Theoretical Predispositions in Experimental Physics: Einstein and the Gyromagnetic Experiments, 1915-1925

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Theoretical predispositions in experimental physics: Einstein and the gyromagnetic experiments, 1915-1925

IN THE MIDST of his work on the general theory of relativity, Einstein became deeply involved in a problem of experimental physics. Collaborating with W. J. de Haas, Einstein helped design and execute experiments to investigate Ampère's hypothesis that magnetism arises from a current circulating about atoms of magnetic substances. In several careful experiments Einstein and de Haas arrived at just the answer they expected, a result now considered to be almost half of what they should have found. Two questions come to mind: Why was Einstein so intrigued with this particular experiment? How did it come to pass that the two experimentalists found what they were looking for?

Einstein and de Haas' result was soon confirmed by an American physicist, S. J. Barnett. Others, including G. Arvidsson, J. Q. Stewart, and E. Beck, then scrutinized the experiment. It took several years before these physicists were able to persuade themselves that Einstein and de Haas' result was not correct despite its striking coincidence with Einstein's theoretical prediction.

1. AMPERE'S HYPOTHESIS AND ITS EARLY TESTS

Within a few weeks of the discovery by Oersted that an electric current can act on a magnetic needle, Ampère showed how to unify the theories of electricity and magnetism. He considered bulk magnetism as well as electromagnetism to be results of electricity in motion. In a bar magnet, for example, the motion occurs around the axis: "It hardly

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seems possible to me [Ampère wrote]...to doubt that there really are such currents about the axis of magnets, or, rather, that magnetization is nothing other than the operation by which particles of steel are endowed with the property to produce...the same electromotive actions as the voltaic battery."[1]

Thus Ampère broke cleanly with the widespread belief that a magnet owes its properties to separate north and south pole molecules that are connected in some unknown way. The poles had no special significance other than their position relative to the currents that composed the magnet.

Ampère later specified that the currents that circulate within magnets are molecular in origin, and he succeeded on that basis in developing a detailed quantitative treatment of magnetism. Although he conceded that all his testable predictions could be reproduced by a law (Biot-Savart) based on the two-pole idea, Ampère insisted that only his theory related the three interactions—current-current, current-magnet, and magnet-magnet—to one cause. He expected that the reduction would bring discoveries. "Those periods of history," Ampère wrote, "when phenomena previously thought to be due to totally diverse causes have been reduced to a single principle, were almost always accompanied by the discovery of many new facts, because a new approach in the conception of causes suggests a multitude of new experiments to try and explanations to verify."[2]

James Clerk Maxwell liked Ampère's hypothesis and strove to test it. But to design an experimental test, he needed to know what a current was, and here very little had been established to his satisfaction. Despite the many similarities between the electric current and a flow of a material fluid, Maxwell cautioned that "we must carefully avoid making any assumption not warranted by experimental evidence, and there is, as yet, no experimental evidence to show whether the electric current is really a current of a material substance, or a double current [positive and negative] or whether its velocity is great or small in feet per second."[3] Still, the possibility that current did involve a material transfer led Maxwell to develop three experiments to exhibit the inertial effects of currents, if such effects existed.

The first experiment Maxwell described in the *Treatise* perhaps

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[1] André Marie Ampère, "Mémoire présenté à l'Académie Royale des Sciences, le 2 Octobre 1820," *Annales de chimie et de physique, 15* (1820), 59-76 and 170-218, on 74-76.

[2] Ampère, "Mémoire sur la théorie mathématique des phénomènes électrodynamique uniquement déduite de l'expérience," Académie des Sciences, *Mémoires, 6* (1823), issued 1827, 175-388, on 303.

[3] James Clerk Maxwell, *Treatise on electricity and magnetism* (Oxford, 1881), 202-203.

dates from 1870, when he queried John William Strutt (Lord Rayleigh), "Have you tried whether the sudden starting or stopping of a current in a coil has any least effect in turning the coil in its own plane as it would be turned if the current were water in a tub?"[4] In figure 1, adapted from Maxwell's illustration, a coil is suspended as freely as possible. If an electrical current involved the transportation of inertial mass, then starting a current through the circuit should cause a change in the angular momentum of the wire. The wire should rotate opposite to the motion of the electricity to conserve angular momentum. It does not appear that Maxwell attempted to perform this test.

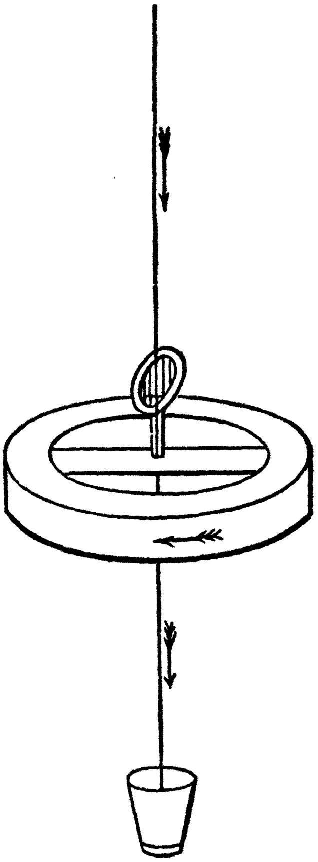

Fig. 1 Maxwell's first experiment, after Maxwell (ref. 3), 201.

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[4] Letter of 18 May 1870, quoted in Robert John Strutt, *Life of John William Strutt* (Madison, 1968), 46.

Maxwell built the apparatus for the second experiment mentioned in the *Treatise* in 1861, to measure the inertial effects of a *constant* current. A current is applied across the coil A in figure 2. The coil can rotate freely on two pins, B and B′. In addition, the entire armature D can tilt towards the horizontal plane as it is attached only at two vertical pins, E and F. The cord, visible on the pulley just above the bottom vertical pin F, is used to spin the armature at a fixed speed. A constant current is applied through the coil by way of two brushes located at the top vertical pin E.

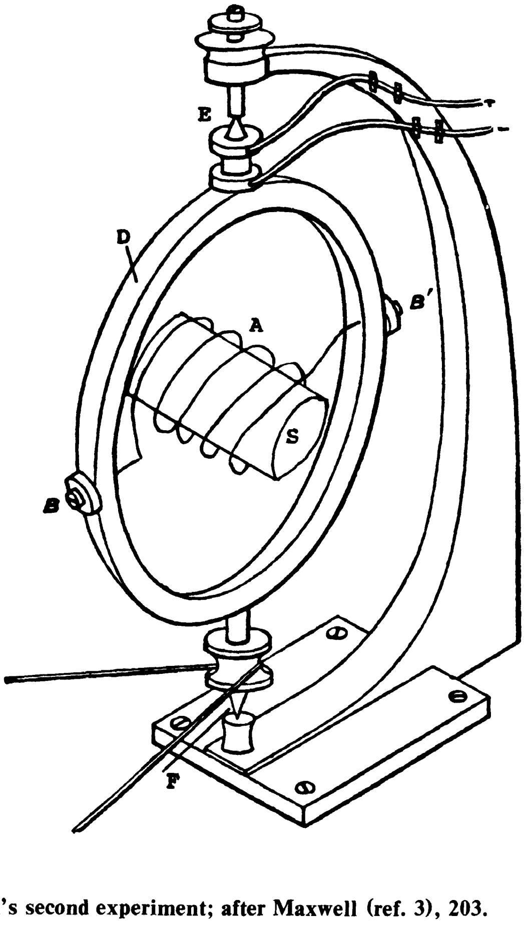

Fig. 2 Maxwell's second experiment; after Maxwell (ref. 3), 203.

If the current carries momentum, the coil will act like a gyroscope precessing about the vertical axis. Depending on the relative directions of the angular momentum of the gyroscope and the rotating armature, the gyroscope either would tip up or down. The effect is familiar to anyone who has rotated a gyroscope's angular momentum vector in a horizontal circle. When rotated one way, the gyroscope will tip upwards, and vice versa. Had Maxwell's experiment yielded a positive result, it would have shown the direction as well as the existence of a material current.

Maxwell tried to test Ampère's hypothesis by inserting an iron bar S into the coil A. He reasoned that the current through the wire would magnetize the iron bar, orienting the microscopic currents surrounding each magnetic molecule and amplifying the tilting he sought to measure. He found nothing and explained his failure as follows:[5]

> The chief difficulty in the experiments arose from the disturbing action of the earth's magnetic force which caused the electromagnet to act like a dip needle [a vertical compass]. The results obtained were on this account very rough, but no evidence of any change in θ [the angle the coil's axis made with the horizontal] could be obtained even when an iron core was inserted in the coil, so as to make it a powerful electromagnet.

Maxwell had little chance of observing the tilt he was looking for. Not knowing about electrons, he could not calculate that the mass transported with the current gave so little momentum that the gyroscopic effect would be miniscule. In 1915 de Haas and his wife, G. L. de Haas-Lorentz, showed that the angle of inclination to be expected in a device like Maxwell's has a tangent of about 0.00013.[6]

Maxwell's third experiment, like the other two, was designed to test whether the carriers of current also transported inertial mass. A short-circuited coil was given an angular acceleration in its own plane. If the unknown carrier of current had inertial mass, it should lag behind the accelerated coil. The resulting current relative to the coil should produce a magnetic field that might be measured. Perhaps Maxwell took the idea for this experiment, which he may not have tried, from an apparatus he built and used in 1863 to measure the resistance of a wire in absolute units.[7] In that work Maxwell rotated a short-circuited wire in the earth's magnetic field and detected the magnetic field resulting from the convection current. He estimated the accuracy of his

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[5] Maxwell, *Treatise* (ref. 3), 205.

[6] W. J. de Haas and G. L. de Haas-Lorentz, "Een proef van Maxwell en de moleculaire stroomen van Ampère," Akademie van Wetenschappen, Amsterdam, Afdeeling Natuurkunde, *Verslagen, 24:1* (1915), 398-404.

[7] Fleeming Jenkins, ed., *Reports of the Committee on Electrical Standards* (London, 1873), Appendix D to report of 26 Aug 1863.

measurement as one part in ten thousand. This may have encouraged him to comment in the *Treatise* that the null results he had obtained in his experiments on the inertia of current were probably significant:[8]

> Few scientific observations can be made with greater precision than that which determines the existence or non-existence of a current by means of a galvanometer....If, therefore any currents could be produced in this way [by accelerating a coil] they could be detected, even if they were very feeble....Since, however, no evidence has yet been obtained of such [currents], I shall now proceed as if they do not exist, or at least that they produce no sensible effect, an assumption which will considerably simplify our dynamical theory.

After Maxwell, Oliver Heaviside, J. J. Thomson, Joseph Larmor, and J. H. Poynting (among others) continued to pursue the connection between current and momentum. At least before 1897, however, they did not consider the momentum associated with currents to be the result of a transfer of ponderable charged matter. Instead, they ascribed energy and momentum to the electric and magnetic fields associated with the currents. Under this widespread assumption, experiments like Maxwell's on the inertia of currents would not have seemed to the point.[9]

H. A. Lorentz's electron theory broke with Maxwell's tradition by postulating charged, ponderable matter on one hand and the ether on the other. By dividing the two, Lorentz made his charged electrons subject to the same forces as uncharged matter as well as to the forces associated with electric and magnetic fields. One consequence of Lorentz's separation of charge and ether was that electrons in motion constitute a material current of the type Maxwell had thought might exist.

In 1907 O. W. Richardson of Princeton University decided to reexamine experimentally Ampère's hypothesis in light of Lorentz's views on the nature of electric currents. Richardson argued as follows: An electron in a circular orbit has angular moment $L = r(m\omega r) = 2ma$, where m is the mass of the electron, ω is the angular velocity, r is the radius of the orbit, and a is the area swept out per unit time. The accompanying magnetic moment is $M = ea$, e the electron charge. Therefore, the gyromagnetic ratio, λ—the ratio of angular momentum to magnetic moment—is independent of the angular velocity and the radius of the orbit:

$$\lambda \equiv \frac{L}{M} = \frac{2ma}{ea} = 2m/e.$$

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[8] Maxwell, *Treatise* (ref. 3), 206.

[9] Cf. Jed Z. Buchwald, *Matter, the medium and electrical current: A history of electricity and magnetism from 1842 to 1895* (PhD thesis, Harvard University, 1974).

Richardson's formula is easily extended to a general closed orbit. More generally, he calculated λ supposing that both electrons and positive particles were in orbit with different areal velocities.[10] The constancy of λ for all orbiting electrons suggested a simple experiment that Richardson tried: a suspended bar of iron, suddenly magnetized, should acquire a corresponding change in angular momentum equal to $(2m/e)M$. Like Maxwell, Richardson ascribed his failure to obtain a rotation to unspecified "disturbing effects"; but as late as 1914 Richardson believed that the experiment would yield positive results in spite of the null outcome of his first attempts.[11] A mechanical analogy to this effect may make it clearer (fig. 3). Suppose two identical gyroscopes are spun in

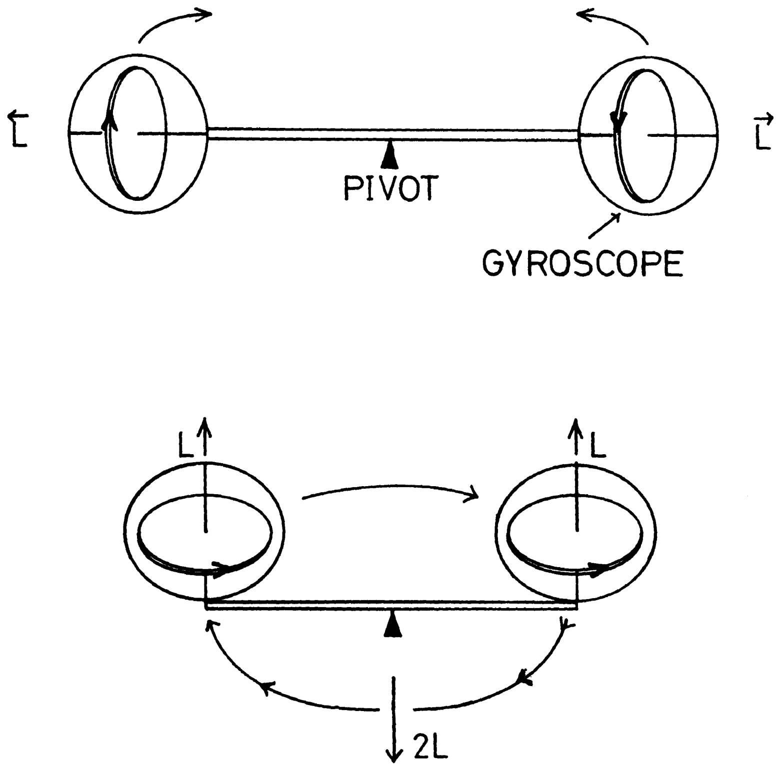

Fig. 3 Mechanical analogy to the Richardson or Einstein-de Haas effect.

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[10] O. W. Richardson, "A mechanical effect accompanying magnetization," *Physical Review, 26* (1908), 248.

[11] O. W. Richardson, *The electron theory of matter* (Cambridge, 1914), 397.

opposite directions with the same angular speed and placed facing away from one another at the ends of a bar. The total angular momentum of the system is therefore zero. Now suppose the bar is placed on a fulcrum around which it can pivot. If the gyroscopes were turned so they stood on end by an agency internal to the rotating arm, the total angular momentum would now be not zero but $2L$. To compensate for the change in angular momentum, the whole system would begin to rotate. Richardson hoped to orient the microscopic gyroscopes constituted by the orbiting electrons and so cause the macroscopic rotation of the magnetized sample. He found no such effect, however.[12]

2. THE EINSTEIN AND DE HAAS EXPERIMENTS

Einstein's interest in testing Ampère's hypothesis goes back at least to the period between 1905 and 1909, when he regularly met with two other young men, Dr. Hans Flükiger and Dr. Hans Rothenbühler, also interested in problems of experimental physics. Occasionally they did experiments in the physics room at the Städtiche Gymnasium in Bern. Perhaps, as one historian suggests, they tried to test Ampère's hypothesis experimentally.[13]

The immediate context of the Einstein-de Haas experiment was Einstein's study of gyrocompasses in order to render an expert opinion in a law case involving the firm of A. Anschütz-Kämpfe. He later wrote: "I was led to the demonstration of the nature of the paramagnetic atom through some technical reports I had prepared on a gyroscopic compass."[14] The consulting job, which was to last for over a decade, arose in 1914, when Anschütz sued the Ambrose-Sperry Company for patent violation. Einstein submitted his opinion on 7 August 1915. After the case was over (Anschütz won) Einstein was called on again to evaluate other cases involving Anschütz in 1918 and 1923. He became such a master of the subject that in 1926 he obtained his own patent relating to gyrocompasses for which he eventually received royalties.[15]

Einstein arrived in Berlin in April 1914 to take up his post as a member of the Akademie der Wissenschaften. He soon received the commission from Anschütz and, while considering the problem, inves-

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[12] Ibid., 252.

[13] Max Flückiger, *Albert Einstein in Bern* (Bern, 1974), 172.

[14] Einstein to E. Meyerson, 27 Jan 1930 (Einstein Archive, Princeton University Library).

[15] Einstein Archives (ref. 14), documents 37/5, 35/392, 35/400-414. Cf. A. Hermann, ed., *Albert Einstein—Arnold Sommerfeld Briefwechsel* (Basel, 1968), 51ff., and T. P. Hughes, *Elmer Sperry* (Baltimore, 1971), 149-150, 167-170.

tigated the related question of Ampère's hypothesis with the help of de Haas and the facilities of the Physikalisch-Technische Reichsanstalt in Berlin-Charlottenberg.[16] Einstein's personal friendship with Lorentz and his strong bonds to the Leyden physics community may have contributed to Einstein's choice of Wander Johannes de Haas (Lorentz's son-in-law) as a collaborator in the experimental project. De Haas had come to work at the Reichsanstalt in January 1914 as a scientific assistant. His work with Einstein began a long experimental career involving many projects related to their early collaboration. The first report of their work comes in a lecture by Einstein to the Deutsche Physikalische Gesellschaft on 19 February 1915.[17]

To obtain a qualitative confirmation of Ampère's hypothesis, Einstein and de Haas needed only show that by magnetizing a suspended iron rod they could set it in rotation. Unknown to them, the apparatus they were using was based on the same principle as Richardson's. Their chief improvement was to oscillate the magnetic field at the resonant frequency of the bar to amplify the effect. However, since they (like Richardson) also wanted to test whether electrons were responsible for the Ampèrean currents, a quantitative measure was needed as well. Here Einstein's theoretical analysis of the experiment gave them the tools to go beyond the simpler experiments of Richardson and Maxwell.[18]

In the first Einstein-de Haas experiment, a fiber G (fig. 4a) is attached on one end to a crossbar H and on the other end to a thin iron cylinder S. Two small mirrors M are mounted parallel to one another on opposite sides of the center of this iron bar (fig. 4b). Coils A and B surround the suspended iron cylinder, above and below the mirrors,

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[16] H.J. Treder, "A. Einstein: Einfache Methode zum Nachweis der Ampèreschen Molekularströme," *Wissenschaft und Fortschritt, 2* (1979), 53; Dieter Hoffmann, "Albert Einstein und die Physikalisch-Technische Reichsanstalt," *Wirkung von Albert Einstein und Max von Laue* (Berlin, 1980), 90-102 (Akademie der Wissenschaften, Berlin, Institut für Theorie, Geschichte und Organisation der Wissenschaftlichen, *Kolloquien*, 21).

[17] Einstein, "H. A. Lorentz, his creative genius and his personality," in *H. A. Lorentz, impressions of his life and work*, ed. G. L. de Haas-Lorentz (Amsterdam, 1957), 5-9; Martin J. Klein, *Paul Ehrenfest*, vol. 1, *The making of a theoretical physicist* (Amsterdam, North-Holland, 1970), 300; Hoffman, "Einstein" (ref. 14), 91.

[18] Einstein and de Haas, "Experimenteller Nachweis der Ampèreschen Molekularströme," Deutsche Physikalische Gesellschaft, *Berichte, 17* (1915), 152-170; "Experimental proof of the existence of Ampère's molecular currents," Akademie von Wetenschappen, Amsterdam, *Proceedings, 18* (1916), 696-711; "Proefondervindelijk bewijs voor het bestaan der moleculaire stroomen van Ampère," ibid., Afdeeling Naturk., *Verslagen, 23* (1914-15), 1449-1464. Cf. the excellent article by V. Ia. Frenkel, "Historiia effekta Einshteina-De Gaaza," *Upsekhi fizicheskikh nauk, 128* (July 1979), 545-557, and E. T. Whittaker, *A history of the theories of aether and electricity* (2 vols., New York, 1973), *2*, 243-245. Also cf. the very helpful work of H. Melcher, "Albert Einstein und die Experimentelle Physik," *Physik in der Schule, 17* (1979), 1-19, esp. 3-6.

leaving the mirrors exposed to reflect a beam of light from an outside source. The adjustable clamp P is used to vary the effective length of the fiber in order to adjust the natural frequency of the cylinder S when in free torsional oscillation.

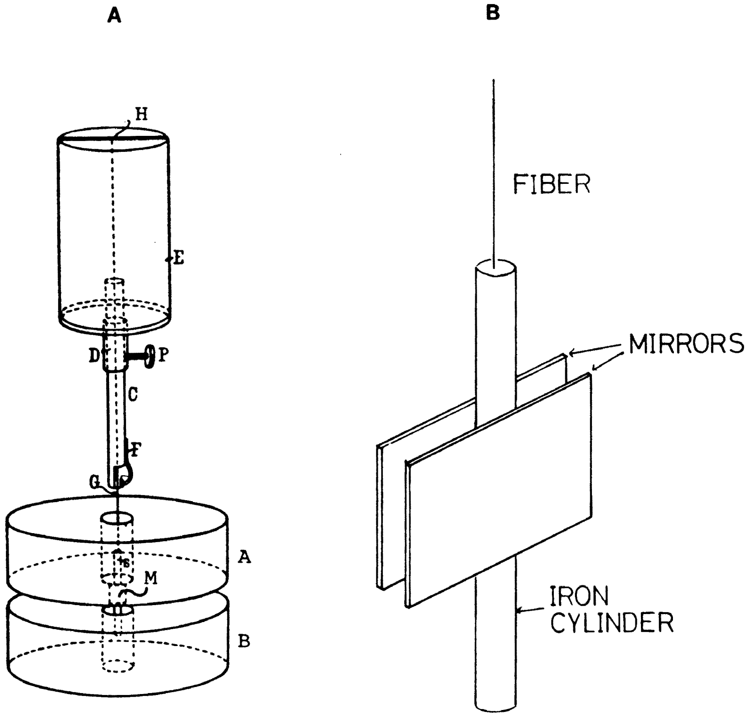

Fig. 4 (a) Schematic of Einstein-de Haas experiment, from Einstein and de Haas, "Experimenteller Nachweis" (ref. 18), 160. (b) Detail of iron sample.

When an oscillating magnetic field is applied by solenoids A and B, S oscillates and reflects a beam of light to a screen. The maximum deflection α of this reflected light beam can be measured even if the movement of the cylinder is very slight. Theoretically, α should be proportional to the torque caused by a change in the cylinder's magnetization M, and inversely proportional to the damping constant P: $\alpha = \frac{(\text{constant})\,\lambda\, M}{P}$. Since α could be measured, and M can be either calculated or measured, only P remained to be determined. In principle P could be directly found by observing the deflections of successive free swings; in practice the deflections were too small to make it possible. Instead, Einstein and de Haas measured α when the magnetic field oscillated at off-resonance frequencies, that is, they measured the Q of the system.

The angular displacement of the fiber, x, satisfies the equation that describes a damped harmonic oscillator:

$$\ddot{x} + (P/I)\,\dot{x} + \omega_o^2 x = (A/I)\cos\omega t.$$

(I is moment of inertia, P damping, ω_o the resonance angular frequency.) A particular solution is

$$\left.\begin{aligned} x &= B\cos(\omega t + \phi) \\ B &= (A/I)\left[(\omega^2-\omega_o^2)^2 + (P/I)^2\omega^2\right]^{-1/2} \end{aligned}\right\}.$$

Let $b = B/B_o$, where $B_o = (A/I)\,(I/P\omega_o)$ = maximum excursion at resonance, and $\nu = 2\,(\omega-\omega_o)$ the width of the resonance curve at the driving frequency ω. In our case, $\omega \approx \omega_o$,

$$\left.\begin{aligned} b &= P/I\left[\nu^2 + (P/I)^2\right]^{-1/2} \\ P &= I\nu\left[b^2/(1-b^2)\right]^{1/2} \end{aligned}\right\}.$$

This expression may be elucidated qualitatively: For a given resonance curve, the greater the moment of inertia, the greater the damping constant must be to achieve the same excursion length on the swings. Therefore P is proportional to I. Since without damping ($P = 0$) the curve would be an infinite spike at the resonant frequency ($\nu = 0$), one sees that P must also increase with ν.

The key experimental quantity, the resonance curve, was not so easy to determine. Using a resonance frequency meter of Hartmann and Braun, Einstein and de Haas could only measure frequencies at steps of a half cycle per second, as the device was equipped with standard coils and capacitors that resonated at certain fixed frequencies. To interpolate to the intermediate frequencies, they used an ammeter to measure the current provided in the generator. The ammeter therefore became their only measure of frequency between the frequencies given directly by the frequency meter.

As the frequency was changed, the double excursion lengths were measured by eye as the light beam oscillated back and forth across a scale some 145 cm from the mirror. Einstein and de Haas found the resonance curve shown in figure 5. From this graph, they took the data reproduced in figure 6, from which they deduced a value for P and for the gyromagnetic ratio. After eliminating excursions too small to measure accurately, they found that their experimental result (L/M = 1.11

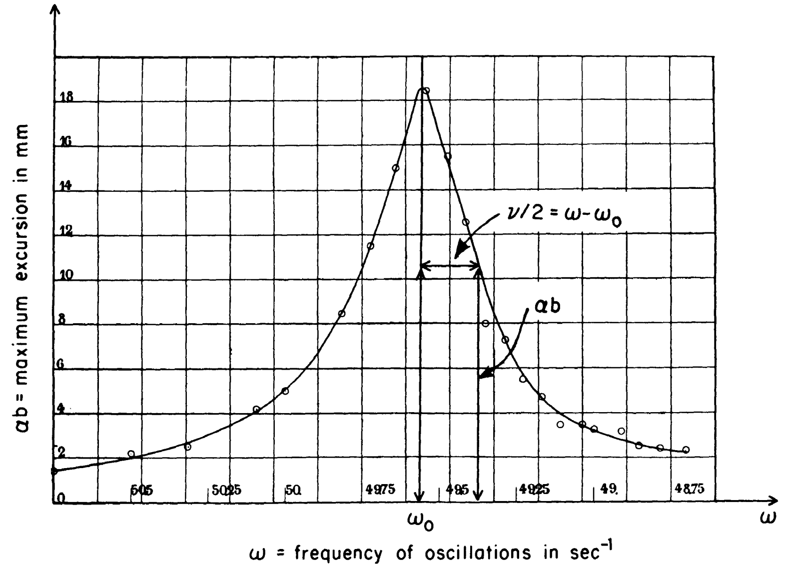

Fig. 5 Determination of damping constant by measurement of resonance curve, from Einstein and de Haas, "Experimental proof" (ref. 18), 708.

Ordinates	ν	b	$\sqrt{\frac{b^2}{1-b^2}}$	$\nu\sqrt{\frac{b^2}{1-b^2}}$
15	0,0911	0,812	1,32	0,120
12	0,152	0,649	0,853	0,130
9	0,221	0,488	0,560	0,124
7	0,293	0,380	0,413	0,121
5	0,403	0,271	0,280	0,114
4	0,489	0,217	0,222	0,108
3	0,618	0,163	0,165	0,0957

Fig. 6 Numerical data used to find the gyromagnetic ratio, from Einstein and de Haas, "Experimental proof" (ref. 18), 710. The column marked "ordinates" shows the light beam deflection in millimeters; ν and b are defined on p. 295, quantity in the right-hand column is inversely proportional to the g-factor.

within a 10% error) was in excellent agreement with their theoretically predicted value: $L/M = 2m/e = 1.13 \cdot 10^{-7}$ gm/emu.

Since $L/M = 2m/e$ was the original prediction for the gyromagnetic ratio for an orbiting electron, it has become standard to define a "*g*-factor" by

$$L/M = (2m/e)(1/g).$$

For an orbiting negative electron, g is 1. For a spinning classical sphere with mass distributed evenly throughout the volume and charge distributed only on the surface, $g = 5/3$. By a suitable disposition of charge and mass, one could create a spinning classical sphere with any *g*-factor desired.

Einstein's theoretical prediction corresponded to a *g*-factor of 1; his and de Haas' empirical result was equivalent to a *g*-factor of 1.02 with an error of 0.10. They accordingly concluded that they had verified Ampère's hypothesis:[19]

> The precision of the agreement may be accidental as our determination must be taken to have an uncertainty of about 10 percent; however, it has been demonstrated that the results of the theory of the orbiting electron sketched at the beginning have been quantitatively established (at least approximately) by the experiment.

(If Einstein and de Haas had kept the three data points they discarded, they would have obtained a *g*-factor about 5 percent higher than their published result.)

Among significant sources of error was the determination of the saturation magnetization. The hysterisis curves of the material were used to determine magnetization as a function of the solenoid's field, but the iron cylinder may or may not have been similar in composition to the standard. Again, the solenoid's field itself was not measured, but calculated from the constants of the coil.[20] Einstein and de Haas recognized several other types of systematic errors: (1) If the axis of rotation does not correspond to the axis of the magnetic field, the cylinder will acquire an alternating horizontal magnetic moment, which can couple with the earth's magnetic field to produce a large disturbance at just the frequency of the Einstein-de Haas effect. (2) The horizontal component of the earth's magnetic field can magnetize the iron bar directly; if there is an alternating horizontal magnetic field in the solenoid, another very strong disturbance will be introduced, also at the frequency of the Einstein-de Haas effect. Each of these disturbances might be

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[19] Einstein and de Haas, "Experimenteller Nachweis" (ref. 18), 170.

[20] Ibid., 169; Einstein and de Haas, "Experimental proof" (ref. 18), 711.

several orders of magnitude stronger than the Einstein-de Haas effect. The torque due to the Einstein-de Haas effect, $T_{\mathrm{Edh}} = \omega \Delta \mathrm{L}$, where $\omega = 50\ \mathrm{sec}^{-1}$ and $\Delta \mathrm{L}$ is the change in angular momentum during one reversal of magnetization: $T_{\mathrm{EdH}} = 2\omega\lambda \mathrm{M} \approx 5 \cdot 10^{-3}$ erg. If the oscillating iron cylinder is 1% off alignment with the solenoid, there will be a magnetization in the horizontal direction of approximately 10 ergs/gauss coupling to a transverse (uncompensated) earth field of about 1 gauss, giving a torque due to misalignment, T_{M}, of 10 erg. Conversely, the earth's transverse field could magnetize the iron cylinder, which has a magnetic susceptibility of approximately $2 \times 10^4 \mathrm{cm}^{-3}$; the resultant magnetization, 2×10^3 ergs/gauss, will couple to the horizontal component of the solenoid's alternating field. Supposing this component to be 1% of the solenoid's field, or 0.5 gauss, we obtain a torque $T_{\mathrm{E}} = 10^3$ ergs. Both disturbances might therefore swamp the Einstein-de Haas effect.[21]

Neutralization of the earth's magnetic field became the most crucial and delicate aspect of the early failure and eventual success of the Einstein-de Haas experiment. At first they used hoops with a radius of one meter with coils wound around them to eliminate the earth's field. The field strengths of the hoops were monitored by an ammeter measuring the current flowing through them. To examine the field in the immediate vicinity of the cylinder, they used a galvanometer and a device that measured the induction of the earth's magnetic field. As a final check on the compensation of the earth's field, they rotated the quartz fiber and then turned on the current oscillators. When they detected no further variation between the amplitude of oscillation from angular position to angular position, they considered the earth's field neutralized.

In later experiments the method proved too crude, and after de Haas returned to Holland on 1 April 1915, both he and Einstein separately began to work on the problem of further reducing the earth's residual horizontal field. De Haas set out to eliminate the first disturbing effect by wrapping the wire of the solenoid on the suspended cylinder. This assured the coincidence of the rotational and magnetic axes. Coupling could still occur, however, between the magnetized cylinder and the earth's transverse field. De Haas arranged a large permanent magnet to compensate the earth's field near the center of the bar, and two smaller ones to compensate it near the poles. Any

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[21] Einstein and de Haas examined two further possible sources of error: Eddy currents, shown not to exist by repeating the experiment using a conducting, but non-magnetizable material of the same dimensions as the iron cylinder; permantly magnetized crystals within the bar, whose components in the horizontal direction might not be reversed by the ambient field.

remaining field was neutralized by a second coil placed at right angles to the cylinder-coil assembly. The two coils were attached in series and a variable resistor was placed in parallel to the horizontal coil. De Haas could then adjust the distance of the coil from the cylinder-coil assembly and regulate the resistor to neutralize the earth's field.[22]

A final innovation of de Haas was to use a current pulse instead of a sinusoidal current by adapting a pendulum to complete a circuit each half cycle. When the pendulum swung in one direction, it completed a circuit sending a current pulse one way through the coil; when it swung back, the current pulse flowed in the opposite direction. After control experiments, de Haas was able to determine that the deviations due only to the Einstein-de Haas effect corresponded to $g = 1.2$. He concluded: "This time again I have not had in view an accurate quantitative determination; yet it may be mentioned that the quantitative agreement between experiment and theory is quite satisfactory. At the same time a way is opened for a later accurate determination of e/m"[23]

By writing that he considered the method a valid way of deriving e/m, de Haas implicitly accepted orbiting electrons as the agents of magnetization required by Ampère's hypothesis. Whereas in modern physics the coefficient of $2m/e$ is taken to be representative of many different phenomena such as nuclear spin and orbital motion, for de Haas and his contemporaries it was axiomatic that ferromagnetism and paramagnetism were due to orbiting electrons. For the moment, however, de Haas put stress on the method and presented his quantitative results modestly.

In private de Haas had already begun to suspect that the difference between $g = 1.2$ and $g = 1.0$ was significant. Apparently he wrote Einstein about it. Einstein replied:[24]

> I am very happy to hear about your work on the effect. [Einstein is referring to the work of de Haas just described.] I also have conducted experiments, in which I reversed the remanent magnetization through the discharge of a capacitor. The experiment won't work yet because

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[22] De Haas discussed a third disturbance: If there is some hysteresis, the horizontal magnetization may not be parallel to the horizontal field during part of the current cycle. If the cycle itself is nonsymmetric, the lag and lead domains may not compensate, and therefore provide a net torsional disturbance. De Haas, "Further experiments on the moment of momentum existing in a magnet," Royal Academy of Amsterdam, *Proceedings, 18* (1916), 1281-1299.

[23] Ibid., 1282.

[24] Einstein to de Haas, n.d. This letter is part of a collection of letters I located in Holland with the assistance of A. J. Kox. They had been among the papers left by G. L. de Haas-Lorentz. I would like to thank Hendrik Antoon Lorentz for making these letters available. A copy of the collection has been sent to the Einstein Project at the Institute for Advanced Study, Princeton.

> despite the short duration of the field (10^{-3} seconds), strong vibrations of the little cylinder set in, hiding the effect. This is naturally avoided with your method. I can believe that your 10% discrepancy with the theory is real. If this is so, however, then it would be very significant.

The experiments of his own that Einstein mentioned here were written up a short time later and received for publication in February 1916 as a "Lecture Experiment."[25] His idea was to reverse the remanent magnetism quickly. Consequently, the solenoid would be on for such a short time that it would not cause the cylinder to oscillate by its direct magnetic coupling to the solenoid. Like de Haas, Einstein used an alternating pulse rather than a sinusoidally varying current. After adjusting the quartz fiber such that the cylinder naturally oscillated at a second or two per cycle, he noted the deviation of the light marker. Each time the beam reached a maximum, he pressed a key pulsing the circuit. This would either markedly amplify or brake the swing, thereby demonstrating at least qualitatively the effect looked for. Again, Einstein made reference to the problem of compensating for the earth's magnetic field and aligning the cylinder properly, but he gave no specific details nor any quantitative results.

After constructing at least four different versions of the experiment, Einstein and de Haas were convinced that they had verified Ampère's hypothesis with orbiting electrons serving as the "current whirls." Qualitatively, all four experiments pointed to a gyromagnetic effect; after some confusion (corrected by Lorentz), the phase relations between current and oscillation clearly indicated the charge on the electrons to be negative. Finally, after two separate quantitative determinations, results were found that can be expressed as:

$$g = 1.02 \pm 0.10 \text{ (Einstein–de Haas 1915)}$$

$$g = 1.2 \text{ (de Haas 1916)}.$$

Even in his later work, de Haas took his measurements in principle to be a measure of $2m/e$, and not a constant by which this quantity was to be multiplied.

Einstein's theoretical preoccupations

Einstein and de Haas noted that if Maxwell's equations held for orbiting electrons, the energy of motion would soon be radiated away.

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[25] Einstein, "Ein einfaches Experiment zum Nachweis der Ampèreschen Molekularströme," Deutsche Physikalische Gesellschaft, *Verhandlungen, 18* (1916), 173-177. The reception is incorrectly given as 25 Feb 1915 (*recte* 1916).

This, they asserted, "is surely not the case," and added the following apparent non sequitur:[26]

> Furthermore it follows from the Curie-Langevin Law that the magnetic moment of the molecule is temperature-independent. Therefore, since the magnetic moment still exists at $T = 0$, there should remain an energy associated with the motion of the orbiting electrons at $T = 0$. Many physicists understandably resist the acceptance of this so-called "zero-point energy."

This abbreviated remark goes to the heart of Einstein's interest in his collaboration with de Haas. Pierre Curie had discovered experimentally in 1895 that the magnetic susceptibility of paramagnetic substances varies with the reciprocal of the temperature. In 1905, using the statistical techniques of Boltzman, Curie's colleague Paul Langevin derived the Curie Law by assuming that each atom carried a magnetic moment m owing to the circulation of electrons.[27] Langevin found the suceptibility to be $\chi = m^2N/(3kT)$, where N is the molar density, k Boltzmann's constant, and T the temperature. For Einstein, Langevin's success in predicting the Curie law gave credence to the assumption that there existed a temperature-independent atomic magnetic moment. Einstein conjectured that this atomic magnetic moment might arise from Ampèrean current loops composed of circulating electrons. Since the electronic motion would have to persist at a temperature when all molecular motion ceased, Einstein thought that it might provide a model for a class of zero-point energies. (Orbital electronic motion is not a source of zero-point energy.) Einstein's concern with zero-point energy dated back to his work in 1907 on the quantized harmonic oscillators of specific heat theory.

In 1911 the zero-point energy was taken up in another context when Planck used it in his "second theory."[28] In the new theory oscillators were allowed continuous absorption of energy but discontinuous emission. Only when an oscillator has acquired an energy equal to a multiple of $h\nu$ can it emit a light quantum. Using these assumptions, Planck claimed that the average energy of an oscillator includes the term $h\nu/2$ even at absolute zero. Planck, however, paid little attention to this energy as he thought that the frequency should be independent of temperature, and therefore, that the term would not contribute to

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[26] Einstein and de Haas, "Experimenteller Nachweis" (ref. 18), 153.

[27] Klein, *Paul Ehrenfest* (ref. 17), esp. 264ff; *Dictionary of scientific biography,* s.v. "Langevin"; Thomas S. Kuhn, *Black-body theory and the quantum discontinuity, 1894-1912* (Oxford, 1978), 210-220, 235-251; A. Pais, "Einstein and the quantum theory," *Reviews of modern physics, 51* (1979), 863-914, esp. 878-883.

[28] Cf. Kuhn (ref. 27), 236-254, 319-320, 340-352.

the specific heat. Einstein, on the other hand, sought experimental consequences of the additional energy.

In 1913, in collaboration with Otto Stern, Einstein pointed out that the rotational motion of a molecule should, by statistical mechanics, depend on temperature.[29] They therefore created a model for diatomic hydrogen for which they could compare predictions for specific heat with and without the assumption of the zero-point energy. Their collaborative work was an indirect continuation of Einstein's analysis of the specific heat associated with a system of quantized harmonic oscillators. After that paper appeared in 1907, Nernst proposed to extend the quantization to rotational as well as vibrational motion. Thus Einstein was continuing an old interest when he and Stern quantized the rotational energy of a molecule by setting the average rotational energy of a molecule, $E = J/2(2\pi\nu)^2$, equal to the Planck's expression for the average energy of an oscillator of frequency ν. Their expression,

$$E = J/2(2\pi\nu)^2 = h\nu/\left(\exp(-h\nu/kT) - 1\right),$$

referred to a collection of molecules all rotating with the same frequency in equilibrium with the radiation.

In order to determine whether or not the zero-point energy should be included, Einstein and Stern calculated the specific heat c from the preceding equation by eliminating ν and forming $c = dE/dT$. They then compared the resulting equation with and without an additional term $h\nu/2$ on the right hand side. The two equations yielded different expressions for specific heat as a function of temperature that could be compared to the experimental data of A. Eucken. Comparison showed that "Eucken's results on the specific heat of hydrogen make probable the existence of a zero-point energy of $h\nu/2$." Thus far Einstein and Stern's argument was based on Planck's radiation law and therefore on the quantum hypothesis. They then reversed their approach. By assuming a zero-point energy, Einstein and Stern contended that no further demands of discontinuity were needed to derive the Planck radiation law. Einstein doubted, however, that "other difficulties" (that he did not specify) could be conquered without the assumption of quanta.[30] Einstein and Stern thus provided a double argument for the existence of one kind of zero-point energy. The inadequacy of their first argument was soon revealed by Ehrenfest,[31] who made the more realistic hypothesis that the molecules have a statistical distribution of

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[29] Einstein and O. Stern, "Einige Argumente für die Annahame einer molekularen Agitation beim absoluten Nullpunkt," *Annalen der Physik, 40* (1913), 551-560.

[30] Ibid., 560.

[31] Klein, *Paul Ehrenfest* (ref. 17), 256ff.

rotational frequencies. Quantizing rotational energy then did not lead to a specific heat formula in good accord with experiment; Ehrenfest concluded that the attempt of Einstein and Stern to justify a zero-point energy had not succeeded.

When Einstein and de Haas began their experiments, they therefore knew that at least part of Einstein's earlier argument for the existence of a zero-point energy had collapsed and that a new one was needed. He hoped to find it in his work on the gyromagnetic ratio:[32]

> The experiment will soon be finished [he wrote to Michele Besso]. It will also have proved the existence of a zero-point energy. A wonderful experiment, too bad you can't see it. And how devious [*heimtückisch*] Nature is, if one wants to approach it experimentally! I've gotten a longing for experiment in my old age.

Einstein well knew that a host of difficulties attended any model of zero-point energy. For example, he pointed out that orbiting electons should by all rights suffer a radiative loss of energy. "No theoretician," he conceded, "can now utter the words "zero-point energy" without breaking into a half-embarrassed, half-ironic smile."[33]

Another consideration relating to the quantum may also have interested Einstein in the experiment. In 1913 Niels Bohr published his first paper on quantum theory in which he accounted for the Pickering lines in terms of orbiting electrons. Soon after the paper appeared, Einstein hailed Bohr's work as "one of the greatest discoveries."[34] Since orbiting electrons were precisely the object of the gyromagnetic experiment, Einstein may have hoped to provide an indirect confirmation of Bohr's theory.

Gerald Holton has stressed the importance that considerations of unity played in Einstein's thought in his formulation of the theory of relativity, and Klein has provided an example from Einstein's approach to the wave theory of radiation.[35] In 1909, for example, Einstein argued that, on one hand, a single electron suitably displaced could generate an expanding spherical electromagnetic wave.[36] On the other hand, a great

[32] Einstein to Besso, 12 Feb 1915, in Pierre Speziali, ed., *Albert Einstein—Michele Besso correspondence, 1903-1955* (Paris, 1972), 57-58. Cf. Einstein, "Experimenteller Nachweis der Ampèreschen Molekularströme," *Naturwissenschaften, 3* (1915), 237-238: "In the electron's orbiting motion we would have a kind of molecular motion that would remain as absolute zero is approached."

[33] Einstein, "Nachweis" (ref. 32), 237.

[34] G. Hevesy to E. Rutherford, 14 Oct 1913, in Klein, *Paul Ehrenfest* (ref. 17), 278.

[35] Holton, *Thematic origins of scientific thought* (Cambridge, 1973), esp. 362-367; Klein, "Einstein and the wave-particle duality," *The natural philosopher, 3* (1964), 5-49, on 7.

[36] Einstein, "Über die Entwicklung unserer Anschauungen über das Wesen und die Constitution der Strahlung," *Physikalische Zeitschrift, 10* (1909), 817-826.

many emitters would be needed to create a collapsing spherical wave for the inverse process, the absorption of radiation by a single electron. It was partially in an effort to restore the symmetry of absorption and emission that Einstein, in 1905, had introduced the light quantum into physics. Again, one explanation replaced the two different accounts of absorption and emission.

Einstein's search for unifying principles may also have engaged him with Ampère's hypothesis, which he regarded as an important synthetic work:[37]

> Since Oersted discovered that magnetic effects are produced not only by permanent magnets but also by electrical currents, there may have been two seemingly independent mechanisms for the generation of a magnetic field. This state of affairs itself brought the need to fuse together two essentially different field—producing causes into a single one—to search for a single cause for the production of the magnetic field. In this way, shortly after Oersted's discovery, Ampère was led to his famous hypothesis of molecular currents which established magnetic phenomena as arising from charged molecular currents.

Yet another great unifying principle could be tested by measurement of Lorentz' electron theory, "tied essentially to Ampère's hypothesis in the demand for a unified conception of the production of electromagnetic fields."[38] Einstein later amplified on Lorentz' contribution to the unification of physics. Before Lorentz, physicists treated the electric and magnetic as conditions governing matter, and the electric field and dielectric displacement as independent entities. In Lorentz' scheme these fundamental vectors of the electric and magnetic fields act on the electrons, which in turn alter the fields during their rearrangement. For Einstein, the experiment on the gyromagnetic ratio was a test of two fundamental unifying principles—Ampère's hypothesis and the Lorentz electron—as well as an examination of various quantum hypotheses.

3. BARNETT: FROM TERRESTRIAL MAGNETISM TO EINSTEIN'S ERROR

There was another path to the gyromagnetic experiments, almost entirely separate from the one that began with Ampère's hypothesis. This other path commenced with terrestrial magnetism, one of the oldest mysteries of physics. Though the attempt to link the earth's mag-

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[37] Einstein and de Haas, "Experimenteller Nachweis" (ref. 18), 152.

[38] Einstein, "Lorentz" (ref. 17), 6.

netic field to gyromagnetic effects would be all but completely abandoned a few years later, it served as the motivating factor for many theoretical and experimental investigations.

In a lecture on spinning tops given to the British Association for the Advancement of Science in 1890, John Perry speculated on the connection between rotation and magnetization. Perry likened the spinning molecules he took to compose matter to a "honeycombed mass with a gyrostat in each cell." Magnetized matter might be nothing else but the state of iron (for instance) in which all the microscopic gyrostats were oriented. This suggested an experiment. If one gave an unmagnetized piece of iron an angular acceleration the little spinning molecules that composed it should experience a torque tending to orient them. Rotation should therefore produce magnetism. Though unsuccessful in his attempts to induce magnetism in this way, Perry attributed his "failure to the comparatively slow speed of rotation which [he]...employed, and to the want of delicacy of... [his] magnetometer."[39]

In 1909 Samuel J. Barnett at Ohio State University proposed a similar connection between rotation and magnetization, taking as his guide the relation of the earth's magnetic field and its rotation. Barnett postulated that magnets are composed of oriented atomic or molecular systems with individual magnetic moments. If the atomic systems in iron, say, had negative electrons orbiting around positive centers, a magnetic field would result if the iron were given an angular velocity.

Barnett performed his first measurements on a steel rod accelerated quickly from zero to ninety rotations per second. Using a ballistic galvanometer, he measured the change of magnetic field produced by the spinning rod at 1/1500 gauss, with a sign associated with the presence of orbiting negative electrons. Unlike Richardson (and later Einstein and de Haas), Barnett was not especially interested in the consequences of his experiments for Ampère's hypothesis, Lorentz' electron theory, or zero-point energies. Instead, Barnett's conclusion addressed his main concern, terrestrial magnetism. "This effect, if substantiated by later work, will account for a minute part of the earth's magnetism, but apparently, only for a minute part."[40] Barnett did not make quantitative theoretical calculation of the expected magnetic field from a collection of orbiting electrons since his primary interest was in the earth's rotationally-generated field. It is therefore anachronistic to assign a *g*-

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[39] John Perry, *Spinning tops and gyroscopic motion* (New York, 1957), 65.

[40] Samuel J. Barnett, "Magnetization by angular acceleration," *Science, 30* (1909), 413.

factor to his result; for later discussion, however, the magnetic field he recorded in 1909 is equivalent to $g = 11$.

Arthur Schuster also examined possible connections between the earth's rotation and its magnetic properties.[41] "We know," he said, "that the earth behaves like a magnet with its axis inclined at an angle of about 12 degrees to the geographical axis of the earth. Is this near coincidence between the two axes merely accidental?" In support of his own suggestion that rotation *causes* magnetization, Schuster examined various candidates for the source of terrestrial magnetism. First, he rejected the idea of a magnetized core of the earth since iron should lose its magnetization "even with the most modest estimate of the internal temperature of the earth." Still, unknown effects of high pressure on the critical temperature of iron might save his hypothesis, and he left open the possibility of a magnetized iron core. Second, he rejected the view that terrestrial magnetism might be caused by a massive rotating current which would rapidly be dissipated. Finally, Schuster dismissed the idea that an external magnetic field might induce a magnetic moment on the earth since there is no evidence of such extraterrestrial magnetic fields. As final evidence for his hypothesis that rotation causes the magnetic moment, Schuster cited the secular variation of the magnetic north about the geographical pole. This, he added, could be explained if the electrons responsible for the earth's magnetic field were free to precess about the geographical pole.

By April of 1915 Barnett had read Schuster's paper, and more importantly, had realized the connection between his work and the attempts of Maxwell and Richardson to measure the gyromagnetic effects that would follow from Ampère's hypothesis. Adapting Maxwell's equation for the torque on a circular wire with a current through it, Barnett showed that he could expect a magnetic field to be produced at the pole of the iron cylinder equal to $H/n = -7.1\times10^{-7}$ gauss/rps.[42] The equations he used to derive this number, which amount to an application of Larmor's theorem, assume $g = 1.0$.

In his experiments Barnett replaced the ballistic galvanometer with a fluxmeter and improved the sensitivity of the measurement by adding a "compensating bar" identical to the rotating steel rod. A coil around the compensating bar is wound opposite to the coil surrounding the test rod. By keeping the compensating bar at rest, the circuit automatically compensates for any flux changes due to extraneous fields such as the

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[41] A. Schuster, "A critical examination of the possible causes of terrestrial magnetism," Physical Society of London, *Proceedings, 24* (1911-12), 121-137.

[42] S. J. Barnett, "Magnetization by rotation," *Physical review, 6* (1915), 171-172, 239-270, on 171, 270.

motor's (fig. 7). Barnett neutralized the earth's magnetic field with several large coils. The outcome after his exhaustive preparations was a value for H/n less than half of that expected for orbiting electrons, and corresponding to a g-factor of 2.3.

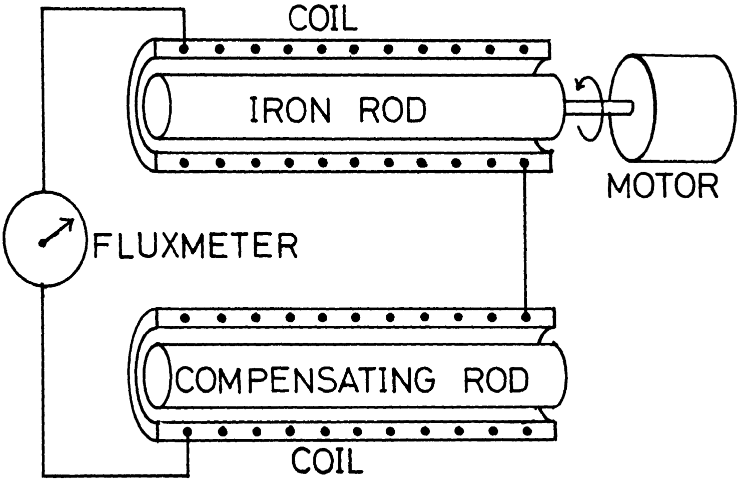

Fig. 7 Schematic illustration of Barnett's method of compensation, after Barnett (ref. 42).

Barnett's effect could account for less than one ten-billionth of the earth's magnetic field. Such a discrepancy would seem to dash all hopes of using rotation to explain the terrestrial magnetism. But, implicitly drawing on Schuster's paper, Barnett suggested that conditions inside the earth might explain the increased magnetization needed to account for terrestrial magnetism.[43] He mentioned two other effects (centrifugal displacement and thermionic displacement of electrons) that might also come into play in the creation of the earth's field but did not elaborate on them. When he again published on the subject two years later, in 1917, he had abandoned his interest in terrestrial magnetism and never again gave it prominence in his papers, even as the source of his original idea. No doubt the work of Einstein and de Haas effected the change.[44]

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[43] S. J. Barnett, "Magnetization" (ref. 40).

[44] Cf. the introductory sentences to S. J. Barnett, "The magnetization of iron, nickel, and cobalt by rotation and the nature of the magnetic molecule," *Physical review*, *10* (1917), 7-21.

Barnett's new experiments employed a magnetometer in place of the fluxmeter. In the old method the motor was turned on and off; the fluxmeter measured the resulting change of flux. With a magnetometer (essentially a suspended coil with a current running through it) the magnetic field can be measured directly since the deflection of the coil or magnet will be proportional to the strength of the field. The magnetometer was more sensitive than the old ballistic galvanometer, but it was also much more susceptible to outside disturbances. Barnett therefore took special care to compensate for the earth's magnetic field, shifts in the rotor's altitude, longitudinal motion of the rotor, temperature variation, and mechanical vibrations. Despite these careful precautions, Barnett's results were not much closer to the now accepted value near $g = 2$. In 1917 Barnett took the proximity of his new result to $g = 1$ as a confirmation of his measurements' validity.

After introducing an equation describing the expected result if the current is due entirely to an orbiting negative electron ($g = 1$), Barnett wrote: "If positive electricity also participates [g should be larger]. *The mean value of...* [g] *obtained in my 1914 experiments was...* [2.0]; and [g] was found to be independent of speed within the limits of the experimental error."[45] Now this is a rather extraordinary remark. Barnett's articles written in 1914 and published in 1915 only reported a result equivalent to $g = 2.3$. The only data Barnett could have been referring to are the raw data from his 1914 experiments: the "weighted mean differential deflection per unit speed...equals 0.057 per revolution per second." If we reduce this mean, we get $g = 2.0$. Barnett makes clear why he had not reduced these data: "After the completion of the work thus described it was decided to repeat the rotations in a region in which the earth's [magnetic] intensity was still more completely annulled....The desirability of this course was realized from the first and was also mentioned by Dr. Rosa at the Philadelphia meeting of the Physical Society, Dec., 1914."[46] From Barnett's perspective in 1915 the data corresponding to $g = 2.0$ were not as reliable as those corresponding to $g = 2.3$. The only result he quoted in 1915 relied on the latter data. Why, then, did Barnett reduce apparently unreliable data for publication in the introduction to his paper of 1917?

Two answers appear plausible. First, Barnett was concerned about receiving credit for his discovery, as is evident in almost all of his publications over the following thirty years. By pointing to data gathered in 1914, Barnett made it clear that his results predated the Einstein-de Haas publication of 1915. But this would not explain why he left out

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[45] Ibid. Emphasis added.

[46] S. J. Barnett, "Magnetization" (ref. 42), 255.

the result $g = 2.3$ from the introduction to his paper of 1917. The mystery, however, is solved in the conclusion to the paper, where Barnett reported that his new magnetometer results ranged from $g = 1.4$ to $g = 1.1$. "The differences are in the same direction as in the earlier experiments on iron, which gave... [$g = 2.3$ and 2.0 instead of $g = 1$]." It is evident that Barnett then *expected* the result $g = 1$:[47]

> But the experimental errors on account of the great difficulties involved, are such that importance cannot in my opinion be attached to the discrepancies. The investigation must be taken as confirming equation (1) [$g = 1.0$] both qualitatively and quantitatively on the assumption that only electrons are in orbital revolution in the molecules of all the substances investigated.

4. THE UNEXPECTED RISE OF THE g-FACTOR

After Richardson's unsuccessful attempt to measure the gyromagnetic ratio in 1908, there were several attempts at the Princeton laboratory to refine the experiment. Finally, in 1915, John Quincy Stewart and Maurice Pate began a series of investigations, perhaps encouraged by Barnett's completion of the converse experiment. One problem that had plagued the Princeton group was one that had led Einstein and de Haas separately to restructure their original technique: as soon as the suspended rod became magnetized, it interacted directly with the solenoid in such a way as to mask completely the searched-for effect.

De Haas had dealt with the problem by wrapping the solenoid directly around the suspended cylinder and using short pulses to reverse the magnetization. Einstein had attacked the difficulty by using extremely short pulses delivered at the resonant frequency to reverse the remanent magnetization of the cylinder. Stewart improved upon Einstein's idea.[48] Instead of reversing the remanent magnetization with a pulsed field, Stewart eliminated it altogether. The magnetic field needed to demagnetize a substance is much less than the coercive force needed to reverse the magnetization. By avoiding strong fields, Stewart greatly reduced the interfering effects associated with them.

Stewart also introduced three fundamental improvements in the elimination of systematic error. First, he designed a system of six square coils arranged on the faces of a cube centered on the suspended sample.

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[47] S. J. Barnett, "Magnetization" (ref. 44), 21.

[48] I. Q. Stewart, "On the moment of momentum accompanying magnetic moment in iron and nickel," *Physical review, 11* (1918), 100-120.

Each pair of facing coils was wired in series. In this way, the earth's magnetic field would be effectively eliminated: first in a rough way by the ratio of coil turns between the vertical and the horizontal coils, and then in a fine way by adjusting the currents through the coils. Second, Stewart made use of narrower and longer wire samples than had Einstein and de Haas. This minimized the amount of demagnetization that

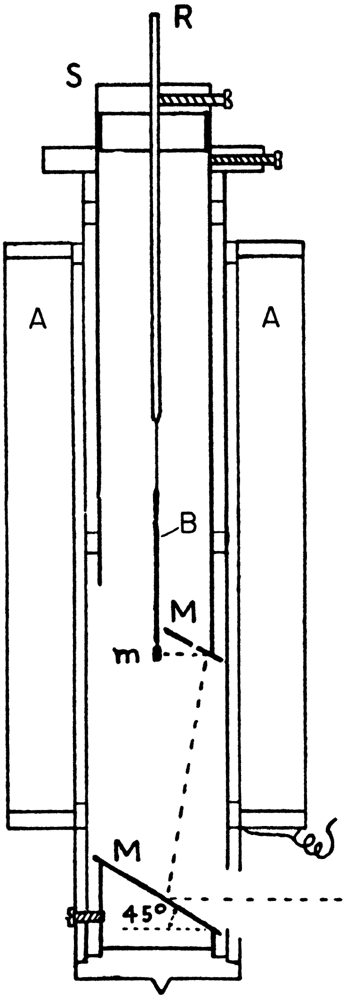

Fig. 8 Stewart's 1918 apparatus of 1918, from Stewart (ref. 48), 102. AA are compensating coils, R,S the suspension mechanism, B the sample, and MM the optical system for determining the angle of twist.

took place due to the action of the poles on the rest of the sample. Finally, Stewart cleverly employed two exploring coils to eliminate the transverse magnetization of the cylinder arising from a small permanent magnetization and induction from the earth's and the solenoid's fields.

By a suitable arrangement of the exploring coils, Stewart could find the magnetic moment of the sample and eliminate it by demagnetizing the sample in small increments until the moment began to be reversed. Then he could measure the free-swinging period of the sample, assured that no magnetic control was being exerted on it by the earth's field. When re-magnetized, the cylinder had a different period; Stewart adjusted the compensating coils so the free period was achieved again, and declared the earth's transverse field neutralized. Similarly, the solenoid's transverse field was eliminated by magnetizing the sample and adjusting the angle between the cylinder and the solenoid until the cylinder oscillated as it would in the absence of applied fields.

Once he had eliminated the disturbances, Stewart experimentally determined the smallest demagnetizing current which was still effective and performed the measurement (fig. 8). His result, averaged over sets of experiments on nine different wires (but excluding sets where the wires were above a certain thickness), was $g = 2.0 \pm 0.2$. One test of the accuracy of Stewart's experiment was that when demagnetization took place from a downwards magnetization, an opposite but equal displacement of the light beam was observed from that observed when the original magnetization was upwards.

Stewart accepted that g is approximately 2, and concluded that one of two possibilities must hold. Either (1) only negative electrons rotate, but they do not fully react on the bulk matter (slippage hypothesis); or (2) positive and negative charges rotate in opposite directions. Ultimately, Stewart dismissed the first possibility (slippage) as being unlikely in light of the coincidence between his results and those of Barnett. Consequently, he concluded positive charge must be orbiting as well. From the measured gyromagnetic ratio and the mass of a positive electron, which he took to be the nucleus of a hydrogen atom, Stewart concluded that "the angular velocity of the rotating positive nucleus is about equal (but opposite in sign) to that of the inner ring of electrons."[49]

Meanwhile in Zurich, another experimentalist, Emil Beck, set out to repeat Einstein's experiment with more precision. Unlike Stewart, Beck continued to use Einstein and de Haas' resonance method, revers-

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[49] Ibid., 120. Stewart must mean "angular momentum," since if one equates the angular velocities, the g-factor cannot be 2. I thank Prof. E. M. Purcell for this observation.

ing the magnetization of the iron cylinder with an oscillating magnetic field. Beck was sufficiently confident both in Einstein's orbiting electron theory and in his own measurements to have written, "In the opinion of the writer this method lends itself very well to an exact determination of the important quantity, e/m." Evidently he entertained no possibility other than $g = 1$.[50]

Three improvements over Einstein and de Haas' experimental method gave Beck this confidence. The first and most important was the elimination of the awkward frequency measuring system employed by Einstein and de Haas. Instead of making a few measurements with a resonance meter and interpolating between the points by varying the current driving the generator, Beck developed a device that gave very accurate measurements of small frequency differences. To do this, he exploited one of the disturbing effects in Einstein's experiment: direct coupling takes place between the magnetized cylinder and the horizontal component of the alternating magnetic field, causing torsional oscillations. In Beck's device a coil is wound parallel to a suspended permanent magnet and in series with the main solenoid. The permanent magnet is suspended by a fiber with the same torsion constant as the one holding the cylinder. The test coil creates a strong oscillating horizontal field that forces the suspended magnetized cylinder to oscillate on its fiber. For any given frequency, Beck could find the length of the fiber corresponding to resonance (maximum excursion length). In this way, very small frequency differences could be measured with great accuracy. An accurate measure of frequency differences led to a more accurate determination of the damping constant, and therefore of the gyromagnetic ratio.

Beck's second innovation was to use a photographic plate to record the deflections of the light reflected from the little mirror mounted on the cylinder. This gave him an additional and direct measurement of the damping constant, which he determined by setting the rod in free oscillation and reading from the developed film the decaying amplitude of the excursions.

Third, Beck had a much improved determination of the solenoid's field, and of the moment of inertia and magnetization intensity of the cylinder, which entered in the calculation of g. To obtain the field inside the solenoid, he used a tiny mirror galvanometer suspended by a wire. (Einstein and de Haas had only calculated this quantity.) Beck also obtained a much better correspondence between his calculated and measured moment of inertia for the cylinder. Finally, he measured the

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[50] Emil Beck, "Zum experimentellen Nachweis der Ampèreschen Molekularströme," *Annalen der Physik, 18* (1919), 109-148, on 113.

saturation magnetization of the cylinder by wrapping around it a coil attached to a calibrated galvanometer. When the magnetic field was suddenly turned on, the rod became magnetized, causing a change in the magnetic field. Since the ambient magnetic field from the coils had already been determined, the meter deflection could be used to determine the magnetization of the rod. When both resonance and photographic measures for the gyromagnetic ratio were calculated and averaged, Beck obtained a result corresponding to $g = 1.8$, a number outside the limits of Einstein and de Haas' error bar. The discrepancy led Beck to check "all causes of error" and to review with special care the alterations he had made from Einstein and de Haas' original procedure.[51]

Before Beck published his work, Einstein came to visit him in Switzerland. Impressed by Beck's work, Einstein reported to de Haas: "In Zurich a really good experimentalist (Herr Beck) has repeated our measurements on the torque exerted on a ferromagnet and only found *half* of the theoretically expected effect."[52] Beck hesitated to announce that he had made a new determination of m/e. Instead, he concluded that either (1) there is a new type of electron, or (2) the nucleus or positive particles circulate in the opposite direction from that of the electrons, or (3) the situation was somehow more complicated than previously suspected.

Beck's and Stewart's results were soon found independently by G. Arvidsson, working at the University of Uppsala.[53] Like Beck, Arvidsson referred to his measurements as a determination of m/e, and he too used the method of resonance by reversing the magnetization of the iron cylinder. But as Arvidsson had not yet seen the results of Beck or Stewart, to him the discrepancy between his result and Einstein's was worrisome. After presenting his data, which averaged to $g = 2.12$, Arvidsson concluded, "In my opinion, one must acquire a more exact knowledge of phenomena involving statistical magnetization in an oscillating field before we can say anything precise about the results."

The measurements of Stewart, Beck, and Arvidsson, all pointing towards $g = 2$, cast the simple model of orbiting electrons into doubt. By the time of the Solvay conference of 1921, the issue was of considerable concern to many of those interested in the physics of the elec-

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[51] Ibid., 144.

[52] Einstein to de Haas, 9 Sep 1919 (ref. 24).

[53] G. Arvidsson, "Eine Untersuchung über die Ampèreschen Molekularströme nach der Methode von A. Einstein und W. J. de Haas," *Physikalische Zeitschrift, 21* (1920), 88-91.

tron. De Haas reported on his experiments at the meeting, and there followed a discussion that included Lorentz, Richardson, and Larmor.

Like Barnett, de Haas succumbed to the temptation of resurrecting earlier, unreliable data and presenting them later along with his final results. Speaking of his experiments with Einstein, de Haas wrote, "The numbers we found for $2m/e$ in our experiments were... [$g = 1.4$] and... [$g = 1.0$]. The second value was almost the classical value... [$g = 1.00$], which led us to believe that experimental errors had made the first too... [large]."[54] This first result, $g = 1.4$, which de Haas reported, came from a set of experiments explicitly rejected by Einstein and de Haas in their paper of 1915. After presenting the calculated and observed double deflections of the light marker, they had not calculated the experimental 2m/e, and for good reason. As they said, to satisfy the conditions specified in the theoretical calculation, it was necessary to have an almost instantaneous reversal of magnetization. For their first experiment this was not the case. Indeed, it was principally this factor that led Einstein and de Haas to repeat the experiment. De Haas did not represent his earlier experiments faithfully by placing the two pieces of data $g = 1.0$ and $g = 1.4$ on equal footing. This democratic action perhaps reflected his growing conviction that Stewart, Beck, and Arvidsson might be right.

After the conference, de Haas published two new sets of data, whose averages were $g = 1.55$ (March 1921) and $g = 1.11$ (July 1921). He explained:[55]

> The other authors cited in this report found double the classical value of e/m. As for me, I am tempted to consider the exact value of the effect *per se* as still an open question. Be that as it may all the observers found a value of e/m which was too large. A part of this torque is therefore disappearing and escaping our observations. The idea was presented that a positive nucleus turning at a high speed could absorb a part of the torque. But this hypothesis seems to me far-fetched and unlikely; I think instead that if the bases of the theory are unimpeachable, other hidden movements must be considered.

Discussing the problem at the Reichsanstalt in Berlin, Einstein repeated de Haas' dissatisfaction with the increasingly well-accepted value of $g = 2$ and urged the great school of measurers to settle the matter: "Can't we investigate exactly the magnetic rotation effect here in the

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[54] De Haas, "Le moment de la quantité de mouvement dans un corps magnétique," *Atomes et électrons* (Paris, 1923), 206-226, on 214 (Conseil de Physique Solvay, *Rapports et discussions, 3*).

[55] Ibid., 226.

Reichsanstalt? There still remains no certainty over the numerical factor [g]."[56]

In October of 1920, after the results of Stewart, Beck, and Arvidsson had been published, Barnett completed a brief report indicating that his earlier work might be defective. Eddy currents, he remarked, had been detected in copper samples when used in place of iron in the magnetometer experiments. "This probably accounts for at least a part of the discrepancy between the results obtained by the two methods [galvanometer in 1915 and magnetometer in 1917]."[57] He could now assert:

> All the rods gave values about [$g = 2$] instead of [$g = 1$], or even [more],... thus again indicating an effect of positive electricity or else indicating that negative electricity alone is involved, but has for the motions responsible for magnetism, a smaller value of m/e than that determined in known experiments.

Barnett repeated these assertions later that year.[58]

By 1922 Barnett had prepared an article on his new research for the *Bulletin* of the National Research Council. There, he stressed his results of 1915 ($g = 2.3$ and 2.0). In another bit of revisionist history, his results of 1917 disappeared with the words, "In 1917 we completed an investigation of steel, cobalt, and nickel by a magnetometer method, and obtained values of [g] which were, as before, all negative, and whose means were intermediate between the values previously obtained for steel and twice those values."[59] This translates to: g was between 1 and 2.

To explain the new (or rather old) result that $g = 2$, Barnett left the Einstein orbiting electron theory to invoke the theories of W. Voigt and M. Abraham. As Abraham had shown, if one takes the charge of an electron to be spread evenly over the surface of a sphere and calculates the mass purely electrodynamically, the ratio $L/M = m/e$ for rotational motions about a diameter corresponds to $g = 2$. A spinning electron with charge distributed through its volume has $g = 5/14$. From these suggestive numbers, Barnett concluded that either (1) positive electrons or "magnetons" are in rotation, or (2) one of the two rotating electrons suggested by Abraham is responsible for the effect, or

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[56] Transcription of meeting of March 1922, reproduced in Christa Kirsten and Hans-Jurgen Treder, eds., *Albert Einstein in Berlin, 1913-1933* (2 vols., Berlin, 1979), *2*, 161.

[57] S. J. Barnett, "Further experiments on magnetization by rotation," Washington Academy of Science, *Journal, 11* (1921), 162-163, on 163.

[58] S. J. Barnett, "Additional experiments on the nature of the magnetic molecule," *Physical review, 17* (1921), 404-405.

[59] S. J. Barnett, "The angular momentum of the elementary magnet," National Research Council, *Bulletin, 3:3* (1922), 235-268, on 242.

(3) a new kind of "magneton," different from the orbiting electron, is the culprit. Although Barnett had no results of his own to report, he was thoroughly convinced that his original results were correct (in agreement with Stewart, Arvidsson, and Beck) and that those of 1917 were spurious.

In 1922 Barnett fell into a feud with Louis Bauer, the head of the Carnegie Institution's Department of Terrestrial Magnetism. Among other issues, Barnett's single-minded commitment to small improvements in his old experiment infuriated Bauer.[60] At the same time, Barnett's laboratory assistant complained that Barnett would not let him undertake any but the most mechanical and routine tasks or "have any part in observations or reductions concerned with the experiments under way."[61] The instrument makers began to despair over the possibility of improving the apparatus in the way Barnett sought. By 1922 over one seventh of the instrument makers' time was devoted to Barnett's experiment alone. Finally, J. A. Fleming (the assistant director of the laboratory) wrote to Bauer recommending that no further work should be committed to Barnett's experiment by the instrument makers:[62]

> In my judgment the mechanical difficulties which Dr. Barnett is trying to overcome in the existing apparatus arise from fundamental mechanical defects...if...they could be temporarily improved...the adjustment probably would not be permanent and might not hold long enough even for any extended, reliable series of observations.

Partly as a result of these pressures, Barnett left the laboratory and continued his work at Cal Tech with his old equipment. It was thus in California in 1925 that Barnett and his wife, L. J. H. Barnett, finished a massive study of the Barnett effect with an exhaustive discussion of errors. A few of the section headings are:

39. Eddy current effects of the lower magnetometer magnet
40. Effect of air currents on bedplate
43. Elimination of thermal effects of magnetometer
47. Error due to thermal effects of journal friction on magnetization
51. Errors from axial displacement of the rotor
53. Errors from the Thomson repulsion effect
54. Errors from mechanical disturbances
55. Errors due to inequality of right-handed and left-handed speeds

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[60] L. A. Bauer to J. C. Merriam (President of the Carnegie Institution), 28 Nov 1922, Carnegie Institution, Washington D. C., Archives, s.v. "S. J. Barnett."

[61] J. A. Fleming to Bauer, 28 Nov 1922 (ref. 60).

[62] Fleming to Bauer, 27 Nov 1922 (ref. 60).

The Barnetts did not want to be fooled again. After 159 sets of observations, they presented their result $g = 1.89$, which they thought accurate to 2%.[63]

With this much more precise data in hand, Barnett abandoned his ideas on the Abraham electron and turned to a quite different field of physics then under intense debate.[64]

> Our phenomenon is undoubtedly connected closely with the Zeeman effect, as our magnetons may be considered to be executing regular precession upon them brought about by the rotation.... As Landé has suggested, the anomaly in the Zeeman effect, which Sommerfeld and Debye had partially explained by the ideas of spatial quantization (now supported in the field of magnetism by the work of Pauli, Sommerfeld, Epstein, Gerlach, and Gerlach and Stern), is probably related to the anomaly in our phenomenon. This anomaly Landé and Sommerfeld have attempted to explain by a process which appears to be equivalent to identifying our magnetons with the atoms in the s-state and attributing to this a value of [g] equal to m/e [$g = 2$] which is approximately the value of [g] given by our experiments.

Once again Barnett had changed the theoretical analysis of his experiment. This time he associated his effect with the spectroscopic phenomena that would shortly be explained by Goudsmit and Uhlenbeck as deriving from electron spin.

If theoretical interpretations still remained vague in 1925, at least the quantitative determination of g was becoming increasingly accurate. Two English physicists, A. P. Chattock and L. F. Bates, used a modification of Stewart's experiment to obtain $g = 1.97$, which was improved by W. Sucksmith and L. F. Bates to $g = 1.99 \pm 0.024$.[65] Barnett too investigated the Einstein-de Haas effect and in 1931 obtained $g = 1.929 \pm 0.006$.[66] Many other variations of the gyromagnetic experiments have since been performed, especially on paramagnetic substances, but perhaps the most exact have been those of G. G. Scott, working at the Research Laboratories of the General Motors Corpora-

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[63] S. J. Barnett and L. J. H. Barnett, "New researches on the magnetization of ferromagnetic substances by rotation and the nature of the elementary magnet," American Academy of Arts and Sciences, *Proceedings, 60* (1925), 126-216, on 215.

[64] Ibid., 128.

[65] A. P. Chattock and L. F. Bates, "On the Richardson gyromagnetic effect," Royal Society of London, *Philosophical transactions,* 223A (1922), 257-288; W. Sucksmith and L. F. Bates, "On a null method of measuring the gyromagnetic ratio," ibid., *Proceedings, 104* (1923), 499-511.

[66] "Gyromagnetic effects: History, theory and experiments," *Physica, 13* (1933), 256-268, on 266. "One of the most important parts of [Barnett's] investigation is...that the magnetic element consists primarily of a Lorentz electron *spinning on a diameter,* and not an electron moving in an orbit." Ibid., 254.

TABLE 1

Summary of gyromagnetic results

EXPERIMENTER	PLACE	PUBLICATION DATE	RESULTS (G-FACTOR)
Barnett	Physical Lab., Ohio State University	1915	1.9 (1914) 2.3 (1915)
Einstein-de Haas	Physikalisch-Technische Hochschule (Berlin)	1915	1.02 ± 0.10 (1.45—dismissed)
de Haas	Teyler Institute	1916	1.2
Barnett	Ohio State University	1917	1.39 to 1.09 (within error of 1.0)
Stewart	Palmer Lab., Princeton	1918	1.96 ± 0.15
Beck	Eidgenossische-Technische Hochschule (Zurich)	1919	1.88
Arvidsson	Physical Institute (Uppsala)	1920	2.12
Barnett	Carnegie Institute (Washington)	1922	"approximately 2"
de Haas	Teyler Institute	1923	1.54 (March 1921) 1.08 (July 1921)
Chattock & Bates	University of Bristol	1922	1.97
Sucksmith & Bates	University of Bristol	1923	1.99 ± 0.024
Barnett	Cal Tech	1925	1.89 ± 0.04
Barnett	UCLA	1931	1.929 ± 0.006
Scott	General Motors	1962	1.919 ± 0.002

tion. Scott reported his best determination of *g* to be 1.919 ± 0.002.[67] By then the spin-orbit and orbit terms were known to be very dependent on the properties of specific substances, and therefore the *g*-factor in itself revealed little of fundamental importance to physics. The gyromagnetic experiments had long since passed from the forefront of research.

5. ON THEORETICAL PREDISPOSITIONS

Our episode in the history of experimental physics (summarized in table 1 and figure 9) is an extraordinary tale. First, Maxwell and

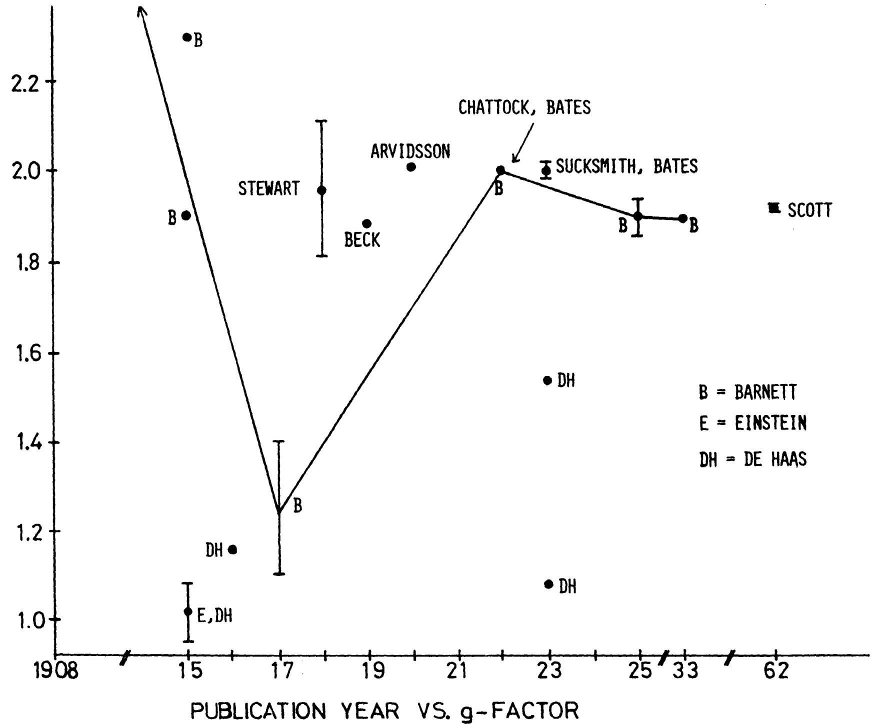

Fig. 9 Publication year vs. *g*-factor. The solid line traces Barnett's results as a function of time.

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[67] "Review of gyromagnetic ratio experiments," *Reviews of modern physics, 34* (1962), 102-109. Cf. S. P. Heims and E. T. Jaynes, "Theory of gyromagnetic effects and some related phenomena," ibid., 143-165.

Richardson found nothing at all in attempting the gyromagnetic experiments. Then, unaware of their research, Barnett began work on the converse effect: when rotating an iron rod, he detected a magnetic field that was more than five times the strength current physics predicts he should have found. After reading Richardson's paper and revising his own experiment, Barnett in 1915 arrived at a value approaching $g = 2.3$ and was quite satisfied with the explanation that positive ions were orbiting in the atom in a direction opposite to the negative electrons. His main conclusion, however, was that this effect, combined with unknown conditions at the center of the earth, might make the earth's rotation the cause of terrestrial magnetism.

Almost simultaneously, Einstein, who unlike Barnett had very strong reason to believe that $g = 1$, performed his experiments with de Haas. They offered a chance to confirm Lorentz' electrodynamic theory, Langevin's explanation of the Curie law, Planck's hypothesis of zero-point energy, and Ampère's hypothesis of molecular current. After at least four different experimental apparatus had been constructed, Einstein and de Haas seemed to have conclusively verified the theory that orbiting electrons are responsible for permanent magnetism. They determined that $g = 1.02 \pm .10$; and, in a second quantitative series of experiments the following year, de Haas found $g = 1.2$. Then Barnett, obviously influenced by Einstein's theory and experiment, repeated his own work and concluded that he too had vindicated the orbiting electron theory: g was somewhere between 1.4 and 1.1.

Three experimentalists, working independently, soon determined that g was *not* equal to one. Stewart, Beck, and Arvidsson each published a quantitative result nearer to twice the Einstein value. Within months Barnett published again, asserting that he too believed that g was approximately 2. In the two years that followed, he improved his result, abandoned Einstein's theory, and adopted one of Abraham's electron theories to explain his result of $g = 1.89$.

Meanwhile, de Haas (1921) repeated his work, now aware that at least four other researchers were finding a g-value near to twice his original one. At the Solvay meeting, he reported a g-value of 1.54, and said that he still considered the value of g to be an open question. After the meeting he repeated his experiments for the last time: his cumulative result of $g = 1.08$ was only a few percent different from his original result with Einstein six years before. The next year, in Berlin, Einstein too maintained that the value of g was still open to question. During this time Barnett refined his method further and in 1925 published a massive paper with an average result of $g = 1.929 \pm .006$. By 1933 the Dirac theory was well known; Barnett was then able to attribute his result to a complex interaction of spin and orbit effects.

Among the explanations for the way theoretical predispositions influence experimental results is one given by Thomas Kuhn in 1961.[68] He argued that the measurements necessary to test new theories often bear on phenomena at the limit of our experimental capabilities. As a result, relative to the size of the effects searched for, random error is very great. This leaves open the possibility for experimentalists and theorists to interpret the necessarily ambiguous results as confirming their theory. Had more precise techniques of measurement been possible, these same results could just as easily have confirmed an opposing theory. For example, Kuhn cites the case of Laplace's prediction of the speed of sound in air. Laplace arrived at a theoretical prediction in excellent agreement (a discrepancy of only 2.5%) with the experimental results of Delaroche and Berard. Their result, though, now seems to differ by over 40% from modern measurements and theory. Kuhn concludes that any measurement like that of Delaroche and Berard must also fit other theories, "and it is only within the experimental spread covered by the phrase theoretical predisposition of the measurer."

The collection of relevant data has a sufficient spread or "scatter" that competing theoretical explanations may both be compatible with the experimental results. However, Kuhn's explanation only applies if *in retrospect* we can see that all and only "relevant" data have been used. Sometimes irrelevant data can be excluded at the time of the experiment. Thus, for instance, in Stewart's experiments, he excluded the thick wires from his average value for g because of demagnetizing effects he thought were systematically distorting the results. Similarly, Barnett excluded his experiments of 1914 because he realized upon their completion that the earth's field had not been adequately neutralized. Unfortunately, at the time of the experiment it is not always possible to identify which data are "relevant" and which must be discarded.

I would like to suggest a different interpretation of the way theory influences the outcome of experiment, one that depends neither on Gestalt-like mis-seeing nor on the large spread of random errors. First, it is crucial that Einstein and de Haas had a theoretical belief—that the current whirls were orbiting electrons—which translated into a definite quantitative prediction. In addition, the measurements under investigation were extremely delicate: the movement of the oscillating reflected light beam from the Einstein-de Haas cylinder is on the order of millimeters, and the Barnett effect depends on the production of a magnetic field of the order 10^{-5} gauss. But most crucially, as a result of systematic errors from a variety of sources, the mean result was shifted in

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[68] Kuhn, "The functions of measurement in modern physical science," *Isis, 168* (1961), 161-193.

different directions often without leaving tell-tale traces of large dispersion in the results as would random errors. In a publication connected with the Solvay congress of 1923, de Haas wrote:[69]

> As to the largely discrepant values found by us and by myself I must remark that these experiments were made in a very short time, and that we were glad already to detect the effect in an unobjectionable way. The numbers serving for the calculation for the effect were but roughly known. So we did not measure the field of the magnetizing coil, we calculated it; moreover the coils were wound rather irregularly and not made for the purpose of the experiment. Also we did not measure the magnetization of the rod, we estimated it. We mentioned all this in our original paper. These preliminary results seemed to us rather satisfactory, and it will be easily understood that we were inclined to consider the value [$g = 1.02$]...as the better.

None of these errors would cause a spread in the results (in fact, Einstein and de Haas gave their probable error as 10%, far from including $g = 2$). Furthermore, such an explanation nowhere requires us to take recourse to the world of Gestalt images. Barnett, too, later tried to explain his determination of $g = 1$:[70]

> A long suspected systematic error has been found in the 1917 magnetometer observations, causing the results to differ considerably from those obtained by the method of electromagnetic induction in 1914 and 1915 is now fully confirmed.

As in the case of the original Einstein and de Haas measurements, the result now considered correct lies outside the range of Barnett's data of 1917 ($g = 1.1$ to 1.4). Some of the other systematic errors later pointed out by Barnett included such seemingly harmless elements as trolleys passing outside, incomplete compensation for the earth's magnetic field, and expansion of the rod during rotation.

The expectation that Barnett had in 1917 was undoubtedly reinforced (as it was for de Haas, Beck, and Arvidsson) by the frequent interchanging in his writing of the two sides of the equation, the measured quantity L/M and its theoretical equivalent $2m/e$. These experimentalists thought they were measuring the gyromagnetic ratio associated with an Ampèrean current whirl, rather than testing Ampère's hypothesis.

It is not enough, however, to say that theoretical predispositions are a purely pernicious factor. In the case of Maxwell, for instance, it is precisely his *lack* of a quantitative prediction (because he had no orbiting electron model) that left him with no idea how big an effect he

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[69] De Haas, "Le moment de la quantité," (ref. 54), 212.

[70] S. J. Barnett and L. J. H. Barnett "Improved experiments on magnetization by rotation," *Physical review, 20* (1922), 90-91, on 90.

was looking for. Had he known, as de Haas and de Haas-Lorentz showed much later, that he could expect a tilt of the apparatus of only 0.00013 radians, he would never have used his experiment as evidence for the non-inertial nature of current. Similarly, it may well have made it more difficult for Barnett in 1908 to have found the effect he was looking for because he had no quantitative prediction of the order of magnitude of the strength of the field he could expect.

The experimentalist would therefore seem to be in a continual dilemma. On the one hand, without a theory, he has no guiding quantitative prediction; he may not be able to find the effect sought, or to dissociate it from disturbances. On the other hand, given a quantitative prediction, the experimentalist is eventually forced to declare (at least implicitly) that here are no more systematic errors. This "stopping place" is, naturally enough, often the predicted result. An experimental nuclear physicist, Martin Deutsch, once put the conundrum as follows:[71]

> It is of course the ambition of every experimenter performing this kind of experiment to make a discovery, to sail safely between the Scylla of intellectual prejudice which makes us reject evidence not readily integrated without preconceived notions, and the Charybdis of irrelevance which has swallowed many working days spent in pursuit of instrumental artifice.

In the series of experiments discussed here, the Scylla was the orbiting electron theory, and the Charybdis included the transverse magnetization of the rod by the earth's field, the eddy currents of Barnett, and the improperly centered magnetic rod.

One might expect that in experiments where both strong theoretical predispositions and a definite quantitative prediction are present, it will often happen that the experimenter will find the result looked for whether or not it corresponds with what is later found to be the case. One might look at some of the other famous experimental factors of two that have arisen in modern physics—parity violation, and the bending of starlight by the sun—or at any number of cases where a new theory disagreed only slightly from the old in its quantitative prediction. In at least some of these I would expect compensating systematic errors to place the prediction and measurement in harmony and to place current experimental results outside of earlier experimental error. Conversely, we can frequently exploit mistaken experimental results to guide us to the experimentalists' often hidden theoretical presuppositions.

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[71] M. Deutsch, "Evidence and inference in nuclear research," *Daedalus* (Fall 1958), 88-98, on 97-98.