Institute *of* **Physics**

History of Physics Group

Newsletter

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Contents

Editorial

There can be few among the world physics community unaware of the many events taking place worldwide to celebrate the centenary of Einstein's 'annus mirabilis'. His five papers of that memorable year are hailed as a turning point ushering in a new era of 'modern physics'. But did the inspiration for all that prodigious activity spring from his creative genius or to what degree was he influenced by the many ideas which went before?*

*It was to consider this question, that our group met at Birmingham University in October last year, after its Annual General Meeting, to hear a series of lectures under the somewhat 'tongue in cheek title': '**Was there life before Einstein?'** focussing on the state of affairs just before the revolution of 1905.*

Over the centuries the 'ether' had been invoked to satisfy many problems; its nature ever changing, it was the medium by which action at a distance could be given credibility. Eventually, it may be said, that it had even metamorphosed into the space-time continuum of Minkowsky, standing between the special and the general theories of relativity. During the 19th Century, however, many physicists were wrestling with all sorts of ideas (including the transmission of electromagnetic waves) in which the material ether was still very prominent.

There are many names which spring to mind here, but it is fitting that our speakers turned the spotlight on the two men who had closest connections with Birmingham University – John Henry Poynting who, in 1880, became the first Professor of Physics at Mason College as it then was, and Sir Oliver Lodge who was it's Principal from 1900 until the end of his working life.

The two men were of a very different mould – Sir Oliver Lodge, a larger than life character with a huge ego, energetic and pursuing a sometimes controversial scientific life, and despite later criticisms of Einstein's general theory, embraced startlingly prophetic ideas in astrophysics. In stark contrast, John Henry Poynting appears quiet, unassuming and somewhat introspective and yet he and Lodge had a close professional friendship, Poynting even offering the mercurial Lodge mathematical advice on occasion.

The articles presented in this issue explore very different views of these men and how their work often pre-echoed the hypotheses which Einstein was to so spectacularly express in the early part of the 20th century.

We are pleased to report the lecture given by Dr. Isobel Falconer at the 2003 AGM which unfortunately we were not able to include in issue 17.

Malcolm Cooper

**See Stuart Leadstone's article 'Anticipating Einstein' in Issue No 17.*

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Discoveries, Theories and Natural Philosophers

Formulating a Theory: J.J. Thomson and Ernest Rutherford's Collaboration on x-ray Ionisation.

Dr Isobel Falconer

One hundred years ago, in 1903, Joseph John Thomson, Professor of Natural Philosophy at Cambridge, published his classic, *Conduction of Electricity Through Gases*, based heavily on the coherence brought to his understanding of gaseous discharge by his work with Rutherford in 1896 on x-ray ionisatio of gases. Their joint paper brought together experimental results and mathematical theory in a close relationship unprecedented in the history of gaseous discharge. The theory they proposed was the, now seemingly straightforward one, that the x-rays dissociated the gas molecules into positive and negative ions, and subsequent recombination of the ions. The conductivity of the gas represented the number of ions free at any time.

What marked out Thomson and Rutherford's work and made it endure was the simple mathematical formulation, the good agreement between theory and experiment, and the power of the theory in assimilating other phenomena. Undoubtedly a number of factors were important in enabling the articulation between experiment and theory, in particular the new phenomena and techniques made possible by the discovery of x-rays. But in this paper I want to concentrate on the way the theory was formulated. What range of ideas and mathematical techniques did Thomson and Rutherford bring to their work and how did each man go about casting both their developing theory, and their experimental results, into mathematical form?

First I will examine the mathematical training of each man, and the way they put it into practice in articulating theory and experiment in some of their earlier work. Then I will discuss the work on x-rays, highlighting the contributions of each to formulating the theory.

J J Thomson

In 1896 J J Thomson was 40 years old. He had been educated primarily as a Cambridge Mathematician, graduating as 2nd wrangler (i.e. 2nd top) in 1880. Cambridge at the time was the centre of mathematical education in Britain, with an influence extending far beyond the university itself. To be a high wrangler (as the first class degree men were called) was a supreme intellectual distinction in Victorian Britain and great kudos attached to the school which had produced the senior wrangler (the top man).

Thomson's 'old school' was Owens College in Manchester, which he entered aged 14. Although he went to study engineering, his recollections show that he was sent to Owens partly because they *had* just produced a senior wrangler (John Hopkinson)[1]. And his maths teacher at Owens was Thomas Barker, himself a senior wrangler, who extended his teaching far beyond the standard schoolboy arithmetic and Euclid, lecturing on the logic of mathematics and on quaternions (Hamilton's system of geometrical analysis in which there was a revival of interest at the time). Barker recognised Thomson's potential and advised him to abandon engineering and try for a scholarship at Cambridge. Thus, even before entering Cambridge, Thomson's mathematical education was geared to the requirements of the Cambridge Mathematical Tripos.

At Owens Thomson was also greatly influenced by Osborne Reynolds, Professor of Engineering, and Balfour Stewart, Professor of Natural Philosophy. From Stewart, Thomson learnt the prevalent Victorian method of reasoning by analogy. Thomson's preferred analogies were always of vortices in the ether. Many factors at Owens contributed to this preference, among them Osborne Reynolds' experiments on vortices and his use of Rankine's textbooks, Balfour Stewart's adherence to vortex atoms and Arthur Schuster's lectures on Maxwell's electromagnetic theory. Rankine, in his textbooks, used vortex models extensively to explain thermodynamics. Equally important to Thomson were Reynolds' own research experiments in the 1870s on vortices. Like many Victorian physicists Thomson's mentors Stewart, Schuster and (indirectly) Maxwell, sought

unification of knowledge in the ether which was thought to be the fundamental medium and the ultimate seat of all phenomena and Thomson followed suit, retaining his belief in the ether until his death in 1940.

Thus, even before he left Owens College and went to Cambridge at the age of 20 the major stylistic themes of Thomson's later work can be traced: the familiarity with advanced mathematics, the use of hydrodynamical analogies within an ether based physics, and the enthusiasm for research. These were reinforced by his intensive Cambridge training in the methods of analytical dynamics (the use of Lagrange's equations and Hamilton's principal of varying action).

By the 1870s analysis was central to the syllabus in the form of analytical geometry and dynamics and a system of coaching had grown up to prepare aspiring mathematicians for the increasingly competitive tripos exam. Thomson's coach, Edward Routh, was far and away the most important influence on his mathematical thinking. Routh was the most famous and successful of all Cambridge coaches. He had been senior wrangler in 1854. His original research was in analytical dynamics and he grounded his students thoroughly in its methods. Physical subjects taught included statics, dynamics, hydrostatics, optics and astronomy, Newtonian planetary motion and Electricity and Magnetism in the analytical mathematical form of Maxwell. But even in 1881 when Thomson took the Tripos, nine years after the founding of the Cavendish Laboratory, students were still discouraged from relating mathematical theory directly to experiment. The Cambridge mathematician, after years of intense coaching, viewed all physical processes as exercises in analytical dynamics.

An example from Thomson's early research shows this very clearly. Here Thomson is applying analytical dynamical methods to electromagnetism[2]. He defined any system using coordinates: x's for the position of bodies in the field, y's for the molecular configuration of bodies and z's for the electrical configuration. He then expressed the total kinetic energy (T) of the system in its most general form:

$$T = T_{xx} + T_{yy} + T_{zz} + T_{xy} + T_{xz} + T_{yz}$$

He considered each term in turn to see what type of phenomena they gave rise to because, 'if from any one phenomenon we get evidence of the existence of any of these terms, Lagrange's equations allow us to anticipate other phenomena'. For example, considering the term T_{xy}: 'The emf arising from this term is

$$-\frac{dT_{xy}}{dz} \quad \text{(see [3])}$$

This will be zero unless this part of the kinetic energy involves the electric coordinates. If it does it '... will indicate an emf depending on the velocities of the bodies in the field, which will be reversed if the velocity of every body in the field is reversed.'

Lengstrom's experiments, in which a ring of insulating material rotating with high velocity acted like a galvanic current, gave some evidence for such a phenomenon, so Thomson went on to consider the implications:

> '... the force tending to increase a molecular coordinate *y* is
>
> $$\frac{d}{dt}\frac{dT_{xy}}{d\dot{y}} - \frac{dT}{dy} \quad \text{[from Lagrange's equations]}$$

... the first term ... indicates a force depending on the acceleration of the bodies in the field'

Therefore he suggested that in Lengstrom's experiment, if the ring was charged and the velocity of rotation increased, then the motion of the molecules in the ring would be altered, which would probably be shown by an alteration in temperature. However, his analysis gave no indication of the magnitude of such a temperature change.

This was a constant weakness of analytical dynamics. It could predict one phenomenon from another but, without a detailed model of the mechanism involved, it gave no indication of the *size* of effect to be looked for. This made direct quantitative comparison between theory and experiment virtually impossible. Thomson realised the importance of quantitative experimental results, but resorted to comparing general trends seeking, in his words, 'to get results whose *magnitude* admit of being compared *roughly* with theory' [my emphasis].

Rutherford

Superficially, Rutherford's education looks fairly similar to Thomson's but the details and context were completely different. Rutherford was born and educated in New Zealand until he came to Cambridge in 1895 at the age of 24. In New Zealand he attended, first Nelson College where he received a broad basic education, and later Canterbury College, Christchurch, part of the University of New Zealand.

As a scholar at Canterbury College, Rutherford had to attend lectures in at least four subjects each year, chosen from Latin, Greek, mathematics, higher mathematics, modern languages, English literature, jurisprudence or logic and moral philosophy, physical science, and natural science[4]. Thus Rutherford, although specialising in mathematics and science, did not study them intensively as Thomson had done. And throughout his university career, he concentrated equally on both, graduating in 1894 with first class honours in mathematics and also in physical sciences.

Like Thomson at Owens, Rutherford was taught mathematics by a Cambridge man. C H H Cook was 6th wrangler in 1872 and became professor of maths at Canterbury College a year later. But, unlike Thomson's teachers, Cook had been brought up in Australia and understood the colonial need for technical expertise. For the next 15 years he campaigned for a school of engineering. In 1885 science and mathematics courses were adapted to the need for technical education, and in 1887 a school of engineering was finally founded with Cook, professor of mathematics, heading it pending the appointment of a professor. It seems likely that Rutherford's mathematical education was far more grounded in practical problems than was Thomson's, although all we know of Cook's teaching is that it was 'able, sound and orthodox'[5].

It is clear from Rutherford's early papers and the books he read that, although no mathematician in the Cambridge sense, he was well grounded in calculus, performing standard integrals and linear differential equations as a matter of routine, and could at least follow

potential theory, Gauss' theorem, Green's theorem, etc. At this level he quite confidently applied maths to science, which he learnt from Alexander William Bickerton.

Bickerton, like Thomson's teacher Balfour Stewart, was an inspiring teacher and an enthusiast for research, but there the similarity ended. Balfour Stewart's background was mainly in pure science and an ether-based physics. Bickerton, on the other hand, had had a varied career in applied science. He was a graduate of the Royal School of Mines who had been a railway engineer, been Hampshire county analyst, and taught technical classes, before going to Canterbury College in 1873. He had a gift for public demonstration and his courses on applied science and applications of electricity at Canterbury College attracted large audiences[6].

Thus, it seems that in science as well as in mathematics, the philosophy of Rutherford's education was practical. He was taught to test his experiments against prevailing theories and to devise theories which made definite practical predictions. This was shown in his first original work on the magnetic viscosity of iron, and the magnetisation of iron in rapidly changing fields, topics which had practical implications for the design of transformers etc.[7] Looking at this work, three important points are apparent:

First, Rutherford's was not an ether-based physics, and he was virtually ignorant of Maxwellian theory. Although he relied heavily on Thomson's *Recent Researches in Electricity and Magnetism* and Lodge's *Modern Views of Electricity* he totally ignored the ethereal and Maxwellian aspects and focussed on the practical implications. He used a number of equations from Thomson's *Recent Researches*, but only those couched in measurable experimental parameters. This forms a contrast to Thomson who, as we have seen, was often led by Maxwellian electrodynamics far beyond the realms of practical possibility.

Second, Rutherford had evidently been taught to relate mathematical formulae very directly to quantitative experimental results by means of graphs, a complete contrast to Thomson who scarcely ever used

curves in his own work. Rutherford's notebooks show that his first recourse on obtaining a set of data was to plot a curve for them, often with the theoretical curve sketched in also. He used this technique to compare experiments with pre-existing theory, to generate formulations of his results, and to extrapolate his data.

The origin of Rutherford's reliance on graphical methods is unclear, but probably lies in Bickerton's teaching of applied science and in Rutherford's chosen subject matter. As well as Thomson and Lodge, Rutherford based his work on Ewing's *Magnetism*, Fleming's *Alternate Current Transformer* and Gray's *Absolute Measurements*. Curve plotting was a standard technique in these more applied works, particularly in discussions of magnetic hysteresis.

Third, in his work on magnetic viscosity Rutherford acquired an important theoretical concept, that of the equilibrium between opposing forces, for instance in the time of rise of current in a coil of known self inductance or reversals of magnetisation in changing fields. This idea was to recur again and again in Rutherford's work: on ionisation of gases, in separation of radioactive products, and in determining radioactive decay series. The equilibrium concept was to Rutherford what ethereal vortices were to Thomson.

Largely on the basis of this work on magnetism Rutherford was awarded an 1851 Exhibition Scholarship to go to the Cavendish. Bickerton's recommendation should be taken very seriously: 'Mr Rutherford has ... a very full acquaintance with both the analytical and graphic methods of mathematics and a full knowledge of the recent advances in electrical science and methods of absolute measurement.'[8]

The collaboration on x-ray ionisation

At the beginning of April 1896 Rutherford had been at the Cavendish for six months, x-rays had been discovered five months previously, and Thomson had been investigating them for three months.

Thomson's work was on two fronts, the mathematical/theoretical investigations of whether x-rays might be longitudinal ether waves, and the experimental/qualitatively theoretical work on the discharge of electrified bodies by x-rays, some in collaboration with J A McClelland.

When Thomson invited Rutherford to join him it was not just because he wanted a gifted experimentalist but, very specifically, he wanted someone with practical skills in high frequency electrical experiment to elucidate a theoretical question - whether the ether could be set in motion by a varying electromagnetic field. This was a necessary condition for the existence of longitudinal electromagnetic waves[9].

This initial experiment has been entirely overlooked, largely because the results were negative and Rutherford himself dropped the subject as soon as possible. Thus, while Thomson, in September 1896, stated that the experiments were not yet complete, and eventually persuaded W C Henderson and J Henry to pursue them, Rutherford had already reported in May to the Commissioners of the 1851 Exhibition that, 'so far as the experiment goes it proves there is no movement of the ether in the neighbourhood of a vibrator,' adding that, 'the method is of course capable of detecting a very minute actual movement of the ether.'[10] He never mentioned the experiment again. It had confirmed his non-ethereal approach to physics, but provided his entrée into x-ray research where he rapidly became central to Thomson's experimental programme. While Thomson continued to devote much of his attention to the more fundamental, but less experimentally tractable, question of the nature of x-rays, Rutherford chose to concentrate on the experimental effects they produced.

Thomson approached the collaboration with a range of theoretical preconceptions about gaseous conductivity and some important experimental skills, while Rutherford had almost none - this was a totally new field for him and he had to learn afresh. Inevitably, the work initially reflects Thomson's methods and concerns. But increasingly, as Rutherford found his feet, he took control.

Since 1890 Thomson had believed that gaseous conductivity was due to the dissociation of molecules made up of oppositely charged atoms, bound together by 'Faraday tubes' of electrostatic induction which he thought of as vortices in the ether. A further development was his suggestion of chain type aggregates, which he called Grotthus chains, in the gas. These would weaken the molecular bond and lower the energy of dissociation and would form a conductor down which a discharge could pass with a velocity approaching that of light.

Thomson worked this theory out in great mathematical detail in *Recent Researches*, analysing the energy and momentum of the Faraday tubes, but was unable to make any quantitative predictions from it, or to isolate any experimentally measurable parameters, because he lacked evidence of how the tubes interacted with material atoms[11]. Despite this, the theory guided most of his work on x-ray ionisation.

He also brought two important experimental discoveries to the collaboration. The first was that x-rays discharge an electrified plate on which they fell by, he supposed, turning the surrounding gas into a conductor. He rapidly turned this into a quantitative technique, and almost all his later experiments on x-ray ionisation used the method. The rate of leak of charge from the plate was used to measure the intensity of x-rays and to monitor the output of the x-ray tubes, or alternatively to measure the conductivity of the gas.

Secondly, Thomson and McClelland reported in March that with increasing potential of the leakage plate, the rate of leak and the current rapidly increased to a maximum value and thereafter remained steady. It became 'saturated'.

Thomson interpreted the saturation current by analogy to the magnetisation curve for iron. He postulated that x-rays produced chains of molecules in the gas along which electricity could pass, as with the Grotthus chains. Generally the chains were randomly oriented, but between electrified plates the chains aligned themselves and a net current flowed. The maximum current was reached when all the chains were aligned.

Although Thomson had these quantitative techniques, he lacked a theory to fit them to, other than by analogy.

The main source of information about the further progress of Thomson and Rutherford's collaboration is a notebook covering the period from 9 July 1896 onwards[12].

It opens with a series of experiments to establish the conditions of experimental geometry, pressure etc, under which the current became saturated. They had found that the saturation current increased with the distance between the plates, i.e. the resistance of a thin layer of gas was higher than that of a thick layer. This fitted Thomson's preconceptions well. If conduction was by long chains of molecules, it was reasonable to suppose that in a confined space the chains were not able to align themselves or function efficiently, leading to a high resistance. Thus these experiments investigated aggregate formation and conductivity. Despite a mass of quantitative results no advances were made as there was still no way of linking the mathematical theory directly to experiment.

Then came a break, and a change of direction. A new page was started in the notebook and a new apparatus described. They took the significant step of blowing the conducting air down a tube before investigating its conductivity, thus isolating it from its source of conductivity, and the first things they investigated were the ways the conductivity could be destroyed (rather than, as hitherto, how it was produced). Was this where Rutherford, with his favoured interpretation of phenomena as an equilibrium between opposing forces, took a hand? Probably, for the first experiment entered in detail was to look at the effect of inserting an electrified wire down the centre of the tube.

After only a couple of runs of the experiment they concluded,

> 'The effect of having a high potential wire around which Roentgenised air is passing is therefore to rob the air of its property of discharging another plate almost completely. From the large difference in the effect obtained at the end of the long tube, it looks as if, the whole of the molecules which are split up

by the rays are used up in the passage of the leak from the high potential wire to the side.'[13]

They added, significantly,

'This would possibly explain saturation curve of the gas.'

This was not added as an afterthought; it is an integral part of the text. The explanation was characteristic of Rutherford: the saturation current represented a balance between the creation of ions by the x-rays and their destruction by the current itself. They had clearly realised this possibility even before performing the experiment and the reinterpretation of saturation current, which allowed a mathematical formulation of their results, had been conceived of before the crucial experiment which made it possible.

Rutherford had abandoned a theoretical approach which, while it had a mathematical form, was incapable of relating to experiment, and sought instead a level of theory at which he could make direct experimental comparisons. Thus he abandoned Thomson's speculations about the nature and production of ions and concentrated on measuring their observable properties. More specifically, he recognised and seized upon properties which could be cast into a form he was already familiar with, that of a equilibrium equation:

$$\frac{dn}{dt} = q - \alpha n^2 - \frac{i}{l\varepsilon}$$

where n is the number of conducting particles per unit volume of the gas, q the rate at which these are produced by the rays, α the rate at which these recombine independently of the passage of the current, i the current through unit area of the gas, l the distance between the electrodes and ε the charge on one particle.

In a steady state dn/dt is zero, so

$$0 = q - \alpha n^2 - \frac{i}{l\varepsilon} \qquad (1)$$

He used this equation to match various parts of the current curve. When the current is small it gives

$$n^2 = q/\alpha$$

hence the number of conducting particles is independent of the current, and the current will be proportional to the emf, corresponding to the straight part of the curve.
In general, though, the current is proportional to *n* times the potential gradient *E/l*. Thus if *U* is the sum of the velocities of the positive and negative ions when the potential gradient is *l*, then

$$i = n\varepsilon UE / l \qquad \text{or} \qquad n = li / \varepsilon UE$$

Substituting this value into equation (1) gives

$$0 = q - \frac{\alpha l^2 i^2}{\varepsilon^2 U^2 E^2} - \frac{i}{l\varepsilon}$$

As the potential *E* increases, *i* approaches a limit of $q\varepsilon\ell$, the saturation current.

This was a step of outstanding significance to Thomson. Having at last achieved a mathematical formulation for gaseous conductivity, he and Rutherford rapidly assimilated other phenomena. They repeated all their saturation current measurements and plotted curves of the results. They used these to estimate the fraction of dissociated molecules in the gas (about $1/3 \times 10^{12}$), to explain why a thin layer of gas had higher resistance than a thick layer (there were fewer free ions) and to estimate the velocity of the ions.

The ionisation theory proved to be a general theory which explained many of the phenomena of the discharge tube, and by 1901 was so well established that the Curies termed it 'classic' when they wrote nominating Thomson for a Nobel Prize[14]. Arrhenius, reporting on Thomson's work to the Nobel Prize committee thought this was his 'greatest achievement' and wrote that:

'By building on the principles he achieved through the study of ionised gases, Thomson has formulated a theory which includes all cases of electrical conduction through gases.... Our knowledge of the nature of

electricity has made a greater advance... in particular through the work of Thomson, than through all previous developments in this area.'[15]

Conclusion

In the x-ray work we see two men, with very different mathematical training, wrestling with the same phenomena. Thomson was an advanced mathematician, well able to formulate a mathematical theory in considerable detail and fully conscious of the value of quantitative experiment. Yet due to the nature of his Cambridge training, he was unable to match the two. In seeking a fundamental, ether based theory, he was missing the vital link of the interaction between the ether and matter which would allow him to relate theory directly to experiment.

Rutherford, while clearly a competent mathematician, was much better grounded in practical science. He had been taught to relate theory directly to experiment by curve matching and if a fundamental theory did not have quantitative experimental implications he abandoned it and sought a less fundamental one which did. It was not pure luck that in x-ray ionisation he found phenomena which could be cast into a mathematical form with which he was familiar; he was actively seeking some such parameters.

Rutherford's work can be characterised as the steady and certain advance of theory through experimentally well justified steps. Nine years later, apropos radioactivity, Larmor wrote to Kelvin, 'The way that Rutherford feels his path through all these mazes without once having to withdraw anything seems to me very wonderful'.[16] In the case of ionisation, this approach paid clear dividends. But this is not to write off Thomson's alternative methods. Thomson's work was a series of wild theoretical guesses, matched only in general terms to experiment: guesses which he frequently had to retract, and did so without embarrassment. Occasionally he was right, as in the case of the electron the following year; a discovery which, I suggest, Rutherford is unlikely ever to have made.[17]

1 Thomson, J.J. *'Recollections and Reflections'* (1936) p15
2 Thomson, J.J. *'On Electrodynamics'*. MS of Fellowship thesis. Cambridge University Library ADD 7654 UD3 from which the quotations in this account are taken.
3 Christopher Green has pointed out that the partial derivative symbol '∂' we would normally use here was probably unknown to Thomson as it was not generally used until 1890's. I am indebted to him for this information.
4 Hight, J. & Candy, A. *'A Short History of Canterbury College'* (1927) pp 26-27.
5 Jenkinson, S.H. *'New Zealanders and Science'* (1940)
6 ibid
7 Rutherford E. *'Magnetization of Iron by High Frequency Discharges'* Transactions of the New Zealand Institute **27** (1894) 481-513; *'Magnetic Viscosity'* Transactions of the New Zealand Institute **28** (1895) 182-204
8 Eve, A.S. *'Rutherford'* p12
9 Thomson, J.J. *'Longitudinal Electric Waves and Rontgen's X-rays'* Proceedings of the Cambridge Philosophical Society **9** (1896) 49-61
10 Rutherford E. *'Report to the Commissioners of the 1851 Exhibition'* 30 May 1896. Archives of the 1851 Exhibition, file 78
11 Thomson J.J. *'Notes on Recent Researches in Electricity and Magnetism'* (1893)
12 Rutherford E. *'Notebook'* Cambridge University Library ADD 7653 NB3
13 ibid. f15
14 Curie, M. & P. *'Letter Nominating Thomson for the Nobel Prize'* (1905) Nobel Archives
15 Arrhenius, S. (1906) *'Justification by the Nobel Committee for Physics of their proposed award for the 1906 prize'* Nobel Archives, Stockholm, 16ff, f14 translation from the Swedish by Karin Morgan
16 Larmor to Kelvin, Cambridge University Library ADD 7342 L38
17 see Davis, E.A. and Falconer, I. J. *'J.J. Thomson and the Discovery of the Electron'* (1997) for the speculative nature of Thomson's work on the electron

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Sir Oliver Lodge and Relativity

Dr. Peter Rowlands
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Two thoughts particularly strike the reader of Sir Oliver Lodge's many writings (over 1100, according to the official bibliography). One is that much of the development of physics is a continuum, the history of which is largely unwritten; for ideas discussed by Lodge, sometimes on an almost casual basis, frequently turn up as 'new' years later in other contexts, and this is no doubt a much more general phenomenon. The second is Lodge's exceptional powers of qualitative thinking. This ability is not as common as we think, and is undervalued because we tend to write our history in terms of mathematical formalisms. Lodge was certainly a capable mathematician, but he tended to leave the creative thinking in this area to others, often giving the impression that he was merely illustrating their mathematical ideas when often he was ahead of them (he referred to himself as a 'light skirmisher'[1]).

Lodge's conception of physics cannot be understood without consideration of the ether. This is a greatly misunderstood concept, and our persistent misunderstanding of the concept has damaged our understanding of both the historical process and the nature of physics.

The ether is, of course, well-known as the 'medium' supposed to be necessary to transmit the electromagnetic waves of Maxwell's theory. However, in Lodge's work, even as early as 1882, it was always a somewhat abstract (nonmaterial) concept, and it became increasingly so over a period of twenty years at the end of the nineteenth century. Lodge's lecture, 'The Ether and its Functions', given at the London Institution on 28 December 1882,[2] characterizes the ether as an absolutely continuous substance filling all space, vibrating as light, shearing into positive and negative electricity, constituting matter by its whirls, and transmitting by its continuity all the action and reaction of which this matter is capable; but it is clear from the context that this 'substance' is not a fully material one.

The history of the ether concept is a very involved one, and cannot be considered briefly without a good deal of distortion. Any full analysis of Lodge's contribution would need to take account of the work of his two closest associates, FitzGerald and Larmor, in addition to that of his more casual acquaintances, Poynting, Heaviside, Hertz, and Thomson, as well as the very significant contributions of his continental contemporaries, Poincaré and Lorentz. However, in approximate terms, Young, in proposing his wave theory of light, had supposed that objects moved freely through a stationary ether (1804); but this caused problems with aberration. Fresnel devised a more complicated theory in which the ether inside material bodies responded differently to the ether outside them (1818), while Stokes proposed that a moving object would drag the ether nearest to it and so change the speed of light (1845).

In their famous experiment of 1887, Michelson and Morley found no change in *c* due to Earth's motion through it. However, FitzGerald and Lorentz supposed, on the basis of electromagnetic theory, that an object such as a Michelson interferometer moving through the ether, but held together by electric forces, would contract by exactly the amount required to keep *c* constant. Stokes's theory was still a possible alternative, so, in 1891, Lodge built a machine to investigate ether drag. He would look for changes in the interference pattern produced by light beams sent along two different paths between two rapidly rotating metal discs. Like Michelson and Morley, he obtained uniformly negative results. The two experiments, when combined, implied that the velocity of light could not be used to detect the presence of an ether, a key aspect of the theory of relativity.

Lodge was President of the Physical Sciences Section at the British Association meeting in Cardiff in August 1891. His Presidential Address announced preliminary negative results on the ether drag experiment; but also contained other material relevant to the development of relativity theory; in particular, it alluded to the fourth dimension, with an early model of a 'world-line': 'events [said Lodge] may be in some sense existing always, both past and future, and it may be we who are arriving at them, not they which are happening', and he illustrated this 'possible fourth-dimensional aspect of time'

using the analogy of a solid cut into thin sections.[3] Interestingly, though 4-dimensional space-time is an important relativistic concept, in this case it had a 'spiritualist' aspect, an idea that goes back to the seventeenth century at least; and Lodge used his Presidential Address, at the same time, to stress the importance of the psychical research, which he had been carrying out in parallel to his work in physics since 1883.

Though the preliminary results of the ether drag experiment had been negative, Lodge carried on with it for several more years. The discussion in his 1892 paper on the experiment predicted the Sagnac effect, in which a beam of light is split by half-silvered mirrors, and the two beams are sent in opposite directions round a loop of mirrors, and made to interfere, as in his experiment.[4] For a stationary apparatus the beams of light will arrive at the detector at same time, but, if the apparatus is rotating, the beam travelling in the direction of rotation has further to travel, so the interference fringes will be shifted. Lodge did not observe the effect, though Sagnac did later. It is now applied as a significant correction to the GPS.

During these years, Lorentz, Larmor, Poincaré, Thomson, Heaviside and others developed Maxwell's theory (sometimes in idiosyncratic ways) to effectively discover most of the individual formulae which now constitute the special theory of relativity (STR), while the work of Larmor and Lorentz led to a kind of abstract electron theory before the discovery of the equivalent material corpuscle by Thomson in 1897. Larmor's work was particularly interesting in being a kind of pre-quantum quantum theory. He had the concepts of discreteness and probability, but couched in a quasi-classical language. He also had positive and negative 'electrons' emerging simultaneously from the ether with opposite rotational strains (left- and right-handed). Lodge contributed to this emerging electron theory with qualitative ideas and some basic calculations. In March 1897, he calculated the electron's approximate size, or classical radius. On the basis of the ether theory, he claimed that atoms containing electrons were mostly empty space, and could be represented by planetary models (1902). While Thomson originally wanted to separate his 'corpuscle' (announced in April 1897) from the Larmor-Lorentz theory, FitzGerald quickly brought in the

term ‘electron’, which he had already persuaded Larmor to use for his independent point-charges. The term had originally been used by FitzGerald’s uncle, Johnstone Stoney, for the fundamental unit of charge.

While Lorentz, Poincaré, Larmor and others, had already produced many of the familiar ‘relativistic’ formulae as by-products of their more specialized models, Einstein’s great advance in 1905 was to produce a kinematical theory that was not model-dependent, as theirs were. He didn’t depend on the electron theory of matter, or rather a particular version of it that was already being superseded. Also, while the work of the electron theorists was really a precursor to quantum theory, Einstein saw that it was possible to do a classical approximation by privileging the idea of ‘light’ in a way that is bizarre if you analyse it from the subsequent view of quantum theory, but actually works in the special case he considered. STR is certainly not an obvious development of what went before, and it is a great disservice to Einstein’s original turn of mind if we think that it is. The strange thing about STR is that Einstein uses the quantum process of light-signalling (almost certainly based on his own discovery in the same year of the light photon) as though it were classical! He creates concepts of simultaneity, light-signalling, and ‘measurement’ of a classical one-way ‘speed of light’, as though they actually have intrinsic meaning, and many people still think they have!

History and physics are also distorted if we fail to realise that Einstein’s coup was succeeded by another, equally brilliant, when Minkowski, in 1907, linked space and time in a 4-vector formalism with an invariant space-time interval:

$$r^2 = x^2 + y^2 + z^2 - c^2t^2 \ .$$

This is what we really mean when we talk about ‘relativity’, and it is what Einstein realised he had to use as the basis for his later general theory (GTR). Einstein’s brilliant coup thus led on to the next great concept, but it was neither obvious nor strictly necessary; and the positions of such original and deep-thinking physicists as Lodge, Larmor and Lorentz are inexplicable if we don’t take this into account. They were not being

reactionary by defending the ether. They were saying that Einstein's theory needed a more fundamental explanation.

By 1912 there were effectively two competing explanations of the ether drift and ether drag experiments. They gave the same answers from different assumptions: the same length contraction, time dilation and mass increase, and absence of an ether effect relating to *c*. The Einstein-Minkowski approach finally won out, about 1915, because it was not model-dependent as the original Lorentz-Poincaré theory was. A few years later its ascendancy was sealed by the success of GTR (1919). Despite this, Lodge's views on relativity never changed. According to his reasoning, *c* must be constant in absolute space, and independent of any motion of the source; but no experiment had yet shown, and no terrestrially-based experiment could show, Einstein's further supposition that *c* was independent of the motion of the observer. Relativity required the equivalent status of motion of source or observer, but Lodge believed that this was an unnecessary assumption: 'The doctrine [of relativity] certainly explains the Michelson experiment, and my experiment; nor has any experiment negatived it so far; and yet – well, it hardly seems consistent with common sense. It seems to me that posterity will formulate the doctrine a little differently.'[5]

While Lodge was prepared to tolerate STR as just another way of saying the same thing as his favoured Lorentz-Poincaré alternative, in which ether remained equally undetectable by experiments on *c*, he didn't like Einstein's later theory at all because he thought that he was being bamboozled by mathematics, with relatively simple predictions being derived from an unnecessarily complicated apparatus. What he disliked most was the press's reporting of Eddington's 1919 eclipse expedition to measure the gravitational bending of light, the experiment which finally clinched the success of GTR. On 7 November, for example, *The Times* addressed its readers with the sensational headlines: 'Revolution in science. New Theory of the Universe. Newtonian Ideas Overthrown'. Lodge himself also seems to have been singled out and set up as a straw man, for *The Times* went on to say: 'It is interesting to recall that Sir Oliver Lodge, speaking at the Royal Institution last February, had also ventured on a prediction.

He doubted if deflection would be observed, but was confident that if it did take place, it would follow the law of Newton and not that of Einstein.' And the article made a point of gratuitously recording that: 'At this stage Sir Oliver Lodge, whose contribution to the discussion had been eagerly expected, left the meeting.'

But Lodge was soon ready with his counter-attack. On 2 December, he took up Eddington's equation for light deflection:

$$ds^2 = -(1 - 2M/r)^{-1}\mathrm{d}r^2 + \text{etc.} + (1 - 2M/r)dt^2 .$$

He immediately sensed that 'gravitational redshift', which had been put forward as a further significant 'test' of GTR (and was essentially represented by the last term in the equation), was merely Newtonian in origin: 'The numerator is the squared velocity of free fall from infinity. And as a beam of light has really fallen from infinity, the expression at once assumes a common-sense aspect'.[6] He even suspected that there was a common-sense way of deriving the full expression for the bending of light.

Two years later, he argued that the new 'quaternion spatial nomenclature' (or 4-D space-time) was more compact than the old Cartesian version with space separate from time; but, though Minkowski had ingeniously incorporated the two quantities into one equation, they still remained separate things.[7] The space-time concept was *not* revolutionary, though it 'may possibly be found to have some metaphysical meaning'. The presence of the 'Maxwellian velocity' (c) in the ether theory had exactly the same effect of relating space and time. Einstein's work was a universal application of earlier results. It was, in fact, a 'fuller realisation' of the theory of the ether. This medium constituted the four-dimensional continuum or physical space-time of Einstein's theory. However, Einstein's theory did not employ the most 'ideal and direct manner', and it was 'unwise to load the new discoveries with an implication that the historical principles of geometry' had 'broken down or been detected as untrue'.

Apart from voicing criticisms, Lodge explored three new or nearly new ideas: gravitational lenses (1919); black holes (1921); and

collapsed matter stars (1921). In a letter to *Nature*, dated 2 December 1919, he proposed that one could introduce 'the simple idea of refractivity, through a diminution of the velocity of light by a gravitational effect upon the ether's elastic or dielectric coefficient, employing the same factor as expressive of a refractive index'.[8]

Jupiter might act as 'gravitational lens', two stars either side of the planet being shifted relative to each other by 1/60 th of a second. If backed by a nebula or any luminous area, the light grazing the sun's rim all round would be brought to a focus at a position 17 times the distance of Neptune, while light from any larger circle would focus still further off in proportion to the area of the circle; from a uniformly luminous area there would be a focal *line* of constant brightness.

Then, in an address to the Students' Mathematics and Physics Society of the University of Birmingham in 1921, he argued that a 'sufficiently massive and concentrated body would be able to retain light and prevent its escaping', but the 'body' need not be a single star; it could be a 'stellar system of exceedingly porous character'.[9] Versions of the classical concept of black hole had been put forward in the eighteenth century by Michell and Laplace; and Anderson had recently resurrected it. However, Lodge showed how it could apply to the whole range of possible scenarios of interest today, and he also put forward the idea of collapsed matter stars. 'For a body of density 10^{12}, – which must be the maximum possible density, as its particles would then be all jammed together, – the radius need only be 400 kilometres. This is the size of the most consolidated body. For anything smaller than that the effect would be impossible.' 'If a mass like that of the sun (2.2×10^{33} grammes) could be concentrated into a globe about 3 kilometres in radius, such a globe would have the properties above referred to; but concentration to that extent is beyond the range of rational attention ... .' However, a 'stellar system – say a super spiral nebula' 10^{15} times the mass of the sun – would not be 'utterly impossible'. 'What becomes of the radiation poured into space by innumerable suns through incalculable ages? Is it possible that some of it is trapped, without absorption, by reservoirs of matter lurking in the depths of space, and held until they burst into new stars?' He spoke of the conversion of radiation into electrons with a velocity of

intrinsic circulation of order *c*. 'On this view the interior of an enormous stellar system could be the seat of the generation of matter ... .'

Such forward thinking is typical of Lodge, and was generally based on good qualitative analysis, rather than random speculation. He also thought that the proton might be composite (which, of course, it is). It was just possible, he said, that 'the progress of discovery' will 'detach from the proton a positive charge more closely akin to the negative electron – in fact an image of it'. (In fact, a process of this type occurs in positive beta decay.) As a result of radioactivity: 'The formation of strange substances and unusual combinations may be expected and the composite nature even of the proton may yet be demonstrated by the emission of something fractional of extreme instability.' He was also one of the first to realise that, although STR didn't need the ether, GTR had to resurrect it. In November 1921 he reported: 'Eddington told me he had asked Einstein in Berlin recently, who said, 'No, I have no objection to the ether; my system is independent of the ether'.'[10] In fact, Einstein used the idea explicitly from about 1915, and even the word.

But Lodge and Larmor, in particular, still felt that something important was missing. Relativity didn't answer the fundamental question. It avoided the problem of the ether rather than tackling it. There were serious unanswered questions, which could only be answered by truly understanding the ether. Special relativity had made that more difficult by giving the impression that the ether had been disproved. Lodge couldn't see why *light* should be privileged as a source of 'information'. We must distrust, he said, the 'popular methods of explanation' for the 'Larmor-Lorentz transformations', in which light is thought of as 'bringing information about events', thus 'giving us rather confused information about what happens to railway trains and embankments'.[11] They inevitably led 'one to ask what light has to do with it; why sound or a messenger-boy should not be used instead; and absurdities of that sort'. Light was 'not of fundamental importance as the unique and only messenger', but rather as the means of measuring experimentally the fundamental constitutional velocity of the ether.

In rejecting light, however, Lodge had to think of another way of getting a handle on the ether. The only other source of information was the theory of matter. Again and again he returned to the necessity of the positive electron. In 1922, he wrote: ‘According to Larmor’s theory the positive and the negative electrons can only differ, or at least must chiefly differ, in one being the mirror-image of the other. One for example might be a concentrated locked right-handed screw twist in the Ether while the other would be a left-handed contortion of the same kind, simultaneously and inevitably produced, and contorted with its fellow by transferable lines of force.’[12] And he continued: ‘Why negative electricity should differ from positive so greatly, or in any respect save in sign, is not at all clear; and it is difficult to understand how one of these entities can have been constructed out of the ether, without the simultaneous production of its opposite partner.’ As late as October 1929, he commented in his review of Larmor’s papers that ‘the author’s dissatisfaction with the concealment or sophistication of the positive electron is manifest’.[13] However, a new theory had emerged which might provide the answer: ‘the names to conjure with’ were now Schrödinger, Heisenberg and Dirac, and the new wave or quantum mechanics would be ‘the beginning of a comprehensive theory of the ether’. Larmor, he said, sees ‘Maxwell in Dirac’.

Lodge and Larmor were absolutely right! The key to understanding the true meaning of STR is to look at the Dirac equation, to which STR is only a classical approximation, and which is inconceivable without the positive electron. It is significant, here, that space and time are not a 4-vector, as each is preceded in the equation by a different operator (or gamma matrix). Quantum Dirac supersedes classical Einstein-Minkowski! The thing that the Einstein-Minkowski formalism leaves out of the equation is the *proper time* (□) and, hence, causality:

$$c^2t^2 - x^2 - y^2 - z^2 = c^2\square^2 .$$

We are told that the space-time combination is an invariant, but not what this invariant is, or why it’s an invariant. Proper time and causality are added in STR as a ‘common-sense’ extra. But there is

nothing common-sense about it at all. Proper time occupies the position that rest mass does in the energy-momentum relation. Causality has a very specific origin in quantum mechanics, which is intimately connected with the idea of the vacuum and nonlocality. And even the rest mass has a vacuum origin in the Higgs mechanism. Einstein was able to dispense with the 'ether' (vacuum) because he left the 'ether' term out of his equation!

Of course, in the historical context, he was right to do so, and his action ultimately makes it possible to identify the term which is most significant in a vacuum context. However, Lodge and Larmor were also right to insist that something was missing and that it would be explained by the theory of Dirac. Dirac's concept of a filled vacuum, in particular, explained the + / – electron in almost identical terms to the ones they had used. Nowadays we use the word 'vacuum' to represent the concept that Lodge and his contemporaries called 'ether'. It is an expression of the nonlocality inherent in quantum mechanics (and anathema to Einstein). It is a kind of expression meaning 'the rest of the universe'. We can't define a fermion without defining its vacuum. Lodge had been moving in this direction from the start.

Lodge's ether was always a more subtle concept than many people have realized: 'Objections to the ether are really objections to the nineteenth century conception based in terms of mechanical models. No such ether exists ... .'[14] 'I have abandoned the old material ether of Lord Kelvin and the nineteenth century in favour of some hydrodynamic or other perfect mechanism at present unknown.'[15] The fact that mass is purely electromagnetic in origin, he said, must mean that all energy, including mc^2, is due to space. Lodge believed that Einstein, in his later work, fundamentally agreed with him. The two men met in Oxford in June 1933. According to Lodge's notes of their conversation, Einstein said that he had gone through three stages with respect to the ether: first, a belief in the old dynamical theory; second, total disbelief; and finally, a belief that the ether is responsible for everything, though a disbelief that it has motion.[16]

Scientific concepts seldom emerge in the clear-cut way that we like to present them, and, though the concept of 'relativity' is predominantly associated with Einstein, many other physicists played a part in shaping the theory. Lodge's contributions, though little understood today, were among the most significant – from early theoretical ideas, like worldlines and the Sagnac effect, through the experimental disproof of ether drag, to the brilliant conjectures concerning gravitational lenses, black holes and neutron stars of his later years. Not least among his contributions is the critical attitude he brought to the foundations of both STR and GTR, and his partial realization, along with Larmor, that the resolution of these difficulties required a deeper understanding in areas that we would now describe as quantum mechanics and particle physics. Some of these difficulties still remain to be resolved today.

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John Henry Poynting – A Sketch for Future Research

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The aim of this contribution is to examine the major themes of Poynting's research, their unfolding during his life and (appropriately at a meeting concerned with Poynting and Lodge who kept up a long friendship built around their work) his interactions with his contemporaries, particularly important since his own letters are mostly lost, and we must look for evidence in the archives of others. My active interest in Poynting over the past decade has been more concentrated on more personal material, searching for lost letters and family reminiscences, to capture this volatile material before it evaporates. I am particularly grateful to a granddaughter, Mrs Elizabeth Ratcliffe who has placed a box file of memorabilia in the care of Birmingham University Library. Fragmentary as many items may be, they can throw useful light on more formal knowledge. There is little chance now of more learning of this kind, and one of the aims of this paper is to sketch a program for the future,

1. Chronology

We must start with significant dates in Poynting's life, from his birth in 1852. According to J.J. Thomson, a friend from Owens College days, his formal education began in his father's school. Thomas Elford Poynting was a Unitarian Minister (at Monton Church, Eccles - now Salford). It was quite usual, for both faith and economic reasons, for Unitarian Ministers to conduct schools, and John Henry and his elder brothers started their education in this way. Thomas Elford himself had a deep interest in science, but systematic study was not possible in the Unitarian College (Manchester) where his own hard-won formal education was completed and the older universities were denied him

by the Test Acts*, and doubly so by the economic stringency of his situation and the demands of a substantial family. None the less, his enthusiasm was passed on to his youngest son, and a foundation laid so that in 1867 he entered Owens College, Manchester, with a Dalton Entrance Exhibition in mathematics, to prepare for London Matriculation, the key to all his future achievements. He was successful in 1869, and then embarked on a B.Sc. London Degree course in Maths and Physics at Owens, and achieved the degree in 1872, entering Trinity College Cambridge with an entrance Scholarship in October of the same year.

For the Maths Tripos he 'read with' E.J. Routh who had an enviable reputation for producing candidates near the top of the Tripos list. Routh's role resembled that of a freelance private coach rather than the supervisor of today. A list survives, in Poynting's handwriting, of the wranglerships amongst Routh's clients, but whether his genius resided in tutorial skills or a prescience in selecting high fliers is not clear. In any case in 1876 Poynting was bracketed 3rd wrangler. For the future it is significant that Routh and James Clerk Maxwell had been undergraduate "contemporaries and close friends at Trinity, and that, when Maxwell returned to Cambridge as Professor of Experimental Physics, the friendship was sustained.

For Poynting, further academic progress at Cambridge required a fellowship, which was awarded on an essay, and to sustain him during its preparation he accepted a demonstratorship at Owens College under Balfour Stuart. Significantly, a fellow demonstrator was J.J. Thomson, and the friendship established lasted throughout Poynting's life. In 1878, he returned to Trinity on a Fellowship, and joined Maxwell at the Cavendish, a collaboration which ended with Maxwell's sudden and untimely death in 1879. In the following year Poynting was appointed Professor of Physics at Mason Science College, in the same year marrying Maria Adney Cropper, the daughter of the Unitarian Minister at Stand, Lancashire.

The rest of Poynting's professional life was spent in Birmingham, and when Mason Science College became the University of Birmingham in 1900, he was appointed Dean of the Faculty of Science, a position

to which he was repeatedly reappointed until 1912, when poor health compelled him to relinquish it. In March 1914, he died of influenza, his condition exacerbated by diabetes. His health had been poor for many years, and he admitted that he was unable to sustain more than six hours work per day, which makes his achievements, both in physics and in the administration of the infant university the more remarkable.

As a memorial to him, colleagues and friends subscribed to produce a volume of collected work. Delayed by World War 1, this was not published until 1920. In addition to research papers, there are discussions of education policy (remember that, in the strict sense Birmingham was the first Civic University, and that, like all the newer institutions of higher education it was entering an unknown domain). Also to be found are extra-curricular lectures to students, and more formal ones for his colleagues in other departments, as well as popular expositions, for instance, for the Enquirer, a Unitarian journal. All these are characterised by a relaxed lucid style which makes them still a pleasure to read.

2. Research Papers

Turning to the Collected Scientific Papers, we can select those papers which time has revealed to be his most enduring work, and group them under the headings of electromagnetism and measurements of the gravitational constant G.

In the first of these, we designate the items as found in the collected papers by their date and title, and accompany them by a few brief comments. In the gravity measurements, the experimental work is a continuous background to his other work, and less attempt is made to distinguish individual papers (which tend to be fewer and longer), but rather to sketch out the experimental approach as it evolved.

(i) Electromagnetism

1877. *'Force on a charged particle inside a spherical shell'*

This was written while a demonstrator at Owens College, the polished mathematics of this paper shows his theoretical skills at the threshold of his career.

1884. *'Transfer of energy in an electric field'*

This paper was the origin of the universally known Poynting vector.

Maxwell's progress through electromagnetic theory was rapid, and left many outposts of ignorance, surrounded rather than overcome - for example matters of energy and momentum carried by electromagnetic waves. Here we have an area which Maxwell's early death left Poynting to explore.

1888. *'The Letters to Dr. Lodge'*
1903. *'The Examination of Dr. Lodge's E.M. Hypothesis'*

The rapid progress Maxwell made with his electromagnetic theory left many (perhaps most) other workers sceptical or uncomprehending: particularly over the displacement current concept. The two papers named above are examples of Poynting's exegisis. The collection of Poynting's letters to Lodge at University College London - the only archived collection - reveal Poynting offering mathematical help to Lodge, as did others. The tone of the letters indicates a close professional friendship throughout, and the slightly bantering manner of the 1888 letter might have strained the relationship, but this originally private letter to Lodge was published at Lodge's express request.

1905. *Presidential address to the Physical Society*.

This summarises work on radiation pressure which sprang from the 1884 energy transfer paper.

1910. *The Bakerian Lecture*

With its clearly perceived astronomical applications, this went beyond Maxwell's own work.

1912. *Pressure of distortional waves in steel*

This may seem inappropriate in this section, but it is the last of a series on wave propagation in solids. These may be mathematical jeux d'esprit, but more probably relate to the aether modelling which was very prominent in the work of Fitzgerald, Heaviside and Larmor, but much less so in Poynting's papers. Mathematical development of this kind would be congenial to one of Poynting's background and *doing* aether modelling, rather than *talking about it* would be entirely characteristic of Poynting.

(ii) Gravity measurement

In contrast to the papers on electromagnetic theory, this work is devoted to the accurate measurement of small quantities, with the patient elimination of potentially larger errors. Maxwell is on record as stressing the importance of accurate measurement, presumably in seeking confirmation of his electromagnetic theory, but his own experimental skills and interests were of a different nature. There is no evidence of discussions between Poynting and himself on this topic, but the temptation to search for the extension of the electromagnetic theory to other interactions, of which only gravity was then known, would be real enough.

Copies of letters from Poynting to scientific instrument manufacturers have recently come to light in Manchester, so Poynting must have initiated this work before taking up his Cambridge fellowship, and certainly his gravity balance was set up in Birmingham in his earliest years there. His chosen method was, and remained, based on the chemical balance, measuring the small change in deflection brought about by bringing up a large mass near to a small mass suspended from one arm of the balance beam, measuring it with a scale and telescope via an optical lever arrangement. The experiments, starting in the 1870's, continued until 1905, and throughout great pains were devoted to the attainment of maximum accuracy, and the observations are listed and tabulated in the major papers. What emerges is a labour of love rather than a necessary chore, and even when a whole year's data were rendered useless by the gradual settling of the building's

foundation it is reported with philosophical calm.

In the quest for accuracy the Oertling chemical balance was replaced by a bullion balance of larger beam though this led to further difficulties with air currents. When the flamboyant C. V. Boys developed a torsion balance with very fine drawn quartz fibres and hence a miniature construction, Poynting was ready to admit that Boys' method was inherently more accurate than his, and his gentility of character, widely commented on, was revealed in its clearest terms. At the same time, Poynting did maintain that it was important for a variety of methods for G measurement to be used and pushed to their limits of accuracy lest they were in fact measuring slightly different things - that there was richness in the nature of gravitational force going beyond the simple inverse square law relationship. This of course is exactly what Faraday had revealed beyond the rather bald concept of Coulomb's law, forming the basis of Maxwell's theory, hence in turn Poynting's starting point.

In his later gravity measurements, Poynting investigated possible anisotropies in gravitational attractions (using quartz crystal spheres rather than steel ones), and also looked for temperature effects following Faraday's lead in the exploration of dielectric properties. Disappointingly he found no measurable effects but the attempt had to be made. In a sense then, we may view Poynting's research as a single study of fundamental interactions, experimental or theoretical according to circumstances.

3. Offices and awards

The scale of institutional science in Britain in the latter part of the nineteenth century was small. The newer universities were in their infancy, with small staffs in departments heavily committed to teaching (of necessity, because student fees were the dominant source of income). As we have seen, the Cavendish Laboratory in Cambridge, the precursor of large scale academic physics research in this country, was only founded in 1872, with J C Maxwell as its director. The first

government institution directly concerned with physics was the National Physical Laboratory, founded in 1899 with Glazebrook, a contemporary of Poynting's, as director. But the scattered individuals concerned were able to interact constructively through the learned societies, with the Royal Society as the most prestigious, and the British Association the widest-reaching with its annual summer meetings held sequentially in major cities, attracting large numbers and great publicity. Further, with the growing specialisation of science, new bodies, like the Physical Society, came into being.

Poynting was elected Fellow of the Royal Society in 1888, and by 1899 was elected President of Section A (Physical Sciences) of the British Association for its Dover meeting. In 1913, though a sick man, he was Vice President of the same section at the Birmingham meeting. By 1905 he was President of the Physical Society, and in 1909 he was appointed to the Council of the Royal Society, becoming its Vice President in the following year. The esteem in which he was held by the scientific community is further indicated by the award of the Adams Prize (1891) and the Hopkins Prize (1893), both by Cambridge University, and by receiving the Royal Medal of the Royal Society in 1905, for his work on radiation. Thus in terms of academic - prominence and centrality of scientific administration, he played a very important role. From the standpoint of this paper all this is important since, in the absence of much of his own archive, the archives of the learned societies can be explored in the hope of reaching a more detailed view - something which has yet to be undertaken.

4. Individual Influences

Since the essence of science is communication, we may be compensated for the loss of so much of Poynting's archive by those of his contemporaries with similar scientific interests.

We can see what might be possible by examining the collection of letters from Poynting to Lodge, with some copies of Lodge's replies,

which is the sole documented correspondence of Poynting's, preserved in the library of University College London. The main thrust of these letters is Poynting's elucidation of Lodge's difficulties with the mathematics of Maxwell's treatise on Electricity and Magnetism.

There must, one feels, be correspondence between Poynting and Maxwell in the Maxwell archive, likewise between Poynting and J.J. Thomson, who produced between them a definitive Textbook of Physics over the years, despite their geographical separation. Larmor, who wrote a cordial obituary for Poynting (as appreciative if less intimate than Thomson's), Heaviside and Fitzgerald also come to mind, though their interests were drawn more towards the aether in which Poynting's interest seems more marginal, as we have seen. Further afield, Hertz must be considered, though there is no evidence of any correspondence on electromagnetic theory between Poynting and any European scientist (though there is a considerable amount on G measurements.)

All these considerations show that it is too early to conclude that we shall never achieve a more rounded view of Poynting's significance in nineteenth century Physics.

*The Test Acts were various statutes making eligibility for public office conditional on professing the established religion.

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Lodge and Poynting

Two brief character sketches

Dr. B.S. Benedikz

In the autumn of 1879 good Sir Josiah Mason viewed the progress of what was to be the last benefaction to his adopted town of Birmingham, the college of science where the young people of the Midlands were to be able to learn the 'pure' sciences after their schooldays and he found it was good. The building over whose construction he had presided with such concentrated energy was now all but ready for habitation, the Articles of Association for the college whose composition he had watched with such direct interest had been completed for five years, and the governing body over whose composition he had taken such care was in being. Nothing was wanting except to appoint staff, to begin work in the autumn of 1880, so that the first students could be admitted at the same time.

It was characteristic of this bounteous knight that though he did not cut corners in the endowment or expenditure of his new college, he was very much in charge of all aspects of it's making, and so, even though he had knowledgeable advisers on the appointments committee (two of whom were the excellent physicians Dr. J Gibbs Blake and Dr. T P Heslop) Sir Josiah's was the decisive voice when it came to the choice of the four successful candidates who emerged from the competition. It is also a characteristic of him that his shrewdness far outmatched his modest formal education, and he showed in these first four appointments an extraordinary acumen in picking young men who were to have truly distinguished careers in their subjects. All four of the first Professors were to end up in the Fellowship of the Royal Society, all were before their day ended to contribute remarkably to the sum of scientific achievement. Micaiah Hill, Professor of Mathematics, did not stay long in Birmingham, but he was to add lustre to the mathematics teaching and research at University College, London, and crown his career as Vice Chancellor of the great if

heterogeneous University of London. William Tilden, Professor of Chemistry did remarkable work in Birmingham before he was tempted away by the Royal College of Science (now Imperial College) in London, where he added a knighthood to his FRS before his day ended, and he is now remembered as one of the splendid band of chemists who found ways to make what had previously been organic products from artificial ingredients, in his case as the man who discovered how to make artificial rubber.

Of the two who were to come and stay, Thomas William Bridge was to acquire renown for his investigations into the animal life of the deepest rifts of the sea (notably of the Challenger Rift) from which he had built up a remarkable collection of samples which were the pride of Birmingham's Zoology museum until the vandalising Lancelot Hogben threw as many of them as he could – and any other departmental possessions not gleaming with novelty – away into a series of dustbins.

The fourth of this remarkable quartet is one of our subjects today. When Sir Josiah and his advisers came to read through the letters of commendation which were given to the finalists in the competition for the chair of physics, which according to the custom of 1860 - 1914 had been printed and distributed to the electors for ease of perusal (there being neither photocopies nor emails in those happy days!) there was one letter which must have outweighed all others – that given to Mr JH Poynting by his chief at Cambridge, Professor James Clerk Maxwell

Clerk Maxwell had not only written this powerful testimonial for his protégé, he had also died shortly before Sir Josiah and his helpers sat down and reviewed the field of applicants for the Chair of Physics and a flood of long and eulogistic obituaries in virtually every national paper (see DNB passim) must have added weight to the support he gave to Poynting. That it was Sir Josiah who chose Poynting I have not the slightest doubt , and in that choice he completed the display of his remarkable acumen in the choice of men from fields about which he can have known as little as I do!

Mr Poynting took up his appointment at the same time as his three colleagues, and they were all at that historic ceremony on 20th October 1880. Sir Josiah completed his life's work of benevolence to Birmingham by handing over the keys of the college as a symbol of its beginning of life, with its staff of seven and its 35 students. The latter were chosen by the teachers, and it says a lot for Poynting and his colleagues in1880 that out of these 35 science students *eleven* were young women!

Having been appointed, at the early age of 28, John Henry Poynting sought a partner for his life. On 9th June 1880 he married Maria Adney, who was to survive him and by whom he was to have a son and two daughters. He was contented with the chair to which he had been appointed, for he showed no signs of hunting for promotion or celebrity, but the excellence of the work he was to produce in Birmingham was to bring its tangible rewards , a Cambridge Sc.D. in 1887, the coveted FRS in 1888 (of which he was to be Vice-President in 1910-11), and the post of Dean of the Faculty of Science of the new University in 1900, which he held until failing health forced him to resign it in 1913.

While Poynting was thus quietly settling in in his new home city, the northern port of Liverpool was getting itself ready to be academically upgraded. Unlike Birmingham, where one determined man had done everything to set the ball rolling, Liverpool set to with a mass of committees out of whose endless deliberations there emerged a college, the third member of the Victorian University which was to lumber on until the component colleges fell apart after the death of the old Queen. In due course, about a year later than Sir Josiah, a college committee sat down to select a Professor of Physics and found itself with fifteen finalists, but, when it came to the point, only one truly outstanding man, whom they duly appointed to start when the college began work in the autumn of 1881.

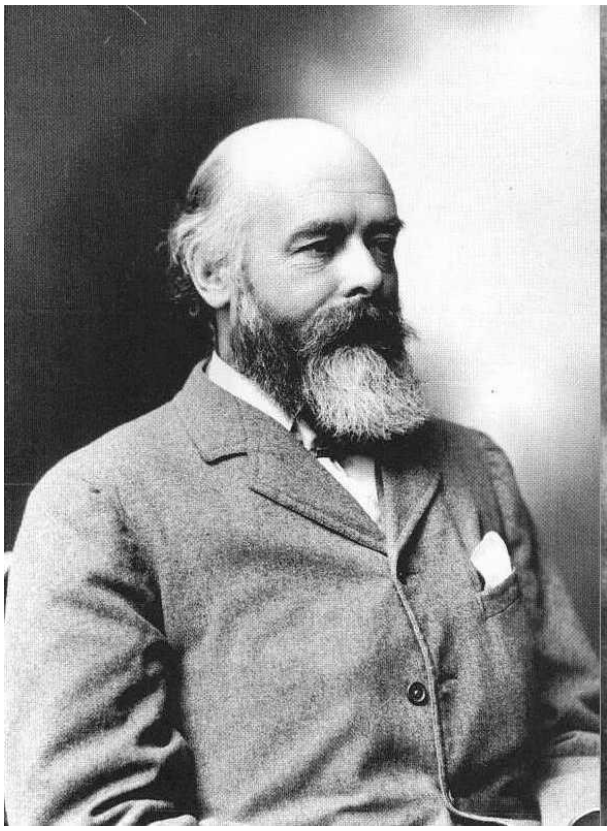
This was Dr. Oliver Joseph Lodge, then in his 30th year, in a career parallel with, but otherwise wildly different to John Henry Poynting's. Lodge, the son of a well-to-do farmer from Penkhull in Staffordshire, who had gone via nondescript schooling to University College

London, where he became attracted to his life's work by a remarkable scientist, Robert Carey Foster, Professor of Physics in the College, who was to raise the subject there from an 18th century gentleman's hobby to a field of European importance. Lodge quickly became a prize pupil and collaborator of the Professors, obtaining his B.Sc. in 1875 after a year's study**,** and (there not being PhD's to be had until many years later) his D.Sc. in 1877. The eldest of nine children (eight sons and one daughter) of his parents, he married (on 22nd August 1877, just after obtaining his doctorate) young Fanny Alexander by whom he was to have six sons and six daughters (some of whom lived into the time when I came to Birmingham), and not surprisingly he needed to look for a position with good pay. This he found at Liverpool, where he flourished for 19 years, producing there all the research by which he became known, being elected FRS in 1887.

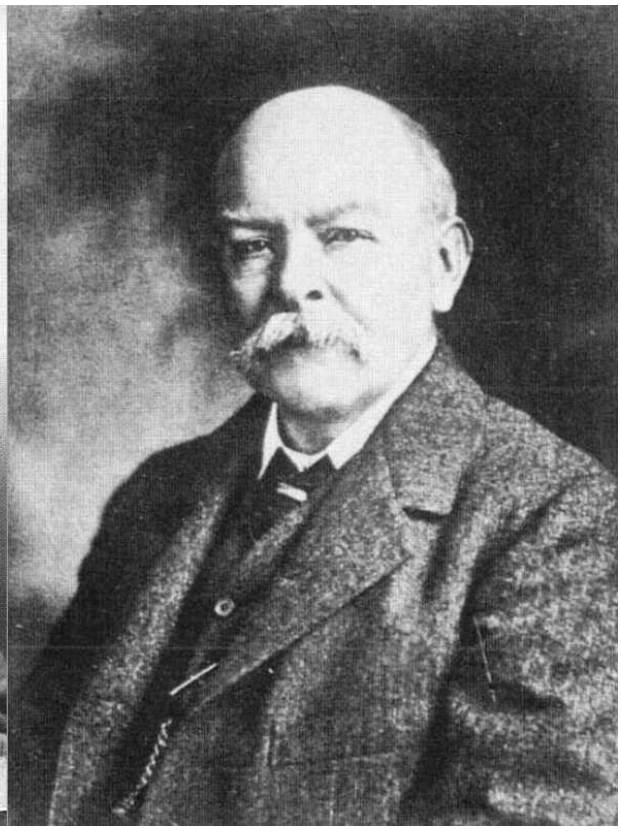
Electricity had fascinated Lodge as much as Poynting from the time when he heard Clerk Maxwell speak about it at a BA meeting in 1873 and what he heard was to set the pattern of much Liverpool research. It was however not merely his distinction as a scientist which led Joseph Chamberlain, a man as fond of getting his own way as Sir Josiah Mason, to select him as Birmingham's first Principal, an act which brought our two men together from a comfortable distance at which they could admire one another to a close proximity in which their very different characters needed to co-exist.

In this short sketch I can only draw your attention to some basic traits of these two great men which are noteworthy in any assessment of their common factors and their differences. Firstly, we can glance at their physical differences:

Poynting's icon (to use the new DNB's jargon) hangs in this very building as the visible sign of its dedication to his memory.



Oliver Lodge



John Henry Poynting

It demonstrates the man very competently, but there is an even better one, a photograph even though this was essentially taken to the greater glory of Joseph Chamberlain! It shows the entire academic staff and lay governing body of the brand new University in the summer of 1901 (it appears to have been taken in the Mason College laundry yard, but more knowledgeable old Masonians assured me that there was a reputable space at the back of the College buildings which could be used without loss of dignity for group photos of this kind. Assembled in all the robes of academic splendour which they could muster, there they sit or stand, the lay members differentiated by their morning coats and top hats. In the front row sits (naturally) Joe, on either side of him Samuel Edwards (Lord Mayor) and CG Beale (Vice-Chancellor). Lodge as Principal is on the far left and Poynting, as the senior of the Deans of the four Faculties of the University, sits on the far right of the picture.

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Even all the glory of the full dress robes of a Cambridge Doctor of Science cannot hide the fact that John Henry Poynting was a short, tubby, homely man who shrank into himself on an occasion of formal splendour such as this. And even more so Oliver Lodge, resplendent in the made-to-measure new Vice-Cancellarial robes (as worn today by his successors) does not hide the fact that he is glorying in his new status. Six foot four, his massive domed head a splendid surmount on his mighty body he is not shrinking here!

And this is a very revealing surface differentiation, for I noted at the time of gathering material for my previous attempt at a description of these two great men, that it covers a basic difference. Poynting was totally immersed in his work, whether as a constantly improving teacher or as the inquisitorial spirit, hunting with unwearying curiosity for answers to some of the great questions of the sphere of physics in which he had immersed himself. By contrast Poynting made not the slightest ripple on the Politics or Social Life of Birmingham and the West Midlands – only in the field of biology did he take an active part, being elected after long service President of the Horticultural Society of Birmingham and, in a practical way Professor and Mrs Poynting ran a farm near Alvechurch until his health failed him and he was obliged to give up agriculture and remove to a house in Ampton Rd, Edgbaston, where he lived for the rest of his life.

Poynting's health was precarious from his thirties, when he was found to be diabetic, and in the days before Banting's discovery of insulin this can have been a most inhibiting trouble as far as daily life went, necessitating a strict and austere regime, (in many ways the miracle is what he succeeded in achieving before his body collapsed). You will hear of these academic achievements, but I cannot pass the years on the farm at Alvechurch without a thought concerning how much it must have taken out of him, even allowing for the devoted help of his wife and family. The pictures do not hide the fact that at fifty plus he was already fading physically, though memorial comments by those who knew him invariably speak of his warmth of spirit, kindness and helpfulness - perhaps the best evidence is that of his former colleague Edmund Fournier d'Albe, who was in some ways influenced by both our great men, but who was very appreciative of Poynting's helpfulness in furthering his work and career, as his generous contribution to Poynting's memorial shows.

Lodge, by contrast, was a large man, robust in body and activity, outgoing in character (as befitted a father of twelve children) and an imposing figure wherever he went, and his robust health enabled him to take a far more active part in the greater activity which is the lot of the Chief officer of an institution such as a University. From the local papers and the Mason College Magazine which changed to the Birmingham University Gazette when the charter of Queen Victoria came into force, we can see how Dr. (all too soon Sir) Oliver was not only an active worker but a deliberately visible one. Where Poynting kept hidden in his office or laboratory unless he was wanted, Lodge strode around Central Birmingham as one who owned the place, and for the first fourteen years of the century, so it was.

The year of the Great War made changes not only in Europe but all over the world, east, west, north and south alike. One of them, unoticed in the outer world, was the death of John Henry Poynting, as quietly as he had lived, a victim at the last of the diabetes which had haunted him and weakened him from giving all his powers to his academic problems. *Felix opportunitate mortis*, he died on 30th March; the Archduke Franz Ferdinand still alive and a continuing peace an apparent certainty.

Lodge, at 63, had to bear the strain of heading an institution which had not been prepared for what followed over the next four years. There is no doubt in my mind that he was neither physically or psychologically prepared for it – nor could he have foreseen the effect which his fiddling with psychic phenomena over the previous 30 plus years was to have on him when a real disaster came close to him. As I suspect all of you present know his young son Raymond went to war and was killed in 1916; his body was never discovered. Though Oliver Lodge was to live another 24 years, he lost touch with the real world and became absorbed in 'etheric' experimentation; there is something infinitely pathetic in this man of great intellect haunting the studios of fake experiments with their fraudulent 'ectoplasmic' phenomena in the vain hope of recovering contact with this lost child – all the more so in that Lodge knew every trick in the fraud's books and constantly saw through them. How far he had got out of touch in his retirement at Normanton in Wiltshire may be seen from the fate of his attempt to reassert himself as a serious physicist with an encyclopaedia of the subject, the text of which lies among his papers in the Heslop room, together with the mass of correspondence from publisher and referees. Unlike Poynting, whose frail physical health had not cracked his intellectual spiritual ability when death claimed him, Lodge's splendid bodily health failed to support his mind when the time of strain came upon him.

Here in Birmingham we had two practitioners of the science of physics over fourteen exciting years. You may therefore be surprised to hear that in the last respect I view them as in some way mirror images of the great men who chose them for their tasks. Poynting's great qualities of carefulness, accuracy and infinite capacity for patient and unrelenting search for his goal, together with his lack of personal flamboyance were the things which found an echo in Sir Josiah Mason's own qualities; Clerk Maxwell's testimonial would have had a powerful impact, but when united with the characteristics visible in the young interviewee (remember, Poynting was only 27 when the Mason College post came his way!) it would have merely swept any doubts from the old knight's mind.

And in the same way, when the flamboyant but superbly able Joseph Chamberlain went a-hunting for the Principal of his new University, whose charter was all but complete except for the name, it is clear that he was not looking for a most careful pedantic master of detail like RS Heath , the sitting tenant Principal at Mason College. Joe was looking for a man who *stood out*, whose abilities were not merely noted by the small group of workers in his narrow academic field, but who would give Birmingham the initiatives to make real the vision of a Midlands university centred in Birmingham, which Joe himself had. In Lodge he found a responsive figure, not merely distinguished in electrical and etheric science, but able and willing to give life to a paper idea – and a very visible physical presence.

Such in brief is my vision of these two remarkable men. They had plenty of individual characteristics and many intellectual and other interests which would have marked them out as very special beings, and Birmingham University (and Mason College before it was chartered) was exceedingly fortunate in having their services in the crucial formative years up to 1914. However, as I look further and further into their careers I have gradually become convinced that in the last resort they owed their final career steps to personal traits; (over and above their academic and administrative abilities, formidable as these were) that in each case some part of their personalities echoed those of the great Birmingham fathers to whom it fell to appoint them, Sir Josiah Mason and Joseph Chamberlain.

* I should point out in parenthesis, that the reasons for Joseph Chamberlain's absence from the great days of Mason's last years were highly legitimate. He was Mayor of Birmingham when the foundation stone was laid, but he had just suffered the great loss of his life, the death of his much loved second wife, Florence, in a still-birth and those who knew anything of JC will know that the blow had paralysed him. He did not get on with Mason, as all who know of the commercial history of Birmingham are aware, but he was in no fit state to attend the laying, and decency was surely preserved by the presence of the Deputy Mayor, Joe's brother, Richard. As for 1880, Joe had by then entered Parliament and had just entered upon his all-absorbing new post of President of the Board of Trade.

Watch this space: the physics of an empty box

Adapted from a radio lecture for the general public

Prof. Denis Weaire
Trinity College Dublin

A fiery heaven in empty space

Michael Faraday once chose a candle as his topic for a series of popular science lectures. Its conversion of chemical energy into heat and light provided him with plenty to talk about. Thomas Huxley settled on a price of chalk to point the way for a journey through the geological ages and the fossil record. Carl Sagan used a grain of salt to illustrate and explore the fundamental laws of nature.

Lacking the eloquence of those three, I might seem to have chosen a particularly unpromising topic for this lecture: the contents of an empty box!

Let us lift the lid and look inside. While we see nothing we all know very well that the box is not really empty. It is filled with air. But ever since Robert Boyle made his wonderful air pump, we have known how to seal up the box and remove the air, more or less entirely – and what then remains? Is there anything left, that we cannot see?

If the answer was to be *no*, this lecture would be distinctly vacuous, but happily we have believed since ancient times that there remains a hidden world in this apparently empty space. It has been the task of the physicist to reveal the invisible – and sometimes to speculate about it, before it could be revealed. The physicist can sing with Porgy & Bess "Ah've got plenty of nothing and nothin's plenty for me".

So what else does our box contain?

For many centuries the official answer was *the ether*, but this could mean many things. So this lecture is about the age-old quest for an understanding of the ether, especially in the nineteenth century.

First we should trace the word back to its origins in Greece. Let me quote Sir Oliver Lodge, who wrote many books on the ether around the turn of the century.

"Appollonius of Tyana is said to have asked the Brahmins of what they supposed the cosmos to be comprised."
"Of the five elements"
"How can there be a fifth" demanded Appollonius "beside water, and air and earth and fire?"
"There is the ether" replied the Brahmin, which we must regard as the element of which the Gods are made: for just as all mortal creatures inhale the air, so do immortals and divine natures inhale the ether".

This ether was associated with a fiery heaven in which souls and gods resided. For the natural philosopher it also made up a nice matched set of fundamental constituents of nature, and could serve to account for whatever could not be handled by the standard ones –rather like the sand wedge in a golf bag.

The Newtonian ether

In throwing off the fanciful science of the middle ages, and concentrating on what could be observed, Isaac Newton and his contemporaries constructed a new view of the world in which the contact and collision of solid bodies was the dominant theme. But even at the heart of Newton's greatest triumph - accounting for the planetary orbits in terms of a new law of gravitation - there lay an uncomfortable paradox. The law of gravitation is one of action-at-distance, between bodies across empty space. As Newton himself said:

"That one body may act upon another at a distance, through a vacuum, without the mediation of anything else by and through which their action may be conveyed from one to the other, is to me so great an absurdity that I believe no man, who has in philosophical matters a competent faculty of thinking, can ever fall into it".

Gravity and other forces which act at a distance were strongly at odds with the new outlook, so it was necessary to retain the ether in one form or another, as a fluid medium through which such interactions could be passed. The ether could also carry light, which was already recognised as a sort of wave or vibration. It was natural then to think of light waves in ether as the analogue of sound waves in air.

In Dublin, Newton's philosophy was taught by Dr. Richard Helsham, who was a physician as well as a physicist. He attended Dean Swift and enjoyed many a good dinner party with him and other Dublin intellectuals. His lectures on Natural Philosophy (published posthumously in 1739) contain an interesting problem, which was to be properly solved a century later by another Irishman, George Gabriel Stokes: what is the drag force on a sphere which moves through a fluid? Helsham's motivation for including this was the recognition that the Earth should move through the ether and might be subject to a drag force, like a soccer ball moving through the air.

There was no evidence of such a drag, nor indeed of any effects of the ether other than the physical properties which it was invented to rationalise. At that stage, arguments about the ether debate were more *ad hoc* philosophy than physics.

One hundred years later, mathematicians such as Stokes had made such progress in describing elastic solids and fluids that they felt ready to construct a full theory of the ether. The ensuing debate occupied the whole of the 19th century, and it is intertwined with two of the greatest achievements of that century. They were the theory of heat, and the development of an understanding of light waves.

The many varieties of material ether

Although formidable mathematics was brought to bear on the ether, it remained elusive. Light waves do not quite correspond to the vibrations of any simple solid or liquid that we know. In an effort to fit the facts, several attempts were made to make analogies with unusual materials.

For example, Osborne Reynolds got very excited by the notion that the ether might have the properties of sand. It was to be *granular*. He recognised that this kind of material had been overlooked by the elasticity specialists and had strange properties. They are indeed very strange - if you put a large stick into a jar of sand you may easily pull it out, but if you simply tap the jar sharply, the sand will instantly settle in such a way that the whole jar can be raised by lifting the stick. If you step on wet sand at the beach, you will see as Reynolds did that sand becomes dry around your foot, when common sense says it should become wetter. Such observations drew great admiration from the likes of Lord Kelvin (who shared with Reynolds his birthplace of Belfast), but only bemusement from Reynolds' colleagues as regards the nature of the ether. His rather undisciplined ideas are well regarded today, for granular materials are a hot topic of research and – to be fair to Reynolds – we don't understand them much better than he did.

Stokes thought the ether was more like a jelly or a wax, or like the cup of thick *chocolat au lait* that Sir Gabriel enjoyed one day in a Paris café, when he wrote to Lord Kelvin in Glasgow about his idea.

Kelvin himself thrashed around with ether models for fifty years. In one of these he conceived the ether as a special kind of liquid foam, and again this has a resonance in materials research today. The hypothesis he made about the ideal structure of a foam of equal-sized bubbles remained controversial for a hundred years. It was overthrown by my research student, Robert Phelan in 1994, when he was the first to find a structure of lower energy - 0.3% less. A headline at the bottom of the front page of the Irish Times read

“Throwing shapes at Trinity”. I have regretted ever since that I had not fed the paper a better headline “Ireland beats Scotland by 0.3%” It was the morning of the international rugby match against that country.

The end of the ether

When Kelvin conceived his foam model, lying in bed in his country house, the idea of a material ether was already in decline. Its death warrant had been signed by James Clerk Maxwell when he produced a combined theory of electricity and magnetism, out of which light waves emerged naturally as fluctuations of electric and magnetic fields.

But even Maxwell himself did not at once discard the idea of an ether. Indeed he described it as follows:

“The vast interplanetary and interstellar regions will no longer be regarded as waste places in the Universe. We shall find them to be already full of this wonderful medium; so that no human power can remove it from the smallest portion of space or produce the slightest flaw in its infinite continuity".

Only after fifty years of refinement and familiarisation of Maxwell’s work did its leading proponents – the Maxwellians - firmly insist that all the properties of light could be found in Maxwell’s theory.

It was against that background that Kelvin maintained his personal determination that the ether was a “real thing”. Your models, said George Francis Fitzgerald, provide at best an allegory of the ether. "Certainly not an allegory on the banks of the Nile " replied Kelvin in a fitting joke for two Irishmen to share.

And even the Maxwellians kept the word *ether* to stand, at least poetically, for empty space endowed with Maxwell’s properties, and perhaps a little more. Listen, for example, to the triumphant George Francis Fitzgerald of TCD in 1888, the acknowledged leader of the

Maxwellians, telling the world the significance of the experiment of Henrich Hertz. (This experiment generated electromagnetic waves, similar to light waves but of long wavelength, by means of an electrical circuit, in accordance with Maxwellian ideas. As well as that fundamental significance it may be regarded as the invention of radio transmission).

Fitzgerald:

"It was a great step in human progress when man learnt to make material machines

when he used the elasticity of his bow and the rigidity of his arrow to provide food and defeat his enemies.

It was a great advance when he learnt to use the chemical action of fire, when he learnt to use water to float his boats and air to drive them.

When he used artificial selection to provide himself with food and domestic animals.

For two hundred years he has made heat his slave to drive his machinery.

Fire, water, earth and air have long been his slaves,

But it is only within the last few years that man has won the battle lost by the giants of old.

Has snatched the thunderbolt from Jove himself.

And enslaved the all-pervading ether!"

Around the same time the material ether was dealt another blow by the experiment of Michelson and Morley, which echoes that old problem in Helsham's textbook. This failed to detect any effect of the bodily

notion of the ether relative to the earth, upon light waves propagating in that ether. In today's physics textbooks, this is given a decisive role in killing off the ether, but it was in reality only one small chapter in its gradual demise. Incidentally, it was not crucial to the inspiration of Einstein's relativity either – but of such convenient myths is school and undergraduate teaching constructed.

Voices from beyond

There is another side to this story which is both amusing and sad. The mysterious ether was eagerly adopted by the spiritualists who became fashionable in the Victorian period, as a pseudoscientific justification of their claims.

By 1870 spiritualism, transplanted from the United States, had taken firm root in England. Mediums, professional and amateur, proliferated. The upper classes delighted in their performances and the leading exponents were national celebrities.

The movement found an early and influential champion from the first rank of the scientific establishment in the person of Sir William Crookes. He was impelled into that dark circle by the tragic loss of a brother. Gradually he was attracted by another emotion - he spoke of "peculiar temptations". These were embodied in the shapely form of Miss Florence Cook. Her seances featured the materialisation of another young girl, Katie King.

The scene is comic. A trivial piece of trickery, practised in the half-light, deceived an eminent man of science, whose hormones must have ruled his head. No wonder that a Hollywood movie has been considered.

Some scientists remained staunchly resistant to the new fashion and the constant invocation of the ether to support it: Faraday, Tyndall

and Kelvin were all outspoken against it. But many others - such as Rayleigh, J.J. Thomson, Ramsay, Crookes and Lodge took what Kipling called "the oldest road, the craziest road of all" leading to nothing but "sorrow in store".

Remember, in order to understand their astonishing credulity, that this was the time when all sorts of new rays emerged in the laboratory. These were both real – in the case of x-rays and various emanations from radioactive substances - and imaginary, the products of self-delusion. The spurious N-rays, discovered in France by Blondlot, were and observed in Dublin by Felix Hackett, who published his findings. It seems that only the UCD students refused to believe him!

From this to the world of occult phenomena was a small step. Of the scientists who took up took it, and became devotees of spiritualism, Oliver Lodge was the most steadfast. He was more of a heavyweight academic physicist than Crookes, and indeed he tempered his advocacy with caution most of the time. In fact he was a Maxwellian, and a great admirer of his colleague across the Irish Sea, George Francis Fitzgerald. One day when walking along the Dublin Quays, I happened to step into Lafayette's old photographic studio, which happily remains there. There, to my amazement, stood on an easel a magnificent photographic portrait of Lodge.

“Do you know who that is?” I said. “ Yes”, said the Manager confidently, “it is Sir Oliver Lodge “ and then he smiled. “But who was Sir Oliver Lodge?” Now he knows.For the studio it was just their prize example of turn-of-the-century work. For me it was like suddenly meeting an old friend.

Despite his fervent support for Maxwell's theory, Lodge still believed that, as an Irish comedian used to say "There's more". The ether was, he said, "the primary instrument of mind, the vehicle of soul, the habitation of spirit".

But like Kelvin, eventually he found himself struggling against a flood tide of scepticism as the new century dawned.

Just when it seemed that it had all been a waste of time, the paroxysm of grief engendered by the Great War created a new clientele of eager believers. In 1915, Lodge's youngest son Raymond was killed in Flanders. In his anguish he turned again to spiritualism and soon made contact with his lost, loved son.

He recounted the whole story in his book "Raymond", with the now customary chapters on life, death and the ether. It was a huge success. As the war drew to a close, the tenth edition was already being printed.

Spiritualism has since declined, but it exerts a powerful hold on a dedicated minority. The Society for Psychical Research, founded in 1882, still exists. It presumably meets regularly to engage in earnest discussions of the ether.

And perhaps this will always be so as long as we yearn for something more than a brief life and bereavement. As Yeats said :

Though grave-diggers' toil is long,
Sharp their spades, their muscles strong,
They but thrust their buried men
Back in the human mind again

The twentieth century

In science we no longer speak of the ether in the empty box. Instead we picture Maxwell's fluctuating electric and magnetic fields – we are as at home with those once-abstract fields as we are with solid matter. We may well then ask: what particular form do these fields take? In particular, how is energy distributed among the electromagnetic waves that bounce around inside the box, if we leave it alone?

This innocent question is one of the great questions of the history of science. In December 2000 physicists congregated in Berlin to celebrate the theoretical solution of the problem by Max Planck in 1900. This is regarded as the birth of quantum theory, the principal

ingredient of 20th Century physics. There is another element of myth in this notion. It was Einstein, five years later, who asserted that Planck's somewhat serendipitous formula was inconsistent with traditional physics.

One of the many consequences of the new quantum theory is that no matter how much you cool the box you cannot take all of that electromagnetic energy out of it: there is an irreducible minimum - the box cannot be emptied!.

Planck's historic formula, which describes what's in the box at any temperature, works surprisingly well if we think of the entire universe as a rather large and almost empty box. The sudden realisation that it was so came crashing in upon cosmology in the mid-sixties and is largely responsible for the current thriving industry of Big Bang Theorists. This came about in a curious way.

Two American scientists, trying to improve microwave communications, found an unexpected background hiss interfering with their efforts. They tried to attribute it to such artifacts as pigeons nesting in their antenna – but, having eliminated all such causes, finally they were driven to recognise that that it came from deep space and might have deep significance. Indeed it has.

In a further twist, cosmologists and astronomers are increasingly sure that there may also be a dark matter out there, not detectable by us in any direct way.

And meanwhile quantum field theorists insist that our box, large or small, is home to many other fields, besides Maxwell's. These represent the possible appearance and disappearance of elementary particles of all kinds.

Our box, once filled with ethereal spirits, and almost emptied again by the Maxwellians, is once more beginning to be very crowded. It is full of invisible and inevitable fields, some that we know, and more that we don't. Sir Oliver Lodge might well look down from that easel in the Lafayette Studio, wink, and say: "Well, didn't I tell you so?"

Looking forward

And what of the future?

Physicists have never been very good at futurology - for example, Rutherford, the father of nuclear physics, insisted that the exploration of nuclear energy was pure “moonshine". But at this millennial moment in time, it's hard to resist the question.

On the grand scale, dark matter will be identified. But not easily, I suspect. It may turn out to be something quite novel. If so, the 21st century will begin as dramatically as did the 20th, and dark matter will then do much more than just tidy up the models of astronomers for such things as the large-scale structure of the universe - the tenuous foam-like structure in which matter is distributed.

Looking in the opposite direction, inwards to the smallest lengths we have ever contemplated - physics on the scale of 10^{-33} cm, we are told that our 20th century physics breaks down, that space itself becomes discontinuous, like the grains of sand of Reynolds or the foam structure that Kelvin suggested. I wish I understood what this kind of new theory really means: I am as bewildered as the contemporaries of Einstein, when he insisted that a fresh start must be made, in 1905. It will certainly lead to something very new and our grandchildren will confidently learn about it at school.

I don't believe we are on the brink of a theory of everything, as some say - it seems to me more a slogan to sell books than to tell truths. Admittedly there is a certain *ennui* in physics today, a sense of convergence towards finality. Not for the first time! It has always been misguided.

Every branch of physics has enjoyed a spectacular century, and like a Caesar's army returning in triumph, we are likely to suffer a bit of hangover. *Been there, done that*. But decline and fall are unlikely.

My own branch of the subject, condensed matter physics, has transformed our society by producing the silicon chip and letting the genie of information technology out of the bottle. It is still busy providing the means to take that revolution yet further in such directions as fibre optics, with quite unforeseeable consequences.

Let me add a final and related thought from interdisciplinary science: surely one of the greatest challenges of the 21st century will be to understand the brain. I hope that physicists can help to provide the mental agility, flexibility and the boldness necessary to comprehend it. In so doing, we may even arrive at some proper scientific theory of consciousness.

And then Sir Oliver would really be pleased.

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Newton's missing experiment?

Dr.Vicente Aboites

Centro de Investigaciones en Optica, Mexico

One of the most important results of Newton's *First Paper on Light and Colours (1672)* and *Opticks* (1704) is that 'the light of the Sun consists of Rays differently Refrangible', or that sunlight is a 'heterogeneous mixture' of 'Rays differently Refrangible'. In agreement with his '*hypotheses non fingo*' assertion, Newton claimed to prove this from 'phenomena' in his one and two prism experiments. In the first experiment a sun light ray was passed through a prism. On the other side of the prism the colour spectrum is displayed and the different degree of refrangibility (or wavelength in modern terms) for each colour can be observed. In the second experiment Newton used two prisms. As before, the spectrum is produced by the first prism but by the use of a screen with a hole at the exit of the first prism, all colours are blocked but one which then passes through the second prism. No new colours were obtained, only the original one.

From the first experiment Newton claimed that sun light is made up of a mixture of differently refrangible rays, and from the second one that the degree of refrangibility is an intrinsic property of each ray and can not be modified. For Newton this result is a theorem (Theorem II, Book, I, Part I of the *Opticks*) and he states that "the proof follows from experiments". The second experiment was crucial to counter objections according to which the colour spectrum from the first prism could have been *created* within the prism. Here there are two interesting points[1]:

i) The observation that red rays are less refracted than blue ones was obtained in prisms of different material e.g. flint glass, water, crown glass. Does it follow that this holds for any transparent medium? Is this just enumerative induction? And,

ii) What if there were a transparent medium – call it "magic glass"- that Newton had not investigated and which reversed the order of refraction? That is, a medium in which red rays are more refracted

than blue rays. As a result of some experiments using water optical elements that we know Newton did, the last question may have indeed been investigated by him and is what here we call "Newton's missing experiment"[2].

As we know[3] in any substance the index of refraction n, is a function of angular frequency ω, and the change of refractive index with frequency $dn/d\omega$, is called dispersion. In 'normal dispersion' the index of refraction $n(\omega)$ increases with ω (or diminishes with wavelength λ, since $2\pi v = \lambda\omega$, where v is the speed of light in the substance). In normal dispersion if white light passes through a glass prism the blue constituent will have a higher index than the red and will therefore be deviated through a larger angle.
However, due to their internal structure, all materials exhibit absorption at certain resonant frequencies. For glasses these resonant frequencies typically occur at wavelengths of about 100nm (well in the ultraviolet and outside our eye detection capability) and this is the reason why we are used to dealing mostly with normal dispersion. In the regions immediately surrounding the resonant frequencies, called absorption bands, the dispersion $dn/d\omega$ is negative and the process is spoken of as anomalous (i.e. abnormal) dispersion. That is, in normal dispersion (within a region of normal dispersion) smaller wavelengths (higher frequencies) have larger indices of refraction whereas in anomalous dispersion (within a region of anomalous dispersion) larger wavelengths (lower frequencies) have larger indices of refraction. Since all substances possess absorption bands somewhere within the electromagnetic frequency spectrum the term anomalous dispersion, is certainly a misnomer. As already said, for glasses and many other substances the absorption bands lie outside the visible region, some exceptions are iodine vapour and fuchsine dye. It is known[4] that anomalous dispersion was first observed in about 1840 by Fox Talbot and the effect was christened in 1862 by Le Roux, however his work was forgotten and eight years later rediscovered by C. Christiansen.

It is interesting to note that in order to observe in a prism 'something somehow looking like anomalous dispersion' (i.e. that the red rays will be deviated through a *larger* angle than the blue Rays) it is not

necessary to have a prism made out of a fancy anomalous absorption material. This can easily be done for example with an air prism immersed in water or (more difficult to build) an air prism inside a glass medium. What is important for the sake of the effect we wish to observe is not only the kind of absorption we have (normal or anomalous) but the *quotient* of the refractive index $n_{\text{medium}}/n_{\text{prism}}$. In most circumstances we have air as medium (n = 1) and a glass prism (n > 1), however in order to observe apparent anomalous dispersion we need only invert the situation, having for example a water or glass medium (n > 1) and an air prism (n = 1).
It is known that Newton used to keep to himself many results of his research, so even if he did not know about materials presenting anomalous dispersion, he may or may not have done the sort of experiments just mentioned with air prisms in water. What would have been the difference for Newton's conclusions if he had also done an experiment in air with a fuchsine filled prism or in water with an air prism? We can only speculate about this question and about "Newton's missing experiment".

I believe that in any case Newton conclusion 'from phenomena' would have been the one previously stated by Worrall[1], i.e. that "the degree of refrangibility would instead be a *relational* affair between a type of ray and a type of transparent material", which is consistent with today's scientific knowledge. On the other hand, the implications of these experiments for Newton scientific methodology are very important[1,2].
To carry out Newton's experiments with water prisms, as well as "Newton missing experiment" (air prisms in a water media), can be a very instructive and interesting experience for any student both from the scientific and the historic point of view.

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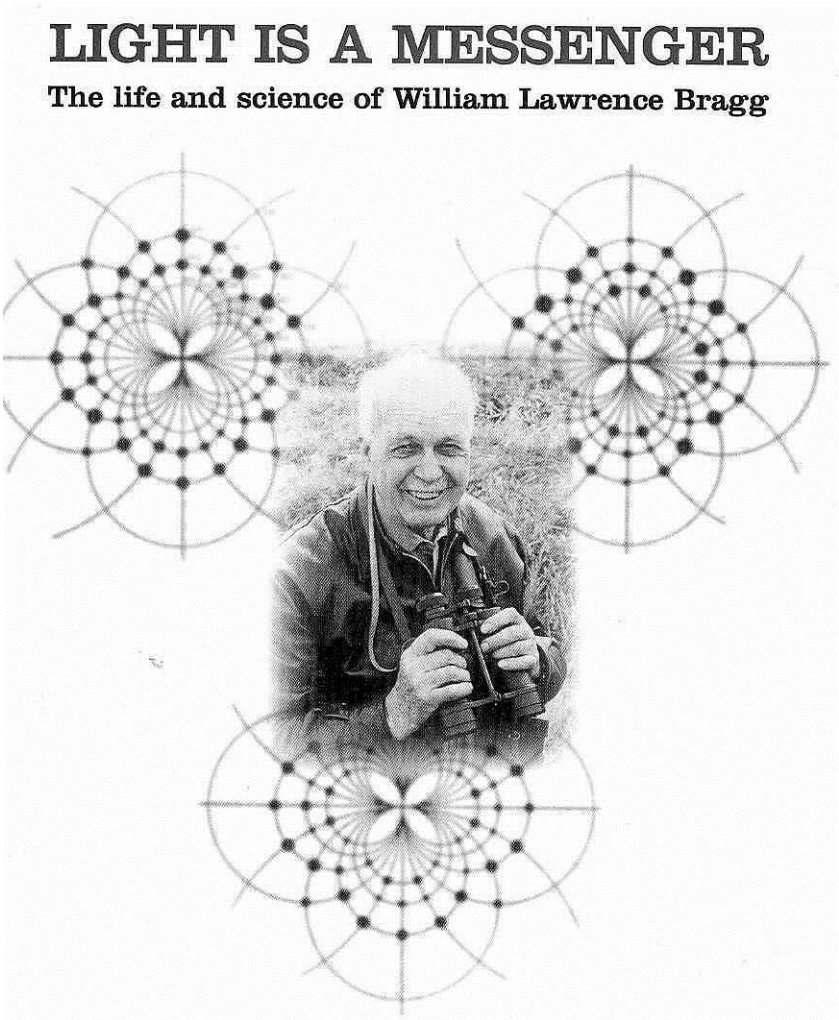
Book Review

Dr. Kate Crennell



Author: Graeme K. Hunter
Pub: Oxford University Press 2004 p301 + xvi introduction, hardback,
Price ~£35
ISBN 0 19 852921 X (Hbk)

The author, Graeme Hunter, says in his introduction that he was inspired to write this biography because William Lawrence Bragg
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remains the youngest winner of a Nobel prize. To distinguish the younger Bragg from his father, William Henry Bragg, the elder one is referred to in this book (and in this review) as 'WHB', the younger man as 'Bragg'

Bragg was also the first to celebrate the 50th anniversary of the award of his Nobel Prize. He was a remarkable man, who died in 1971, yet this is the first attempt at a biography, possibly because his interests covered a wide range of topics so that several biographers would be needed to do justice to the technical activities of their subject.

There are ten chapters, each devoted to a period of Bragg's life:
Adelaide 1886 - 1908,
Cambridge 1909 - 1914,
World War I,
Life in Manchester 1919- 1930, and following a nervous breakdown, 1931 - 7,
National Physical Laboratory 1937 -39,
World War II,
Cambridge 1943 - 53,
The Royal Institution 1954 - 66,
Retirement 1966 - 71,

Within each chapter, family events and holidays are mixed in with his scientific activities; the author states (page xv) that this is 'a scientific biography rather than a biography of a scientist' unlike those written about other crystallographers such as J.D. Bernal or Dorothy Hodgkin whose biographies were not written by practising scientists. There is a great deal of science, fascinating to a crystallographer interested in the early development of the subject, but perhaps rather too much for others reading the book in order to learn about the man. Since this is not a crystallography text book, I am not reviewing the science. There are some fascinating details such as Bragg's work during World War I on 'sound ranging' using several microphones to detect the sound of cannon and then use 3D geometry to decide where the guns were located so that our artillery could be trained in that direction to destroy the enemy weapons.

Unlike Einstein or Rutherford, Bragg did not have a charismatic personality; he was a private man with a conventional home life, as can be seen in the family photographs. Nor like Einstein or Rutherford, did he stick to one branch of science, he thought of himself as a physicist but his work touched on chemistry, mineralogy, biology. He may be said to have founded the field of protein crystallography, yet he easily became depressed and felt ignorant of mathematics and chemistry. He was not interested in administration but he was enthusiastic about lecturing on science to the general public. He also had an artistic temperament, encouraging his wife to use electron density maps as embroidery patterns. Before his friend C.P. Snow gave his lecture on 'the Two Cultures' Bragg was active in promoting the idea of science as providing a good all round education despite the poor coverage of scientific issues in the media.

He did not get on well with either of his parents, (see Page 104 for relationship with his mother and page 142 that with his father, WHB, whose mother had died young and his father abandoned him to be brought up by his uncles.) Bragg's younger brother, Robert, was much more cheerful and merely laughed at their mother's exaggerations instead of trying to reason with her. He was killed at Gallipoli in 1915, had he lived, perhaps Bragg would have learnt from him how to handle his parents. Bragg was born in Australia in 1890 and brought up there for approximately the first 20 years of his life. The contrast between sunny Australia and what were then grey gloomy cities of Leeds and Manchester, may well have contributed to his bouts of depression. Seasonal Affective Disorder (SAD) had not been discovered then, perhaps all he needed was some artificial sunlight.

This lack of communication did not extend to his students, he had a gift for choosing extremely able students, many of whom later became eminent 'founding fathers' of crystallography. Later students included Max Perutz and David Blow who were encouraged to work in new fields in which Bragg, using his ability to grasp the essential point of problems, could see ways to tackle with experiments which others thought impossible.

His understanding of the relationship between 2D diagrams and the 3D world enabled him to devise the 'Bubble raft' model as a way of thinking about dislocations in metals. During his time at the RI he encouraged lectures aimed at explaining science to the general public and gave some of the 'Christmas lectures' himself, some even televised but it seems unlikely that any recordings are still around. A few snippets of film have survived, and some were shown during the History of Physics meeting in Birmingham Nov. 2004.

Bragg originally suggested to Watson that he write the popular science book *'The Double Helix'* about the discovery of the structure of DNA and wrote the preface. A large part of the final chapter on Bragg's retirement is taken up with this controversial work. In a time when Britain was more discriminatory than it is today, Bragg had encouraged minorities to work in science. Several women and Jews worked in his laboratory; he nominated Kathleen Lonsdale to be one of the first 2 women Fellows of the Royal Society.

Comments on book production:

1 I would have preferred the photographs to be printed near the text to which they are relevant. Instead all 20 black and white photographs are bound together without any reference to them in the Contents page.

2. The author has been poorly served by the OUP editorial staff, who could have suggested that since this is the first biography of this important scientist it would have been useful to have a few appendices, giving a list of his major publications, a bibliography of the larger works consulted, and a time line listing his major achievements for reference purposes.

Anyone searching for information has only the chapter headings and index to guide them. Although there are many entries in the index, it lacks the clarity and ease of use of a 'time line'.

3. The reference list is over long. The 1008 references occupy pages 252 to 291. However, these are not all unique references. In my experience most authors of scientific papers refer to a given publication using the same reference number no matter how many times they refer to it.

4. The book is printed on unusually shiny paper, causing annoying reflections in some types of lighting

Conclusions:

The author has worked hard to include much detailed science and many fascinating historical records of Bragg's, travels and scientific life but the chronological approach of this book fails to give an overall picture of how Bragg succeeded in making so many momentous discoveries.

So should you buy this book?

Yes, if you want a detailed history of Bragg's achievements and are prepared to browse through it looking for specific facts.

No, if you want an easy to read reference work where you can quickly look up details such as when he was knighted.

Selected Bibliography (taken from the list of references)

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ISBN 0-905927-43-5 308 pages.
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The History of Physics Website

This is reached from the IOP main pages by selecting first 'Groups' and then the 'History of Physics' from a list of groups. The top page is just a statement of our history and aims and it rarely changes.

On the left is a panel with a list of topics, click on the item you want to see. These are updated as I receive information. Please send me your news items for the 'Latest News' pages and any new links relating to the history of physics for the 'Links' page which I have re-ordered into general physics and physics related sites, specific areas of physics, biographical sites and Museums. In the 'Archive' pages you can find a list of previous meetings and some of the past newsletters. I would be most grateful for a more complete list if anyone can send me details.

Please can you look at the pages about 'Blue Plaques' where I have tried to collect details of all the Blue Plaques which have been funded by the Institute of Physics. This has some biographical information about some of the people, I am still looking for details of others and the O.S. Grid Reference of each location to allow visitors to find the plaque more easily.

Please can you try to photograph any plaques which are near you, or you happen to see on holiday, and send me either an image or a print which I will return after scanning. My aim is to get photographs of each plaque and its surroundings on to the web site together with related links to each person.

Comments, corrections to these pages and suggestions for new pages are always welcome.

Kate Crennell (web editor)

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Bob Chivers 1948 - 2004

It is with regret that we have to report the death in November last year, of former committee member Bob Chivers. Neil Brown remembers Bob's quiet but committed enthusiasm.

Bob had attended occasional meetings of the History of Physics Group and at one such meeting in 1998 he indicated that he might be willing to join the group committee and (in the usual manner of such organisations) the offer was accepted with alacrity at the Annual General Meeting later in the year.

He was always very interested in seeing that the evidence of the work of physicists was not lost and he spent much time over several years clearing up the papers of a former colleague who had worked (if my memory is correct) in the field of acoustics. Bob was concerned to find a 'good home' for books and papers that, though not necessarily very old, were rare and unusual. I was pleased to be able to help by placing some of these in the Science Museum Library, which was very grateful for them.

The History of Physics Group committee will miss his contributions, and, though I cannot claim to have known him closely or had very frequent contact with him, I will miss him. He gave valuable service to that committee, working particularly closely with my successor as Honorary Secretary, Sophie Duncan.

He remained a member until October 2004, shortly before his death. We need people like Bob who are prepared, quietly and unostentatiously, to do 'good works' even though they might not think of them in those terms.

Neil Brown.

Former honorary secretary of group.

Future events

History of Physics Group:

Provisional notice:

October 29th, 2005 University of Glasgow

AGM and lecture series on the theme – ' Lord Kelvin'

In line with usual practice, the Annual General Meeting of the group will be combined with a series of lectures – this year on William Thomson, aka Lord Kelvin - details to be announced later. It is also hoped that a visit to the recently opened exhibition 'Lord Kelvin – Revolutionary Scientist' at the Hunterian Museum would be included.

April 4th – 5th, 2006 University of Liverpool

An International Symposium to mark the Centenary of the birth of Herbert Fröhlich FRS'

Further details to be announced later.

Other organizations

June 23rd, 2005 Collins Barracks, Dublin

'Science and Technology in Ireland 1780 – 1920'

Organized by the British Society for the History of Science and **The Royal Irish Academy**

More details at www.bshs.org.uk/conf/2005ireland/

July 1st - 3rd, 2005 **Keele University**

'The History of Electrical Engineering'

Organized by School of Chemistry and Physics and **the IEE History of Technology Professional Network**

More details from Alan Darlington, email: pha56@phys.keele.ac.uk

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July 15th – 17th, 2005 **University of Leeds**

International History, Philosophy and Science Teaching Group's 2005 Conference in conjunction with
'The annual meeting : BSHS 2005'

More details at: www.bshs.org.uk and www.ihpst2005.leeds.ac.uk

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September 3rd – 10th, 2005 **Dublin**

'The BA Festival of Science in Dublin'

One of the UK's biggest science festivals bringing together scientists and science communicators who reveal latest developments in research to a general audience.

Also involved is the **British Society for the History of Mathematics**, organising the Thursday session under the title of:
'Maths that changed the world: key mathematical ideas from three ancient cultures'

More details at: www.the-ba.net/the-ba/Events/FestivalofScience

September 11th – 13th, 2005 Science museum, London

International conference:

'Cross-connexions: Communications, Society and Change'

'The aim is to stimulate scholarly research in the history of telecommunications and to bring together all those interested to discuss developments in telecommunications.'

Organized by The Newcomen Society, IEEE, Connected Earth and the Science Museum.

October 7th – 8th, 2005 Oxford University

'Euclid and his heritage'

A Clay Mathematics Institute Conference on the occasion of the publication, for the first time, of a complete digital edition of the oldest surviving manuscript of Euclid's Elements.

More details at: www.claymath.org/euclid

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News

The group's chairman, Denis Weaire, is to join the Royal Society's Library Committee this year and will be nominated in November to take over as Chairman. "I am very excited by this opportunity to learn more about the Societies historic archives and collection, and the exhibitions that they support" he told us.
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Abstract

J. Phys. B: At. Mol. Opt. Phys. **38** (2005) S437-S448

1905-a miraculous year

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Abstract

The article discusses Einstein's famous papers of 1905 - his miraculous year – and deals with their physical and historical context as well as their fundamental impact on modern physics. It shows that the papers are not isolated, but connected with each other by Einstein's deep-seated conviction of physical atomism and his criticism of an ether. They are concerned with specific problems that can be characterized as 'borderline problems' since they go beyond the traditional divisions between mechanics, electrodynamics and thermodynamics.

This paper is part of a special edition of ***Journal of Physics B*** issued to celebrate the centenary of Einstein's 'miracuous year'.

Further details from: custserv@iop.org

Book Notice

'The Correspondence between **William Rowan Hamilton** and **Peter Guthrie Tait'**

Edited by David R. Wilkins, Trinity College Dublin

Wilkins has spent many years analysing this and other Hamilton material, so this will be an authoritative version of an important mathematical dialogue.

Quaternions, Wave optics, Conical refraction etc.

To be published by TCD Physics Department, July this year.
For further information contact:

Physics Departmental Office
Sniam Building
TCD
Dublin 2, Ireland email: physics@tcd.ie

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Wanted!

Articles, letters, comments - *long or short* wanted for your Newsletter.

Please send to *Malcolm Cooper, Editor*

Also news items for your website –

Please send to *Kate Crennell, Web Editor*

Contact addresses – see page 4
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