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A purely local experiment—Poynting and the mean density of the Earth*

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Abstract. These days John Henry Poynting is best known for his association with the Poynting vector, which describes the flow of energy in an electromagnetic field, and little is known of his life or work. Yet in the 1890s he caught the popular imagination as 'the man who weighed the Earth'. His experiment, using a novel method with a common balance, was part of a heroic tradition, gained him Cambridge University's Adams Prize, and set new standards of precision. Yet in performing this experiment, Poynting seemed to step outside the traditions of late nineteenth century Cambridge University where he was educated, and it is probably significant that he was a Unitarian throughout his life. This paper examines Poynting's experiment and his interpretation of it in the context of the rest of his life and work.

Keywords: density of the Earth, gravitational constant, beam balance, common balance, J H Poynting

In 1891 John Henry Poynting announced the results of an experiment to determine the mean density of the Earth. In many ways the experiment was a failure: it added little to our knowledge of gravitation, or the Earth, and the method was superceded even before the results were published. In scientific terms it was far less important than his theory of the transfer of electromagnetic energy. Yet his experiment, in the heroic mould, gained him Cambridge University's Adams Prize and caught the popular imagination: he was known locally as 'the man who weighed the Earth'.

Poynting himself is something of an enigma. 'John Henry Poynting was a man admired of all who knew him, beloved of all who knew him well', wrote G A Shakespear (1920), one of Poynting's close colleagues. Yet today, he is generally remembered solely from his name attached to the Poynting vector, which he formulated in 1884 to describe the flow of energy in an electromagnetic field. The real man lurking behind the vector has been overlooked by both physicists and historians and little remains to cast light on the qualities of scientific judgement and personal understanding for which he was revered. J J Thomson, for instance, counted his friendship with Poynting 'one of the greatest joys of my life .... He had exceptionally sound judgement, a very original and acute mind . . . a genius for friendship and a sympathy so delicate that, whether you were well or ill, in high spirits or low, his company was a comfort and a delight. During a friendship of 40 years I never saw him angry or impatient, and never heard him say an unkind thing about man, woman or child' (Thomson 1936, pp 22–3). It is in these testimonies that we begin to see the coherence of Poynting's scientific and professional work and his Unitarian faith. He was sympathetic not only to his friends but, 'with humanity in general and especially with the poorer classes. As a magistrate he tempered judgment with mercy and his experience in this capacity confirmed him in the opinion that delinquents in general were as often sinned against as sinning'. And, 'In politics he was a liberal, and perhaps the thing for which he stood more strongly than anything else was freedom and liberty of thought: the thing of which he was most intolerant was bigotry, and indeed in an intimate acquaintance of 20 year's duration the only time I ever heard him speak of any man with bitterness was in reference to a case of religious intolerance' (Shakespear 1920).

Thus he upheld the Unitarian ideals of tolerance and public service. These ideals, and a philosophical appeal to reason rather than dogma in science as well as in religion, underpinned his scientific work.

John Henry Poynting was born in 1852 at Monton, near Manchester, the youngest son of Thomas Elford Poynting, Unitarian Minister of Monton. Initially he seemed destined to become a conforming member of the group of 'Cambridge Mathematical Physicists' of whom Kelvin, Stokes, Maxwell, Thomson and Larmor are the best known and who were mainly concerned with matter, the ether and electromagnetism.

He was educated at first at the school run by his father, then, in 1867, he went on to Owens College, Manchester with an Entrance Exhibition in Mathematics. It seems likely that here, studying natural philosophy under William Jack, Poynting learnt the painstaking experimental method evident in his later work on gravity. In 1872 he took an external London BSc, studying physics with Jack and mathematics with Thomas Barker. In the same year he obtained an Entrance Scholarship at Trinity College, Cambridge. He

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read mathematics, was coached by Routh, rowed for his college and graduated as 3rd wrangler (i.e. third in his year) in 1876. (This was all standard stuff for a Victorian 'Cambridge Mathematical Physicist'.)

After graduating, Poynting returned to Owens College as a demonstrator. Within a year he had work published—not on electromagnetism, nor on light, energy, heat or vortex atoms, but on the statistics for drunkenness in England and Wales, and on the use of a common balance to determine the mean density of the Earth. These two elements, public service and painstaking experimental measurements of gravitational effects, were to dominate much of his career. The gravitational experiments, started in Manchester in 1877, were moved successively to Cambridge, where Poynting obtained a fellowship in 1878, to Mason College Birmingham, where he became the first Professor of Physics in 1880, and then to the new buildings of the University of Birmingham, in 1910. Poynting was one of the prime movers in the development of Mason College into the University of Birmingham in 1900 and was largely responsible for the planning and organization of the new technological departments and their buildings. He became Dean of the Faculty of Science in 1900. In addition to the effort involved in twice planning and equipping a new physics laboratory, preparing lectures, doing research and caring for a growing family (he married in 1880 and had three children), he found time to serve as a Justice of the Peace and as a member of the licensing committee and, at various times, as President of The Physical Society, Vice President of the Royal Society, on the Gassiot Committee of the Royal Society and on the Sights Test Committee of the Board of Trade. Poynting carried out all this despite continual poor health, the first signs of diabetes, of which he eventually died in 1914.

We can only speculate about the origins of his commitment to public service and to gravitation measurements. His Unitarian background was undoubtedly important in interesting him in matters such as drunkenness and licensing, and this was reinforced by his marriage to Maria Cropper, daughter of John Cropper, Unitarian Minister of Stand. His concern with the proper use of statistics is the first real evidence of the Unitarian influence on his science, for this is something he certainly did not acquire from his Cambridge education and was out of line with Cambridge interests. Unitarian social reformers, on the other hand, had been collecting and using population statistics since the 1830s.

Sir Arthur Schuster (1910, p 30) has left us an account of the beginnings of Poynting's gravitation work. 'Poynting had been acting as demonstrator to Balfour Stewart, who was then making experiments at Owens College, Manchester, to find, if possible, a change in weight in lead discs when first placed vertically and then horizontally on the balance plate. In trying to calculate the attraction of surrounding bodies which were sufficiently near to produce a disturbing effect when the position of the lead disc was changed, it occurred to Poynting that a large mass which could be introduced or removed from underneath the balance pan might affect the apparent weight sufficiently to allow the gravitational constant to be determined.'

This account raises a question. Why did this problem interest Poynting? His mentor, Balfour Stewart, was undoubtedly testing the constancy of gravitational attraction under various conditions. As well as the experiment with lead discs, in 1874 he attempted to detect a change in weight on chemical combination (Thomson 1936, p 20). Such questions formed a valid part of the search for an explanation of gravity and, hopefully, a consequent grand unification of physics. But Poynting deliberately eschewed these questions, at least in print, until much later. He generally emphasized that what he was doing was measuring the mean density of the Earth. Although his final paper on the experiment (to the Royal Society in 1891) gave the measurement of G, the gravitational constant, as the first aim of the experiment, he rapidly passed on to discuss the use of the results to measure the mass of the Earth. This seems a singularly uninteresting aim for a Cambridge Mathematical Physicist. Maxwell, despite his support for Poynting, was to refer to it as 'tedious' (Maxwell 1877).

The impression left by his writings is that it was the sheer instrumental challenge that appealed to Poynting. In the many histories of methods of weighing the Earth which he gave in later years, Poynting recounted with enthusiasm the difficulties encountered by many 'Earth weighers', from the blizzards and sandstorms in the Andes which hampered Bouguer's measurements, to the fires and floods in mines suffered by Airy (see e.g. Poynting 1894). He evidently saw himself as part of a grand instrumental tradition, and his determination to make a novel method work carried him through 13 years of experiments.

The novelty of Poynting's method lay in the use of a common balance, rather than a torsion balance. Very briefly, the principle is this. Consider a balance with two equal masses, M_A and M_B. They are both equally attracted to the Earth with a force

$$F_A = GM_E M_A/R^2 = GM_E M_B/R^2 = F_B$$

where F_A, F_B are the forces on M_A and M_B respectively. M_E is the mass of the Earth, R is the radius of the Earth and G is the universal gravitational constant.

If a large, extra, mass M is put under M_A at a distance r, then M_A is attracted down by an additional force,

$$F'_A = GM_E M_A/R^2 + GMM_A/r^2$$

while the force on M_B remains constant. Hence the balance tilts, giving a measure of

$$F'_A - F_B = GMM_A/r^2.$$

Since M, M_A, r and $F'_A - F_B$, are known, G can be calculated and from this the mean density of the Earth can be obtained from the expression

$$g = GM_E/R^2 = G\Delta V/R^2$$

where Δ is the density and V is the volume of the Earth. Hence $\Delta = gR^2/GV$.

Inevitably, it is not as simple as this. Allowance has to be made for the attraction between M and M_A before M is moved into position, for the attraction of the beam, and of M_B to M, and methods devised for measuring or cancelling these. Also, the apparatus had to be operated from a distance sufficient to prevent disturbances by the operator.

Traditionally, the common balance was considered unsuitable for measuring such small forces, first because of the greater disturbance produced by changes of temperature, such as convection currents and unequal expansion of the arms of the balance, and second because of the errors arising from raising the beam between each weighing, resulting in varying flexure of the beam and inconstancy of the points of contact of the knife edges and planes. However, Poynting was determined to make the balance method work and set up preliminary experiments in the cellar under the chemistry lab at Owens College, using an Oertling chemical balance with a 16" beam (probably purloined from the chemistry labs). H F Newall (1910, p 105) has left us a vivid account of Poynting's experimental environment later on in the basement of the Cavendish Laboratory at Cambridge: '. . . I was allowed to use a fine balance in the basement-room, where Poynting was making his preliminary experiments with reference to redetermining the mean density of the Earth. It was of importance not to disturb the operations: and so before going into the balance-room the procedure was to see if the operator was 'stalking his balance.' There is still, I believe, a hole in the south wall of the balance-room; and it was through this hole that the excessively delicate balance was observed with the help of a telescope by the experimenter, who had to protect his instrument from the attraction and temperature-effects of his own body.'

Poynting solved the problem of raising the beam by leaving it lowered all the time and clamping it in position while the weights were changed but, despite all precautions, disturbances due to temperature remained a problem, particularly in unsettled weather.

He was sufficiently encouraged to want to go further, though, using a larger balance which he hoped would reduce the significance of temperature disturbances. He published his results, emphasizing the instrumental nature of his investigations (Poynting 1879, p 12). 'The results . . . have no value in themselves, but they serve as an example of the employment of the balance for more delicate work than any which it has yet been supposed able to perform,' and that, 'the difference between two weights in any one series of weighings can be measured with a greater degree of accuracy than has hitherto been supposed possible.'

Poynting sent his account to Trinity College, Cambridge as a fellowship dissertation and Clerk Maxwell acted as referee. Maxwell seems to have entered fully into Poynting's instrumental view of the work, remarking to Joule, 'You see that the age of heroic experiments is not yet past' (Schuster 1910, p 31) and elsewhere that 'It is rare, especially in this country, to find the requisite mathematical ability combined with mechanical ingenuity made available for so delicate and tedious a research by the still higher endowment of patience and constant pertinacity.' (Maxwell, 1877).

Poynting obtained the fellowship and moved his experiments to the basement of the Cavendish Laboratory in 1879, while Maxwell helped him obtain a grant from the Royal Society for a new balance specially made by Oertling. This large bullion balance with an extra rigid beam of about 125 cm long (123.329 cm) was the balance which Poynting used for the next 11 years, while the experiment suffered a series of refinements and interruptions. The balance is still in use today at the National Physical Laboratory for weighing secondary standards (figure 1).

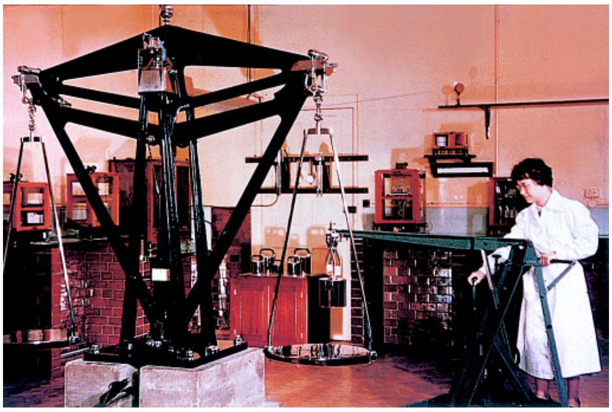

Figure 1. Poynting's balance at the National Physical Laboratory. Courtesy of the National Physical Laboratory. Crown copyright 1974. Reproduced by permission of the Controller of HMSO.

The first of many interruptions was his appointment as Professor of Physics at the new Mason College, Birmingham, in 1880. The move to Birmingham may well have been prompted by Poynting's Unitarian faith and his desire to get married, which he did a few months later. Poynting's future at Cambridge had seemed rosy—he had a fellowship, was highly regarded by established Cambridge physicists and had good working conditions. But although at that time fellows had recently been allowed to marry, their salaries were still based on the assumption that they were single and living in college. Birmingham undoubtedly meant more money, and Poynting clearly already had a lady in mind. Moreover, Cambridge was still Anglican in outlook. Religious tests for BA degrees had been removed only in 1871, the year before Poynting went up as an undergraduate and he may well have found Cambridge spiritually uncongenial. Birmingham, with its strong Unitarian presence and the challenge of setting up a new college for an industrial population, was probably a much more attractive proposition.

The experiment was set up in the basement at Mason College in Edmund St, Birmingham (figure 2). It is apparent that the work was sporadic. To quote Poynting (1892, p 566), 'Many times work has been begun and results have been obtained, but examination has shown them to be affected by large errors which could be traced and eliminated by further improvements in the apparatus.' Other things also intervened. Poynting's departmental duties always came first, and other research interests diverted him: theoretical investigations of the changes of state in the early 1880s and of the transfer of energy in an electromagnetic field, resulting in Poynting's theorem, in 1884–85. He was also in poor health and at some point in the 1880s he and his family moved to Alvechurch, 12 miles out of Birmingham, where Poynting took up farming in his spare time. They remained here until 1901 when pressure of university work forced a move back to Edgbaston.

In 1888 Poynting returned to the experiment: '. . . after a complete determination . . . of the mean density, when I

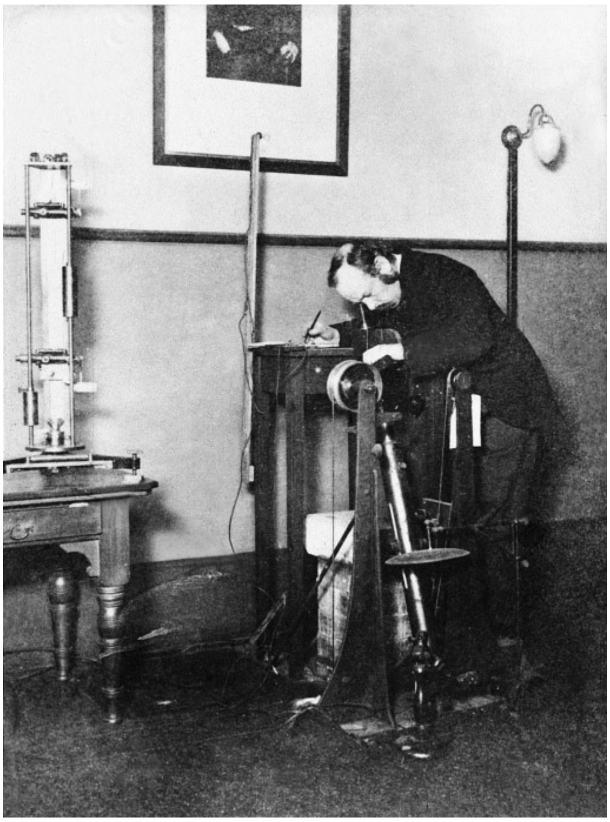

Figure 2. Poynting ‘stalking his balance’ at Mason College, Birmingham. The balance, in the basement, is observed through a telescope from the floor above. Reproduced courtesy of Birmingham City Library Services.

supposed that the work was finished, an examination of the results showed some curious anomalies, which I could only ascribe to a tilting of the whole floor on the displacement of the mass.’ (Poynting 1892, p 567). This tilting, moreover, was gradually increasing, probably due to the floor of the new building settling down and becoming increasingly rigid.

At this stage Poynting added a balancing mass to the turntable (figure 3)—the attraction of this to all other parts of the apparatus now also had to be allowed for.

At the beginning of 1890 the modified apparatus was back in working order and it was then that Poynting published his final measurements. His results were:

$$G = 6.6984 \times 10^{-8} \text{ dynes cm}^2 \text{ g}^{-2}$$

$$\Delta = 5.4934 \times \text{density of water.}$$

He later picturesquely summed up the sensitivity and accuracy of his experiment (Poynting 1894, p 22): ‘Imagine a balance large enough to contain on one pan the whole population of the British Islands, and that all the population were placed there but one medium-sized boy. Then the increase in weight which had to be measured was equivalent to measuring the increase due to putting that boy on with the rest. The accuracy of measurement was equivalent to observing from the increase in weight whether or no he had taken off one of his boots before stepping on to the pan.’ (figure 4).

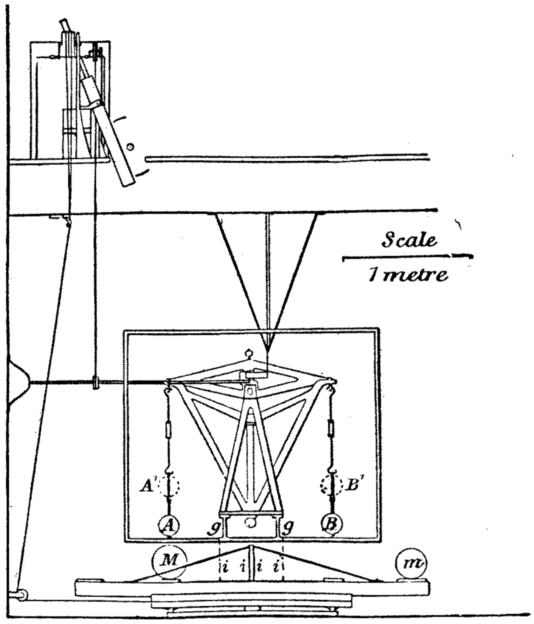

Figure 3. The final form of Poynting’s experiment—elevation of balance-room and observing-room. The front of the case is removed and the front pillar is not shown. The pointer and mirrors are at the back. Taken from Poynting (1892).

Poynting’s experiments had taken him 13 years. During this time von Jolly and König and Richarz had initiated common balance experiments on gravitation, and Poynting’s method was decisively overtaken by the torsion method following Boys’ discovery of the torsion properties of quartz fibres (von Jolly 1880, König and Richarz 1884, Boys 1895). Boys also pointed out that the problems of temperature effects would be reduced by making the apparatus as small as possible, which he proceeded to do, rather than going for size as Poynting had done. All in all, as Poynting himself concluded when he published his results (Poynting 1892, p 565), ‘. . . It might appear useless to add another to the list of determinations, especially when, as Mr Boys has recently shown, the torsion-balance may be used for the experiment with an accuracy quite unattainable by the common balance.’ He went on to justify his publication though, ‘I think that in the case of such a constant as that of gravitation, where the results have hardly as yet begun to close in on any definite value, and where, indeed, we are hardly assured of the constancy itself, it is important to have as many determinations as possible made by different methods and different instruments, until all the sources of discrepancy are traced and the results agree.’

This may be pure justification—he did not want to waste 13 years of effort—but it is the first indication we have of Poynting taking a more global, or fundamental, view of gravitational problems than the very parochial one of weighing the Earth. He states, ‘. . . we are hardly assured of the constancy [of G] itself . . . ’ This had clearly been a concern of Balfour Stewart’s, but Poynting had shown little explicit interest in it. Now, perhaps, he began to, for his Adams Prize Essay of 1894 on the Mean Density of the Earth includes speculations that the gravitative field of crystals might differ along their axes. His next experimental

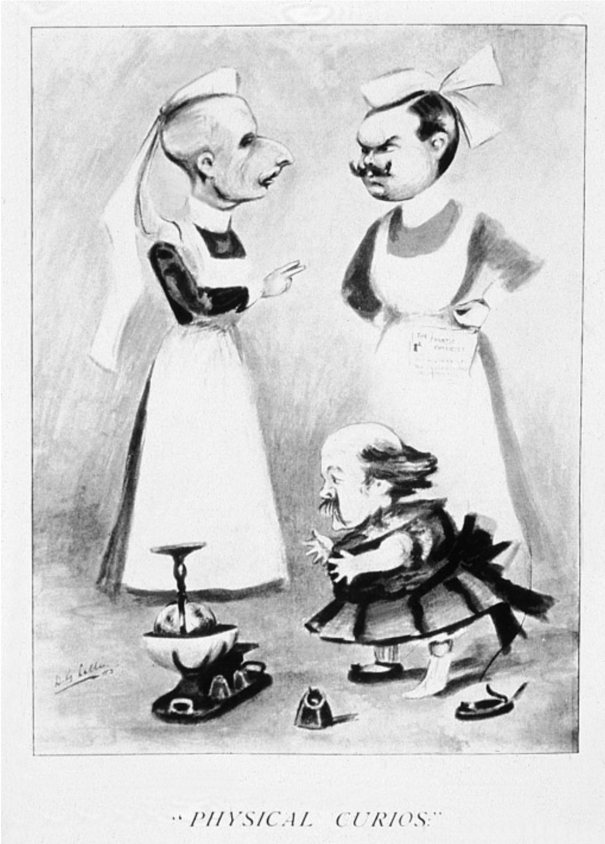

Figure 4. Professor Henry Poynting weighing the Earth. Cartoon by Denis Lillie, a student at Birmingham. The nurses are members of Poynting's staff, Dr G A Shakespear and Dr A de P Denning. From the student magazine, *The Mermaid*, frontispiece to volume 1 no 6, July 1905. Reproduced courtesy of the University of Birmingham.

work, published jointly with P L Gray in 1899, tested this speculation.

Poynting reasoned that if the attraction between two spheres was different according to whether their axes were parallel or crossed, then there would be a directive action on one sphere in the field of the other. He and Gray attempted to detect such directive action by rotating one sphere and attempting to force rotational oscillations of the other. They found no significant oscillations within the limits of accuracy of the experiment.

By this time Poynting was becoming involved in the work on radiation and the pressure of light which occupied much of the rest of his life, but he did perform one more gravitation experiment (Poynting and Phillips 1905) to determine if a change of temperature had any effect on weight. Again he used a common balance, a small 6″ beam one this time, balanced by two weights, and the whole apparatus was evacuated. He then heated one weight with steam, or cooled it with liquid air, to see whether the balance tilted. Again the results were negative.

What, then, did Poynting make of his experiments? Not a lot, it seems. To the scientific community at large they added evidence for the universal nature of gravitation. But this interpretation of gravitation experiments was still being negotiated, and Poynting rejected this view. He constantly and deliberately referred to all determinations of G as measurements of the mean density of the Earth, even remarking in a lecture in 1900, 'Professor Boys has almost indignantly disclaimed that he was engaged on any such purely local experiment as the determination of the mean density of the Earth. He was working for the Universe, seeking the value of G, information which would be as useful on Mars or Jupiter or out in the stellar system, as here on Earth. But perhaps we may this evening consent to be more parochial in our ideas and express [his] results in terms of the mean density of the Earth.' (Poynting 1900, p 282).

Why? By 1889 Poynting had developed a philosophical antipathy to giving fundamental status to 'laws of nature'. As Lodge (1920, p xii) wrote, 'Poynting was strongly inclined, almost unduly, to limit the province of science to description, and to regard a law of nature as nothing but a formulation of observed correspondences.' In these views of Poynting's we may discern an echo of the influential Unitarian, James Martineau's, rejection of the Scriptures as the ultimate religious authority and his appeal to man's individual reason. Poynting rejected the laws of nature as ultimate scientific authority, recognizing that they were nothing more than man's own attempt to apply reason to the phenomena of nature and that the laws were only as good as the reason devising them. In Poynting's words (Poynting 1903, p 729), 'These laws are not fixed—are not promulgated by Nature herself. They are our descriptions of the likenesses which we think to observe when we would watch her actions.' Furthermore, 'If this is a true account of the nature of physical laws, they have, we must confess, greatly fallen off in dignity. No long time ago they were quite commonly described as the Fixed Laws of Nature, and were supposed sufficient in themselves to govern the Universe. Now we can only assign to them the humble rank of mere descriptions . . . . Let us remember that there is no such thing as a failure to obey a physical law. A broken law is merely a false description.' (Poynting 1899, p 616).

Thus, with no firm evidence for the universal and constant value of G, Poynting emphatically refused to echo Boys' claims for the universe and his results remained firmly Earth-bound.

Indeed, I would go further: Poynting positively wanted to find some variation in G. He saw this as the only route to finding an explanation for gravitation, for, ' . . . we explain an event, not when we know 'why' it happened, but when we show 'how' it is like something else happening elsewhere or other when—when, in fact, we can include it as a case described by some law already set forth,'(Poynting 1899, p 6) and hence, 'This unlikeness, this independence of gravitation of any quality but mass, bars the way to any explanation of its nature.' (Poynting 1900, p 294).

Thus, in the end, Poynting achieved a union between his concerns with a heroic experimental tradition and the search for a fundamental unified theory of nature so typical of the Cambridge physicists. If he seemed to have achieved little, at least he had established something: ' . . . We at least know something in knowing what qualities gravitation does not possess, and when the time shall come for explanation, all these laborious, and at first sight, useless experiments will take their place in the foundation on which that explanation will be built.' (Poynting 1900, p 294). That explanation was not to be found by assuming the fundamental and universal nature of gravitation, but by rejecting Boys' claims and seeking small differences in purely local experiments.

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