayleigh's psychical research led him to "inhibit discussions of problems depending on disconnected and shifting hypothesis".

The metaphysical relationship between matter, mind, free-willed causation, spiritualism and colour perception were problems resting on both disconnected and shifting hypothesis in 19th century Britain. Rayleigh tried to remove the epistemic paradoxes surrounding human perception from his psychical research. However, because psychical research specifically

⁵⁶ Rayleigh's (1871: 81) first colour mixing experiments occurred in July. He embarked on his diffraction grating printing experiments in the following "autumn" and "winter" months (Rayleigh 1872a: 157).

⁵⁷ Rayleigh (1896)

⁵⁸ Rayleigh (1897)

⁵⁹ Rayleigh (1879: 407) stresses that the sensitive flame was valuable to his research into "loops" and "nodes" precisely because it granted him access to inaudible waves. "The Flame is affected where the ear would not be affected".

⁶⁰ Noakes (2004)

targeted phenomena at the causal bridge of physical law and free-willed mind, he found it impossible to do so. This led him to abandon active work in the field and remain permanently agnostic toward any amount of evidence supporting genuine spiritual agency in nature. This can be seen as an effort to direct his scientific attention away from experiment and theory which incorporated causal connections between mind and matter.

When Rayleigh encountered similar problems in his colour research, he could not simply doubt evidence for the existence of “colour”. His solution was to become dualistic about the mental and physical aspects of colour, resulting in efforts to separate them wherever possible. In his research into spectroscopy and optical science, Rayleigh worked to remove human perception and phenomenological colour from his laboratory work on light. He continued to research “colour” in terms of physical light or the physiology of colour blindness, but moved away from theorizing about any objective aspects of “colour” based on subjective introspection into phenomenological colour experience.

Closing thoughts

It is not the purpose of this article to suggest that the developments in question were wholly contingent on the context I have presented, or to second-guess the theoretical and methodological decisions of Lord Rayleigh. However, a critic may still contend that science simply explains reality, that scientists like Rayleigh also believed that science explains reality, and that to presume that Rayleigh developed physical scientific theories or practices to accommodate psychical or spiritual ideas is misinformed anachronism at best. However, the premise of my argument is entirely consistent with this view, and my conclusions do not conflict with it.

My argument rests on the assumption that if a person believes that science tells us about reality, and if this person established scientific conventions for exploring and describing reality, then what their founder(s) believed about the reality that their practices were designed to explore and describe is relevant to those conventions. Following from this premise, I presented evidence to support the view that Lord Rayleigh initially believed that laboratory research could reveal “spiritual” realities, but that this changed after his efforts to do so. If indeed Lord Rayleigh believed that what science can and does reveal is determined by reality, then surely it is reasonable to consider that a change in what he thought science could reveal involved a change in how he understood the reality it was *intended* to reveal? Surely

any such change in his beliefs about the type of reality we inhabit, and the capacities of science to explain it, could have affected his scientific practice if he intended for those practices to explain reality? Viewing things this way, my discussion of Rayleigh's psychical and early colour research simply recognizes the evidence that his understanding of the capacities of those sciences, and the nature of the reality they were intended to explore, changed. I then consider how this change in his understanding of science and reality may have affected his later scientific work involving colour.

At the very least, what can be taken from my argument is that reliance on evidence from subjective experience posed problems for both psychical and colour research. For various reasons, then, it would be unsurprising that efforts were made to remove subjective colour in the increasing push for greater experimental precision in physical science. However, lingering skepticism may yet remain about my interpretation of Lord Rayleigh and perhaps that is a good thing. Rather than a destructive process, I believe the debate about how to view science in history should be seen as a constructive exercise. On the one hand, it keeps scientists aware of the philosophy and history upon which their preferred scientific theories and practices may be genuinely contingent. On the other, it keeps historians and philosophers skeptical about their own unquestioned practices and motivations, and forces more fidelity in examining the scientific theories and practices that they historically and philosophically contextualize.

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As Jim Grozier mentioned in his introduction to the open meeting two speakers – Cormac O’Raifeartaigh of Waterford Institute of Technology, and Isobel Falconer – had recently published papers on their topics, and have kindly provided links to them rather than writing duplicate articles.

These are:

<https://arxiv.org/ftp/arxiv/papers/1608/1608.01520.pdf>

<https://physicstoday.scitation.org/doi/10.1063/PT.6.3.20181030a/full/>

- Editor

Robert Pohl: the Father of Solid State Physics

Colin Hempstead**Prologue**

As I have grown older I have collected a set of facts, experiences and memories; I have accreted prejudices some of which hardened with time but many were sloughed off with age. Now at 82 I must remember that many ideas come and go, and sometimes they return modified as techniques change – perhaps ‘big data’ and ‘algorithms’ and computer programs presage the final success of Baconian science. Or, perhaps, the ‘age of curve fitting’ will dawn.

In my first post-degree years I worked as a physicist in Research and Development helping to design infra-red optical systems for guided missiles, other lab jobs were aimed at some properties of graphite in reactors and developing semi-conductor devices. When I taught physics I used historical themes in my classes. Eventually, under the belief that engineers and scientists required to be ‘liberalised’, Teesside Polytechnic, now a university, paid me to teach my hobby, the history of science. My UCL Masters Degree allowed paradise to arrive.

I thought a PhD would help, and since I had no idea how long I would be allowed to teach a hobby I chose a subject that would allow me to return to teaching physics. The subject I chose for a PhD was a ‘History of Semiconductors’.

As I searched bibliographies in 1972 – tedious then – a number of papers written by Robert Pohl and his associates from 1920 to 1939 surfaced. Clearly a visit to Göttingen was called for, if there were any archives of Pohl's I had to see them. In particular I wanted to know why Pohl changed his research interests after 1919.

Influences

My own experiences indicated that life was episodic, uncontrolled and 'chancy'. However, I had come under the influence of two lecturers at UCL, the pragmatic rationalist Laudan and the temperamental anarchist Feyerabend. To the influence of these men I have to add the work of Karl Popper and Imre Lakatos.

These gentlemen were poles apart in their views, and all were excellent teachers, but all were philosophers in essence. Their narratives were produced in order to persuade others to agree with their views – they all had their favourite philosophies.

Preparations

A letter from Göttingen in the spring of 1974 brought me the news the Pohl was still alive at c90 years of age. I decided to arrange an interview with him and met him in Krefeld, in July 1974.

I was concerned that my history should be based on solid original testaments. Two living physicists, Alan Wilson and Neville Mott, who had known Pohl, were still alive in 1974, and were interviewed. Others corresponded with me and I used their memories to check the accounts.

I discussed the idea with other historians at Teesside. In general the opinion was sniffy. Indeed one colleague considered real history derived from texts only. But some of them thought that I wasn't really one of them! However, I had some supporters and conversations led me to send my interviewees some questions to allow them to give their view of their own history. This technique turned out to be fruitful.

A more significant problem 42 years ago was that of age. Could any old man's memory be worthwhile as a source? I tested that by looking up 'the Sperrin', a plane I first saw in 1955. The picture the web came up with was

the very plane I remembered. Now I am able to say that many of the memories of the old are OK, providing the interviewer keeps out of the discussion, and does not push answers with personal agendas.

With my memory confirmed, I believe that Pohl gave me a 'true' biography. Many of his claims were underlined by his publications and in letters from a few of his younger associates.

The Interview

During the late morning of the 25th July 1974 I met Pohl in Krefeld. He sat at a table, wearing a suit and pullover, although room was warm. I now recognise that age needs warmth. Between us there were some journals and physics books. His voice was firm, loud and clear, he mainly used German and apologised for his poor English. From time-to-time he interjected, 'Do you understand?', and I usually did!

Before Pohl spoke to me I had been impressed by Laudan's "research traditions" and Lakatos's "research programs", although I was unable to jump onto either idea as my own bandwagon. In 1930/34 Bernhard Gudden, a younger colleague of Pohl's, in lengthy reviews of the 'state of the art', provided the first, general classification of solids. Now the class of semiconductors was generally agreed. This information was in the public domain and it led me to fit Pohl in a Lakatosian "research program". His answers to my questions shook my confidence in the opinions of these 'world-class' philosophers.

For example, I had slotted Pohl in a 'narrative' within his time, his knowledge, and his publications. However, he told me he had intended to continue with the work he had developed before the First World War. None of his publications touched on the things that took him to particular studies.

Pohl's approach to physics and his own work did not seem to fit any philosophical views of science and its method. His answers to my questions were often surprising. Clearly if he subscribed to a philosophical tradition he was a follower of Galileo.

By the time Pohl began his researches a considerable body of 'facts' had been assembled on semiconductors. However, very few works on the history of the solid state have been produced. In 1919 the Old Quantum Theory had been applied to substances such as selenium, with little success.

In retrospect it can be seen that the careful, ingenious experiments characteristic of Pohl *et. al* were seminal. Their use of good crystalline specimens enabled them to underline the significance of small amount of impurities in materials such as diamond and alkali-halides. Very good pure crystals into which known impurities could be added were *sine qua non*.

Although many attempts had peppered the 19th century scientific world with descriptions and theories of electricity none was able to cover everything. Indeed there was no satisfactory general classification of solids.

Pohl began:

“Like old people I shall start, with the history of all this and tell you something how all this began. This will make you understand best how I came to do this sort of work.

After I had finished my studies in 1906 I worked mainly in Berlin and during the holidays in Hamburg. In Hamburg I mainly worked under Bernhard Walter; I worked in the field of X-rays and did experiments on the diffraction of X-rays. In Berlin I was mainly concerned with the external photo effect in alkali metals. I mainly worked with Peter Pringsheim. Working together we found the so-called selective photo-effect of the alkali metals. We examined the relationship between the number of the electrons and the wavelength. We reached the conclusion that the selective photo-effect on the surface was really the effect of single absorbed atoms. Then followed the terrible First World War. My friend Pringsheim was in Australia as a guest of the BA. He was interned, (behind barbed wire). I was called up as a military engineer and was given the rank of colonel although I had never been in military service before.

I had to do research into spark telegraphy (radio-telegraphy). I also worked on the production of un-damped electrical oscillations. In Berlin in the Physical Colloquium I was the special referee for the work of Philipp Lenard, whose work dealt mainly with Phosphorescence. Thus I knew Lenard's work very well. His papers were difficult to read since he wrote enormously long sentences and the most important facts were always in the footnotes. All this became very significant in my work.”

Towards the Solid State

“In 1919 I started in Göttingen. I had been given a chair in 1916 but I was in the army so I couldn't accept it at the time. I intended to continue my work concerning the selective photo-effect. Now follows something rather strange. It was impossible to produce a high vacuum in Göttingen, there was no liquid air available. It was impossible to obtain liquid air in Göttingen in 1919. I was half annoyed and suggested, half in joke, that we should take the exact opposite – that is, a solid body, We would introduce single atoms into the solid body and try to demonstrate the selective photo effect on these single atoms. I didn't believe that electrons liberated by light could run through the entire crystal but would only move forwards and backwards thus increasing the dielectric constants. The experiment worked out very well, and we managed to increase the dielectric constants. We were able to prove it by the method of undamped electrical oscillations, by producing a difference tone. When we tried using other phosphors, Balman's luminous paints, calcium sulphide etc., here we no longer got the change in the dielectric constant - it remained a special property of ZnS. Here we found properties similar to selenium, that is the irradiation with light produced a change in the electrical conductivity. You know that the phenomena using Se are very complicated. The same applies to the phosphors and in addition the phosphors were only available in the form of a powder; first thought was that we had to try and get decent crystals.

We started using diamond at about this time, and we found to our surprise that it showed the same photoconductivity. One small diamond, a battery of a few hundred volts and a single match were sufficient to show photoconductivity. Using the diamond we found that if electrons are liberated the absorption spectrum changes towards the long waves. We called this excitation using the term by analogy with phosphors. If you excite phosphors by light to cause it to emit light it also absorbs light at longer wavelengths. Thus after the liberation of electrons a different absorption spectrum appears. We carried out a number of experiments on diamond. It was extremely difficult to obtain a pure diamond. I travelled round various diamond merchants and borrowed diamonds, but unfortunately none of them was pure, fortunately the one we had used before had been a particularly good specimen.

I now remembered some of the work Röntgen had done, in Munich, and Joffe, one of his students in Petersburg. Röntgen had irradiated rock salt with X-rays and had found that it showed photoconductivity. We said to ourselves we would like to explain Röntgen's work in a similar way to that in diamond. Röntgen's work was extremely long with not very clear passages in it.

In the process of absorption electrons are liberated and a new spectrum is formed with longer wavelengths etc , Initially we used natural NaCl etc. Afterwards we said to ourselves that we would try the same experiment with other alkali halides, KBr, KI etc. We took a physical chemist, Izaak Kolthoff, into our institute as a guest, his task was to find out how one could produce crystals from alkali halogens, he developed a method to produce these crystals and it was used to a large extent on an industrial basis, In Germany too we have got a small factory which produces these crystals according to this method, it has become entirely accepted and commonplace to produce crystals in this fashion. Since then most papers written in Göttingen were done about alkali halogens in their crystal form. Not only about photo-conductivity but also about the elementary photo-electric processes in photography the specific ultra violet absorption which determines the refractive index. In all the work the crystals used were alkali halogens.

Pohl, Franck and Born were all Professors in Physics in Göttingen in the 1920s and 30s and Born was influential in the development of matrix mechanics. I learnt from Pohl that experimental physics was his *metier*:

“I suppose I should be ashamed to admit this, but I was never very interested in theory I was always much more interested in experimental practice.”

For his work this was a problem, as he suggested to me that theories come and go, but that the (scientific) facts remain.



L-R Reich, Born, Franck, Pohl, c. 1920

“As a consequence our work was not noticed all that much initially, for we did not relate at all to the atomic model of Bohr. For after the First World War only those things that related to the atomic model of Bohr were taken to be related to physics. There was the marvellous work of James Franck but of course James Franck in spite of the fact that he was a friend and a student at the same time as myself did not want to know anything about our work for it was not related to Bohr's atomic model. This continued until Mott, Seitz etc came along and said stop! there's something of relevance.

With the newly appointed Franck and Max Born, Pohl started what came to be known as the splendid era of Göttingen as one of the world's centres for atomic physics ... The students at Göttingen had the choice among Pohl, the master of experimentation and demonstration; Franck, the genius at reconciling experiments with theory; and Born, the absolute theorist.”

[Complete Dictionary of Scientific Biography. 2008. Encyclopedia.com.](#)

A Costly Error

Of course, eventually his work and that of his colleagues was recognised and is very significant in the history of the physics of the solid state. However there was a glaring omission in Pohl's published work. Why did he not use the Hall Effect? In 1873 Maxwell's *Electricity and Magnetism* was published: and Willoughby Smith 'discovered' photoconductivity in Selenium. Existing theories were unable to account for such a simple property of a solid. Nevertheless the expectations were that field theories – the 'bee's knees' – would be able to describe photoconductivity. However, there were many omissions in Maxwell's book and the work was hard to understand. I wondered why Pohl turned to metal halides; his answer was another nail in the coffin of my 'belief' in philosophy of science. In the interview Pohl explained the reason:

“Bernhard Gudden was a student of Rieker, but he hadn't finished his course yet. In July 1919 he did his final examination under my guidance. I was very much impressed by him and I gave him a position straight away as one of my assistants. We started to experiment together and the work I described previously was the first work we did together. Now as I have said before, phosphor showed the same difficulties as Se and on top of that it appeared as a powder.

Following a suggestion of Gudden we tried to measure the Hall Effect of the diamond. We positioned the diamond within the poles of a large electromagnet; however we forgot to fasten down the poles, connected up and immediately our one usable diamond had been pulverised. This was my own fault for I hadn't been careful enough. I made the mistake to say that I don't want to hear any more of the Hall Effect, one diamond has been spoiled and that's enough. So we made the mistake of discontinuing those measurements with the Hall Effect, which later on were carried on in America and were quite conclusive, and they became very important in constructing the transistor. This is the real reason why we changed over to alkali halogens.

In the 1920s American experimentalists used the method to such effect, that in 1931 A. H. Wilson was able to produce his foundational paper regarding semi-conductors, and in 1948 a few Bell Labs scientists developed the first true transistors.

In 1938 a crystal analogue for a triode was made by a colleague of Pohl.

“The first gentlemen to become interested in this work were in England. They were Sir Neville Mott, then in Bristol now in Cambridge, John Mitchell, born in NZ in the same village as Rutherford who is now Professor of Physics in Charlottesville, USA and F Seitz now the President of one of the Universities in New York. This was when they recognised the relevance of the work we were doing. In 1964 I was invited to the USA when I celebrated my 80th birthday, they celebrated it too. This is how all this started.

From Physics to Biology

I asked him: “How did your work with solids relate with other work in physics?” He replied:

“My answer might be strange. It played an important part in biology. You know that Vitamin D comes into existence if cod liver oil is irradiated. Professor Adolf Windaus who worked in Göttingen at that time tried to find out exactly what Vitamin D was. One knew that it comes into existence if ergosterene is irradiated with ultra-violet light. Chemically one couldn't detect any changes. After this I went skiing with Windaus in South Germany. We talked about this and I said I intended to examine the ultra-violet absorption – it looked to me very much like the excitation of the diamond, etc. I first irradiated cholesterol with ultra-violet light. Windaus gave me some preparations made of cholesterol and I tried to detect the excitations. I discovered selective absorption spectra and I told him 'Herr Windaus; this is not the cholesterol at all, but a certain impurity in it that has this particular absorption spectrum.' This work has been published in the Göttingen Academy of Sciences. By means of these absorption spectra one very soon found that it wasn't cholesterol at all, but very small quantities of ergosterene that were mixed in. In the same way that Cu is present in very small quantities in ZnS here we found very small quantities of ergosterol in cholesterol, and thus Vitamin D could be produced in a pure culture. Thus the work done with the excitation of phosphors led to the discovery of a very important fact in biology.

Another technical thing was when we examined the optical behaviour of some of the very thin alkali-halogens. Layers of alkali halogens steamed is open* for example rock salt and thus reduced

their reflection. A PhD student of mine, Bauer, had found and suggested this method. I wrote into his thesis that this is technically correct. One can think for example of the method used on visual optical glass, a procedure referred to later on as 'blooming'. So you see this too was a consequence of our research.

Towards the Transistor

“Baedeker was one of the first officers killed during the First World War. He was really the first discoverer of the fact that bodies without metallic binding can have a current of electrons running through them and produce the Hall Effect even if there is the sign of the holes. He had found this out already, but I didn't know about his work, *Electronic conduction in crystals without metallic bindings*.

In 1933 in a public lecture I said that it is possible to replace the vacuum tubes in the radio by crystals. I can show you certain experiments which led to this conclusion. It proves the possibility that the valve can be replaced with crystals. In 1938 we demonstrated that. it was really possible. It was another three to five years that Bardeen and Brattain came up with a similar result. I know all three of these gentlemen very well indeed. We used to sit together in Göttingen and drink wine. They have earned the merit of making technical use of the fact of electron conduction. The fact that it was possible in principle was shown by us first of all. Bardeen, Brattain and Shockley – those were the three. Shockley with his fast cars. Of course he has acquired a number of enemies because of his opinions on race relations. An extremely clever man this Shockley. But the very important thing in this book is about Baedeker, normally history starts with some theories or something or other. But this proves that all the experimental work had been done by Baedeker.”

“We got all excited so we made contact with Pohl, [and] we [went] to see him. I can't remember, [1938] but we organised a conference on solid state physics in Bristol and invited him to come along with others. But Pohl I would call the father of solid state physics, I don't know any earlier systematic investigation of anything in this area.”

From interview with Neville Mott, 1/11/74

War Work

In WW1 Pohl was asked to produce continuous radio-waves for communications purposes. After the war when he rejoined Göttingen he faced the financial aftermath of the war, and the work he intended to do was impossible to carry out. Hence he began to study solids; Since the Hall Effect was banned by Pohl, nevertheless directly his work during WW1 has vanished in later developments in electrical engineering and latterly in optical systems. However, his war-work in telecommunications provided instruments which he used to study semi-conductors. It employed a super-het system to replace the Hall Effect.

Between the World Wars Pohl remained in post and he retired in 1944.

When asked about guided missiles, he replied:

“The idea of guided missiles was already in existence: during the first world war when the same office that employed me as a military engineer employed a Swede who was trying to devise an electronic means of directing missiles. In the Second World War, of course, these attempts were realised. As far as I know steering by light was not tried out during the war. I never worked for the military, my old teacher Emile Warburg took care of that. He used to say that after the war there will be peace again and then one has to work together with those who used to be the enemies and that is why physicists should keep out of all this.”

Perhaps his work was foundational in the development of modern devices which depend on deep scientific knowledge of the properties of solids. There is, perhaps a sad irony that warfare catalyses the use of pre-war knowledge to make weapons and medicine, but little of the truly new is invented *ab initio* during conflicts.

* steamed is open (sic) - this is clearly incorrect - *editor*

A partial history of cosmic ray research in the UK

*Alan Watson**School of Physics and Astronomy, University of Leeds***Introduction**

One of the puzzles confronting physicists in the late 19th Century was why residual charge on a body leaked away even when surrounded by heavy shielding. The effect seems to have been first spotted by Coulomb in 1785 when, during his inverse-square law studies, he noticed that the charged gilded-metal spheres that he had suspended by silk threads discharged unexpectedly rapidly. The discovery of X-rays, radioactivity and the electron in the 1890s did much to clarify understanding of ionisation and led C T R Wilson to proposeⁱ that '*the continuous production of ions in dust-free air could be explained as being due to radiation from sources outside our atmosphere, possibly radiation like Röntgen rays or cathode rays, but of enormously greater penetrating power*'. To test his hypothesis Wilson took an electroscope into the Caledonian Railway tunnel near Peebles but found the rate of discharge to be essentially the same inside as out. He concluded, as had the German scientist H Geitel, that ionisation production, at a rate of about 20 ion pairs cm⁻³ s⁻¹, was a property of the air itself.

Wilson's initial surmise was vindicated, when Victor Hess ascended in a balloon to 5 km and found that the rate of discharge of his electroscopes increased rapidly as he floated upwards. He inferred that the earth was indeed being bombarded by a penetrating radiation initially known as *Höhenstrahlung* or *Ultrastrahlung*. In 1927, during a meeting of the British Association for the Advancement of Science in Leeds, Robert Millikan introduced the name *cosmic rays*, reflecting his erroneous belief that cosmic rays were photons rather than the nuclei of atoms, an idea that became established only at the end of the 1930s. UK interest in the phenomenon *per se* was relatively slight although a Royal Society Discussion meeting on '*Ultra-Penetrating Rays*' in 1931ⁱⁱ was attended by Geiger, Rutherford, Wilson, Chadwick and other luminaires. At the time the study of nuclear physics dominated the research program at the Cavendish with the cloud chamber⁶¹ being developed as a supremely important tool, particularly through the efforts of P M S Blackett.

⁶¹ Outside of the UK one often reads of the 'Wilson Chamber'. I regret that this term is not common here.

In 1933, Blackett and Occhialini combined Cambridge cloud-chamber expertise with Italian skills in coincidence circuitry, due to Bruno Rossi, to make a ‘counter-controlled’ cloud chamber. With this device they observed cascades of positive and negative electrons just one year after Anderson had discovered the positronⁱⁱⁱ. Studying these cascades became one of Blackett’s interests and this was to have a major influence on studies of high-energy cosmic-rays in the UK post-WWII.

The major British interest was to use this free high-energy beam for what we now call ‘particle physics’. The positron, the muon, the charge pions and the first kaons and hyperons (the ‘strange particles’) were found in cosmic rays using cloud chambers and nuclear emulsions: the pions and the first strange particles were detected in Bristol and Manchester in 1947. The history of this work is well-documented^{iv}. By 1953 accelerators could produce protons of energy sufficient to make pions so that the use of cosmic rays for particle physics was largely superseded.

Extensive Air Showers

In 1938 Pierre Auger and Roland Maze^v, testing an improvement in the resolving time of coincidence circuitry, found serendipitously that Geiger counters, even when placed 300 m apart, discharged in coincidence more frequently than expected. They established that cascades of particles, named *extensive air-showers*, were spread over large distances at ground level and, using the new theory of quantum electrodynamics, they argued that the most energetic primaries had energies of $\sim 10^{15}$ eV. Such an energy is unreachable at accelerators today or in the foreseeable future and an astrophysical thrust developed to discover where and how such energetic particles were produced.

In 1938, Auger and Rossi, both *en route* to the USA, spent time in Blackett’s Manchester Laboratory, then the UK centre for cosmic ray work. Their visits further stimulated Blackett’s interest in cascade phenomena. As a consequence Bernard Lovell and John Wilson, Blackett’s young colleagues, placed two cloud chambers some metres apart and demonstrated that parallel tracks were seen in each chamber^{vi}, a visual confirmation of the inferences from counter experiments.

On 3 September 1939, two days after the start of WWII, Lovell and Wilson were at a radar site in East Yorkshire^{vii}. They noticed traces on the radar screens which the operators attributed to ionospheric effects but which

Lovell and Wilson speculated might be due to the reflection of the radar beam from ionisation trails left by air showers. In 1941 Blackett and Lovell proposed this as a method for shower detection^{viii}. Despite Eckersley^{ix} having pointed out that the electron damping was much more rapid than they had estimated, so reducing the likelihood of detecting anything, after the war Blackett encouraged Lovell to test the idea. The work was carried out at Jodrell Bank where ionisation trails left by Perseid and Giacobinid meteor showers were soon detected. Blackett only discovered Eckersley's letter after the move to Jodrell Bank and Lovell comments⁷ that had it been found sooner radio astronomy work there might never have started. Air shower searches ceased and although their detection by the radar method was mentioned in the case for what is now the Lovell telescope, of the two only Blackett retained interest in high-energy cosmic rays.

After WWII, when the UK atom bomb was being developed at Harwell, the Director, John Cockcroft, encouraged fundamental research to go on 'outside the wire'. In 1950 Bruno Pontecorvo, who had moved there from Chalk River where he had worked with Auger, was asked to establish an array of detectors to study air showers. The first array^x used only Geiger counters, and so lacked the capability of measuring directions. Pontecorvo knew of work at MIT, under Rossi's direction, to develop a liquid scintillator that would allow the times to be determined at which a shower sweeping across an array hit the scintillators so enabling the direction of the incoming cosmic-ray to be measured. The scintillator used was a benzene/para-terphenyl mixture. As para-terphenyl was expensive, Pontecorvo asked John Jelley to investigate the efficiency of the light output as a function of the quantity used. Jelley found that even with no additive, he could detect light from benzene, and from other liquids including distilled water^{xi}, as muons passed through. The light was Cherenkov radiation and this led Neil Porter to develop a large water-Cherenkov detector^{xii} subsequently used effectively for shower studies. Porter's detector is compared (figure 1) with one currently deployed at the Pierre Auger Observatory¹³. Little has changed!

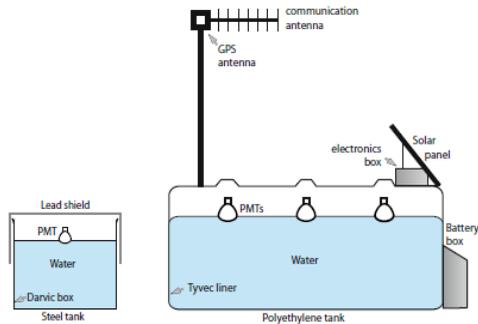


Figure 1 A comparison of the tank (left) developed by Porter¹² and one used at the Pierre Auger Observatory^{xiii}.

Blackett was also interested in Cherenkov light and was the first to calculate the threshold energies for electrons and muons to produce such light in air^{xiv}. He estimated that the light from cosmic rays made up $\sim 10^{-4}$ of the night-sky background and that signals from air showers might be seen in Cherenkov light using the human eye as a detector. According to Lovell, Blackett made an attempt to see such flashes^{xv}. Alas there is no record of his efforts but the idea was taken up by Jelley and Bill Galbraith, also at Harwell, with a more practical approach. With a photomultiplier looking downwards at an upward-pointing search light mirror they observed signals which they were able to attribute to air-showers^{xvi}. Air Cherenkov-light as a method to study showers blossomed in places with more congenial climates than the UK, notably in the Pamirs through the efforts of scientists from the Lebedev Institute. However, Neil Porter developed the technique further after moving to Ireland and one of his former students, Trevor Weekes, played a pivotal role in creating the new astronomy of TeV gamma rays through the detection of a clear signal from the Crab Nebula with the Whipple Observatory in Arizona^{xvii}. Important contributions by Michael Hillas (Leeds), an alumnus of the Harwell team, were used in this study. Currently over 250 sources are known to emit gamma rays of ~ 1 TeV and above with a world-wide effort by ~ 1200 physicist underway to create the Cherenkov telescope array (CTA), a \$300M project. Physicists from Durham, Leicester, Liverpool and Oxford are involved.

Work at Harwell ended in the late 1950s when the airfield was requisitioned for the Culham Fusion Laboratories. The leaders, Ted Cranshaw and Galbraith, move to other areas of physics. Blackett, however, wished to see shower studies continue but in the universities.

A collaboration of cosmic-ray physicists led by Wilson formed, leading to the construction of an array covering 12 km^2 at Haverah Park on the Yorkshire Moors, about 20 miles from Leeds University. Blackett had required that the site be no more than 30 miles from a University so that some of those involved would be able to do their teaching easily. In addition to the Leeds University team, groups from the University of Durham and Imperial College were crucial to the early efforts with the University of Nottingham joining later. Senior people in these four institutions had all been in Blackett's orbit in Manchester.

A prototype array of $4 \times 34 \text{ m}^2$ detectors of a design similar to that of Porter, set out in a star-like arrangement with three 500 m arms, was used to take data from late 1962. I joined the project in October 1964 and helped construct the 12 km^2 detector. In all over 200 water-tanks, each containing 2.7 tonnes of clear water, drawn from a bore-hole near the site of the battle of Marston Moor, were deployed in small clusters over the area. Key measurement of the distribution of arrival directions were made^{xviii} and the energy spectrum was shown to extend to nearly 10^{20} eV ^{xix}. An important method, targeted at discovering the mass of the primary particles was developed^{xx} that has been of great use in the Pierre Auger Observatory, the giant successor of the Haverah Park project. This was needed as one of the conclusions from that work was that at $\sim 10^{20} \text{ eV}$ the flux of particles was only ~ 1 per km^2 per century.

A limitation of detectors on the ground is that the energy of the particle responsible for creating the shower must be estimated using predictions from models of hadronic interactions extrapolated from the lower energies available at accelerators. Fortunately the shower, through the same process that creates aurorae, lights up the sky with isotropically-emitted fluorescence-radiation enabling a calorimetric estimate of the primary energy. This technique has been developed particularly by teams at the University of Utah^{xxi} and is now a key part of the instrumentation at the Auger Observatory in Argentina and the Telescope Array^{xxii} operated by a US-Japanese collaboration in Utah.

The Auger Observatory was constructed in Argentina by a consortium of over 400 scientists from 17 countries. Jim Cronin, who won the Nobel Prize in 1980 for the discovery of CP Violation, and I initiated the project in 1991 and led it for many years. The Observatory covers 3000 km^2 and is shown in figure 2. One of the most important results is the finding that above $8 \times 10^{18} \text{ eV}$ there is a highly significant anisotropy in the arrival

direction of cosmic rays^{xxiii}. This is illustrated in figure 3. The observation, significant at 5.2 sigma, establishes that cosmic rays above this energy come from outside of our galaxy, an idea first put forward over 50 years ago. The result was hailed in *Physics World* as a ‘Top Ten’ breakthrough for 2017.

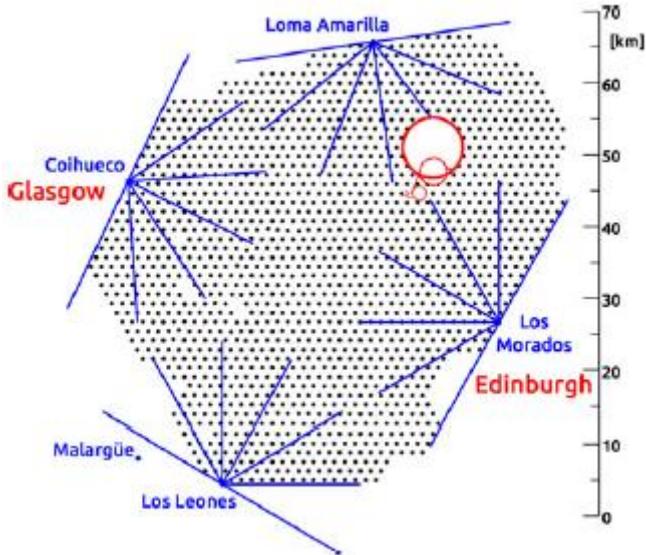


Figure 2: The layout of the Pierre Auger Observatory near the city of Malargüe in Mendoza Province, Argentina. The $1600 \times 10 \text{ m}^2$ water-Cherenkov detectors are overlooked by 24 fluorescence telescopes. Each dot represents a water-Cherenkov detector (figure 1). The fluorescence telescopes are at the blue radiants (Los Leones etc.). The size of the array can be compared with the accelerator complex at CERN (circles) and by the separation between Edinburgh/Glasgow (~40 miles). The area is similar to that inside the M25.

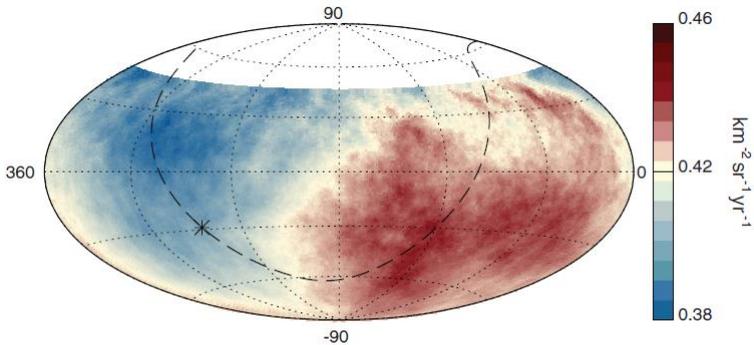


Figure 3: The plot shows the fluxes of particles above 8×10^{18} eV in Galactic coordinates smoothed with a 45° top-hat function. An asterisk marks the galactic centre and the galactic plane is indicated by a dashed line²⁰.

The work of UK physicists over ~80 years, particularly influenced by P M S Blackett, has contributed greatly to successful work in the field of ultra-high energy cosmic rays. A popular account of the work at the Auger Observatory and at its Northern Partner, the Telescope Array, can be found in ‘Meet the Ultras’ in *Physics World*^{xxiv}. A history of work on Extensive Air-Shower was written^{xxv} to mark the centenary of the discovery of cosmic rays.

ⁱ C T R Wilson Proc Roy Soc A London 68 151 1901

ⁱⁱ Royal Society Discussion Meeting on ‘Ultra Penetrating Rays’ Proc Roy Soc A London 132 331 1931

ⁱⁱⁱ P M S Blackett and G P S Occhialini Proc Roy Soc A London 139 699 1933

^{iv} L M Brown and L Hoddeson *The Birth of Particle Physics* Cambridge University Press 1983

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- ^{vii} B Lovell Notes and Records of the Royal Society London 47 119 1993
- ^{viii} P M S Blackett and C B A Lovell Proc Roy Soc A London 177 183 1941
- ^{ix} A letter was sent by T L Eckersley to Blackett in March 1941. See [7] above for details.
- ^x T E Cranshaw and W Galbraith Phil Mag 45 1109 1954
- ^{xi} J V Jelley Proc Phys Soc A 64 82 1951
- ^{xii} N A Porter et al. Phil Mag 3 826 1958
- ^{xiii} Pierre Auger Collaboration, Nucl Inst Method A 798 172 2015 (diagram from ref 25)
- ^{xiv} P M S Blackett Physical Society of London Gassiot Committee Report 34 1948
- ^{xv} B Lovell Biographical Memoirs of Royal Society 21 1 1975 (memoir of P M S Blackett)
- ^{xvi} W Galbraith and J V Jelley Nature 171 349 1953
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- ^{xix} M A Lawrence, R J O Reid and A A Watson J Phys G 17 733 1991
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- ^{xxi} R M Balustraitis et al. Nucl Inst Method A 240 410 1985
- ^{xxii} T Abu-Zayyad et al. Nucl Inst Method A 689 87 2012
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- ^{xxv} K-H Kampert and A A Watson European Journal of Physics H 37 359 2012

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