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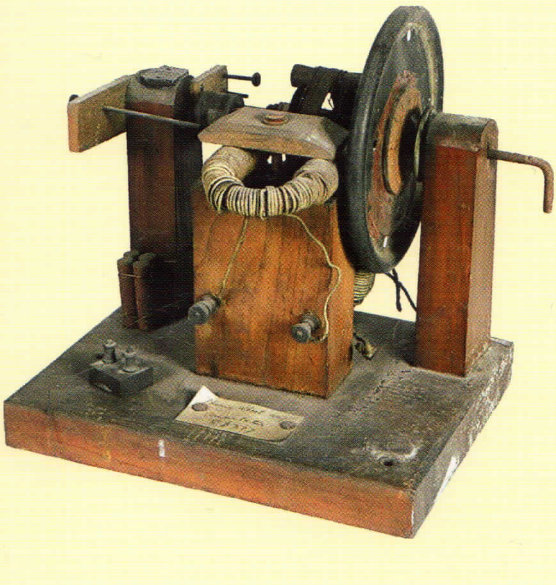
Newsletter



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Cover picture: From the Rayleigh collection, Terling Place.

Phonic wheel or synchronous motor. The coils of two horseshoe magnets (one vertical and one horizontal) were supplied with voltage pulses created by a vibrating tuning fork connected to a d.c. source. The flywheel has four holes through which the tuning fork blades could be viewed to check synchronicity.

Contents

Editorial

One of the sad duties of an editor from time to time is to arrange the publication of obituaries and following the death of Hugh Montgomery earlier this year his colleagues, John Roche and Stuart Leadstone, write of their appreciation of his full and active life.

In 1992 he wrote an article entitled *'The Visible and Invisible in Physics'* which was never published and I am particularly pleased to be able to include it this issue as I feel it is so appropriate that such a full and active life be celebrated by bringing one of his articles to a wider audience.

It deals with what might be described as philosophical matters which deeply affect how physicists may consider the fundamental basis of physics. He starts at the 6th Century BC outlining the conflict between Reason and the Senses, discusses Plato and Platonism through the works of Galileo and Newton, commenting finally on how quantum physics may be viewed in this context.

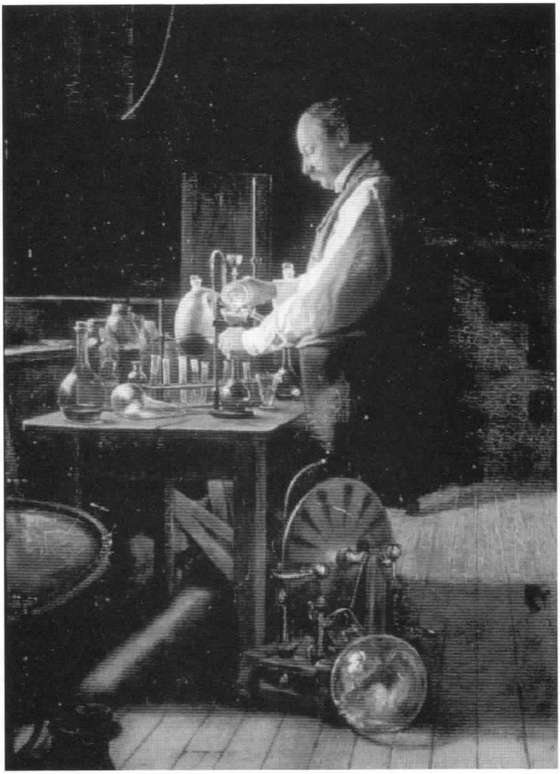
As I was reading through this amazingly compact text, I seemed to identify some abstruse concepts of modern physics with Plato's 'Eye of the Soul'. What heresy was this? Should one abandon the evidence of the senses? Certainly not! But on reaching the end of the article I realised that it did make me think about it in a new and useful way.

Rarely does one come across such a clear and concise exposition of the problems which arise when considering the implications of the macroscopic in terms of the microscopic and vice-versa. And it does so in a way which draws upon the history of physics confirming it to be one of the most powerful tools in the teaching and learning of our natural philosophy.

Malcolm Cooper

Meeting Report

'The Rayleigh Laboratories' held at 76 Portland Place, 17th July



John William Strutt, better known as Lord Rayleigh (3rd Baron Rayleigh), was the subject of our latest half day meeting held at 76 Portland Place on 17th July. Well over 50 people filled the well appointed Franklin lecture theatre to hear three excellent talks each giving a different aspect of his life and work.

It is not often that our lectures on illustrious physicists include accounts from a member of the family but we were treated to just that from the charming 6th Baron speaking about his ancestors. As well as an illustrated factual talk about his forebears, he also revealed secrets such as what's the origin of 'Bobs your uncle', why were Express Dairies *express* and who *was* Black Jack? His colourful painting of the family background set the scene admirably for our other speakers.

The laboratories at Terling Place, the ancestral home, are very much as they were 100 years ago and a project to produce a complete photographic record of this unique 'time capsule' and its contents is almost complete. Prof. Davis, in his talk 'The Laboratories and Work of the 3rd Baron Rayleigh' used many of these photographs to take us on a most effective and fascinating virtual tour of the place where Lord Rayleigh lived and worked for so much of his life.

The work which earned him the Nobel Prize for Physics in 1904, the discovery of argon, was described by our last speaker, Sir John Meurig Thomas, who explained that, he had been investigating the density of nitrogen and was 'much puzzled' by the difference between prepared and atmospheric nitrogen. It was some 18 months later, after giving a lecture at the Royal Society, that he met another who was working on the same problem, Prof. William Ramsay. They immediately struck up a friendship and it tells of great openness and generosity of character that within 4 months they had both isolated the new gas - Argon.

As has been said, the laboratory has changed little over the years and this is due to the dedication to conserving this marvellous scientific heritage by the Strutt family and I'm sure all our members would wish to express our gratitude for that priceless foresight. The meeting ended on an amusing note as the 6th Baron emphasised this responsibility to the future 7th Baron who, was present and had, no doubt, been listening attentively to his father's lecture!

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Unfortunately the transcripts of these lectures could not be available in time to be included in this issue but it is planned to publish them in another issue.

Malcolm Cooper
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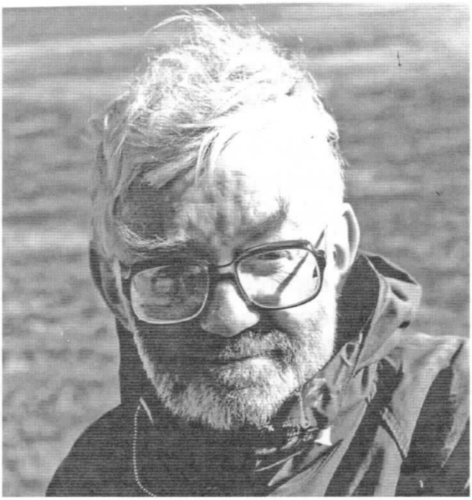
It is with great regret that we announce the death of Hugh Montgomery on 23rd March. Hugh was a staunch supporter of the group, serving on the committee for many years. John Roche and Stuart Leadstone offer their personal obituaries.



Dr Hugh Montgomery
1931 – 2008

Hugh Montgomery's life in physics falls into four periods: his early years in Oxford were followed by a decade at Harwell; thirty years were then spent largely in teaching in the University of Edinburgh and finally there was the deeply contemplative period of his retirement. For most of his career, his research in physics was the study of the solid state at low temperatures. After he graduated with a first class honours in 1952, Merton, his college in Oxford, awarded him with a Harmsworth scholarship and he began the research for his doctorate at the Clarendon Laboratory. In 1954, Merton elected him to a Junior Research Fellowship which he resigned after a year because he felt more exciting work was being done at the Atomic Energy Research Establishment at Harwell. He remained at Harwell until 1967 and was the co-founder of the Solid State Physics Division there. He spent 1967 at the Argonne National Laboratory, Illinois. He was head-hunted by Edinburgh University and was to spend the rest of his career in Edinburgh until his retirement in 1997.

His first paper in 1956 was published jointly with the supervisor of his D.Phil., the low temperature physicist Kurt Mendelssohn (1906–1980): it examined heat transport due to the scattering of electrons and phonons by imperfections in a metal. He followed this with a study on the thermal conductivity of lead caused by lattice vibrations at a temperature of about 1 Kelvin above absolute zero. This was communicated by Mendelssohn to the Royal Society and published there in 1958.

At Harwell Hugh continued his low-temperature work, measuring the specific heat capacities and resistivity of various metals, and also working with infra-red spectroscopy He worked on lattice dynamics of various crystals, cryogenic thermometry, vapour pressure scales, various magnetic properties in the solid state, superconductivity, the study of isotopes, the properties of engineering alloys, and the measurement energy levels in metals. The year 1970 was Hugh's most productive year: he published a joint paper in the *Physical Review* from his work at Argonne, and two articles in the *Journal of Physics C: Solid state physics*. He continued this research at Edinburgh.

For about ten years before his death Hugh changed his direction of research to the critical explanation of concepts in classical electromagnetism. Between 1999 and 2007 he published three papers, two in the *European Journal of Physics* and one in *Apeiron*. The latter paper was substantial. He focused on 'unipolar induction' - the electromotive forces that develops when a cylindrical magnet is rotated about its axis, or when a disc is rotated in a plane perpendicular to a magnetic field. While Hugh's technical papers followed the rigorous norms of that research, his papers on unipolar induction are models of pedagogy (in the best sense), clarity, and historical accuracy. Reading these papers again, I believe that Hugh solved most of the knotty interpretive problems of unipolar induction.

Apart from his peer-reviewed research, Hugh had many other scholarly interests. He was one of the rare physicists who are as much at home in the humanities as in the sciences. His knowledge of history and literature was both wide and deep, especially the history and literature of Scotland, the country he loved most, and of exploration, the activity, after physics, he enjoyed most. He was a member of the Scottish Arctic Club and the photograph accompanying this article was taken on one of several arduous expeditions he made to North East Greenland. So, understandably, the history of his own subject engaged him from early in his career While an undergraduate at Oxford he attended a course in the history of science, presumably with the historian of science, Alistair Crombie, and always maintained an interest in the subject afterwards. He also became interested in the philosophical aspect of certain physics concepts, particularly in the philosophy of space. As a result of this interest, he studied the philosophy of science for a term at Cambridge under Professor Mary Hesse.

Hugh was intensely interested in the history of ideas. In two short studies, which I hope will be published, he condensed a lifetime of reflection into the methodological foundations of mathematics and physics. This was expressed in a measured and elegant style, which was nurtured by his scholarly reading of the history of science, and of the history of philosophy. My great regret is that I did not read these pieces while he was still alive. With respect to the foundations of mathematics he lucidly balances the intuitive process of discovery in mathematics, with systematic deductive development, and with the construction of axiomatic foundations. He brought out, far more clearly than I had ever understood previously, the tension between the rigorous abstract foundations of mathematics and the scientific explanation of nature. With respect to the methodological foundations of physics I was not prepared for the depth of his analysis by which, in a few sentences, he articulated the essence of Galileo's method, and of physics to this day. Hugh saw that Galileo, in his new physics, brought together Plato's idealisations from the World of Forms, with the potential for idealisation in the natural world through experiment. Hugh regretted that, in much of quantum theory, physics has returned to idealisation without a foundation in nature.

During 1999-2000 Hugh wrote up some of his views on the relationship between science and Christianity. He was a severe critic of over-confidence in the cognitive claims of science: 'If you believe that of all members of society the scientist has the wisest insight into the deepest problems of human experience, your faith in science is going to be hard to sustain...Speaking personally, one of my strongest reasons for clinging to Christianity is my horror of a mental world built upon nothing but scientific knowledge'.

But Hugh was critical of any easy integration of science and religion. Speaking positively he wrote '...there is a deep link between Christianity and science, and it emerges I think in Genesis I, *And God saw everything that he had made, and behold it was very good'*. On the other hand, although 'we need both science and Christianity, we cannot demand that we can hold onto both without any problems arising... There is a genuine tension between Christianity and science'. Hugh was particularly critical of John Polkinghorne's view that 'God intervenes constantly in the workings of the natural world'. He also insisted 'that the role of science is not to accept [Christian] orthodoxy'. He concluded that 'Physics and theology...are not joined as easily as Polkinghorne would have us believe'.

In her moving address at Hugh's Service of Thanksgiving, his sister Lesley commented that 'Hugh inherited his passion for ideas from his father – and from his mother a streak of charming daftness that endeared both of them to children.' Hugh's 'charming daftness' concealed a brilliant, scholarly and overly modest mind.

John Roche 21 May 2008

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A personal reminiscence by Stuart Leadstone

I met Hugh through the History of Physics Group. Our first encounter was at the History of Gravitation meeting organised by Peter Tyson in the University of Birmingham in October 1991. During the tea-break Hugh and I found ourselves occupying the same small volume of space-time. Whether it was our similar North-of-England accents, our common reserve, or our shared enthusiasm for the history of physics, I am not sure, but we quickly found an ease of communication which was to characterise our friendship for the next $16^1/_2$ years.

As a mere school teacher I was immediately struck by the seriousness and respect which Hugh – a university academic – exhibited in response to many issues in physics which I subsequently raised with him. These covered a wide range: Torricelli's theorem in hydrodynamics; the sling-shot effect in space travel; energy considerations in a CRO; the correct
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interpretation of "mass-energy equivalence" – to name but a few. Hugh's responses to my queries were invariably characterised by deep thought and meticulous attention to detail, and he enlightened me on many points. The reassurance from a "professional" that my concerns were non-trivial was both a mark of Hugh's generosity of spirit and a source of consolation and strength to one labouring in the nightmare world of secondary education.
Hugh had special knowledge and expertise in electromagnetism, and he lectured on the topic in the Edinburgh University Physics Department over many years. In his retirement he had "homed in" on the curious phenomenon of "unipolar induction", writing two articles for the European Journal of Physics (1,2) and communicating with a number of other physicists in different parts of the world who were equally intrigued by what has been termed "Faraday's Final Riddle: Does the Field Rotate with a Magnet?" Hugh's most recent thoughts on this topic were expressed in a letter to one of his many correspondents less than a week before he died.

Hugh reflected deeply on matters philosophical and religious. His thoughts in these realms were invariably stimulating and challenging, and his writings were characterised by both erudition and humour. Behind a benign and avuncular exterior there lay a rapier-like mind tempered with grace and charm – the hallmark, one might say, of the ideal scholar.

It was a privilege to be present at the Service of Thanksgiving for Hugh held in Currie Kirk, Edinburgh on 2nd April, and I thank his sister Lesley and other family members for the care and inspiration manifestly evident on that occasion. I value the friendship with Hugh forged through the History of Physics Group, and, with other members of the Group who knew him, I mourn his passing greatly.

1. H. Montgomery *Unipolar induction: a neglected topic in the teaching of electromagnetism.*
   Eur.J.Phys. **20** (1999) 271-280.

2. H. Montgomery *Current flow patterns in a Faraday disc.*
   Eur.J.Phys. **25** (2004) 171-183.

The Visible and Invisible in Physics

Dr. Hugh Montgomery (Written December, 1992)

Thomas Huxley once claimed that science is just organised common sense. Darwin's Bulldog was a biologist, speaking in the nineteenth century, and his statement seems peculiarly inadequate to describe physics, particularly physics in the twentieth century. This sometimes looks more like organised insanity, and its great success has been bought at the price of accepting concepts which are strangely counter-intuitive. A perfectly ordinary object like a table is presented in terms of a swarm of things called protons and electrons, and any question the layman asks about these entities tends to be answered in the negative. The physicist describes the comprehensible in terms of the incomprehensible, and then has the effrontery to call it an explanation. Any discipline has its appalling pitfalls and difficulties, but I think that the gulf between the layman and the expert is wider in physics than in almost any other subject. (My own sympathies are with the layman.) The physicist must take some blame for the situation, but I think that the main reason why the layman does not understand him arises from the peculiar nature of the questions the physicist has to ask.

The trouble began right back at the birth of physics, in Ionian Greece in the sixth century B.C. One of the two fragments attributed to Thales of Miletus states that "Everything is made of water." It is too easy to interpret a statement like this in a completely anachronistic way, and we must take note of the other fragment ascribed to him, which says that "Everything is full of gods". But it does seem likely that his intended meaning includes the following two arguments:

a) Nature can be understood on its own terms, and
b) Nature is not what it seems.

Already we have the battle lines set for the conflict between Reason and the Senses which would plague physics throughout its history. Heraclitus of Ephesus put it very neatly when he said:

"The eyes and ears are bad witnesses for men, if the mind cannot interpret what they say."

In the fifth century Democritos of Abdera tried to set up a purely materialistic theory of the universe, and he managed to intensify the problem. He noticed that the physical world appeared as a strange mixture of constancy and change, and he taught that everything consisted of innumerable unchanging atoms, in ceaseless motion in the Infinite Void. These atoms possessed shape, size and movement, and they could not penetrate each other. In themselves they had no colour, taste, smell or sound, and they were invisible to the human eye. The visible was explained in terms of the invisible; the existence of atoms could not be deduced from sensory experience, and it had to be introduced as a metaphysical assumption.

Greek Atomism provided a reassuring feeling that "the truth is there just beneath the surface", and it encouraged the study of Nature by experiment. However as Democritos realised, it is not obvious how one proceeds from experimental observations to a determination of the properties of the invisible atoms. The same basic problem was addressed a generation later by Plato in Athens, and his conclusion was diametrically opposed to that of the Ionians; he insisted that Nature *is* what it seems, and that it cannot be understood on its own terms.

In his famous metaphor human beings are presented as prisoners bound within a dark cave, and the natural world is merely a set of flickering shadows on its walls. The duty of the philosopher is to free himself from bondage, and to turn away from the shadows towards the True Light, which is visible but only to the Eye of the Soul. Reality consists of the Forms, the Ideal Patterns laid up in Heaven, and the world revealed by the senses can imitate the Forms only in an imperfect way. Hence the natural philosopher should abandon observation and experiment, and should contemplate the Forms by mathematical reasoning.

This raised a basic problem: how could the philosopher become aware of the Forms in the first place, if sensory experience could not lead him to them, and could at best remind him of a truth which he already unconsciously knew? Plato's most uncompromising answer to this question derived from his Pythagorean belief in Reincarnation.

In an existence before our birth we are fully aware of the Forms, and:

> *"Not in entire forgetfulness,*
> *And not in utter nakedness,*
> *But trailing clouds of glory do we come*
> *From God, who is our home"*

Hence for Plato sensation is worthless compared with reason, and the purpose of education is to remind us of the period before our birth into this corrupted world. (It is very strange how many physics lecturers, while utterly rejecting Plato, present the subject as if their students merely need to be reminded of truths which they really know already.)

Clearly there is no possible way in which we can reconcile physics with Platonic philosophy. Physicists since 1600 simply have not felt the same contempt for the natural world which pervades Plato's thought, and observation and experiment are an essential ingredient in any tradition in modern physics. But it is the other ingredients in physics, particularly the use of highly abstract mathematics, which raises the ghost of Platonic arguments. In the history of science Plato is sometimes presented as a dreadful aberration, whose ideas are so seductive and dangerous that the sooner they are forgotten about the better. Clearly this is not my own belief, otherwise I would not have mentioned him. I feel that Physics bears the same relationship to Platonism as Christianity does to Judaism; in each case the debt to the earlier system of belief is enormous, but it has always to be repudiated.

This ambivalence of physics towards Platonism appears most starkly in the writings of Galileo. Plato's pupil Aristotle had rejected the theory of Forms, and he insisted that an understanding of the natural world must be based on observation. When Galileo was making his great telescopic discoveries he might have regarded Aristotle as an ally against the crushing weight of authority and tradition. But in the Dialogue Concerning the Two Chief World Systems, it is Aristotle, not Plato, who is the object of Galileo's attack. Simplicio constantly refers to observational evidence in his defence of the Aristotelian system, and Salviati promotes the Copernican system mainly with rational arguments. Salviati even claims that the great genius of Copernicus lay in the fact that he rejected the evidence of the senses. Galileo's telescopic discoveries were vital, but they did not by themselves establish the truth of the Copernican system; they could equally well be interpreted in terms of the Tychonian system, which Galileo rejected on rational grounds. Galileo was not a Platonist, but he realised instinctively that the common sense approach of medieval Aristotelianism was a bigger threat to the future of physics than the most absurd of Plato's speculations.

Let's look a little closer at Galileo's attitude towards experiment. The drawings he made of the Moon and the stars are meticulous, and his experiments on bodies rolling down inclines leave no doubt that he was determined to learn how they actually behaved. But he insisted that the "Book of Nature is written in the language of Mathematics", and this led him to draw a distinction between primary and secondary properties. He believed that primary properties such as shape, size and motion, can be quantified and treated mathematically, and secondary properties such as colour, taste and smell cannot. By straining his sense data through the sieve of mathematics he was removing most of the features of the natural world which made it familiar to everyday experience. In doing this he was clearly following in the steps of Democritos; but whereas Democritos applied the primary properties to the invisible atoms, Galileo applied them to visible and tangible bodies. In order to understand the essential features of his mechanical systems he had to idealise them, and ultimately he was returning to the old dilemma of explaining the visible in terms of the invisible.

This dilemma is implicit in all of Newton's work, though it rarely finds its way to the surface. Newton loathed Platonism though he was deeply impressed by the Greek Atomists, and his physics is a careful balance of experiment and mathematical reasoning. His concept of universal gravitation supplied the unifying principle which was missing in Galileo's mechanics, particularly when applied to the solar system. He tried to avoid any metaphysical speculation on the cause of gravitation, and declared that "Hypotheses non fingo" - although he sometimes fingit Hypotheses with gusto. His beautiful experiments on light were interpreted in terms of an atomic or corpuscular theory, although he fully admitted that the existence of the light corpuscles could not be demonstrated directly.

By the beginning of the nineteenth century Newtonian mechanics was fully established, but his corpuscular theory of light was looking increasingly vulnerable; by the middle of the century it had been largely superseded by the wave theory of Huygens and Young. This coincided with the rise of Positivism in physics, which was a ferocious attempt to cast out all hypothetical theories and to base physics entirely on sense data. This had great success in mechanics, where Newton's theory was reformulated by Ernst Mach, and also in thermodynamics, where the chief advocate of the Positivist approach was Pierre Duhem. They argued that the purpose of physics is not to provide an explanation in the ordinary sense, but rather to provide an elegant ordering of experimental phenomena which would lead to successful predictions of future phenomena. In particular, any theory which regarded atoms as real entities had to be renounced, because the existence of atoms could not be demonstrated directly.

Positivism was a heroic programme, but by the end of the nineteeenth century it had become bankrupt. The sheer wealth of new phenomena, in radioactivity, electrical discharges in gases 'and low temperature research, pointed inexorably to a hypothetical world whose existence had to be accepted without our being able to see into it directly. There is a sad story of Ernst Mach on his deathbed in 1916, when a friend brought him a speck of radium and a scintillation screen to play with. As he watched the tiny flashes of light on the screen, Mach capitulated and said "1 believe in the existence of atoms".

But if the existence of atoms was no longer in doubt, research into the nature of atoms soon plunged physics into the deepest revolution in its history. It was found that atoms are not like the geometrical structures which were imagined by Democritos, and they are not like the billiard balls which were studied by Galileo; they move according to a different set of rules known as quantum mechanics. Unfortunately atoms are not entirely different from billiard balls either; we can speak of their mass, position, velocity and energy, but the meaning of these words undergoes a sea change in the quantum world. At a given moment in time, a billiard ball can have a certain position and a certain velocity. But if an atom has a precisely defined position it cannot simultaneously have a precisely defined velocity, and vice versa. This is Heisenberg's Uncertainty Principle. I obviously cannot go into details, but let me point out a common misconception.

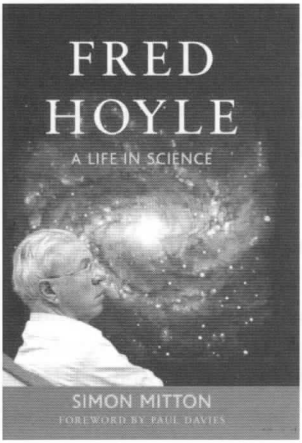
It is not true to say that at a given time an atom has a definite position and a definite velocity, but that it is impossible for us to know these quantities simultaneously. If we know exactly the velocity of an atom, it does not possess a definite but secret position. The Uncertainty Principle does not simply limit our knowledge, it also limits the atom's state of being. One of the most disturbing features of quantum mechanics is that the old distinction between knowing and being is no longer entirely tenable.

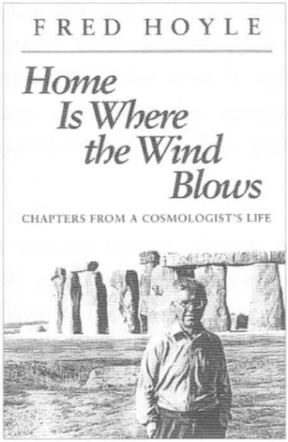
It is very doubtful to what extent quantum mechanics provides an explanation of the atomic world; atoms cannot be seen even with Plato's Eye of the Soul. What quantum theory does is to provide an extremely powerful and elegant mathematical calculus, which is highly successful in predicting new phenomena. Curiously this does not represent a return to nineteenth century positivism, because quantum theory has to make a large number of hypothetical postulates which are not based directly on experiment. We cannot see atoms, but without atoms we cannot predict the phenomena which we can see.

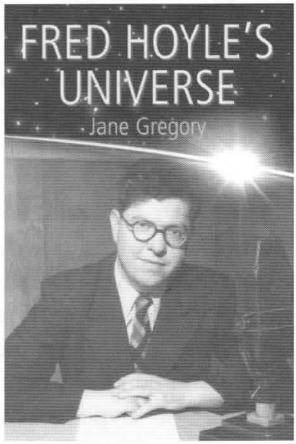
Many physicists are quite content with this state of affairs, because the mathematical structure of quantum mechanics allows them to conduct fruitful research, and they are not concerned with the philosophical problems it creates. However an important minority of physicists have never been able to accept quantum mechanics at the most fundamental level. Einstein felt that it represents Mathematics without Understanding, and his later years were spent in a vain search for a more satisfying theory. In a strange way this brings us back to Plato. Although he did so much to promote the use of mathematics in the study of the natural world, he also insisted that mathematics was not the highest form of knowledge:

"Geometry and the studies that accompany it do in some sort lay hold on reality; they are, as we see, dreaming about being, but the clear waking vision of it is impossible for them as long as they leave the assumptions on which they are based undisturbed, and cannot give any account of them. For if the premiss is something one does not know, and the intermediate steps and conclusion are strung together out of things one does not know, by what alchemy can this mere consistency ever become knowledge?"

Book Reviews







'Three Sides of Fred Hoyle'

Fred Hoyle: A Life in Science (Aurum, 2005)	*Simon Mitton*
Fred Hoyle's Universe (OUP, 2005)	*Jane Gregory*
Home Is Where the Wind Blows (OUP, 1994)	*Fred Hoyle*

Reviews by Jim Grozier

(Originally published in 'Fusion' - the Open University Physics Society)

Reading two biographies of the same person might be considered a little strange, when there are all those other books out there that need reading; reading three is surely downright obsessive. Like a lot of research, it was motivated by a mystery.

Being of the post-war generation, I was too young to listen to Hoyle's radio broadcasts in the early 1950s, but he was always there in the background – on TV and in the papers, I suppose, although any memories of specific occasions have long since evaporated – but his very distinctive Yorkshire accent was hard to forget. I can remember the *Sun* (when it was a broadsheet!) running a strip cartoon series on what were then considered to be the two rival theories of the Universe – the Big Bang and Steady State theories. The latter was championed by Hoyle and his colleagues Hermann Bondi and Tommy Gold, and I have had a soft spot for it ever since; after all, any theory which avoids the idea of a beginning to space and time should not be disregarded lightly. And of course, when the science fiction serial *A For Andromeda* (co-written by Hoyle) came on the television I was glued to the set, as were doubtless many others of my age. But it was a bit of "local interest" that got me started on the biographies.

Simon Singh, in his excellent book *Big Bang*, describes the genesis of the Steady State theory as having occurred when Hoyle and his colleagues went to see the film *Dead of Night* in a Guildford cinema in 1946;[1] the cyclical nature of the film's plot gave them an idea for a model of the Universe. Having gone to school in Guildford, I found myself wondering which cinema this was; in the 1960s there were the *Odeon* and *Astor*, although both are now closed, and the former has been demolished. I suggested to my local Institute of Physics branch committee that, if it turned out to be the Astor, it might be considered a suitable site for a blue plaque; and, at a public lecture in Guildford, I even interjected, following a mention of the Steady State Theory by Professor Jim Al-Khalili, that it had been invented there. I was also fascinated to hear what had brought the three to that part of Surrey, namely the radar research laboratories of the Admiralty Signal Establishment at Witley, which I immediately recognised as the same place that later housed the National Institute of Oceanography, where I had my first summer job, as a labourer. (I can still remember how my limbs ached from stripping lead off miles of underwater cable that the Admiralty had left behind – a task we were given when there was nothing else to do).

Imagine my embarrassment, then, when I read the opening sentences of Jane Gregory's third chapter: "One evening in 1946, Hoyle, Bondi and Gold strolled across Cambridge to the cinema. The film they were going to see was *Dead of Night* ..."[2]

Who was right – Singh or Gregory? And how could one of these eminent authors have got it wrong? Of course, I probably wouldn't have read any of these books if I had not attended a brilliant lecture on Hoyle a few weeks earlier, given by Simon Mitton, and bought his book as a result; but Mitton says only that they saw the film at "a local cinema", and gives no clue regarding which locality it belonged to. Then someone gave me a copy of Gregory's book, and on discovering the contradiction over the cinema location, the obvious next step was to read the autobiography; I was hooked.

In Arthur Golden's *Memoirs of a Geisha*, the (fictional) translator says in a preface that "A memoir provides a record not so much of the memoirist as of the memoirist's world. Autobiography, if there really is such a thing, is like asking a rabbit to tell us what he looks like hopping through the grasses of the field. How would he know? If we want to hear about the field, on the other hand, no one is in a better circumstance to tell us – so long as we keep in mind that we are missing all those things the rabbit was in no position to observe." So maybe one should not compare biography with autobiography; still, the autobiography gives us a valuable insight, if only from what the author leaves out. For of course there is another factor present in an autobiography, other than the question of perspective: that is, the selection, or, to put it another way, censorship, of material by the author. We see this most strikingly in the context of the very thing that started me reading these books, namely the genesis of the *Steady State Theory*, for it is missing from the autobiography altogether; there is a retrospective reference to the theory at the end of the book, but it is given in the context of the theory's later incarnation at the hands of Hoyle, Burbidge and Narlikar. There is no detail of the development of the original version, nor any mention of the numerous wartime discussions by Bondi, Gold and Hoyle in Bondi's house in Dunsfold. In fact, Bondi and Gold are not even properly introduced; in contrast with Hoyle's accounts of first encounters with other famous scientists such as Hubble and Fermi, the two with whom his name was to be most closely linked simply appear from nowhere during his account of the wartime radar work. It is almost as if a chunk of his life had been air-brushed out of existence. This is not really so hard to understand: writing in the mid-1990s, by which time the Steady State Theory (at least in its original form) was well out of the running, he is clearly re-assessing the importance of his earlier activities, but it is a pity that he went so far with the self-censorship.

So the autobiography turned out to be of no help with my quest for the true location of that cinema. I contacted both Singh and Gregory, and eventually – on the advice of the latter – consulted the Hoyle Papers at St John's College, Cambridge. Here, a paper written by Hoyle in the 1980s confirms the Cambridge venue, and, although based on a 40-year-old memory, gives enough detail to convince one of its accuracy. Singh's version – the Guildford connection – apparently came from an interview with the late Tommy Gold; but this too was an old memory, and the extract he quoted to me did not contain the sort of corroborating details that are present in the Hoyle archives; so on that basis I concluded that the odds are on Cambridge. Singh does also quote a passage by Hoyle in his book which refers to the film, but has not been able to give a reference for it. (Big Bang does not have a detailed bibliography like the two biographies, but only "Further Reading"; I suppose that is one of the things that distinguish popular science books from more serious works of scholarship).

But why two biographies? Well, they were both published in the same year – 2005 – but it is not clear whether the two authors knew of one another, nor who started first, although they both quote extensively from the autobiography. Happily, they are not identical. Each has its strong and its weak points, and they concentrate on different aspects of Hoyle's life; so the three books should be seen as complementing one another, rather than competing.

All the same, one might expect them not to deviate from one another on the basic facts, or at least, one would not expect the two biographies to disagree; but they do. For example, in the autobiography, Hoyle says that at his first meeting with his tutor at Cambridge, P.W.Wood, he was told that his mathematics was "not good enough for a real scientist"; on asking Wood what he should do, the tutor replied that he "might consider Mathematics, Part I" and advised Hoyle to go away and think about it. (He had been accepted by the college as a student of science, not mathematics).[3] Gregory concurs: "Rather than enter straight away for the natural sciences degree, Hoyle's tutor suggested he take Part I of the Mathematical Tripos".[4] Yet Mitton tells it like this: "Fred had a very difficult first encounter with his tutor, Mr P.W.Wood. He hesitantly explained that he did not wish to pursue natural sciences after all and would prefer to switch to mathematics. This must have shocked Wood …" [5]

We should not presume to judge who is "right" and who is "wrong" here. There were no other witnesses to the encounter between tutor and student; it is unlikely that Mitton could have interviewed Wood, who was already due for retirement in 1945, but he might have got it from a third person who knew him, or the source could have been Hoyle himself at an earlier time in his life, when his memory was more reliable, since Mitton knew him for nearly thirty years. However, it has to be said that no such reference appears in Mitton's bibliography.

From reading these three books it is clear that there are a number of interesting aspects to Hoyle's life. Probably the best-known is the discredited Steady State Theory, but there was also a major piece of work on nucleosynthesis – the origin of the chemical elements – in stars and supernovae, in collaboration with the nuclear physicist Willy Fowler, and the astronomers Geoffrey and Margaret Burbidge, which culminated in a landmark 1957 paper commonly known by the first letters of their names – B2FH – and which many people felt should have entitled Hoyle to a share in Fowler and Chandrasekhar's 1983 Nobel Prize. Then there was a fascinating but destructive rivalry with the radio astronomer Martin Ryle, mainly over the evidence for and against the Big Bang and Steady State theories, which does not show the protagonists (particularly Ryle) in their best light, to say the least.

Hoyle was clearly not a man to shirk controversy; we see this, not only in his defence of the Steady State Theory against an increasing weight of evidence against it, but also in the very first papers he published in astronomy, in a collaboration with Ray Lyttleton which started in 1939. These papers dealt with the accretion of interstellar dust onto stars, a topic which has since entered the main stream of astronomical thinking, but was not favoured by the establishment figures of the day. However, they went on to use this model to explain ice ages and other geophysical phenomena, in what Simon Mitton describes as "a spectacular leap across disciplines" which constituted "a mild invasion of the intellectual territory of earth and atmospheric physicists" [6]. The rejection of the first paper by the Royal Astronomical Society, precipitated by the authors' refusal to make changes suggested by the referees, heralded a life-long conflict between Hoyle and "the authorities". Mitton devotes a whole chapter to this first clash with scientific orthodoxy. My own reaction, on reading this chapter, was that, although there is nothing wrong with the idea of accretion in itself, for an astronomer to use it to explain such things as the earth's climate inevitably brings the word "maverick" to one's mind. But then Hoyle was perhaps the kind of genius who could arrive at the right conclusion by working something out by himself from first principles, without the need to consult the established experts – so that if the experts got upset, it was not necessarily Fred's fault.

Gregory does not dwell on the story of the accretion papers, but she does give a detailed account of a later controversy, once again concerning interstellar dust – now posited as a possible carrier of life, and bringer of epidemics such as influenza, in a collaboration with Chandra Wickramasinghe in the late 1970s and 80s. This of course upset a whole new swathe of experts, and was perhaps a step too far, especially when one considers that Hoyle and Wickramasinghe even flirted with creationism at one point. Perhaps the most embarrassing episode of all was their assertion, in 1985, that the Natural History Museum's Archaeopteryx fossil was a fake.

The image of Fred Hoyle as a modern scientific icon, a working-class boy who made good and became one of the foremost, and most popular, astronomers of his time, is somewhat tarnished by these revelations. But Hoyle's reputation is rescued by his first-class work on nucleosynthesis, which many would say was his greatest achievement – and also his least controversial. Yet even here he seems to have been determined to inject some controversy, by claiming, as he does in the autobiography, that his successful prediction of a resonance of the carbon-12 nucleus at 7.65 MeV (essential for the production of carbon, and therefore also of heavier elements, in stars) was derived from the anthropic principle. This is a very strange thing for a cosmologist to say, and even stranger when he is pretending to be a nuclear physicist. (Gregory echoes the claim, as does Singh in his book; but Mitton, in an otherwise very comprehensive account of this topic, does not mention it).

To me, the anthropic principle is not really a principle at all – at least, not in the sense of a generally accepted premiss, such as Hamilton's Principle or the Principle of Special Relativity; it says something along the lines of "isn't it odd that all the physical constants have just the right values for life to have arisen?" If it is to be used as a *principle*, it should be in the context of why the constants have these particular values (several theories having indeed been put forward), which is a far more fundamental question.

Once one has set the fundamental constants to these values, the existence of the resonance (and indeed all other measurable quantities) follows from them, and the anthropic principle has no rôle to play, and nor indeed does life, except insofar as it is needed to observe the abundance of carbon. Hoyle's argument for the resonance surely boils down to this: "a resonance at 7.65 MeV would allow carbon to be made easily in helium-rich stars, and consequently make it an abundant element; it is an abundant element; therefore the resonance exists". (Mitton quotes Hoyle as having effectively said this, but does not credit the quotation). This is a quasi-logical way of arguing: one may say, perfectly logically, "A implies B; A is true, therefore so is B", or alternatively "A implies B; B is false, therefore so is A"; but Hoyle's line of reasoning, as I have expressed it above, amounts to "A implies B; B is true; therefore so is A". This does not actually follow logically (because B could have arisen from some other cause) but it is in practice how most scientific hypotheses come to exist. One observes a particular phenomenon (for instance the abundance of carbon) and constructs a theory – which could be one of many possible theories – to explain it (for instance the existence of the resonance). One then does what Hoyle persuaded Willy Fowler to do – look for the resonance – to test the theory.

One odd thing about *Home Is Where The Wind Blows* is its use of American spellings. Fair enough, the book was first published by a US firm, University Science Books; but then it isn't just spellings. Apart from American words like "fall" and "sidewalk", there is the statement that "I had arrived in Cambridge a frail young fellow weighing about 115 pounds …" [7] – so he has even picked up the American way of describing one's weight! In Britain, certainly in my generation, body weight has always been expressed in stones and pounds (although the US style may be more prevalent in the UK nowadays, like so many other aspects of American culture); so it is a fair bet that, in 1933, the 18-year-old Hoyle would have weighed himself in stones and pounds too; writing about it some 50 years later, however, he has "translated" the weights into the conventional American units. This could, of course, still be down to the publisher insisting on the book using American terms throughout, but that "translation" process would hardly extend to the introduction of a term like "most everything", which appears in the book. A better explanation is that Hoyle simply absorbed American habits on his very many visits across the

Atlantic to visit colleagues at Caltech and elsewhere (often to the consternation of the Cambridge students he was supposed to be supervising).

Jane Gregory devotes several of her chapters to Hoyle's administrative activities, including in particular his rôle as midwife to the Anglo-Australian Telescope, and the long and ultimately successful struggle for the establishment in Cambridge of the Institute of Theoretical Astronomy (IoTA). These stories are told in all three books, but Gregory's account is by far the most comprehensive. The IoTA story stretches over several of her chapters, with many references to official documents. Such a record could be of immense value to a future historian of UK science policy, but it is perhaps rather too much of an in-depth study for a general biography.

The IoTA affair was a turning point in Hoyle's life. Although the Institute ushered in a "golden age" for theoretical astronomy in Cambridge, it was beset with problems from the start – problems which were to lead to Hoyle's resignation from Cambridge a few years down the line, when it merged with the Observatories to become the Institute of Astronomy, under a new Director. In fact, Mitton starts his book with Hoyle's last day at the Institute, 19 August 1972, before commencing the conventional biographical narrative.

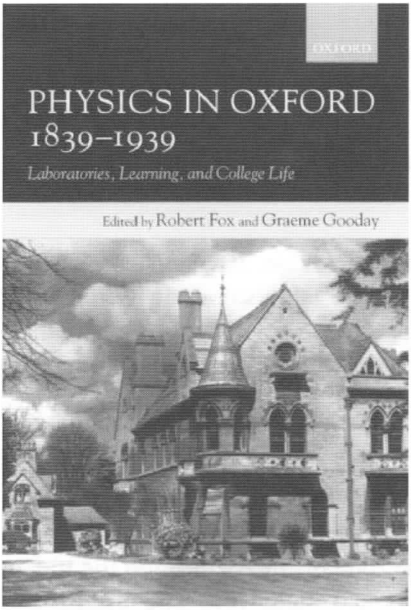
Simon Mitton's lecture ended on a very sad note. At the end of the questions session, an elderly gentleman at the back of the hall put his hand up and told us about a Cambridge reunion he had attended, where he had seen Hoyle standing alone; no-one would talk to him. This was at the end of Hoyle's life; I did not catch the exact year, but, I think it was in the 1990s, by which time his bizarre theories on life in outer space had presumably cut him off from much of the scientific mainstream; or perhaps he had simply alienated too many people over the years.

The commonly held stereotype of the Yorkshireman is that of a gruff, outspoken, opinionated character who does not suffer fools gladly. To what extent Hoyle resembled this stereotype is not clear, although if he did it might explain why Simon Mitton had the 18-year-old Hoyle making demands of his tutor within a few days of his arrival in Cambridge. Certainly the years of playing truant from school (which do not seem to have prevented him winning a scholarship to Cambridge) would suggest that he had quite an "independent streak", at least in early life; and there are comments in the autobiography that tend to suggest that he did not always much care about the impact of his words. Unlike Ryle, Hoyle was apparently not given to losing his temper, but it does seem that he was either strongly for something or strongly against it (where the "something" could be a person, a group of people, a project or an institution), with precious little in between. His carpings about those he disliked were, for me, the hardest parts of the autobiography to read; furthermore, even my limited knowledge tells me that some of these judgments were surprisingly ill-informed.

Was Fred Hoyle a victim of the mandarins of the scientific establishment, or of his own uncompromising personality? Well, read the book and decide for yourself. But *which* book? Well, as mentioned before, they concentrate on different aspects of Hoyle's life; if you want more of a scientific slant, read Mitton; if you want to know about the bureaucratic machinations going on in the background, try Gregory, whose main interest is in "science in public" although she has studied physics, and – like Mitton – has worked in science publishing. With the exception of the rather turgid chapters on IoTA, Gregory's book definitely wins over its rival in the readability stakes, with Mitton's prose somehow failing to "flow" well; on the other hand, Mitton benefits not only from a knowledge of the science but from a personal acquaintance with his subject (he was for a time one of Hoyle's research fellows), although he did not commence work on the book until after the astronomer's death in 2001.

But the best "read" of all is of course the autobiography, as long as one reminds oneself that it is not a full account of this fascinating life. For completeness, read all three!

1 Big Bang, Simon Singh, Fourth Estate, 2004, p. 343
2 Gregory, p 36
3 Hoyle, p 91
4 Gregory, p 10
5 Mitton, p 33
6 Mitton, pp 64-65
7 Hoyle, pp 103-104



Physics in Oxford 1839-1939

Edited by Robert Fox and Graeme Gooday

OUP *2005*
ISBN *0198567928*
386pp *Hardback £71*

Contents:

Reviewed by:
Prof.Denis Weaire
Trinity College Dublin

'Outfoxed by Cambridge'

I recently dined at High Table in an Oxford College. It was New College, which is of course very old. At the head of the table sat a patrician figure, who shall be nameless here though recognizable to many – a scholar of extraordinary accomplishments and deep historical insight. It transpired that that his greatest passion was foxhunting.

From *Physics in Oxford* I have learnt that the same could be said of John Sealey Townsend, first Wykeham Professor. In his case, research receded further and further from his thoughts as he rode his horse through the traffic to the laboratory that he had founded and fought for. The bright and ambitious protégé of G F Fitzgerald in Dublin and J J Thomson in the Cavendish had become a reactionary don, rejecting quantum mechanics (not generally popular in Oxford anyway) and other new-fangled physics, while still holding on to the reins of power as well as those of his horse.

The academic profession offers a unique opportunity to build an empire and then to destroy it, because the displacement of the powerful professor is not easily accomplished before the statutory age of retirement. Oxford physics has had more than its fair share of this lamentable phenomenon. The peculiarities of the university, which are not confined to giving things strange names, are surely to blame. The system of independent colleges, a wonderful invention at its best, was taken to greater extremes here than elsewhere. The close and largely closed community of the College exerted a gravitational pull on the university teacher, and provided a reason, or an excuse, to be less than assiduous in pursuit of the objectives of the central institution. "Cantankerous isolation", by which Soddy is characterised in the book, was common.

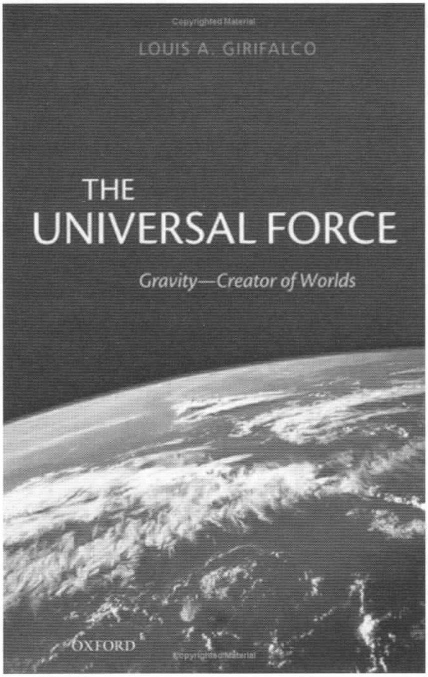
More inclined to *chercher la femme* than the fox, Erwin Schrödinger failed to adapt to the mores of Magdalen College, and left. (Eventually he ended up in Dublin, which could hardly have been less intolerant but was large enough for him to hide in the cracks.)

Your reviewer has managed to complete several paragraphs without mentioning Cambridge, the elephant-in the-room in any discussion of Oxford physics. The inevitable comparison with the all-conquering Cavendish recurs throughout the book. Claims are made that it departs from the apologetic line of previous works, even rejects them. But apologies abound, and the facts repeatedly speak for the success of Maxwell, Stokes, Rayleigh, Thomson, Rutherford and Bragg, and the relative failure of their Oxford counterparts. After three hundred and fifty pages of agonised analysis, the Light Blues still win the Boast Race by ten lengths.

Nevertheless, this volume is fascinating, and offers a cast of characters in comparison with which their Cambridge counterparts look like the "Grey Men" from the grammar schools who invaded Oxbridge in the 1960's – too serious and self-absorbed. Consider the case of Lindemann. How many physicists played tennis with the both the Tsar and the Kaiser? (Not mentioned here). Or were close confidants of Churchill? Or carried on their conscience the policy that brought about fire-bombing of Dresden? Or brought to Britain as refugees many of the superlative talents that kept it at the forefront of world physics, at the end of the period of this book? To his credit he remained a positive influence on Oxford physics to the end, although his academic contributions dwindled, early on.

It is also an invaluable reference text, thoroughly professional throughout in its treatment of sources and in its guarded judgments. It makes a good case for the classical physics to which Oxford clung for too long, even if it leaves a taste of remorse for opportunities missed.

At times the exegesis of the Oxford system's oddities threatens to overwhelm the reader with arcane detail, but in a university that calls an old college "New", someone has to tell the rest of us what all the words mean.



The Universal Force
Gravity – Creator of Worlds

Louis A Girifalco

Oxford University Press 2007
ISBN 978-0199228966
288pp Hardback £19.99

Reviewed by Dr. Peter Ford
University of Bath

This book describes our understanding of gravity, a force which permeates our whole life and universe without many people being fully aware of it. It has been written by Louis A. Girifalco, who is a Professor of Materials Science at the University of Pennsylvania and a distinguished academic and author. Not unsurprisingly the book tends to discuss the subject historically. The first eight chapters give an interesting description of that great flowering of activity, which mainly took place in the late sixteenth and all of the seventeenth century, resulting in the formulation of the laws of motion and of planetary movement and where the ideas of force and gravitation began to be developed. The life and work of all of the famous names are discussed: Copernicus, Galileo, Newton, Kepler, Tycho Brahe and others. The author has a considerable flair for providing interesting biographical details of such people and in so doing brings their characters to life. He is good at setting the historical scene. Thus chapter 5 opens with a delightful description of three men sitting round a table in a London coffee house one cold day in January 1684 deeply engrossed in conversation which is unintelligible to those who surround them. The three men are Halley, Hooke and Wren and they eventually get on to discussing the motion of the planets around the sun. They all felt that in order to understand this it was necessary to introduce an attractive force varying as the inverse square of the distance from the sun. However, they believed that this was extremely difficult to prove mathematically. Halley felt that the only person capable of achieving it was Newton whom he decided to consult. This resulted in Newton's famous book *Principia*, which has had such an enormous influence in our understanding of the natural world.

The book continues with a chapter giving a good description of early work on electricity and magnetism leading to the seminal work on electromagnetism due to Faraday and Maxwell, which among many other developments led to the beginnings of field theory. This is followed by an account of the ether, which naturally leads into the work of Einstein. A chapter is devoted to the life and work of Einstein and included an interesting comparison between him and Newton. Einstein's famous papers of 1905, his *Annus Mirabilis*, are described. These are his work on the Brownian motion, leading to a much greater belief by physicists in the existence of molecules, his paper on the explanation of the photoelectric effect in terms of particles of light or photons, for which he was awarded the 1921 Nobel Prize in Physics, and his work on special relativity and its extension leading to his derivation of the energy-mass relationship, the most famous equation in physics. Two chapters are devoted to special relativity where the author gives a good description of the concepts of time and space as applied to moving and stationary objects and the verification of the seemingly paradoxical conclusions that Einstein reaches. The book continues with Einstein's work on general relativity and the ideas of curved space and warped time. Einstein believed that light would be deviated when it passed close to a massive object such as our Sun or other star due to its gravitational field. Einstein's worldwide fame originally stemmed from an expedition to an island off the coast of West Africa in 1919 to observe the total solar eclipse taking place. It was mounted by the British Astronomer Arthur Eddington, the most famous living astronomer at that time. The expedition was successful in observing during the eclipse the deviation of light rays from other stars as they grazed the surface of the Sun in a manner that Einstein had predicted. This observation struck a chord in the public imagination largely because a British scientist had verified the predictions made by a German. It had great symbolism as an important act of reconciliation between Britain and Germany coming so soon after the ending of the horrific First World War.

In chapter 18, which has the title "Crunch", the author emphasises the universal nature of gravity. Compared with the electromagnetic force, it is extremely weak (many orders of magnitude so) and unlike the electromagnetic force it is always attractive in nature and can never be shielded. The force of gravity is an absolutely essential factor in our understanding of important aspects of cosmology such as the formation of galaxies and eventually to individual stars. Newton was the first to appreciate the importance of gravity in cosmology. He realised that in a finite universe the gravitational force should eventually coalesce all matter into a single mass. Surprisingly early some scientists were speculating on the consequences of extreme forms of gravitational attraction. Thus in 1738 John Mitchell from Yorkshire was proposing a situation in which the density and mass of a star had become so great that the escape velocity from it exceeds that of light. Many years later these were detected and the emotive name "black hole" was coined by the eminent American scientist John Wheeler. Other highly extreme forms of gravitational attraction discussed in the book are the white dwarfs and neutron stars, which are the death throes of stars.

The historical time line and physics discussed in this book follow a well trodden path. Indeed much of the material of the present book also appears in the book *From Clockwork to Crapshoot – A History of Physics* by Roger Newton, which I reviewed in Newsletter No 22, (August 2007). However, this in no way denigrates the value of Girifalco's book. He writes in a highly lucid and interesting manner and succeeds admirably in bringing the subject to life. What he discusses in the book forms an important part of our intellectual, scientific and I believe cultural heritage and should be widely disseminated in books and the media to as many people as possible. In my opinion this book by Louis Girifalco is a "good read" and I strongly recommend it to others.

New commemorative statue of James Clerk Maxwell

Above is an artist's impression of a new statue, commissioned by the Royal Society of Edinburgh and which will take a prominent place in George St., Edinburgh. It is hoped that the statue, commemorating one of the world's great physicists will be unveiled later this year on November 25th.

The event includes the full day '**James Clerk Maxwell Conference**'

Speakers include Prof. Alexander Stoddart, Sculptor and Prof. Malcolm S Longair.

For a full description of the making of the statue take a look at the very comprehensive website at:

http://www.royalsoced.org.uk/maxwell/index.htm

A message from our Web Pages Editor, Kate Crennell

1. Easier Access to the History of Physics Group Web Pages

The Institute of Physics home page on the web is reached at http://www.iop.org . Personally I find this page very cluttered with many small topics none of which obviously point to our Group pages, so to find our pages more quickly I use the alternate way of going directly to them, at http://hp.iop.org This shows our History of Physics Group 'home' page at once. A line of topics maintained by the Institute is at the top of the page. Another column of topics is usually seen on the left side of the page (it may be elsewhere if your Internet browser is not the same as that of the IOP web designers). Look for the heading: 'Subject Groups', below that is another heading 'History of Physics', and below that a list of topics leading to pages which I update via the IOP Science Support Officer. Look at 'News' for any news I have been sent such as awards or obituaries, 'Events' contains details of forthcoming meetings and conferences, please send me information about any which you come across to share with other members. The last line 'History of Physics Group Calendar' is maintained by the IOP.

2 What did the Physicists do for us?

I have recently been watching a fascinating series of television programs on the UKTV History channel, with titles like 'What did the Tudors do for us?' where 'Tudors' is replaced by other historical periods such as 'Victorians'. These programs are presented by Adam Hart-Davies and were originally made for the Open University. I thought it might be a useful thing for the IOP to make a section of their Website to attempt to show how physicists of the past have contributed to our daily lives today. Perhaps they could discuss it with Adam Hart - Davies and make a whole series about physicists through the ages, which might be made available to schools on DVDs. Both the IOP and 'Physics World' have collected useful historical information which could be used as a basis for some entertaining history, but unlike the Institution of Electrical Engineers neither of them find it interesting enough to keep it in an archive. For example in 1999 they published '125 years, The Physical Society and The Institute of Physics,' which has some useful Appendices, a list of past presidents and their photographs, a list of Nobel prize winners amongst the members and a list of the medals and awards. Unfortunately, the rest of the book was mostly about the administration of these societies and the reader is left to guess what physics these people did and how it benefited us. 'Physics World' used to have a 'Best of history' section on their website so I did not make a note of the many individual links to historical articles published over the years. Unfortunately, in a recent 'improvement' to their site this important section was lost and my emails to them about it are not answered. Then there are the people after whom Institute Headquarters Rooms are named. When the Institute rents rooms to other societies for conferences, surely it would be useful to have a small leaflet available for visitors explaining why these were important people? There was such a leaflet for the newer part of the headquarters building but it is no longer available. Join me in urging the IOP itself and IOP Publishing to maintain an Archive of historical information, including photographs, which they collect and keep it readily accessible for future generations.

Group AGM Notice

Our AGM this year will be held on 10th December at Liverpool University and the associated half day lectures will be organized jointly with the Physics Dept to celebrate its distinguished reputation in physics.

The lectures will be held in the Leggate theatre in the newly opened Victoria Gallery and Museum, which now houses all the University's art and science collections in the very beautiful interior of the University's Victoria building which was designed by Alfred Waterhouse in 1892. There will be an opportunity to view the galleries as well as attend the meeting. The venue details for our AGM will be announced later.

There will be four talks, beginning at about 11.00 am, and ending about 4.30 pm, with a buffet lunch and light refreshments. The speakers will be Dominic Dickson, Peter Rowlands, Gerard Hyland and Mike Houlden, and the meeting will cover the work of Oliver Lodge, Lionel Wilberforce, Charles Barkla, James Chadwick, Otto Frisch, Herbert Frohlich, Joseph Rotblat, and many others, leading right up to the present day.

There will also be an exhibition of documents, photographs and artefacts, and a DVD featuring several of the major players.

History of Physics Group Committee

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* Peter Ford, our new chairman of the History Group of the Institute of Physics, has recently retired from the Physics Department of the University of Bath. His email will remain as given above for the time being. -Ed

Wanted!

Articles, Letters, Queries

- long or short

wanted for your Newsletter

Send to Malcolm Cooper, Editor

email: mjcooper@physics.org

and

news items for your website

Send to Kate Crennell, Web Editor

email: kmcrennell@physics.org

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Remember!

***faster* access to History of Physics Group Web Pages via:**

http://hp.iop.org
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