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Testing universal gravitation in the laboratory, or the significance of research on the mean density of the earth and big G, 1798–1898: changing pursuits and long-term methodological–experimental continuity

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Abstract This article seeks to provide a historically well-informed analysis of an important post-Newtonian area of research in experimental physics between 1798 and 1898, namely the determination of the mean density of the earth and, by the end of the nineteenth century, the gravitational constant. Traditionally, research on these matters is seen as a case of "puzzle solving." In this article, the author shows that such focus does not do justice to the evidential significance of eighteenth- and nineteenth-century experimental research on the mean density of the earth and the gravitational constant. As Newton's theory of universal gravitation was mainly based on astronomical observation, it remained to be shown that Newton's law of universal gravitation did not break down at terrestrial distances. In this context, Cavendish' experiment and related nineteenth-century experiments played a decisive role, for they provided converging and increasingly stronger evidence for the universality of Newton's theory of gravitation. More precisely, the author shall argue that, as the accuracy and precision of the experimental apparatuses and the procedures to eliminate external disturbances involved increasingly improved, the empirical support for the universality of Newton's theory of gravitation improved correspondingly.

1 Introduction: measuring gravitational force

As gravitational forces are very small, gravitational experiments in the laboratory are highly susceptible to extraneous disturbances (Cook 1996, pp. 50–51, 70–71; Gillies 1997, p. 153; Chen and Cook 1993, xii, pp. 5, 34–57). Measuring gravitation in the

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laboratory is therefore far from unproblematic and to the present day this difficulty persists.[1] Accordingly, it should not come as a surprise that it took well over a century *after* the publication of the first edition of Newton's *Principia* (1687) until the first precision measurements of gravitation were excogitated and performed. Newton himself had been rather sceptical on this matter and in his posthumously published *De mundi systemate liber* (1728) he had suggested that, if two spheres with a diameter of 1 foot are placed at a distance of 1/4 in. from one other, they would not come together by the force of their mutual attraction in less than 1 month.[2] Moreover, he had noted that a hemispherical mountain of 3 miles high and 6 miles broad, would not succeed in drawing a pendulum 2 min from its primary perpendicular.[3]

Newton's theory of universal gravitation was well-confirmed at planetary distances and, to a significant extent, it was founded on astronomical observations (Kuhn 1996, p. 31). In Propositions I–II of Book III, Newton established that the primary planets are drawn by an inverse-square centripetal force directed *quam proxime* towards the sun's centre and that the circumsaturnian and circumjovial planets are drawn by an inverse-square centripetal force directed *quam proxime* towards the centre of Saturn and Jupiter, respectively (Newton 1999, p. 802).[4] This conclusion was warranted by astronomical observation, which indicated that the celestial bodies satisfied Kepler's area (*quam proxime*) and harmonic rule (exactly), and by the systematic dependencies Newton had established in Propositions I–IV of Book I of the *Principia* between, on the one hand, the presence of an inverse-square law directed *quam proxime* to the centre of the attracting body and the attracted body describing Kepler's area rule *quam proxime* and, on the other hand, between the periodic times varying as the 3/2 power of the radius and the centripetal force varying inversely as the square of the radius (Newton 1999, pp. 444–451). An essential feature of the systematic dependencies Newton had established in Book I is that they are rigorously deduced from the laws of motion, and thus backed-up by them. In Propositions III–IV of Book III, Newton established that the moon is drawn by an inverse-square centripetal force towards the centre of the earth. Since astronomical observation shows that the motion of the moon satisfies Kepler's area rule *quam proxime*, it follows—given the systematic dependency between the area rule and the presence of a centripetal force—that the moon is drawn by a centripetal force towards the centre of the earth. However, because the moon is a solitary satellite, Newton could not use the route via Corollary 6 to Proposition

[1] See Gillies (1997) for further discussion. The precision with which the inverse-square law can be established is about one part in 10^4, whereas that of the inverse-square law in electrostatics is about one part in 10^{16}. Chen and Cook (1993, p. 5) comment as follows: "One reason is the very great sensitivity of electrical measurements as compared with mechanical measurements, the other is the fact that electrical detectors can be completely enclosed within a conducting Faraday cage, whereas it is not possible to build a completely enclosed gravitational Faraday cage and still have access to a mechanical detector."

[2] See Newton (1728, p. 27): "Hujusmodi globi duo, quartâ tantùm digiti parte ab invicem distantes, in spatiis liberis, haud minori quam mensis unius intervallo, vi mutuæ attractionibus accederent, ad invicem." Newton's calculation is, however, mistaken. See Poynting (1894a, p. 10, 1894b) on this matter.

[3] See Newton (1728, p. 27): "Sed nec montes toti suffecerint ad sensibiles effectus: Ad radices montis hemispærici alti tria milliaria & lati sex, pendulum vi montis attractum non deviabit scrupulis duobis primis a perpendiculo."

[4] The author has kept the discussion of Newton's *Principia*-style methodology to its bare essentials referring the reader instead to Ducheyne (2009, 2005), Harper (1998, 2002), Smith (2002a,b) for detailed accounts.

IV of Book I—as he had done in the preceding propositions of Book III. Newton, however, showed that, on the assumption of the inverse-square law, the acceleration of the moon in the region of the earth comes out equal to Huygens' measurement of terrestrial acceleration, which was one of the few direct indications in support of Newton's claim that gravitation was preserved all the way down to the surface of the earth. Whilst Propositions I–IV of Book III mainly involved "deductions from phenomena," which were backed-up by the systematic dependencies as established in Book I (and, *ultimately*, by the laws of motion), Propositions V–VIII of Book III explicitly contained several inductive steps that were guided by Newton's *regulae philosophandi*. The boldest leap, although having some mathematical support in Proposition LXIX of Book I, was Newton's generalization of the proportions he had established to bodies *universally*.[5] Newton had done little, except for taking into account the acceleration at the surface of the earth in Proposition IV of Book III, on the gravitational forces between bodies at smaller distances.[6]

It therefore remained to be shown that Newton's law of universal gravitation did not break down at smaller distances and in this context Cavendish' experiment and related nineteenth-century experiments played a decisive role, for they provided *converging and increasingly stronger evidence* for the universality of Newton's theory of gravitation.[7] From this perspective, several interconnected historical-systematic questions emerge: first, *how was such converging and increasingly stronger evidence in favour of Newton's theory of universal gravitation established*; secondly, *what about the development of the experimental procedures followed and the characteristics of the experimental apparatuses set to use*; and, finally, *is there is a discernable methodological–experimental unity in 100 years of experimental research and, if so, what does it look like.*

Several elements of Newton's law of universal gravitation can be subjected to empirical testing. An experimental physicist may chose to test *whether gravitational*

5 Newton was well aware, for instance, that the application of Law III to celestial bodies and bodies universally was an inductive generalization. It is worth pointing out that the theory of universal gravitation was predicated under the fourth *regula philosophandi* (Newton 1999, p. 796).

6 Newton conceived of an *experimentum crucis*, which involved measuring surface gravity, to decide between the theory of universal gravitation and a vortex theory. Newton stated that, on the assumption that the earth is an oblate sphere of homogeneous density, surface gravity at the equator results from the combination of two effects, namely the centrifugal forces (at the equator) *and* the gravitational forces arising from the inverse-square forces directed towards the individual parts on an oblate earth (Newton 1999, pp. 830–831; see Greenberg 1995, pp. 1–14, for discussion). By contrast, Christiaan Huygens, who explained gravity in mechanical terms, claimed that the earth's centrifugal forces at the equator alone are sufficient to explain the different lengths of seconds-pendulums. By consequence, the variation of surface gravity with latitude is larger according to Newton's theory than according to Huygens' (Schliesser and Smith forthcoming). It was only in the eighteenth century, however, that the matter was settled in favour of universal gravitation.

7 As is widely known, Kuhn (1996, p. 27) stressed that the empirical work undertaken to articulate a paradigm theory consists in "resolving some of its residual ambiguities and permitting the solution of problems to which it had previously only drawn attention." Conceiving normal science as mere "problem-solving" may come at the risk of underestimating the evidential significance of eighteenth- and nineteenth-century science. Compare: "*Exploring the agreement between theory and experiment into new areas or to new limits of precision* is a difficult, unremitting, and, for many, exciting job. Though *its object is neither discovery nor confirmation*, its appeal is quite sufficient to consume almost the entire time and attention of those physical scientist who do quantitative work" (Kuhn 1961, p. 174 [italics added]).

force really varies inversely proportional to the square of the distance,[8] *whether gravitational force is indeed proportional to the product of the masses involved* (Mackenzie 1895, pp. 333–334), *whether the constitution of the masses makes a difference* (Mackenzie 1895, pp. 321–323, 326–333), *whether it makes a difference when the masses are not* in vacuo (Austin and Thwing 1897), *whether the law of universal gravitation holds at smaller distances than celestial ones*, and *what the value for G is (and, even, whether G is really constant)* (Chen and Cook 1993, p. 2). The eighteenth- and nineteenth-century experimental work, which will be surveyed in what follows, set out to test the last two of these research questions.

To conclude this introduction, the author shall dedicate some words on the structure of this article. From the late eighteenth century onwards, quantification, standardization, accuracy, and precision measurement became increasingly salient (Hankins 1985, p. 50; Home 2003, pp. 371–374; Frängsmyr et al. 1990; Wise 1995). When Henry Cavendish (1731–1810), who according to Russell McCormmach was "the first after Newton to possess mathematical and experimental talents at all comparable to Newton's," (*DSB*, III, 195) was nearly 67, he published what would become his last substantial scientific article:[9] "Experiments to Determine the Density of the Earth," which in 1798 appeared in volume 88 of the *Philosophical Transactions of the Royal Society of London* —in fact, the only journal in which he published (Jungnickel and McCormmach 2001, p. 169). Despite popular thinking to the contrary, Cavendish never saw his experiment as an attempt to measure the gravitational constant. He worked entirely within a mathematical framework based on proportions, whereas the concept of a constant of universal gravitation can only be conceived within a mathematical framework of equations and absolute measurements.[10] That Cavendish tried to measure "big G" is therefore a textbook anachronism (Moreno Gonzáles 2001; Falconer 1987; Lally 1999; Jungnickel and McCormmach 2001, p. 444, footnote 87). Unfortunately, the myth of Cavendish and G has persisted, not only in physics textbooks, but also in the scholarly literature (Kuhn 1996, pp. 27–28; Baigre 1995, pp. 113–116). In addition to addressing the issue alluded to in its title, Cavendish' (1798) article provided a measurement of the gravitational interaction between laboratory-sized bodies. Cavendish' article will be discussed in detail in Sect. 2. In nineteenth century physical experimental practice, information on the probable error of an experimental result, the stability of the experimental environment, and the exclusion of external disturbances became all the more important (Gooday 1997; Schaffer 1995; Olesko 1995). In Sects. 3 and 4, nineteenth-century post-Cavendish experiments will be discussed

[8] This was first explicitly tested in 1895 by Mackenzie (1895, pp. 334–339; Cook 1988, pp. 717–718), who showed that no deviations from Newton's inverse-square law occurred. Recent experimental research has confirmed this result. For example, in 2001 at the University of Washington Newton's *inverse-square law* was tested down to 218 μm using a metal ring, suspended from a torsion pendulum, and containing ten equally spaced holes. No deviations occurred during this ingenious experiment (Hoyle et al. 2001).

[9] In 1809 Cavendish (1921, II, pp. 287–293) published one more article on a manner of improving the division of astronomical instruments (Cavendish 1921, II, pp. 287–293). Cavendish (1798) article is reproduced in Cavendish (1921, II, pp. 249–286.)

[10] Cornu and Baille (Cornu and Baille 1873, p. 957) (see *infra*) seem to have been one of the first physicists who insisted on a determination of G in absolute terms: "Nous avons donc commencé par une étude complète de la balance de torsion, surtout au point de vue des *mesures absolues*."

and analysed. In the early to mid-nineteenth century, Cavendish' quest for the mean density of the earth continued as new experimental setups, apparatuses and physico-mathematical treatments were excogitated to solve the issue at stake. An overview of these researches will be provided in Sect. 3, in which the work of Baily, Reich, Cornu and Baille, Airy, Sterneck, Jolly and Wilsing will be discussed. During the late nineteenth century, the quest for the density of the earth would ultimately lead to the introduction of the gravitation constant G.[11] An overview of these researches will be provided in Sect. 4, in which the experimental work of Poynting, Boys, Braun, and Richarz and Krigar-Menzel will be discussed. In Sect. 5, the author shall, on the basis of the historical material surveyed in the previous sections, develop an account of the evidential significance of this long-term episode in the history of science. In the final Sect. 6, the author shall conclude this essay by clarifying the relationship between the post-Newtonian experimental methodology, which we have surveyed in Sects. 2–4, and that of Newton.

2 The terminus *a quo*: Cavendish' article on the density of the earth

The apparatus

The apparatus set to use in Cavendish' famous experiment was a refinement of an apparatus originally contrived by John Michell (1724–1793),[12] who "did not complete the apparatus till a short time before his death, and did not live to make any experiment with it."[13] After his death it came into the hands of the Jackonian Professor at Cambridge, Francis John Hyde Wollaston (*DSB*, XIV, pp. 484–486), who, Cavendish wrote, "not having conveniences for making experiments with it, in the manner he could wish, was so good as to give it to me" (Cavendish 1798, p. 469; Jungnickel and McCormmach 2001, pp. 441–442). Figure 1 shows a longitudinal vertical section through the apparatus and the room, *GGHHGG*, in which it was placed. Cavendish' description of the experimental setup is to be found in Cavendish (1798, pp. 469–473).[14]

[11] Determining G accurately is quite difficult since it involves the absolute measurements of time, distance and mass (Chen and Cook 1993, p. 197).

[12] Michell and Cavendish's cooperation is discussed in McCormmach (1968). On Michell, see: Hardin (1966), Schaffer (1979), Gower (1982); DSB, IX, pp. 370–371. McCormmach's forthcoming biography of Michell, *Weighing the World: The Reverend John Michell of Thornhill*, will be a welcome addition to the scholarly literature on Michell.

[13] No draft material connected to Cavendish' famous experiment has surfaced so far (McCormmach 1995, p. 22).

[14] Useful discussion of the Cavendish experiment is to be found in Titchmarsh (1966), McCormmach (1995, 1998), Falconer (1999a), and Jungnickel and McCormmach (2001, pp. 440–450). However, it must be noted that most of the above accounts are not very detailed when it comes to the specifics of Cavendish' calculations. In discussing Cavendish' results, the author shall preserve his original mathematical reasoning, which appears somewhat archaic in comparison to our contemporary tools of mathematical computation: just as Newton, Cavendish used proportions only.

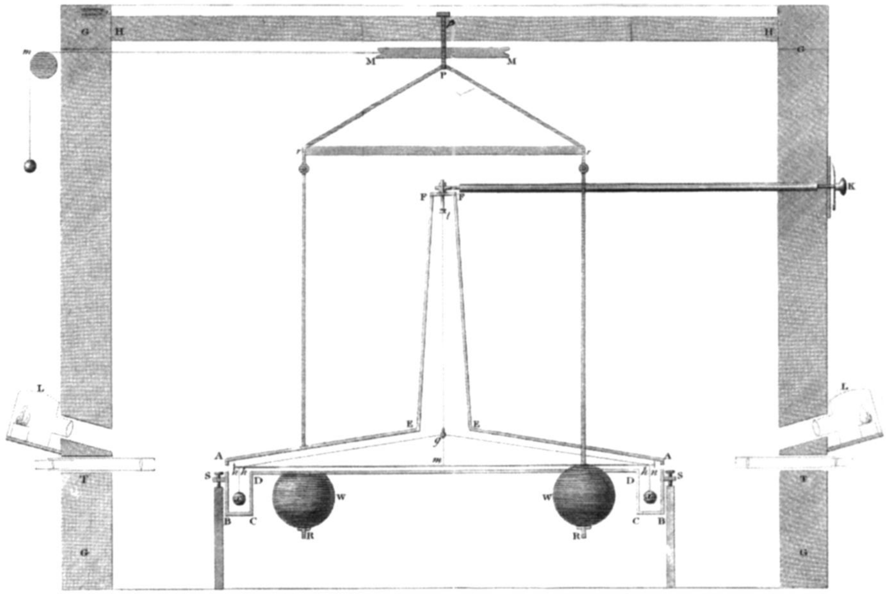

Fig. 1 Cavendish' apparatus (longitudinal vertical section). Taken from Cavendish (1798, p. 526). Courtesy of The Royal Society

In order to guard against sources of error, the room, measuring 10 feet in height and as many feet across, remained shut throughout the experiment and the effects were observed from outside of the room by means of telescopes (T) and lamps (L), which were installed at both sides of the room and which pointed to the verniers placed inside the case (Cavendish 1798, p. 471).[15] In this way, the most significant source of error, *scilicet* variation of temperature, could be guarded against significantly, according to Cavendish (1798, p. 471). Two leaden balls x and x, which have a diameter of about 2 in. (or about 5.08 cm), were suspended by the wires *hx* from the arm *ghmh* which is itself suspended by the slender wire *gl* with a length of about 40 in. (or 1.016 m). Given the fact that the wire is sufficiently slender, "the most minute force, such as the attraction of a leaden weight a few inches in diameter, will be sufficient to draw the arm sensibly aside" (Cavendish 1798, p. 469). Cavendish (1798, p. 470) computed that the force by which the balls are attracted in proportion to their weights is as 1 to 50,000,000. To determine the force by which the balls and the arm are drawn against the restoring force of the twisted wire, the arm was placed in such a way so as to enable it to move freely as a "horizontal pendulum." The arm *ghmh*, measuring 6 feet (or roughly 1.83 m) consisted of a slender deal rod *hmh* strengthened by a silver wire *hgh*, which "is made strong enough to support the balls, though very light" (Cavendish

[15] Jungnickel and McCormmach (2001) note that in these experiment Cavendish "brought the earth into his place of privacy, his home [in an outhouse of Clapham Common], where he experimented on it on his own." On Cavendish' personality traits, see Jungnickel and McCormmach (2001, pp. 303–309); on Clapham Common, see Jungnickel and McCormmach (2001, pp. 324–331).

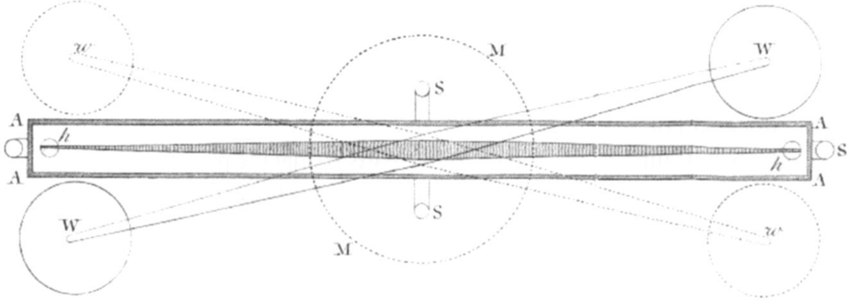

Fig. 2 Cavendish' apparatus (view from above). Taken from: Cavendish (1798, p. 527). Courtesy of The Royal Society

1798, p. 472).[16] The two lead balls x and x are placed in the narrow wooden case *ABCDDCBAEFFE*, which is set horizontally and which is supported by posts fixed firmly in the ground to which it is attached to by four screws (S).[17] The wooden case served to protect the arm from air currents. *FK* represents a wooden rod, which, by means of an endless screw, turns around the support and to which the slender wire *gl* is fastened. By means of *FK* Cavendish could manipulate the position of the arm *ghmh* from outside till the arm settles in the required position without any danger of touching either side of the case. The wire *gl* is fastened to its support at the top and to the centre of the arm at the bottom by brass clips in which it is pinched by screws. Two lead weights W and W are suspended from the copper rods *Pr* and *rR* and the wooden bar *rr*, which was in-between the rods. This devise was attached to the centre pin *Pp* which was attached to the ceiling *HH* of the room and placed above the centre of the apparatus. To *Pp* the pulley, *MM*, around which the cord *Mm* was attached so that one can alter the position of the weights W and W from outside. Figure 2 depicts a view from above of the instrument.

When the weights W and W were in the first position—indicated by full lines—they conspired in drawing the arm in the direction *hW*; when the weights are in the second position—indicated by dotted lines—they attracted the arm in the contrary direction *hw*. As in the second position the arm was drawn aside in such a direction as to make the index point to a higher number on the ivory slips, Cavendish considered this as the "positive position of the weights." The weights W and W were furthermore prevented from striking the instrument by pieces of wood, fastened to the wall of the room, which stop the weights as soon as they come within one fifth of an inch (or 0.508 cm) of the case. Cavendish found that "the weights may strike against them

[16] In an accompanying footnote, Cavendish pointed out that this setup is easier to construct, meets less air resistance and involves less complicated computations to ascertain how much the rod was attracted by the weights.

[17] In Fig. 1, the longitudinal vertical section of the apparatus, only two of the four screws are depicted. All four screws are depicted on Fig. 2 (see *infra*). Cavendish noted that the box in which the balls are moved is pretty deep "which makes the effect of the current of air more sensible than it would otherwise be, and is a defect which I intend to rectify in some future experiments" (Cavendish 1798, p. 497).

with considerable force, *without sensibly shaking the instrument*" (Cavendish 1798, p. 473 [italics added]). Moreover, "[i]n order to determine the situation of the arm" (Cavendish 1798, p. 473), slips of ivory, which were divided to a twentieth of an inch (or 1.27 mm), were place within the case, as near to each end of the arm as could be possibly done without touching them. To the original slips on each side a nonius was added, which in its turn was divided into five parts so that the position of the arm could be measured to one 100th of an inch (i.e. to 0.254 mm). Once the arm is set to rest and its position was observed, Cavendish (1798, 474) moved the weights W and W closer to the balls x and x so that "the arm will not only be drawn aside thereby, but it will be made to vibrate, and its vibrations will continue for a great while."

Attempts to determine the density of the earth were undertaken prior to Cavendish' experiment with the torsion rod. A well-tried method consisted in measuring the deflection of a plumb line in the vicinity of a large mountain.[18] This was the method which was used in Nevil Maskelyne's (1732–1811) famous experiment at Mount Schiehallion in Scotland.[19] In a short but acutely written article, Jacob (1813–1862) pointed out that "the Cavendish experiment is the one which may be relied on as giving a good approximation to the truth, within limits or error (when conducted with proper precaution)" (Jacob 1857, p. 295). In the Cavendish experiment "we are dealing with disturbing masses whose amount is exactly known," whereas in the method promoted by Hutton "we may approximately measure the mass of the mountain *above* the surface, we do not know how much may be added or abstracted *below*; and we have no right to assume that the mountain is merely a detached mass resting upon the general surface; it will almost certainly have *roots* differing in density from the surrounding country" (Jacob 1857, pp. 297–298 [underscore added]).

Measurements and their computation

After having provided the description of the experimental setup, Cavendish explained how he was about to determine the *point of rest of a vibration* and the *time of vibration*. To establish the point of rest, it was necessary "to observe the extreme points of the vibrations, and from thence to determine the point which it would rest at if its motion was destroyed, or the point of rest, as I shall call it" (Cavendish 1798, p. 474). To do so, Cavendish (1798, p. 474) observed three successive extreme points of a vibration and took the mean between the first and third of these extremes, as the *extreme point of*

[18] This method seems to have been tried for the first time by Pierre Bouguer (1698–1758) Bouguer (1749, pp. 372–373). See furthermore: Howarth (2007b, pp. 230–231). On Bouguer, see *DSB*, II, pp. 343–344.

[19] See Maskelyne (1775) and Ranalli (1984) and Reeves (2009) for further discussion. On the Astronomer Royal Maskelyne, see *DSB*, IX, pp. 162–164. Cavendish was involved in the mathematical parts of Maskelyne's (and Hutton's) experimental work on the matter (Jungnickel and McCormmach 2001, pp. 259–261). See furthermore: Hutton (1779), in which he arrived at a mean density of the earth relative to water of 4.5 (p. 93), Playfair (1811), which contains a correction of Hutton's value into 4.55886 (p. 374), and Hutton (1821), which contains Hutton's final determination of the density of the earth: 5 (p. 281). On Charles Hutton, see *DSB*, VI, pp. 576–577; on John Playfair, see *DSB*, I, pp. 34–36. The same method was again used in 1856 (James 1856), which arrived at a value of 5.417 ± 0.054. On Maskelyne's, Hutton's and Playfair's contributions to the determination of the earth's mean density, see furthermore Howarth (2007b, pp. 231–233).

vibration in one direction, on the one hand, and, on the other, he took the mean of the extreme point of vibration and the second extreme as the *point of rest*, "for as the vibrations are continually diminishing," he observed, "it is evident, that the mean between two extreme points will not give the true point of rest."[20] He then determined the time of vibration by observing the two extreme points of a vibration and the times at which the arm arrived at two given divisions between the extremes, which were on different sides of the middle point and not very far from it. From the above, he computed the middle point of the vibration and, by proportion, the time at which the arm comes to this middle point. After a number of vibrations he repeated this procedure and divided the interval of time, between the arrival of the arm to the two middle points, by the number of vibrations, which gives the time of one vibration.[21] "To judge the property of this method," on must consider "in what manner the vibration is affected by the resistance of the air, and by the motion of the point of rest" (Cavendish 1798, p. 476). Cavendish, however, argued that in both cases the effect will be inconsiderable. First, "as the time of coming to the middle point is before the middle of the vibration, both in the first and last vibration, and in general is nearly so, the error produced from this cause must be inconsiderable." Secondly, insofar as the point of rest can be considered as moving uniformly, the time of two successive vibrations "will be very little altered; and, therefore the time of moving from the middle point of one vibration to the middle point of the next, will also be very little altered" (Cavendish 1798, pp. 476–477). It is relevant to note that Cavendish was a careful observer who was very knowledgeable of the calibration of scientific instruments—in fact, he was very active at times when the instruments of The Royal Society were being calibrated[22]—and "in his experimental work he showed a thorough understanding of the theory of errors" (Jungnickel and McCormmach 2001, p. 149, 174).

"The" Cavendish experiment is in fact a concatenation of 17 related experiments.[23] The specific determinations of the motions of the arm and the times of vibration in each of these experiments will not be discussed: The author shall restrict himself to a discussion of the obtained results, which Cavendish summarized on page 520 of his article, and their computation. The third and fifth column contain the distances traversed by the arm and the times of vibration as found in the 17 foregoing experiments (see Fig. 3). The second column shows the starting positions of the arm and the directions in which it was moved.

[20] In addition, Cavendish pointed out the following: "It may be thought more exact, to observe many extreme points of vibration, so as to find the point of rest by different sets of three extremes, and to take the mean result; but it must be observed, that notwithstanding the pains taken to prevent any disturbing force, the arm will seldom remain perfectly at rest for an hour together; for which reason, it is best to determine the point of rest, from observations made as soon after the motion of the weights as possible." (Cavendish 1798, p. 474).

[21] Cavendish (1798, p. 478) notes that the error in the result is much less, when the forces required to draw the arm aside was deduced from experiments made at each experiment, than when it is taken from previous experiments.

[22] See for instance Cavendish's unpublished piece "Boiling Point of Water, At the Royal Society, April 18, 1766" (in Cavendish 1921, II, pp. 351–345) and Cavendish (1776). For further discussion, see Jungnickel and McCormmach (2001, pp. 220–224).

[23] These were performed in 1797 on 6, 7, 12, and 20 August, 6, 18, and 23 September and in 1798 on 29 April, 5, 6, 9, 25–28, and 30 May. The article was read shortly afterwards, i.e. on 21 June 1798.

The following Table contains the Result of the Experiments.

Exper.	Mot. weight	Mot. arm	Do. corr.	Time vib.	Do. corr.	Density.
1	m. to +	14,32	13,42	′ ″	-	5,5
	+ to m.	14,1	13,17	14,55	-	5,61
2	m. to +	15,87	14,69	-	-	4,88
	+ to m.	15,45	14,14	14,42	-	5,07
3	+ to m.	15,22	13,56	14,39	-	5,26
	m. to +	14,5	13,28	14,54	-	5,55
4	m. to +	3,1	2,95		6,54	5,36
	+ to —	6,18	-	7,1	-	5,29
	— to +	5,92	-	7,3	-	5,58
5	+ to —	5,9	-	7,5	-	5,65
	— to +	5,98	-	7,5	-	5,57
6	m. to —	3,03	2,9	-	-	5,53
	— to +	5,9	5,71		-	5,62
7	m. to —	3,15	3,03	7,4 by mean.	6,57	5,29
	— to +	6,1	5,9			5,44
8	m. to —	3,13	3,00		-	5,34
	— to +	5,72	5,54	-	-	5,79
9	+ to —	6,32	-	6,58	-	5,1
10	+ to —	6,15	-	6,59	-	5,27
11	+ to —	6,07	-	7,1	-	5,39
12	— to +	6,09	-	7,3	-	5,42
13	— to +	6,12	-	7,6	-	5,47
	+ to —	5,97	-	7,7	-	5,63
14	— to +	6,27	-	7,6	-	5,34
	+ to —	6,13	-	7,6	-	5,46
15	— to +	6,34	-	7,7	-	5,3
16	— to +	6,1	-	7,16	-	5,75
17	— to +	5,78	-	7,2	-	5,68
	+ to —	5,64	-	7,3	-	5,85

Fig. 3 Summary of Cavendish' measurements. Taken from: Cavendish (1798, p. 520). Courtesy of The Royal Society

What will be discussed, however, are the ways the experiments differed and the procedures by which external forces were singled out. In the first three experiments, Cavendish (1798, p. 478) used a copper silvered wire, which, as he soon found out, was not stiff enough so that "the attraction of the weights drew the balls so much aside, as to make them touch the sides of the case." However, he decided to make some experiments with it. In order to make sure that the vibrations were not produced by magnetism, he changed the iron rods, by which the leaden weights were suspended, for copper ones, and a result of this it turned out that "there still seemed to be some effect of the same kind, but more irregular, so that I attributed it to some accidental cause, and therefore hung on the leaden weights, and proceeded with the experiments" (Cavendish 1798, p. 479). Furthermore, Cavendish observed that:

> if a wire is twisted only a little more than its elasticity admits of, then, instead of setting, as it is called, or acquiring a permanent twist all at once, it sets gradually, and, when it is left at liberty, it gradually loses part of that set which it acquired; so that if, in this experiment, the wire, by having been kept twisted for 2 or 3 h,

> had gradually yielded to this pressure, or had begun to set, it would gradually restore itself, when left at liberty, and the point of rest would gradually move backwards; but, though the experiment was repeated twice, I could not perceive any such effect. (Cavendish 1798, p. 485)

In the experiments made thereafter, he replaced the original wire by a stiffer one. In the fourth experiment, Cavendish observed that, although, as in the previous experiments, on moving the weights from positive to negative the effect of the weights increased on standing, the effect diminished on moving them from negative to positive. He then determined whether the balls or weights could have acquired polarity from the earth's magnetic field or whether magnets placed in the vicinity of the case could alter the observed effects (fifth experiment) (Cavendish 1798, pp. 490–491). Upon closer scrutiny, these putative causes indicated no significant difference, according to Cavendish. He found, however, that differences in temperature did make a difference (sixth to ninth experiment) (Cavendish 1798, pp. 496–497). Next, he compared the results when starting the experiment with the index placed very closely to the case without touching it (ninth to eleventh experiment), with the index in its usual position (12th to 14th experiment), and with the index placed very closely to the case without touching, but now in the opposite direction (15th experiment). Two additional experiments concluded the observations which Cavendish provided to support his case. Cavendish' experiments provided information about two important values: the motion of the arm and the time of its vibrations. In the following paragraph, we will see how he came up with an ingenious way to determine the density of the earth relative to the density of water in terms of the observed values for the motions of the arm and the time of vibration—in the remainder of his article, these terms were denoted by B and N, respectively.

Determining the density of the earth

Cavendish' computation of the earth's density assumed "that the arm and copper rods have no weight, and that the weights exert no sensible attraction, except on the nearest ball." He added that he would examine "what corrections are necessary, on account of the arms and rods, and some other small causes" (Cavendish 1798, p. 509). Cavendish first determined the force required to draw the arm aside, which is determined by the time of a vibration. He treated the motion of the arm as a horizontal pendulum which he compared to the motion of a regular (vertical) pendulum. This is a crucial feature of the experiment's setup: because the balls are set in a plane orthogonal to the direction of the earth's gravitational field, Cavendish succeeded in eliminating gravitation's downward pull from the experiment considerably. As the distance between the centres of the two balls, x and W, is 73.3 in., the distance of each from the centre of motion is 36.65 in. Moreover, the length of a pendulum vibrating seconds "in this region" is 39.14 in. Therefore,

> if the stiffness of the wire by which the arm is suspended in such, that the force which must be applied to each ball, in order to draw the arm aside by the angle

A, is to the weight of that ball as the arch of A to the radius,[24] the arm will vibrate in the same time as a pendulum whose length is 36.65, that is, in $\sqrt{\frac{36.65}{39.14}}$ s,[25] and therefore, if the stiffness of the wire is such as to make it vibrate in N seconds, the force which must be applied to each ball, in order to draw it aside by an angle A, is to the weight of the ball as the arch of $A \times \frac{1}{N^2} \times \frac{36.65}{39.14}$ to the radius [(*)][26]. (Cavendish 1798, p. 509)

As the ivory scale at the end of the arm is 38.3 in. away from the centre of motion and each division is $\frac{1}{20}$ of an inch from the centre of motion, it subtends an angle at the centre whose arch is $\frac{1}{766}$ i.e. $\frac{38.3\,\text{in.}}{0.05\,\text{in}}$. By filling in A in (*), *the forces which must be applied to each ball to draw the ball aside by one division, is to the weight of the ball as* $\frac{1 \times 36.65}{766N^2 \times 39.14}$, that is *as* $\frac{1}{818N^2}$ *to 1* (**). Secondly, it is required to find "the proportion which the attraction of the weight bears to that of the earth thereon, supposing the ball to be place in the middle of the case, that is, to be not nearer to one side than the other" (Cavendish 1798, p. 510). At this point, Cavendish introduced a correction factor. He observed that, "[w]hen the weights are approached to the balls, their centres are 8.85 in. from the middle line of the case; but, through inadvertence, the distance, from each other, of the rods which support these weights, was made equal to the distance of the centres of the balls from each other, whereas it ought to have been somewhat greater."[27] As a consequence of this, the effect of the weights in drawing the arm aside is less than it would otherwise have been, to wit, in a ratio of 0.9779 to 1.[28] Each of the weights weighed 24,390,000 grains or roughly 158 kg,[29] which is equal to the weight of 10.64 spherical feet of water, i.e. equal to the weight of 10.64 times the volume of a sphere of water with a diameter of 1 foot. The radius of one spherical feet of water is therefore 6 in., as 1 foot equals 12 in. Therefore, *the attraction of a weight on a particle placed at the centre of a ball at 8.85* in. *from the centre of that weight is to the attraction of a spherical foot of water on an equal particle placed on its surface as* $10.64 \times 0.9779 \times \left(\frac{6}{8.85}\right)^2$ *to* 1. Furthermore, the mean diameter of the earth is 41,800,000 feet and, therefore, if the mean density of the earth is to that of

[24] What Cavendish is stating here is equivalent to saying that the force restoring the pendulum's motion (F_r) to the vertical through an angle A is to the weight of the ball times $\sin(A)$ (Falconer 1999a, p. 475a).

[25] As in this case $\frac{x_1}{x_2}$ is proportional to $\frac{t_1^2}{t_2^2}$, it follows that $\sqrt{\frac{x_1}{x_2}}$ is proportional to $\frac{t_1}{t_2}$. If x_1 is 36.65 in., x_2 is 39.14 in. and t_2^2 is 1, it follows that t_1 is proportional to $\sqrt{\frac{x_1}{x_2}}$ or, from what is given, proportional to $\sqrt{\frac{36.65}{39.14}}$.

[26] Insertion added. This is equivalent with saying that the force exerted on the balls (F_e) swinging along a simple pendulum is to the restoring force (F_r) as $\frac{T^2}{N^2}$. Because the restoring force is proportional to the weight of the ball (W_b) times $\sin(A)$ (see supra), F_e/W_b is proportional to $\sin(A) \times \frac{T^2}{N^2}$ or to $\sin(A) \times \frac{36.65}{39.14} \times \frac{1}{N^2}$.

[27] Baily (1843, p. 89) pointed out that "yet it would have been more satisfactory to have known that no alteration in that distance was perceptible during the whole of the series."

[28] This step follows from basic geometry. Since it is neatly described and illustrated in Mackenzie (1895, p. 89), footnote *, the author will omit further discussion of this step.

[29] Assuming that 1 grain equals 64.79891 mg.

water as D to 1,[30] *the attraction of a leaden weight on a ball will be to the attraction of the earth on that same ball* as $10.64 \times 0.9779 \times \left(\frac{6}{8.85}\right)^2$ to 41,800,000 D, i.e. as 1 to 8,739,000 D[31] (***).

Although Cavendish did not make this point explicit, this conclusion relied on Newton's law of universal gravitation.[32] Let F_W be the weight of W on ball x, F_e the weight of the earth on ball x, and $F_{\mathrm{H_2O}}$ the weight of the water sphere and let the diameters (d), densities (ρ) and radiuses (r) be represented similarly. The ratio $\frac{F_W}{F_\mathrm{e}}$ is given by $10.64 \times 0.9779 \times \left(\frac{6}{8.85}\right)^2 \times \frac{F_{\mathrm{H_2O}}}{r^2_{\mathrm{H_2O}}} \times \frac{r^2_\mathrm{e}}{F\mathrm{e}}$, which is proportional to $10.64 \times 0.9779 \times \left(\frac{6}{8.85}\right)^2 \times \rho_{\mathrm{H_2O}} \times \frac{d_{\mathrm{H_2O}^3}}{d_{\mathrm{H_2O}^2}} \times \frac{d^2_\mathrm{e}}{\rho_\mathrm{e} \times d^3_\mathrm{e}}$. Now, since $d_{\mathrm{H_2O}} = 1$ in., $\frac{F_W}{F_\mathrm{e}}$ is proportional to $10.64 \times 0.9779 \times \left(\frac{6}{8.85}\right)^2 \times \frac{1}{\rho(e/\mathrm{H_2O})} \times \frac{1}{d_e}$, which equals $10.64 \times 0.9779 \times \left(\frac{6}{8.85}\right)^2 \times \frac{1}{D} \times \frac{1}{41{,}800{,}000}$. Therefore, $\frac{F_W}{F_e} :: \frac{1}{8{,}739{,}000\, D}$. Previously, we have established that the force required to draw the arm through one division of 2.54 in. is to the weight of the ball as 1 to 818 N^2 (**). By dividing (**) and (***), we establish that *the attraction of a weight on a ball is to the force required to draw the arm through one division 2.54 in. as* N^2 to 10,683 D, from which it follows that the density of the earth relative to the density of water, D, is given as $\frac{N^2}{10{,}683B}$, where B is the number of divisions in hundredths of an inch and N is the observed period in seconds. By adding correction factors (1) and (4), which are discussed in the next paragraph, to the above formula, Cavendish corrected $\frac{N^2}{10{,}683B}$ to $\frac{N^2}{10{,}844B}$ and by this proportion he arrived at the values in column 7 in the table on Fig. 3 (Cavendish 1798, p. 517).

Before Cavendish proceeded to compute the value of the density of the earth relative to the density of water on the basis of the values of N and B he had established in his experiments, Cavendish provided six correction factors:

> [F]irst, for the effect which the resistance of the arm to motion has on the time of the vibration: 2d, for the attraction of the weights on the arm: 3d, for their attraction on the farther ball: 4th, for the attraction of the copper rods on the balls and arm: 5th, for the attraction of the case on the balls and the arm: and 6th, for the alteration of the attraction of the weights on the balls, according to the position of the arm, and the effect which that has on the time of vibration. None of these corrections, indeed, except the last, are of much signification, but they ought not entirely be neglected. (Cavendish 1798, p. 511)

30 Therefore D is proportional to the density of the earth divided by the density of water. As will be denoted here, $D = \frac{\rho_e}{\rho_{\mathrm{H_2O}}}$, or more succinctly, $\rho_{(e/\mathrm{H_2O})}$.

31 Charles Hutton (1821, p. 287) correctly pointed out that the ratio should have been 1 to 8,740,000 instead.

32 Cf. Falconer (1999a, p. 475b). In his manuscripts, which are preserved at the Duke of Devonshire's (Derbyshire) library in Chatsworth House, Cavendish did not seems to written anything on the specifics of Newton's methodology (personal communication with Russell McCormmach, 19 March 2010). On the status of theories in Cavendish' work and that of his contemporaries, see McCormmach (McCormmach 2004, Chapter 3, pp. 49–77).

Cavendish computed that, (1) if the time of a vibration is given, the force required to draw the arm aside is greater than if the arm had no weight, namely in a proportion of 11,660 to 11,262, i.e. ca. 1.0353 to 1 (*first correction factor: the resistance of the arm to motion*; Cavendish 1798, pp. 512–513), (2) that the power of the weight to move the arm, by means of its attraction on the nearest part of it, is 0.0139 of its attraction on the ball (*second correction factor: the attraction of the weight on the arm*; Cavendish 1798, pp. 513–514),[33] (3) that the effect of the attraction of the weight on both balls, is to that of its attraction on the nearest ball as 0.9983 to 1 (*third correction factor: the attraction on the farther ball*; Cavendish 1798, p. 515), (4) that the attraction of the weight and copper rod on the arm and both balls together is to the attraction of the weight on the nearest ball as 1.0199 to 1 (*fourth correction factor: the attraction of the copper rods on the balls and arms*; Cavendish 1798, p. 515), (5) that the attraction of the case on the balls cannot exceed $\frac{1}{1,170}$ part of the weight and that the whole force is "so small as not to be worth regarding" (*fifth correction factor: the attraction of the case and the balls and the arm*; Cavendish 1798, p. 515–517), and, finally, (6a) that "[i]f the time of vibration is determined by an experiment in which the weights are in near position, and the motion of the arm, by moving the weights from the *near to the midway position*, is d divisions, the *observed time* must be diminished in [...] in the ratio of $1 - \frac{d}{185}$ to 1,"[34] (6b) that in order to correct "the motion of the arm caused by moving the weights *from a near to the midway position,or the reverse*, observe how much the position of the arm differs from 20 divisions, when the weights are in the near position: let this be n divisions, then, if the arm at that time is on the same side of the division of 20 as the weight, the *observed motion* must be diminished by the $\frac{2n}{185}$ part of the whole; but otherwise, it must be as much increased,"[35] and (6c) that "[i]f the weights are moved from *one near position to the other*, and the motion of the arm is $2d$ divisions, the *observed motion* must be diminished by the $\frac{2n}{185}$ part of the whole" (*sixth correction factor: the effect of the alternation of the attraction*; Cavendish 1798, p. 519 [italics added]).[36]

Given the formula which Cavendish had obtained to calculate the density of the earth relative to the density of water, $D = \frac{N^2}{10,683B}$, where N is the (possibly corrected) time of vibration expressed in seconds and B the (possibly corrected) motion of the arm expressed in twentieths of an inch, he could compute D as inferable from each experiment. The results of these computations are to be found in column 7 of the table depicted in Fig. 3. In the concluding section to his article, Cavendish recorded:

> From this table it appears, that though the experiments agree pretty well together, yet the difference between them, both in the quantity of motion of the arm and in the time of vibration, is greater than can proceed merely from the error of

33 Cavendish (1798, p. 514) furthermore noted: "It must be observed, that the effect of the attraction of the weight on the whole arm is rather less than this, as its attraction on the farther half draws it the contrary way; but, as the attraction on this is small, in comparison of its attraction on the nearer half, it may be disregarded."

34 See entries 7 and 14 in column 6 of the table in Fig. 3.

35 See entries 1–7, 12, 14, and 16 in column 4 of the table in Fig. 3.

36 See entries 13, 15, and 17 in column 4 of the table in Fig. 3.

> observation. As to the difference in the motion of the arm, it may very well be accounted for, from the current of air produced by the difference of temperature; but, whether this account for the difference in the time of vibration, is doubtful. If the current of air was regular, and of the same swiftness in all parts of the vibration of the ball, I think it could not; but as there will most likely be much irregularity in the current, it may very likely be sufficient to account for the difference. (Cavendish 1798, p. 521)

Next, he derived an average of 5.48[37] for the density of the earth relative to the density of water and noted that "the extreme results do not differ from the mean by more than,38, or 1/14 of the whole, and therefore the density should seem to be determined, to great exactness" (Cavendish 1798, p. 521). By doing do, he had succeeded in determining a previously "imponderable quantity" (Hacking's terminology; Hacking 1983, p. 236). Thereafter, he warded off two possible objections:

> [1] It, indeed, may be objected, that as the result appears to be influenced by the current of air, or some other cause, the laws of which we are not well acquainted with, this cause may perhaps act always, or commonly, in the same direction, and thereby make a considerable error in the result. But yet, as the experiments were tried in various weathers, and with considerable variety in the differences of temperature of the weights and air, and with the arm resting at different distances from the sides of the case, it seems unlikely that this cause should act so uniformly in the same way, as to make the error of the mean result nearly equal to the difference between this and the extreme; and, therefore, it seems unlikely that the density of the earth should differ from 5,48 by so much as 1/14 of the whole.
>
> [2] *Another objection, perhaps, may be made to these experiments, namely that it is uncertain whether, in these small distances, the force of gravity follows exactly the same law as in greater distances. There is no reason, however, to think that any irregularity of this kind takes place, until the bodies come within the action of what is called the attraction of cohesion, and which seems to extend only to very minute distances.* With a view to see whether the result could be affected by this attraction, the author made the 9th, 10th, 11th, and 15th experiments, in which the balls were made to rest as close to the sides of the case as they could; but there is no difference to be depended on, between the results under that circumstance, and when the balls are placed in any other part of the case. (Cavendish 1798, pp. 521–522 [numbering and italics added])

Finally, when pointing to the discrepancy between his own result and that obtained by Maskelyne, Cavendish (Cavendish 1798, p. 522) concluded his article with the words: "But I forbear entering into any consideration of which determination is most to be depended on, till I have examined more carefully how much the preceding determination is affected by irregularities whose quantities I cannot measure." Cavendish' last article was of tremendous importance for "it brought the precision of astronomical

[37] The third value on column 7 on Fig. 3 should have been 5.88 instead of 4.88. As a consequence of this, Cavendish' mean result should have been 5.448 (Baily 1843, p. 90). Interestingly, in Proposition X of Book III, Newton had noted that "it is likely that the total amount of matter in the earth is about five or six times greater than it would be if the whole earth consisted of water" (Newton 1999, p. 815).

observation down to earth, to experimental science" (Jungnickel and McCormmach 2001, p. 450). By means of his experiment, Cavendish had not merely determined the mean density of the earth, more importantly, he had provided *a test for the universality of Newton's theory*: he had shown that the law of universal gravitation holds "in these small distances" and, hereby, he had provided evidence that the law of universal gravitation holds at smaller distances than celestial ones. Obviously, he did not provide a test for the gravitational inverse-square law (Lauginie 2007, pp. 126–127). In short, he had demonstrated that *robust*[38] gravitational interactions occur between terrestrial bodies.

3 Post-Cavendish determinations of the density of the earth

The publication of Cavendish' article on the mean density of the earth, drew scholarly attention in Europe, particularly in England, Germany and France. Accounts of Cavendish' experiment were soon published in German in 1799 and again in 1827 (Gilbert 1799; Muncke 1827),[39] and in 1815 a complete French translation of Cavendish' original article appeared (Chompré 1815). Occasionally, his results were contested, as will be shown in what follows.[40] Most scientific attention, however, was devoted to improving Cavendish' results, i.e. to *excogitating more reliable measurement techniques*, to *constructing variant and different experimental apparatuses*, which suffered less from external disturbances than Cavendish' original torsion rod,[41] and to *introducing new idealizations*[42] of the phenomena under consideration.[43] In addition to torsion and plumb-line experiments, new methods for determining the density of the earth were established during the early nineteenth century: most notably, by means of a regular pendulum,[44] by comparing the swings of pendulums near and below the

[38] In the sense that they were shown to be *independent* from the surrounding variations in temperature or air currents. See the discussion in Galison (1987, pp. 2–3).

[39] See Gilbert (1799) and Muncke (1827). In the *Vorrede* to the first volume of *Annalen der Physik* (1799), the first editor of the journal, Gilbert, had praised the experimental results in Cavendish (1798) article for their exactness (Jungnickel and McCormmach 2001, p. 456). In 1806 Heinrich Wilhelm Brandes (1777–1834) derived a variant formula to establish the time of oscillation more precisely (1806, p. 301, 310). On Brandes, see *DSB*, II, pp. 420–421. A useful work on the history of geodesy in Germany is Torge (2009).

[40] In Hutton (1821), Cavendish was accused of several calculation errors. However, much of Hutton's criticism was simply mistaken (see Baily 1843 for ample discussion).

[41] Compare with Baigre's (1995, p. 119) analysis of post-Cavendish experiments in terms of continually addressing residual phenomena.

[42] My favourite quote on scientific idealization in the context of the determination of the mean density of the earth is the following: "Now none of these methods give the mean density as a direct result; for the result obtained, the earth's total attraction, is $= g \times$ the sum of (all the particles respectively by the squares of their distances) instead of $g \times$ (the total mass divided by the square of the radius or mean distance): and to assume the equality of these, is to assume the earth to be a sphere, and to have its matter arranged in concentric shells or layers of equal density throughout each layer, both of which we know to be untrue" (Jacob 1857, p. 296).

[43] In their case-study, Baird and Faust (1990, esp. p. 170, 172) have emphasized that the accumulation of *scientific instruments* and *instrumental techniques* are important factors for scientific progress.

[44] See Poynting (1894a, pp. 22–40, 1894b), for further discussion of particular experiments performed by this method, and Howarth (2007a), for discussion of the role of the pendulum in early geophysics.

surface of the earth, and by means of a common balance.[45] Rather than providing a complete overview of all nineteenth-century experiments on the determination of the density of the earth and G, in this and the following section the author shall provide a detailed overview of the most significant developments in these areas of research. The author starts his analysis by discussing Cavendish-like torsion experiments.

Post-Cavendish torsion experiments: 1843–1873

In 1843 Francis Baily[46] (1774–1844) recorded that Cavendish' aim in his 1798 article "appears to have been more for the purpose of exhibiting a *specimen* of what he considered to be an excellent method of determining this important enquiry, than of deducing a result that should lay claim to the full confidence of the scientific world" (Baily 1843, p. 8).[47] He emphasized that Cavendish' results were approximate and few in number. For this reason The Royal Society had set out:

> not merely to repeat the original experiments of CAVENDISH in a somewhat similar manner, but also to extend the investigation by varying the magnitude and substance of the attracted balls—by trying the effect of different modes of suspension—by adopting considerable differences of temperature—and by other variations that might be suggested during the progress of enquiry. (Baily 1843, p. 10)

Just as Cavendish had done, Baily performed the experiments in a room of his private house—37 Tavistock Place in London. He introduced an "inverse" Cavendish torsion rod and determined the times of vibration and the resting points in a different way. Most importantly, whereas Cavendish (and Reich (1838)[48]) had suspended the masses from above and supported the torsion from below, Baily reversed the modus operandi. An inverted T-shaped mahogany box, which contained the suspended torsion rod, was attached to the ceiling by "a very stout plank." By means of several strong iron screw bolts, passing through the ceiling, the plank was made the firm support of a triangular wooden frame (see Fig. 4a) (Baily 1843, p. 11). Baily recorded that "[t]he whole of the mahogany box is completely insulated from every part of the frame work, and from any contact with those portions of the apparatus that are near it. It consequently remains undisturbed either by walking about the floor, by working the

45 Howarth notes: "The first systematic measurements of the force of [*sic*] Earth's gravity field in the eighteenth century began as an essentially geodetic exercise, with the aim of providing an alternative, and perhaps more precise, route to defining the Figure of the Earth than that provided by arc-length measurements. This was accompanied by a keen interest in obtaining a value for the mean density of the Earth" (Howarth 2007b, p. 255).

46 On Baily see Miller (1986) and Ashworth (1994).

47 Baily refers to Cavendish (1798, p. 522) to back up his claim. An extended abstract of Baily's (1842) article appeared under the same title in *Philosophical Magazine*.

48 In this essay the author shall not go into the details on Reich's first article on the mean density of the earth. Reich's second article on the matter will be discussed in what follows.

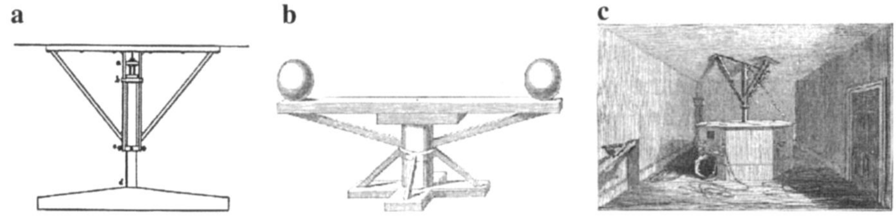

Fig. 4 **a–c** (left to right: **a**, **b**, **c**) Francis Baily's torsion-rod experiment. Taken from: Baily (1843, p. 11, 12, 13), respectively. Courtesy of The Royal Society

masses, or by any other commotion within the room." (Baily 1843, p. 20).[49] Below the centre of the mahogany box, a solid wooden cross piece was firmly screwed to the floor (see Fig. 4b), "on which has been raised a circular frame work, embracing and supporting a copper ring; within which ring a large round wooden pillar moves on an iron pivot, which bears upon a small metal cup" (Baily 1843, pp. 11–12). On top of the pillar, a deal plank was fastened horizontally, which supported two large leaden balls or masses, which were firmly fixed onto it:

> The height of this pillar has been so constructed that the centres of the masses should be placed in the same horizontal plane (or nearly so) as that intended to be the plane of the centres of the small balls alluded to. On the upper surface of the plank, and as nearly as possible in the centre of motion, a round piece of ivory, about 1 in. in diameter, has been inlaid. The centre of motion having then been accurately determined, two black cross lines were so drawn on the ivory that their point of intersection always indicated its position. (Baily 1843, p. 12)

In order to minimize the "influence of any accidental or sudden change of temperature in the room," (Baily 1843, p. 13) an octagonal wooden frame was built around the horizontal portion of the mahogany box and the support of the leaden balls (see Fig. 4c). Baily remarked that "[n]othing can exceed the ease, the steadiness, and the facility with which these large bodies are moved: and during the many thousands of times that they have been turned backwards and forwards, I have never observed the least deviation from the most perfect accuracy" (Baily 1843, p. 15).

Baily experimented with small balls of six different materials (platinum, lead, zinc, glass, ivory, and hollow brass) and diameters and he used different modes of suspension of the torsion rod, "one by means of a *single* copper wire, as practiced by CAVENDISH and REICH; the other by means of *double* lines, as proposed by GAUSS in magnetic experiments" (Baily 1843, p. 26) and torsion wires of varying length and material (silk, brass, iron) (Baily 1843, pp. 24–31). With respect to the support to which the suspension lines, Baily recorded:

> Before I close this account of the suspension lines, I cannot but advert to the firmness and stability of the support to which they were attached. In order to

[49] The torsion box was furthermore lined with tin foil, which was connected with a copper wire that communicated with the ground for the purpose of carrying off any slight electricity that might exist in the box (Baily 1843, p. 19).

> satisfy myself on the requisite point, at the time of the original construction of the apparatus, I made various attempts to create a sensible disturbance in the motion of the torsion rod, by causing the doors to be frequently and violently slammed—by jumping heavily on the floor of the room—and also *above* the ceiling—and in other different ways, having a similar tendency: but, in no instance could I observe the least effect upon the lateral motion of the rod. I have also frequently tried the same experiment, when different visitors were present, since the apparatus has been completed: and have moreover many times not only accidentally, but also designedly, made a regular series of experiments for determining the Density of the Earth, during the most violent storms that I have ever witnessed, when the wind has been so boisterous, and blowing in such gusts, that the house has been shaken to its centre. But in no instance have I ever seen the least disturbance in the lateral motion of the torsion rod, nor any difference produced in the results of the experiments. [...] But a moment's consideration will convince a person conversant with the subject, that no *dancing* motion of the suspension line, even if it did exist, would tend to produce an irregular *lateral* or *angular* motion in the torsion rod; and this is the only anomalous motion we need guard against.[50] There is also another remarkable circumstance with this subject, which I think it requisite likewise to place on record. When the torsion rod has been in a state of repose, I have frequently shaken the torsion box, by rapidly moving the ends backward and forward from side to side 50 or 60 times or even more: but I could never discover that this disturbance of the box caused the least motion in the torsion rod, which still retained its stationary position. [...] Yet notwithstanding this torpid state of the torsion rod, if the slightest change of temperature be applied near the *side* of the torsion box, or if either *side* near the balls be sprinkled with a little spirit of wine, the torsion rod is immediately put in motion and the resting point undergoes a rapid change. (Baily 1843, pp. 30–31)

Baily noted that Cavendish' (and Reich's) experimental setup suffered from the unprotected state of the torsion box: "[i]n both cases the masses were brought up almost *close* to the outer side of this wooden shaft, but without actually touching it: but no mention is made of the application of any intervening substance to guard against a change of temperature on the approach of the masses" (Baily 1843, p. 35). In order to screen off this source of anomaly, Baily (1843, pp. 38–39) made sure that the surfaces of the masses could not approach the torsion box nearer than about an inch, "conceiving that this increased distance would be a sufficient protection," and gilded the masses "for the purpose of preventing the effect of [heat] radiation, from whatever source it might arise" (Baily 1843, p. 41). In a period of 18 months, Baily performed nearly 1,300 experiments. Although many of them were made with a view to discover the anomalies caused by differences in temperature, some 1,000 experiments were

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50 At this point, Baily inserted the following footnote: "On one or two occasions, when a very fine wire and the heaviest balls I have used, I have noticed a slight *trembling* of the torsion rod, when a loaded wagon has been passing the house. But although this motion was closely and designedly watched, I never could discover the least *angular* deviation in the torsion rod. And it may be proper to add that no trembling agitation of this kind took place when violent storms and gusts of wind blew against the house: nor have I since observed the occurrence here alluded to."

used to determine the mean density of the earth. Baily (1843, p. 44), nevertheless, admitted that discordances occasionally occur, "which cannot wholly be attributed to change of temperature, but to some other occult influence with which we are at present unacquainted."

Baily's procedure for determining the times of vibrations was more accurate than the one put to use by Cavendish: whereas Cavendish was contented with determining the time of a vibration for a whole series of changes in the positions of the masses for a single experiment—thereby assuming that the times of vibration are constant, Baily (1843, pp. 50, 51–56) determined the time of vibration for every change of position of the masses in an improved way.[51] He used a similar procedure for determining the resting points (Baily 1843, p. 52). Baily also computed the probable error (E) of the performed experiments by means of the formula $E = \frac{\sqrt{S}}{n} \times 0.674489$ (where S is the sum of the squares of all single experiments minus the square of the general mean result of the whole multiplied by the number of single experiments (n)) (Baily 1843, pp. 84–85).[52] Baily's method of determining the earth's mean density was totally different from Cavendish'. His computations were derived from a series of analytic formulae provided by the Astronomer Royal, George Biddell Airy (1801–1892)[53] (Baily 1843, pp. 99–111), which assumed that the weights involved occur in vacuo.[54] Basically, Baily-Airy provided a more complex equation for Cavendish' $D :: \frac{N^2}{B}$ proportion,[55], namely: $D = \frac{3}{4} \cdot \pi \times \frac{L}{G \times R \times \left(1 + M - \left(\left(\frac{5M}{2} - \varepsilon\right) \times \cos^2 \mathrm{lat.}\right)\right)} \times \frac{i}{h} \times \frac{E}{F} \times \frac{N^2}{B}$, where

[51] See especially: "CAVENDISH always took the *second mean* of the extreme points as the true position of the resting point: and always compared his *last* true resting point in one experiment, with the *first* true resting point of the next succeeding experiment, for the purpose of determining the deviation [...]. [...] For CAVENDISH always continued the motion of the torsion rod for an indefinite period after the determination of the resting point for the deviation, and deduced the mean time of vibration from observations made at the beginning and end of that period: not perhaps bearing in mind that, during that period, the time of vibration might be (as, indeed, it often is) subject to change. Whereas, on the contrary, the author always considered the true time to be that which occurs during the motion of the very vibrations that are employed for determining the resting points; having had frequent experience of sudden changes in the time of vibration, without any apparent cause: which changes, though perhaps not always very great, might sometimes sensibly affect the results, of not carefully attended to" (Baily 1843, pp. 55–56).

[52] The introduction of experimental error was a typical nineteenth-century development (Home 2003, p. 373).

[53] On Airy, see *DSB*, I, pp. 84–87.

[54] Baily computed the weight in vacuo (W_v) on the basis of the weights as measured in air (W_a) with the formula $W_v = W_a \times \left[1 + \left(\frac{\text{specific gravity of air}}{\text{specific gravity of the body}}\right)\right]$ (Baily 1843, p. 113). In a theoretical article published in 1840, Luigi Federico Menabrea (1809–1896) provided a series of analytic formula on the basis of which the density of the earth could be computed. Cf.: Menabrea (1840, p. 312): "C'est pour cela que je me suis proposé, non point de refaire les calculs numériques de l'auteur, ce qui ne présenterait pas d'intérêt, d'autant plus qu'il s'agit en Angleterre de procéder à de nouvelles expériences de ce genre, mais de reprendre cet intéressant problème de physique sous le point de vue purement analytique, pour déduire des équations primitives du mouvement, les formules qui servent à déterminer la densité de la terre." Menabrea derived analytic formulae which took the resistance or the air and the spherical shape of the earth into account. On Menabrea, see *DSB*, IX, pp. 267–268.

[55] Gosselin (Gosselin 1859, pp. 482–485) derived a variant formula which expressed the mean density of the earth relative to the density of water (D), namely: $D = \frac{0.00025 \times 766 \times T^2}{1{,}000n}$, where T is the time of vibration and n is the motion of the arms. In deriving this formula, Gosselin relied on a value for the earth's

the overall constant equals $\frac{3}{4} \cdot \pi \times \frac{L}{G\times R\times\left(1+M-\left(\left(\frac{5M}{2}-\varepsilon\right)\times\cos^2\text{lat.}\right)\right)} \times \frac{i}{h} \times \frac{E}{F}$ (Baily 1843, p. 54, 117). The second part of the constant, $\frac{L}{G\times R\times\left(1+M-\left(\left(\frac{5M}{2}-\varepsilon\right)\times\cos^2\text{lat.}\right)\right)}$, where L is the length of a seconds pendulum (at the latitude at which the experiments were performed) in inches, G is the number of grains in one cubic inch of water, R is the earth's polar axis in inches, M is the proportion of the centrifugal force at the equator to the gravity there, i.e. nearly 1/289, ε is the earth's ellipticity, i.e. nearly 1/300, and lat. is the latitude of the place at which the experiments are performed, depended only on the dimensions of the earth and the second pendulum and it could therefore be considered as being constant (Baily 1843, pp. 110–111). The third part, $\frac{i}{h}$, where i is the distance of the index from the centre of the small ball, i.e. 11 in.,[56] and h is the value for one of the divisions of the index' scale, i.e. $\frac{1}{26}$, depended on the dimensions of the apparatus only and it could likewise be considered as constant. The first three parts give rise to a general constant (Baily 1843, pp. 117–118). The fourth part of the constant, $\frac{E}{F}$, where E is the moment of the force of attraction and F is the moment of inertia, constituted a special constant which depends on the weight and diameter of the small balls and on the weight and length of the torsion rod employed, respectively (Baily 1843, pp. 118–120). The final part of the general formula, $\frac{N^2}{B}$, is to be established by experiment.

The mean result of this large body of experiments gave a value of 5.6747 for the mean density of the earth with a probable error of 0.0038 (Baily 1843, ccxlvii). As we have seen, Baily improved upon Cavendish' procedures of eliminating external sources of disturbances. More specifically, Baily ensured the stability of the mahogany box and the suspension lines, added an octagonal frame to protect the apparatus from variations in temperature, used balls of different materials, and experimented with both single and double suspension wires. He also computed the times of vibration and the resting points in a more accurate way and based his conclusion on a very large body of data. In addition, he introduced a more complex approximation for Cavendish' $\frac{N^2}{B}$ expression and determined the margin of error of the obtained experimental result. Despite these improvements, Baily pointed out that occasionally unexplained discrepancies occurred which he ascribed to an "occult influence with which we are at present unacquainted."

Accordingly, in a series of articles published in the 1870s, Marie-Alfred Cornu (1841–1902), who was Professor of Experimental Physics at the *École Polytechnique* in Paris, and Jean-Baptistin Baille (1841–1918), who was Professor of Optics and Acoustics at the *École de Physique et de Chimie Industrielles* in Paris and a close

Footnote 55 continued
surface gravity g, i.e. "la force accélératrice [de la terre] [...] qui sollicite cette mass m' située à la surface du globe terrestre," of 9.809 m/s^2 (Gosselin 1859, pp. 475–476). Gosselin's formula gave slightly higher values for D than the one obtained by Cavendish. On the basis of the above formula, he determined that the mean density of the earth equals 5.69 (actually: 5.6825) (Gosselin 1859, p. 485).

56 However, Baily (1843, pp. 119–120) notes that when δ, the observed distance, is not exactly equal to 11 in. the distance should be corrected by $\frac{\delta^2}{11^2}$.

friend of Paul Cézanne and Émile Zola, provided a correction to Baily's results.[57] They pointed out that Baily did not sufficiently take into account a systematic error caused by the inversion of the attracting weights on their pivot,[58] which caused some minute trepidations.[59] Moreover, Baily himself never succeeded in accounting for the decrease of the value for the mean density of the earth whenever heavier balls were used. Cornu and Baille noted that Baily's assumption, that the last elongation for one position of the masses may be taken as the first for the succeeding position, was the cause of this. As a result of this, the centres of swing are too high in the negative position and too low in the positive one (Poynting 1894a, pp. 55–56, 1894b). Once the required correction was applied to Baily's result, Cornu and Baille obtained 5.55 as the corrected value for the mean density of the earth (Cornu 1878, pp. 701–702).[60]

A post-Cavendish pendulum experiment: 1852

In 1852 Ferdinand Reich (1799–1882), who was Professor of Physics at the *Bergakademie Freiburg*, published the results of a series of experiments he had performed to determine the mean density of the earth. The method of processing the observations was similar to that of Baily (Reich 1852, p. 390). However, the apparatus Reich used was entirely different (see Fig. 5) (Reich 1852, pp. 392–394). Reich used a single tin mass b suspended along a silvered copper wire of 2.270 m. In order to prevent any air circulation, the space between the moveable axis of suspension, which pivots on an endless screw, and the upper part of the surrounding case CD was filled with a pouch with cotton lining. The lower part of the case was cylindrical and surrounded by a horizontal turntable, which could be turned 180° and on which the mass A is placed. Reich established a value of 5.5832 ± 0.0149 for the mean density of the earth (Reich 1852, p. 418). In order to determine the degree of influence of (dia)magnetism, he experimented with a diamagnetic attracted mass of bismuth and an attracted iron mass, which gave the following values for the mean density of the earth, respectively: 5.5333 ± 0.0403 and 5.6887 ± 0.0312 (Reich 1852, p. 422, 425). Rather than making any rash conclusions on the basis of this, Reich insisted on future research on the

[57] The opening sentence of their article goes as follows: "Depuis la découverte de la loi de l'attraction universelle par Newton, un problème expérimental d'une grand importance s'est naturellement posé aux astronomes et aux physiciens, à savoir: la détermination de la valeur numérique de la constante qui exprime l'attraction réciproque de deux unités de masses placées a unité de distance" (Cornu and Baille 1873, p. 954). Although they clarify the relation between G and the density of the earth (Δ)—by supposing a spherical approximation of the earth—and provide a corrected value for Δ, no exact determination of G was provided. On Cornu, see *DSB*, III, pp. 419–420.

[58] See: "Ces études nous ont conduits à reconnaître dans les expériences de nos devanciers des causes d'erreurs systématiques, qui, dans les observations de Baily en particulier, ont eu une influence très-marquée." (Cornu and Baille 1873, p. 700).

[59] See: "lors de l'inversion des masses très-pesantes (160 kilogrammes) et leur arrive sur les buttoirs, il se produit inévitablement des trépidations et des chocs qui se transmettent partiellement au levier par l'intermédiaire de l'air ou la suspension: il s'introduit donc de nouvelles forces" (Cornu 1878, p. 701).

[60] Details on their calculations are lacking. For a brief description of the main features of the experimental setup, see Cornu (1891).

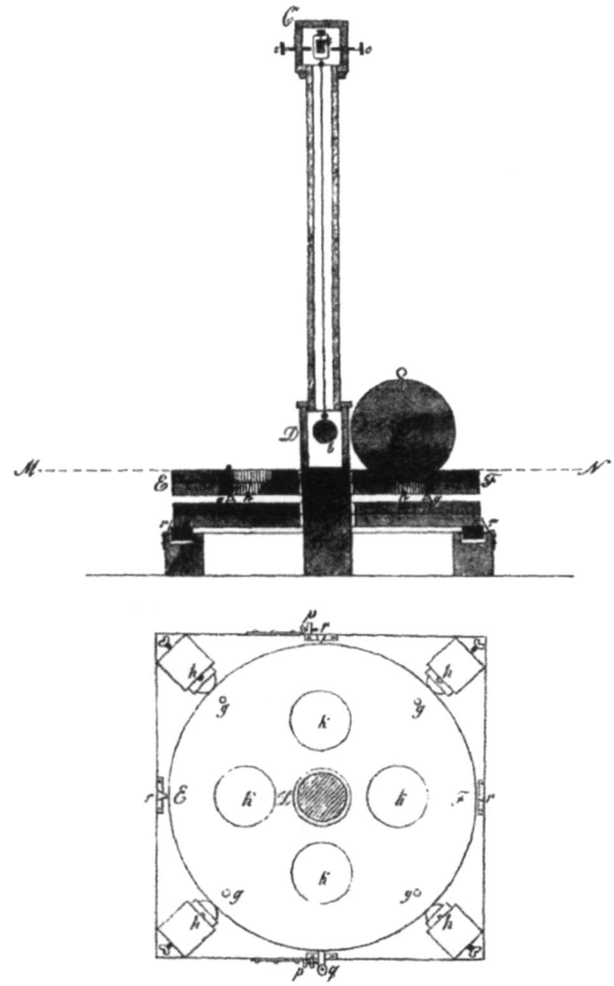

Fig. 5 Reich's experimental apparatus. Taken from Reich (1852, p. 394)

matter.[61] The significance of Reich's experiment lies in the fact that it corroborated Cavendish' result by using a different experimental apparatus. As we shall see in the following subsection, not all experimental setups were adequate to determine the mean density of the earth.

61 In fact, he wrote: "Jedenfalls dürfte sich aber ergeben, dass zwischen der Bleimasse und der Zinnkugel störende magnetische oder diamagnetische Wirkungen nicht anzunehmen sind" (Reich 1852, p. 426). On probability and experimental error in early nineteenth-century physical science, see: Olesko (1995, esp. pp. 105–117).

Post-Cavendish pendulum experiments near and below the surface of the earth: 1856–1885

The aim of performing pendulum experiments above and below the earth's surface is to infer the earth's mean density on the basis of the observed difference in the rates of two invariable pendulums, one at the top and the other at the bottom of a deep shaft, which were set to swing simultaneously. In order to do so, G.B. Airy[62] established the following formula for determining the mean density of the earth: $\frac{D}{d} = \frac{\frac{3c}{R}}{\left(1+\frac{2c}{R}+\frac{G}{g}\right)}$, where D is the mean density of the earth's sphere,[63] R is the radius of the earth's sphere, d is the mean density of the outer spherical shell with a thickness c (equal to the depth of the mine) which surrounds the earth's sphere, G is the pendulum's gravity at the bottom of the mine, and g is the pendulum's gravity above the mine (Airy 1856, p. 297).[64] As c, R and d—Airy took to latter to be 2—are known, we only have to establish $\frac{G}{g}$, which Airy computed to be $1.00005185 \pm 0.00000019$, in order to determine D (Airy 1856, p. 330). By filling in the parts of the equation, it follows that $D = 6.566 \pm 0.0182$ (Airy 1856, p. 342). The Achilles' heel of Airy's result lied in the uncertain value for the mean density of the outer spherical shell, d—moreover, his result was dependent on both the accuracy of the pendulums involved and the ability of the observers involved to make observations with considerable precision. In the same article, to which we have already referred previously, Jacob pointed out that "Mr. Airy has indeed shown that, in the case of his experiment, it is sufficient if we know, as regards the upper shell, the form and density of that portion which is in the immediate neighbourhood of the place of observation, without attending to irregularities of distant parts; but he has *not* shown that variations of density *below* and *near* to his lowest station would not sensibly vitiate his results" (Jacob 1857, p. 296 [underscore added]).[65]

The aim of the experiment performed by Robert (Daublebsky) von Sterneck (1839–1910),[66] at the Adelbert shaft in Přibram (Böhmen) in 1883 was identical to Airy's experiments, namely "aus den Unterschieden der Schwingungszeiten eines Pendels auf der Erdoberfläche und in verschiedenen Tiefen unter derselben die Äderungen der Schwere im Innern der Erde mit zunehmender Tiefe zu bestimmen" (Sterneck 1883, p. 59).[67] The motion of the two pendulums was simultaneously observed in comparison to the same clock which was installed at the surface of the shaft and which was connected by means of an electrical circuit to the two clocks accompanying each of

62 On Airy, see *DSB*, I, pp. 84–87 and *NDSB*, I, pp. 24–26.

63 Airy's approximation supposed that the earth's figure was that of sphere and that it consisted of homogeneously concentric spherical shells.

64 Furthermore, the error (δ) in D caused by an error in G is given as $\frac{\delta D}{D} = \frac{\delta G}{G} \times \frac{D}{d} \times \frac{R}{3c}$.

65 Compare with Poynting (1894a, p. 621, 1894b): "All to the methods which I have so far described use, as you will have noticed, natural masses to compare the earth with, and herein lies a fatal defect as regards exactness. We do not know accurately the density of these masses and what is the condition of the surrounding and underlying strata. We can really only from at best rough guesses."

66 Sterneck was Major-General at the Militär-geographische Institut in Vienna.

67 See Brillouin (1895) for useful discussion and additional illustrations.

the pendulums (Sterneck 1883, pp. 68–69). The accuracy implied thereby was a major advance in comparison to Airy's method. In order to compute the mean density of the earth, Sterneck used the same formula Airy had previously relied on—Sterneck (1883, p. 90) estimated the value of d to be 2.75. When filling in the formula Sterneck arrived at the following determinations of D, i.e. the mean density of the earth: 5.71 at 516 m, 5.81 at 747.9 m, and 5.80 at 972.5 m below the surface, which gives a mean of ca. 5.77 (Sterneck 1883, p. 89, p. 91).[68] Two year later, Sterneck performed similar experiments at the Abraham shaft in Freiberg. On the basis of the experiments collected there, he computed the following values of D: 5.66 at 97.42 m, 6.66 at 257.04 m, 7.15 at 414.20 m, and 7.60 at 534.08 m below the surface (Sterneck 1883, pp. 113–114).[69] Despite the improvements Sterneck made in comparison to Airy, his result was equally liable to error caused by the uncertain value for the mean density of the outer spherical shell. In this context, Poynting adequately pointed out that "[t]he true value of experiments on gravity below the earth's surface would appear to lie not in their use to determine the mean density of the earth, but rather in their indication, by anomalies, of irregularities in density in the region round the place of the experiment" (Poynting 1894a, p. 38, 1894b; cf. Radau 1887, p. 237). In other words, the method followed by Airy and von Sterneck introduced a considerable uncertainty with respect to the computation of the attractive forces. Cavendish' method did not suffer from this problem—however, due to the sensitivity of the apparatus successful ways to eliminate disturbing forces had to be sought for.[70]

Post-Cavendish experiments with the common balance: 1881

The first detailed common balance experiment seems to have been performed by Philipp J. G. von Jolly. In 1881 Jolly (1809–1884)[71] had devised an experiment using a common balance by which he sought to measure the increase in weight with decrease

68 At the end of his article, Sterneck (Sterneck 1883, pp. 91–92) hypothesized that the mean density of the outer shell varies as the distance only.

69 Based on the computed values for D and the temperature measurements he had simultaneously performed, Sterneck (Sterneck 1883, pp. 117–119) suggested that the increase of gravity with increasing depth is nearly proportional to the increase of temperature. Poynting, however, correctly noted that we should ascribe the increase of gravity with depth to underground variations in density. "In our ignorance of the conditions below the lowest level yet reached in mining, there is no difficulty in accepting the explanation," he commented (Poynting 1894a, p. 36, 1894b).

70 Wilsing wrote: "Darum bezeichnet die Coulomb'sche Erfinding der Torsionswage und ihre Anwendung auf das vorliegende Problem durch Cavendish einen erheblichen Fortschritt zur exacten Lösung des Problems. Bei der grossen Empfindlichkeit dieses instruments konnte nämlich die Anziehung wenige Centner schwerer Kugeln, derer Masse sich ohne Schwierigkeit durch directe Wägung bestimmen liess, messbar nachgewezen werden, und damit trat die Dichtigkeitbestimmung der Erde in den Kreis der Laboratoriumsversuche ein. Allein die grosse Empfindlichkeit der Torsionswage, durch welche die Unsicherheit in der Berechnung der anziehenden Kräfte beseitigt wird, hat Uebelstände anderer Art zur Folge, deren zuerst Francis Bailey Erwägnung thut, welcher sich mit Rücksicht auf die vorliegende Aufgabe eingehend mit dem Studium der Torsionswage beschäftigt hat" (Wilsing 1887, p. 35).

71 On Jolly, see *DSB*, VII, p. 160 and Soffel (2009). Jolly was Professor of Mathematics and later Professor of Physics at the University of Heidelberg. Later in life he moved to the University of Munich, where one of his students was the young Max Planck.

of the distance from the earth's surface. He used a balance in which in each of the two pans two identical 5 kg spheres, which were filled with mercury, were placed. Two additional scale pans, in each of which an air-filled sphere of equal volume to that of the mercury-filled spheres was put, were suspended to each of the pans above (for the details see Jolly 1881, pp. 332–333). Next, on one side of the balance, the mercury-filled sphere and the air-filled sphere were interchanged (Jolly 1881, p. 334). Due to the increase of the weight caused by the decrease of the distance from the earth's surface, the balance was no longer in equilibrium and small weights had to be added on the other side of the balance to restore the equilibrium. In addition, a large lead sphere with a radius of 0.4975 m and a weight of 5,775.5 kg was placed below the interchanged spheres and the procedure was repeated. Jolly calculated that increase of weight caused by the massive lead sphere was 0.589 mg (Jolly 1881, p. 350). On the basis of the formula $\rho = \frac{r^3\delta}{Ra^2} \times \frac{Q}{q}$, Jolly established that the mean density of the earth, ρ, is equal to 5.692 ± 0.068, where r is the radius of the lead sphere, i.e. 0.4975 m, δ is the mean specific weight of the lead sphere, i.e. 11.186, R is the distance from the centre of the mercury-filled sphere and the centre of the earth, i.e. 6,365,722 m, a is the distance from the centre of the mercury-filled sphere to the centre of the lead sphere, i.e. 0.5686 m, q is the observed increase of weight, i.e. 0.589 mg, and Q is the weight of the mercury-filled sphere, i.e. 5,009,450 mg (Jolly 1881, pp. 350–351).

Coda

The up-shut of the nineteenth-century research on the mean density of the earth, which we have surveyed so far, was that several independent and relatively reliable determinations of the mean density of the earth—of varying degrees of accuracy and precision[72]—provided measurements which were quite close to one other (see Table 1, for a summary of the results which the author has discussed in this section). The implication of the Cavendish experiment, i.e. that robust gravitational interactions occur between terrestrial bodies, was confirmed independently of Cavendish' original experiment. Moreover, as experimenters had become increasingly skilled in eliminating external disturbances, the evidential support for the claim that the law of universal gravitation holds at smaller distances than celestial ones had become increasingly stronger.[73]

72 Terminological clarification: "accuracy" refers to the closeness of an experimental result to the true value; "precision" to the fineness of the scale involved. The percentage by which these experimental results differ from the present-day value are: ca. 1.21% (Cavendish), ca. 2.90% (Baily), ca. 1.24% (Reich), ca. 19.06% (Airy), ca. 0.63% (Cornu and Baille), ca. 3.21% (Jolly), and ca. 1.16% (Wilsing) (see Table 1). Baily's and Reich's results are the most precise, whilst Cornu's and Baille's are the least precise.

73 Near the end of the nineteenth century, a new method to determine the mean density of the earth was devised. In 1887 and 1889 Johannes Wilsing published two accompanying studies on the determination of the density of the earth by means of a vertical pendulum balance, which were both appeared in *Publicationen des Astro-Physikalischen Observatoriums zu Potsdam* (on Wilsing, see *DSB*, IV, p. 414). The use of a brass vertical pendulum with a length of 1 m was a notable feature of the apparatus involved. The idea was that a vertical pendulum balance would provide more reliable results than a horizontal pendulum balance, insofar as it could be considered as a rigid system and insofar as it is protected from bending by its (vertical) position (Wilsing 1887, p. 36). By the choice of materials magnetic effects were guarded against. Precautions were

Table 1 Summary of the determinations of the mean density of the earth

Cavendish	1798	5.448
Baily	1843	5.6747 ± 0.0038
Reich	1852	5.5832 ± 0.0149
Airy	1856	6.566 ± 0.0182
Cornu and Baille	1873	5.55
Jolly	1881	5.692 ± 0.068
Wilsing	1889	5.579 ± 0.012

The present-day value for the mean density of the earth is ca. 5.515

4 The *terminus ad quem*: research on big G, 1892–1898, or "working for the Universe"

Near the end of the nineteenth century, scientists continued to work on the mean density of the earth. However, by then the research focus had shifted: determining G had became the centre of scientific attention—an interest which continues to this very day.[74] The determination of the mean density of the earth was from then on seen as a corollary to the determination of big G. On this matter, Poynting recorded that although the scientific articles, which will be discussed in the section at hand, provided a determination of the mean density of the earth, "they have *a more general aspect* and may be regarded as determining the exact expression of Newton's Law of Gravitation" (Poynting 1913, p. 84 [italics added]). In the same context, he remarked, in an address to The Royal Institution of Great Britain, entitled 'Recent Studies in Gravitation' (1900), that "Professor Boys has almost indignantly disclaimed that he was engaged on any *such purely local experiment as the determination of the mean density of the earth. He was working for the Universe, seeking the value of G, information which would be as useful on Mars or Jupiter or out in the stellar system as here on the earth*" (Poynting 1920, p. 633 [italics added]).

By adding G, which indicates the strength of gravitation, to Newton's original (proportional) formulation of the law of universal gravitation a major advantage was created: gravitational forces could be determined and formulated in absolute terms, i.e. in terms of standard units. As a consequence, masses and densities could from

Footnote 73 continued
also made to minimize changes in temperature and air flow. The pendulum, to which at each end two brass balls—weighing 533.93 and 545.10 g (Wilsing 1887, p. 59)—were attached, was strengthened near its middle by means of a frame, inside of which a non-sharp agate knife-edge—which is above and very near to the centre of gravity of the pendulum—rests on a concave agate bearing (Wilsing 1887, p. 37). Two iron cylinders, weighing 325 kg each, served as the attracting masses (Wilsing 1887, p. 39). The masses were installed at opposite sides of the pendulum balance in such a way so that the centres of each cylinder were on the same horizontal plane as the centres of their corresponding balls. In order to compute the mean density of the earth, the double deflections, caused by lowering the upper mass and simultaneously raising the lower mass, and the times of vibration were required. Wilsing obtained a value for the mean density of the earth of 5.594 ± 0.032 (Wilsing 1887, p. 85). In his second study, in which he had paid extra attention to possible changes in temperature, Wilsing corrected the value for the density of the earth to 5.579 ± 0.012 (Wilsing 1889, p. 141).

[74] On the role of G in contemporary theoretical physics, see Damour (1999).

then on be calculated in absolute terms. These advantages were, however, *a by-product of the fact that G was empirically shown to be relatively stable*. Late nineteenth century experimental physics had succeeded in empirically establishing the value of the strength of gravitational interaction, which was inferred from experimental setups which involved terrestrial bodies.

Poynting

In 1892 John Henry Poynting's (1852–1914)[75] article on the determination of the mean density of the earth and the gravitational constant appeared in the *Philosophical Transactions of the Royal Society of London A*.[76] In its introduction, Poynting wrote:

> It might appear useless to add another to the list of determinations, especially when, as Mr. Boys has recently shown, the torsion balance may be used for the experiment with an accuracy quite unattainable by the common balance. *But I think that in the case of such a constant as that of gravitation, where the results have hardly as yet begun to close in on any definite value, and where, indeed, we are hardly assured of the constancy itself, it is important to have as many determinations as possible made by different methods and different instruments, until all the sources of discrepancy are traced and the results agree*. (Poynting 1892, p. 565 [italics added]).

In his article, Poynting set out to experimentally determine "the attraction of one known mass M on another known mass M' a known distance d away from it,"[77] i.e. to determine G (Poynting 1892, p. 566).[78] He added that "[t]he law of universal gravitation states that when the masses are spheres with centres d apart this attraction is GMM'/d^2, G being a constant—the gravitation constant—the same for all masses." "Astronomical observation fully justify the law as far as M'/d^2 is concerned," these "do not, however, give the value of G, but only that of the product GM for various members of the solar system" (Poynting 1892, p. 566).[79] Once G is known, the earth's mean density is easily derivable: the attraction of the earth—approximated as a sphere—on

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[75] For an account of Poynting's person and work, see: Poynting (1920, vii–xxvi). See, furthermore, *DSB*, XI, pp. 122–123.

[76] Poynting's endeavour to use the common balance to measure the density of the earth dates back to more than a decade before the publication of his 1892 article (see Poynting 1920, pp. 7–42 and Falconer 1999b).

[77] Poynting (Poynting 1892, p. 566) referred to this method as the "'Prepared Mass' method."

[78] Poynting used the following metaphor to describe the aim of the experiment: "Imagine a balance large enough to contain on one pan the whole population of the British Islands, and that all the population were placed there but one medium-sized boy. Then the increase in weight which had to be measured was equivalent to measuring the increase due to putting that boy on with the rest. The accuracy of measurement was equivalent to observing from the increase in weight, whether or no he had taken off one of his boots before stepping on the pan" (Poynting 1894a, p. 626, 1894b).

[79] In 1900 Poynting (1920, p. 630) wrote: "If [...] we compare the accelerations due to different pulling bodies, as for instance that of the sun pulling the earth, with that of the earth pulling the moon, or if we compare changes in motion due to the different planets pulling each other, we can compare their masses and weigh them, one against the other and each against the sun. But in this weighing our standard is not the pound of kilogramme of terrestrial weighings, but the mass of the sun."

any mass M' is given as $\frac{GV\Delta M'}{R^2}$—where V is the volume of the earth, Δ is the mean density of the earth, and R is the radius of the sphere of the earth—which equals $M'g$, where g is the accelerative force of the earth. Therefore, $\Delta = \frac{gR^2}{GV}$. The apparatus used in Poynting's experiment, which was installed in a basement room at Mason College in Birmingham,[80] is depicted on Fig. 6. During the experiment, air currents and variations in temperature and air pressure were avoided as far as possible.[81] All weights were made of an alloy of lead and antimony (Poynting 1892, p. 578). Two nearly equal masses A and B were suspended from a balance. The weight of A equals 21,582.33 g and that of B equals 21,566.21 g (Poynting 1892, p. 579). A and B are furthermore placed within a wooden case. Mass M, weighing 153,407.26 g, is placed underneath A. Once we have observed the change in position of the beam, which has a length of 1.23329 m (Poynting 1892, p. 571) we turn M 180° degrees so that M is now underneath B. Once M has switched sides, we observe the position of the beam once again. In this configuration, the attraction is taken away from A and added to B. In order to eliminate the attraction of M on the beam and the suspending wires, we raise A and B to the equally higher positions A' and B', "[f]or the *difference* between the two increments of weight on the right, is due solely to the alteration of the positions of A and B relative to M, the attraction on the beam remaining the same in each" (Poynting 1892, p. 567).[82] In order to compensate for the tilting of the floor which arose when M was moved, an additional mass m, which is nearly half as big as M, i.e. 76,497.4 g, was installed twice as far from the axis and on the opposite side of M (Poynting 1892, pp. 567–568, 579). Due to the addition of m, the "resultant pressure was now always through the axis" and no "tilting of the floor when the turntable was moved" could be detected (Poynting 1892, p. 569). Both M and m were steadily placed on a turntable, which could be manipulated in the room above the basement. A scale was fixed horizontally to the end of the telescope by means of which the subsidiary riders attached to the centre of the balance beam could be monitored, and hence the tilt of the beam.

"Assuming that a spherical mass M attracts another spherical mass M' when their centres are D centimeters apart with a force of GMM'/D^2 dynes," Poynting (Poynting 1892, p. 603) stated, "we can express the change of vertical pull due to the change of position of the masses as $G\times$ a function F of the masses and distances." In addition of M's pull on the weights A and B, there is a (vertical) pull, E, exerted on the beam and the suspending rods. Assume that M produces a vertical pull of n dynes.[83] In this case, $n = GF + E$. When the weights A and B are raised to positions A' and B', they will be undergo a vertical pull of n' dynes. Let f be the function of the masses and the new distances corresponding to F, so that $n' = Gf + E$. As noted above, what we are interested in is the difference between the forces n and n'. From what is given,

80 It was first set up at the Cavendish Laboratory "through the kindness of Professor Clerk Maxwell" (Poynting 1892, p. 566, cf. p. 569).

81 For the detailed discussion of Poynting's (Poynting 1892, pp. 565–602) experimental setup, see Part I of his article.

82 These vertical displacements were measured by means of a standard cathetometer (Poynting 1892, pp. 588–591).

83 The "dyne" is an old unit of force; 1 dyne equals 10^{-5} N.

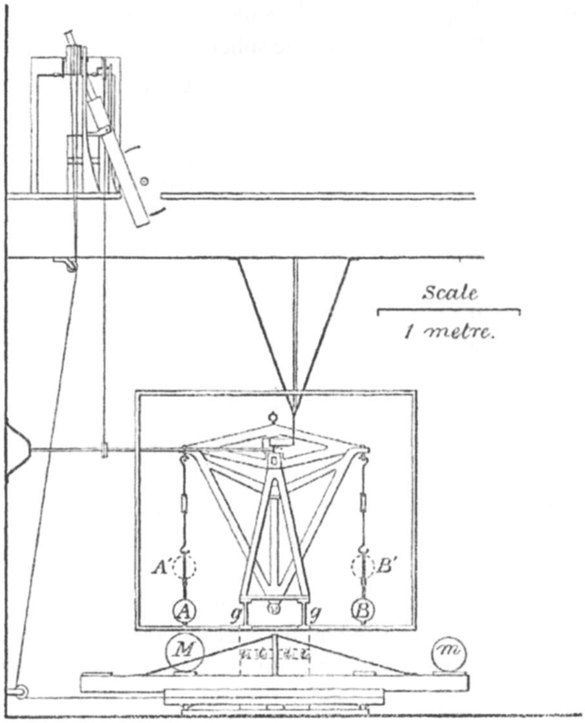

Fig. 6 Poynting's common balance. Taken from Poynting (1892, p. 568). Courtesy of The Royal Society

$n - n' = GF + E - (Gf + E) = G(F - f)$, so that E is eliminated. Given this formula, it follows that $G = \frac{n-n'}{F-f}$. $F - f$ can be expressed in terms of the masses A, B, M and m, the fixed distances, L and l, and the variable distances D, d, H and h (see Fig. 7), namely as $\frac{M(A+B)(1-\theta)}{D^2} - \frac{MD(A+B)}{(D^2+L^2)^{3/2}} - \frac{mH(A+B)}{\left(H^2+\left(l-\frac{L}{2}\right)^2\right)^{3/2}} + \frac{mH(A+B)}{\left(H^2+\left(l+\frac{L}{2}\right)^2\right)^{3/2}} - \frac{M(A+B)}{d^2} + \frac{Md(A+B)}{(d^2+L^2)^{3/2}} + \frac{mh(A+B)}{\left(h^2+\left(l-\frac{L}{2}\right)^2\right)^{3/2}} - \frac{mh(A+B)}{\left(h^2+\left(l+\frac{L}{2}\right)^2\right)^{3/2}}$ (Poynting 1892, p. 606).[84] Filling in this equation, Poynting established that $F - f = 4{,}826{,}997.2$ (Poynting 1892, p. 611). Furthermore, $n - n' = \frac{2bwgB(A-a)}{L}$ (Poynting 1892, p. 606), where b is the length of the small rider beam, w is the mass of each rider, A is the mass deflection divided by the rider deflection in the lower position, a is the mass deflec-

[84] $1-\theta$ is a correction factor to account for the holes drilled into the masses A and B, which is nearly equal to 1 (Poynting 1892, p. 604). On the status of scientific laws, Poynting recorded in his presidential address to the Mathematical and Physical Section of the British Association (1899): "If this is a true account of the nature of physical laws, they have, we must confess, greatly fallen off in dignity. No long time ago they were quite commonly described as the Fixed Laws of Nature, and were supposed sufficient in themselves to govern the universe. Now we can only assign to them the humble rank of mere descriptions, often tentative, often erroneous, of similarities which we believe we have observed" (Poynting 1920, p. 600, cf. pp. 686–698).

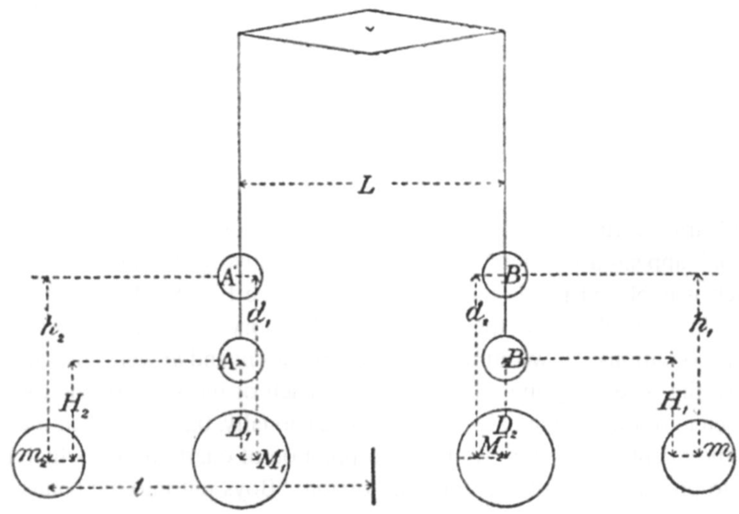

Fig. 7 Poynting's common balance. Taken from Poynting (1892, p. 602). Courtesy of the Royal Society

tion divided by the rider deflection in the lower position and g_B is the gravity at Birmingham. Having solved both $F - f$ and $n - n'$, Poynting was able to complete the equation $G = \frac{n-n'}{F-f}$, which was equal to 6.6984×10^{-8} (Poynting 1892, p. 612). Once G was established, the mean density, Δ, could be determined by the formula $\frac{a^2 L(F-f)}{2bwV\left(1+\frac{\varepsilon}{3}-\frac{3m}{2}+\left(\frac{5m}{2}-\varepsilon\right)\sin^2 52°28'-\frac{41}{10{,}000{,}000}(A-a)\right)}$, according to which Δ is equal to 5.4934 (Poynting 1892, p. 607). When looking back on the experiment a couple of years later, Poynting remarked:

> At last my long catalogue of experiments is brought to an end, or rather it is brought up to the present time, for such researches have no end. Each generation will try to add another decimal place to the result or find out the errors of its predecessors. And even now there are many workers in the field, indeed, there is almost an epidemic of earth-weighing (Poynting 1894a, p. 627)[85]

In his 1900 address to the Royal Institution of Great Britain, already referred to in a footnote, he concluded:

> So while the experiments to determine G are converging on the same value, the attempts to show that, under certain conditions, it may not be constant, have resulted so far in failure all along the line. No attack on gravitation has succeeded in showing that it is related to anything but the masses of the attracting bodies

[85] In 1900 Poynting would remark the following: "But gravitation still stands alone. The isolation which Faraday sought to break down is still complete. Yet the work I have been describing is not all failure. We at least know something in knowing what qualities gravitation does not possess, and when the time shall come for explanation all these laborious and, at first sight, useless experiments will take their place in the foundation on which that explanation will be built (Poynting 1920, p. 644).

and the attracted bodies. It appears to have no relation to physical or chemical condition of the acting masses or to the intervening medium. (Poynting 1920, p. 643)

Boys

In 1895 Charles Vernon Boys' (1855–1944)[86] article "On the Newtonian constant of Gravitation" appeared in print.[87] Boys' experiment involved a 0.9 in. mirror torsion rod, which was placed inside the central tube *T* (see Fig. 8). The experiment was performed in the vaults under the Clarendon Laboratory at Oxford University during favourable conditions.[88] Two attracted golden masses *m* and *m* were suspended—one 6 in. above the other—by fine quartz fibres on each of the sides of the torsion rod. The diameters of the golden balls were 0.2 (occasionally, 0.25) inch and the distance between their centres was 1 in. By the incorporation of quartz fibres, which have the property to produce a small and constant torsion, "Boys put into the hands of physicists a means of making torsion balances for the measurement of small forces far exceeding in delicacy and accuracy anything hitherto used" (Poynting 1913, p. 68). The torsion rod and the small masses attached to it had to be reduced in size so that the fine quartz fibres could carry their weights. This entailed that the apparatus suffered less from variations in temperature and air currents; on the other hand, the variables to be measured were rendered considerably minute. Boys argued that by reducing the size of the apparatus its sensibility could be increased. The two attracting lead masses *M* and *M* were hung from the two diametrically opposed tubular pillars *P* and *P*. Theses masses could be smoothly moved around *T* by turning the little wheel *D*, which by means of the action of the train of wheels *WWW* turned the lid *L*. The attracting masses were hung at the same level as their closest attracted mass. The edge of the flange was divided in degrees and could be read on the vernier *V* to 0.1°. The readings of the vernier were observed by the aid of a small telescope, which was installed at a distance. The diameters of the lead balls were 4.25 (occasionally, 2.25) inches and the distance between their centres was 4 or 6 in., depending the size of the balls being used. Boys computed that the maximum deflection of the attracted balls is produced when the lead balls are moved from their + to − position through an angle of 65° (Boys 1895, p. 46).

By filling in the formula $G = \frac{PS}{4QD}$, where G is the gravitational constant, P is the mean value of the observed deflections in scale divisions, S is the actual couple

[86] On Boys see, Strutt (1944).

[87] The author shall not go into the minute details of Boys' sophisticated apparatus, see Boys (1895, pp. 1–37) for the details or Boys (1889) for an early description of the apparatus. In its introductory words, Boys wrote: "The Cavendish experiment *for determining the constant of universal, from which the density of the earth may be calculated*, is so well known that there is no occasion to describe it" (Boys 1889, p. 253 [italics added]).

[88] Boys recorded: "The daytime, of course, is out of the question, owing to the rattling traffic in St. Giles', about a quarter of a mile away; and all nights except Sunday night the railway people are engaged making up trains and shunting, which is more continuous and disturbing to the steadiness of the ground than a passing train" (Boys 1895, p. 47).

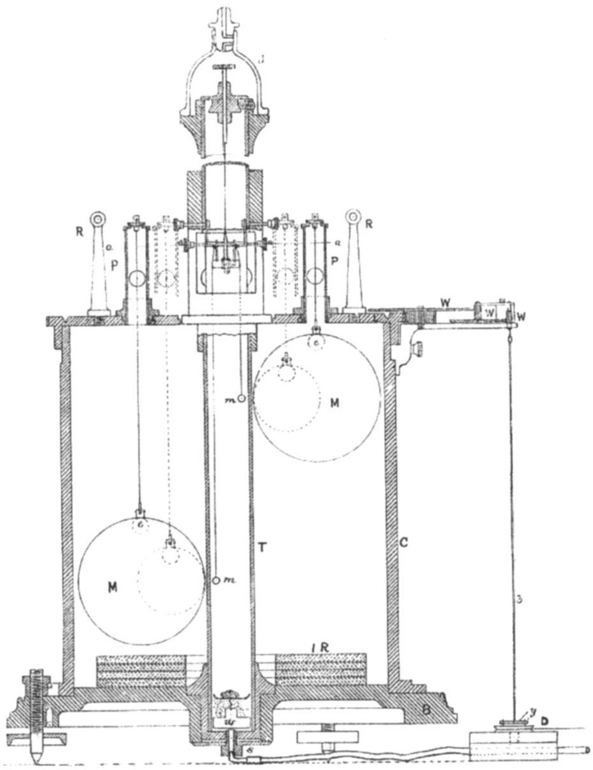

Fig. 8 Vertical section through Boys' apparatus. Detail taken from Boys (1895), Plate 1. Courtesy of The Royal Society

needed to twist the torsion fibre trough an angle of one unit (= 57.3°), Q is the numerical coefficient of G,[89] and D is the actual distance from the scale to the mirror in tenths of a scale division.[90] Completing the formula, gives $G = \frac{3695.4\times0.00119598}{4\times1942.882\times139{,}965} = 4.06312 \times 10^{-9}$ (Boys 1895, p. 62). When multiplied with the required factor for the conversion from cubic inches to cubic centimeters, 16.3861 (Boys 1895, p. 7), the value for G becomes 6.6579×10^{-8} and, correspondingly, we obtain a value of 5.5268 for the mean density of the earth (Boys 1895, p. 62).

89 When Q is multiplied by G, we obtain "the actual moment produced upon the torsion fibre by the action of the balls upon one another upon the supposition that the balls are all spheres, and act as if they were concentrated in their centres" (Boys 1895, pp. 58–59).

90 For Boys' derivations see Boys (1895, p. 53, 56 [for P], p. 35, 60 [for S], p. 59 [for Q] and pp. 17–18 [for D]).

Braun

Just as in the articles of Boys and Poynting, the primary focus of Carl Braun's (S.J., 1837–1907) 1896 article, in which he presented the results of the experiments he had begun in 1892, was on the determination of G.[91] A reviewer, Wadsworth, commented as follows on Braun's experiment:

> The work of Dr. Braun in this same field, which is fully described in the above memoir, is perhaps less elegant and finished than that of Professor Boys as regards some of the details of the design, construction, and manipulation of the apparatus, but, in view of the great length of time devoted to it, the variety of methods of observation employed, the careful consideration of all sources of error, and the painstaking means adopted to eliminate them as far as possible from the measurements; it must, I think, be admitted as worthy of ranking the work of the latter in point of accuracy, which is perhaps the highest praise that can be bestowed upon it. (Wadsworth 1897, p. 159)

In the corner of his room, Braun had attached a square tile to a wall. A ring was attached to the surface of the tile and a plate of glass, adequately cut, was placed inside the ring (see Fig. 9a, b). The brass torsion rod was hung from the top of a system of axial tubes by means of a brass suspension wire (see Braun 1896, pp. 189–192, for the details of the experimental setup). In the central tube, which is supported by a tripod, another tube was inserted, and in this tube a third tube was placed. The torsion arm, from which two gilded brass balls of an average weight of 54.2657 were suspended on equal heights at a distance of ca. 24.6 cm from each other, was triangular and consisted of copper wires of 1 and 2 mm. The whole apparatus was covered by a bell-jar within which a vacuum could be created. Around the bell-jar, two masses were suspended from another ring installed above. Two sets of balls were used: one set of brass masses—the first mass weighing 5.1590 kg, the second weighing 5.0905 kg; the other set of hollow globes filled with mercury—the first mass weighing 9.18475 kg, the second weighing 9.10757 kg. By the choice of materials, influences of the earth's magnetic field were negligible. In addition, the temperature was kept as constant as possible and because the created vacuum was so tight the pressure inside the glass cover remained constant. Braun studied two different types of effects: the motion of the torsion rod when the masses were at an equal height (horizontal or deflected movement of the rod) and the downward and upward motion of the torsion rod when

[91] See: "Von den drei Grössen 1° Gravitations-Constante (C), 2° Masse der Erde (M), 3° mittlere Dichte der Erde (D) ist die erstere in wissenschaftlicher Hinsicht die wichtigste, sofern sie die Constante für ein allgemeines Naturgesetz ist und wahrscheinlich im ganzen Universum Geltung hat. [...] *Die dritte Grösse (D) ist noch mehr von diesen Quantitäten abhängig und ist eigentlich von geringere Wichtigkeit*" (Braun 1896, p. 188 [italics added]). Braun explicated the relation between C, M and D, as follows. The earth's mass equals the product of its volume and density ($M_e = V_e D_e$). Furthermore, the volume of an oblate spheroid is given by $\frac{4\pi a^2 b}{3}$, where a is the equatorial radius and b is the polar radius. The accelerative force at the surface of the earth—approximated by an oblong spheroid—at latitude φ, $g_{(\varphi)}$, equals $\frac{MC}{\rho^2(\varphi)}$, where $\rho_{(\varphi)}$ is the radius at latitude φ, and C is the gravitational constant. Therefore: $C D_e = \frac{g(\varphi)\rho^2(\varphi)}{v}$ (hence: C is given by $\frac{g(\varphi)\rho^2(\varphi)}{VDe}$ and D_e is given by $\frac{g(\varphi)\rho^2(\varphi)}{VC}$) (Braun 1896, pp. 188–189).

the ring, from which the masses were hung, was turned sideways in an oblique angle (vertical or oscillatory movement of the rod). Both effects were observed in a separate series of experiments and both were treated by two different methods: a deflection method ("Deflexionsmethode"), which was inspired by Cavendish, and an oscillation method ("Oscillationsmethode"), which was inspired by Reich (Braun 1896, pp. 201–205, 205–211).[92] By determining the actual torque produced by the masses, which could be computed from the moment of inertia and the times of swing of the balls, Braun had two independent routes to calculate the gravitational constant, C (and, consequently, the mean density of the earth, D), at his disposal (Braun 1896, p. 201, 241, 253, respectively).[93] By combining the results established by both methods, Braun (Braun 1896, p. 258*c*) concluded that $D_e = 5.52700 \pm \text{ca.}\, 0.0014$ and that $C = 6.65816 \pm 0.00168 \times 10^{-8}$.[94] This result matched quite well with the value for C which Boys' had established: 6.6579×10^{-8}.[95] To give an idea of the smallness of the forces involved: the mean deflective attraction on each of the balls was 0.00031 dynes (or 0.00031×10^{-5} N) and the mean oscillatory attraction on each of the balls

[92] See, furthermore, Braun (1896, pp. 211–221, 221–226) for the correction factors which Braun introduced for both methods.

[93] Braun (1896, p. 189) compared the actual deflection produced with a theoretically derived value for the deflection, which assumed an initial value for C. The aim of the article was to determine experimentally how much the actual deflection is and, on the basis of this, Braun sought to establish by how much the initial value for C needed to be corrected. Cf.: "Das Princip dieser Methode is nun sehr einfach. Ist die Zinkscheibe mit den daran hangenden Massen M um einen Winkel c gedreht, so kann die Torsionskraft berechnet werden, welche durch die Anziehung der massen M gegen die Kugeln m hervorgebracht wird. Und da die Torsivkraft des Drahtes aus dem Trägheitsmoment unde der Schwingungszeit berechnet ist [...], so kann auch die Ablenkung berechnet werden, welche durch jene [...] Stellung der Massen bewirkt werden muss, sofern die vorausgesetzte Gravitations-Constante C richtig ist. Aus den Beobachtungen anderseits ergibt sich in der oben [...] beschriebenden Weise, wie gross die wirklich bewirkte Ablenkung ist. Aus dem kleinen Unterschied swischen diesen beiden Wirkungen ergibt sich dann leicht, um wie viel jenes C corrigirt werden muss, um das wahre C zu erlangen, und damit auch D" (Braun 1896, p. 201).

[94] In the same year Ronald (Loránd) von Eötvös' article 'Untersuchungen über Gravitation und Erdmagnetismus' appeared in print, which contained a section on the gravitational constant and the density of the earth (Eötvös 1896, pp. 385–392). Eötvös placed a torsion balance similar to that used in Baily's experiment, i.e. a "reverse Cavendish torsion rod," which was surrounded by a case and which he installed between two equal pillars of lead (for the figure, see Eötvös (1896, p. 387). He then compared the time of vibration of the rod in the longitudinal direction, i.e. parallel to the line connecting the two pillars, to that in the transversal direction, i.e. perpendicular to the line connecting the pillars (Eötvös 1896, p. 388). Given these data he was able to complete the following equation: $\frac{1}{T_l^2} - \frac{1}{T_t^2} = \frac{8f\sigma(1-\varepsilon)}{\pi}$, where T_l is the time of vibration in the longitudinal direction, T_t is is the time of vibration in the transversal direction, f is the gravitational constant, σ is the density of the pillars, and $1-\varepsilon$ is a correction factor (Eötvös 1896, pp. 389–391). Given this formula, Eötvös could now determine f, for which he found a result of 6.65×10^{-8} (Eötvös 1896, p. 392). In his article Eötvös' did not bother to mention the value for the mean density of the earth. On Eötvös' contribution to the study of the earth's surface gravity, see Howarth (2007b, pp. 245–249). On Eötvös, see *DSB*, IV, pp. 377–381.

[95] On which Wadsworth remarked: "Each is admitted to be uncertain by at least one and perhaps two units in the fourth place, so that the agreement to even the fifth figure is more likely to be a striking coincidence than an indication of real accuracy obtained" (Wadsworth 1897, p. 163). On the same page, he remarked, furthermore, that: "Results obtained by other methods, notably the one obtained by Poynting, (1880–1891) by the balance method, have differed quite widely from the above, and whilst they are undoubtedly less accurate than the latter, so far as accidental errors of observation are concerned, it may be that the Cavendish method is subject to some constant source of error yet unsuspected and undiscovered."

was 0.00045 dynes (or 0.00045×10^{-5} N) (Braun 1896, p. 256). Braun concluded his article by remarking that the obtained results are provisional and by adding that:

> From a purely scientific point of view, the issue, as to whether Newton's law of universal gravitation $q = MmC : r^2$ holds absolutely exactly, could indeed consider as quite settled. Only from a natural-philosophical point of view, there remains an important question. *Since, first and foremost, the reasons which speak for the correctness of the law of gravitation are quite removed to prove the absolute exactness of the same* [law]*; additionally, there are also good reasons which justify doubt, namely concerning the factors M and m and the factor 1/r*^{2}. First of all, it is namely not unlikely that the attraction is stronger at infra-microscopic distances than that which follows by the formula [of universal gravitation]. Since by accepting this assumption, the possibility would be allowed that molecular forces could also be attributed to gravitation—so that the somewhat unnaturally seeming necessity that several heterogeneous attraction forces need to be accepted, would be cancelled out; and, secondly, [since] according to the only fairly supportable mechanical explanation of the gravity, this must go back to pushes of the ether atoms, it seems quite unavoidable that the attraction of a enormous body must be smaller than stated by the formula [of universal gravitation]. (Braun 1896, p. 257 [italics added])[96]

Richarz and Krigar-Menzel

By their choice of apparatus, Franz Richarz (1860–1920) and Otto Krigar-Menzel (1861–1930) sought to accommodate some inaccuracies in the method of Jolly,[97]

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[96] Author's translation of: "Allerdings könnte man vom rein wissenschaftlichen Standpunkt aus, für welchen das Newton'sche Gravitationsgesetz $q = MmC : r^2$ als absolut genau gilt, die Frage als einigermassen abgethan ansehen. Allein vom naturphilosophischen Standpunkt aus gibt es doch noch ein gewichtiges Fragezeichen. *Denn zunächts sind die Gründe, welche für die Richtigkeit des Gravitationsgesetzes sprechen, weit entfernt, eine absolute Genauigkeit desselben zu beweisen, und anderseits gibt es auch gute Gründe, welche einen Zweifel rechtfertigen, und zwar sowohl hinsichtlich der Factoren M und m , als des Factors 1/r*^{2}. Es ist nämlich erstens nicht unwahrscheinlich, dass für infra-mikroskopische Distanzen die Anziehungskraft stärker sei, als der Formel entspricht. Denn mit dieser Annahme würde eine Aussicht eröffnet, dass auch die Molecularkräfte auf die Gravitation zurückgeführt werden könnten, so dass die etwas unnatürlich scheinende Nothwendigkeit, m e h r e r e heterogene Anziehungskräfte annehmen zu müssen, entfiele;—und zweitens nachdem die einzige einigermassen haltbare mechanische Erklärung der Gravitation diese auf Stösse der Ätheratome zurückführen muss, scheint es ganz unausweichlich, das für enorm grosse Massen die Attraction kleiner sein müsse, als die Formel angibt." I am indebted to Christian Straßer for checking and improving the above translation.

[97] See: "Während bei Jolly sich besonders an den 21^m Drähten der Einfluss auch geringer Luftströmingen sehr stark geltend machte und stets eine erhebliche Temperaturdifferenz zwischen dem Orte der oberen und der unteren Wageschalen herrschte, war Poynting von solchen störenden Einflüssen bei den weit kleineren Dimensionen seines Apparates fast ganz frei" (Richarz and Krigar-Menzel 1898, p. 4).

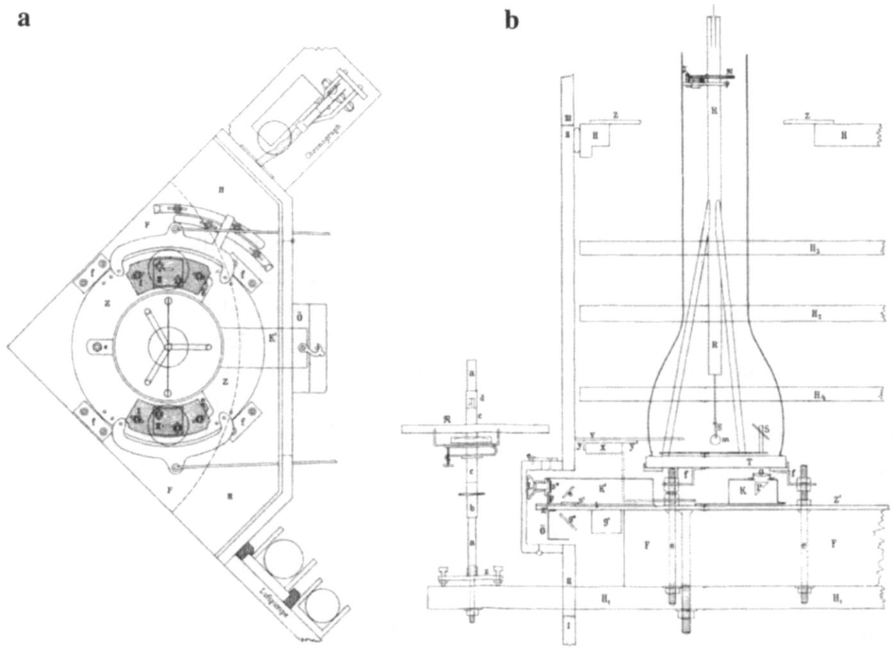

Fig. 9 **a–b** Horizontal (*left*) and vertical (*right*) section of Braun's apparatus. Taken from: Braun (1896), Fig. 4, Table II and Fig. 2, Table I, respectively

Poynting,[98] Boys and Braun[99]. Their apparatus was essentially an improved version of Jolly's balance: two scales were connected by a bar of 2.25 m and underneath them two additional scales were placed (Richarz and Krigar-Menzel 1898, p. 4). Experiments were performed in a room inside the Citadel of Spandau in Haselhorst (Richarz and Krigar-Menzel 1898, pp. 6–12). Air pressure, temperature, and atmospheric humidity was carefully monitored and air currents were minimized (Richarz and Krigar-Menzel 1898, pp. 12–25, cf. 29–40). The first of two nearly identical 1 kg spherical brass masses[100] was placed in the left scale above, whilst the second was placed in the right scale below. Next, the mass on the upper left scale was put on the lower left scale and the lower right mass on the upper right scale. The differences between the two equilibriums gives the double decrease of weight as height increases (Richarz and Krigar-Menzel 1898, p. 4). Finally, this procedure was repeated

98 See: "Gegenüber Poynting konnte eine bedeutend grössere gravitirende Masse angewendet werden, da diese nicht, wie bei Poynting, hin und her geschoben werden musste" (Richarz and Krigar-Menzel 1898, p. 5).

99 See: "Bei Braun (und bei Boys) handelt es sich jedoch um kleine Massen, die in kleinem Abstande auf einander gravitiren, deren Wirkung aber in Folge günstiger Anordnung sehr sicher messbas ist. Hier kommt die Unsicherheit der Massen- und Längenbestimmungen sehr wohl in Betracht; ja—kleine Asymmetrien oder Inhomogenitäten können die Sicherheit des Resultates ganz erheblich gefährden" (Richarz and Krigar-Menzel 1898, p. 113).

100 Richarz and Krigar-Menzel (Richarz and Krigar-Menzel 1898, p. 41) experimented with three types of brass masses: one gilded, one platinized, and one half-gilded and half-platinized.

in the presence of an enormous parallelepiped block of lead (its weight was no less than 100,000 kg!) (Richarz and Krigar-Menzel 1898, pp. 16–19), which served as the attracting weight and which was posited between the upper and lower balance scales. This block increased the weight of the masses in the upper scale and decreased the weight of the masses in the lower scales.

The general outcome of Richarz and Krigar-Menzel's experiments (in total 52) established that the average difference in weight in the absence of the lead weight is 1.2453 ± 0.0016 mg and that, when the lead weight is installed at its appropriate position, the average difference in weight is -0.1211 ± 0.0014 mg, taking into consideration the air which is pushed away by the masses and the required correction factor for variations in temperature (Richarz and Krigar-Menzel 1898, pp. 67–84, esp. p. 83 [temperature correction], pp. 55–66 [correction for the pushed-away air]). As the difference between the vertical accelerative forces in the lower and higher position *without the lead weight*,[101] $g_u - g_o$, equals $0.0005183 \times (1.2453 \pm 0.0016)\frac{\text{cm}}{\text{s}^2}$ (Richarz and Krigar-Menzel 1898, p. 48, 51) and, analogously, $g_u - g_o - (k_o + k_u) = -0.0005183 \times (0.1211 \pm 0.0014)\frac{\text{cm}}{\text{s}^2}$, it follows from experiment that the total added vertical attraction, which is *produced by adding the lead weight*, $k_o + k_u$, equals $0.0005183 \times (1.3664 \pm 0.0021)\frac{\text{cm}}{\text{s}^2}$ (Richarz and Krigar-Menzel 1898, p. 84, 110). The theoretically derived value for $k_o + k_u$ was shown to be equal to $10,594.0 \times G$ (Richarz and Krigar-Menzel 1898, p. 107, 110). Combining both formulae, $G = \frac{0.0005183 \times (1.3664 \pm 0.0021)}{10594.0} = (6.685 \pm 0.011) \times 10^{-8} \frac{\text{cm}}{\text{g} \times \text{s}^2}$ (Richarz and Krigar-Menzel 1898, p. 110). In order to establish the mean density of the earth, Δ, Richarz and Krigar-Menzel relied on the following formula: $\Delta = \frac{3g}{4\pi R(p) G\left(1 + a - \frac{3}{2}c\right)} = 5.505 \pm 0.009$— where $g = 9.7800\frac{\text{m}}{\text{s}^2}$, $R_{(p)}$ = the earth's polar radius, i.e. 6,356,079 m, a = the earth's ellipticity, i.e. 0.0033416, and, c is the proportion of the centrifugal force at the equator to the gravity in Berlin, i.e. 0.0034672 (Richarz and Krigar-Menzel 1898, p. 111).

Coda

In this section, we have surveyed how, on the basis of entirely different apparatuses, converging and reliable experimental determinations—of varying accuracy and precision[102] established for the gravitational constant and the mean density of the earth (see the summary in Table 2). In other words, in the late nineteenth century, converging measurements were established for the strength of the gravitational force and it was further confirmed that between terrestrial bodies there were robust gravitational interactions. In this section, the author has also brought to the fore how, in each of

[101] The subscripts "u" and "o" refer the German words "*u*nter" and "*o*ben".

[102] The percentage by which these experimental results for G differ from the present-day value are: ca. 0.36% (Poynting), ca. 0.25% (Boys), ca. 0.24% (Braun), and ca. 0.16 % (Richarz and Krigar-Menzel); the percentage by which these experimental results for the mean density of the earth differ from the present-day value are: ca. 0.39% (Poynting), ca. 0.21% (Boys), ca. 0.22 % (Braun), and ca. 0.18 (Richarz and Krigar-Menzel). The value for G recommended by CODATA (2006) is equal to $6.67428 \pm 0.0010 \times 10^{-8} \frac{\text{m}^3}{\text{kg} \times \text{s}^2}$ or $6.67428 \pm 0.0010 \times 10^{-8} \frac{\text{cm}^3}{\text{g} \times \text{s}^2}$.

Table 2 Summary of the determinations of G and the mean density of the earth

Poynting	1892	$6.6984 \times 10^{-8} \frac{cm^3}{g \times s^2}$	5.4934
Boys	1895	$6.6579 \times 10^{-8} \frac{cm^3}{g \times s^2}$	5.5268
Braun	1896	$6.65816 \pm 0.00168 \times 10^{-8} \frac{cm^3}{g \times s^2}$	5.52700 ± 0.0014
Richarz and Krigar-Menzel	1898	$6.685 \pm 0.011 \times 10^{-8} \frac{cm^3}{g \times s^2}$	5.505 ± 0.009

The present-day value for G, which was updated in 2006, is $6.67428 \pm 0.0010 \times 10^{-8} \frac{m^3}{kg \times s^2}$ or $6.67428 \pm 0.0010 \times 10^{-8} \frac{cm^3}{g \times s^2}$

the discussed experimental setups, efforts were made to ensure the stability of the apparatus and to guard against external disturbances—for instance, air currents were minimized, variations in temperature, air pressure and humidity were avoided or kept constant. As the accuracy and precision of the experimental apparatuses and the procedures to eliminate external disturbances involved improved in comparison to those discussed in the previous section, the empirical support for the universality of Newton's theory of gravitation improved correspondingly.

5 On the genesis of stronger evidence

In the preceding sections, the author has indicated that the evidential support for the *universality* of Newton's law of gravitation became increasingly stronger. Moreover, the author has suggested that the increasing accuracy and precision of the values for the mean density of the earth and the gravitational constant resulted from a long-term learning process, to which generations of experimental physicists contributed, and new technological possibilities. Here the author shall expand on these matters.

A salient feature of a good experimental apparatus is that it produces a stable or robust phenomenon in a controlled environment and therefore a reliable outcome.[103] "Control" entails two things: first of all, it refers to the elimination or keeping constant of disturbing factors;[104] secondly, it refers to the factors, which are varied during the experiment, being maximally (quantitatively) determined.

If we study the development of scientific research on the mean density of the earth and ultimately the gravitational constant between 1798 and 1898, it becomes apparent that experimental physicists became increasingly skilled in eliminating sources of error. In this context, Ian Hacking has adequately noted that "serious repetitions of an experiment are attempts *to do the same thing better—to produce a more stable, less noisy version of the phenomenon*" (Hacking 1983, p. 231 [italics added]). For instance, although Cavendish made explicit attempts to eliminate disturbing factors,

103 See Knorr-Cetina (1999, esp. pp. 26–28) [on the laboratory as an enhanced environment], Pickering (1981, p. 218) [on relatively closed systems], and, Radder (1988, pp. 63–64) [on closed systems].

104 Peter Galison (Galison 1987, p. 248) records: "Experimental culture is grounded in expertise—the ability to eliminate kinds of backgrounds and an instinctive familiarity with the valid limits of an apparatus."

Baily pointed out that the screening-off procedures in Cavendish' experiment were not entirely waterproof and, accordingly, he sought to overcome the problems associated with them. As we have also seen, improved experimental skill was not limited to the area of elimination of disturbing factors, but equally applied to the area of measurement techniques—Baily's improvement of Cavendish' measurement techniques for determining the time of vibration and the resting points, which was in its turn criticized by Cornu and Baille, is a notable example of this. New methods for eliminating disturbing factors were being devised as new, more fine-tuned scientific apparatuses emerged. Also, the number of eliminated factors increased—for instance, (dia)magnetic effects (Reich), air pressure (Poynting and Braun), and the humidity of the surrounding air (Richarz and Krigar-Menzel) were added to the picture. In addition, as a means of compensating for disturbing influences, correction factors were introduced—see, for instance, the correction factors introduced in the experiments of Cavendish, Poynting, Braun, Richarz, and Krigar-Menzel.

As we have seen in the previous sections, experiments, which set out to determine the attractive force between two *known* bodies at a *known* distance, were more successful than experiments which relied on an undetermined factor. Recall the uncertainty of the density of the outer shell of the earth implicated in Airy's and Sterneck's experiments with pendulums at and below the surface of the earth. Varying specific factors was equally important,[105] for by doing so it was possible to track potential sources of error—in case the varied factors made a difference—or to add to the stability of the phenomena at hand—in case the varied factors made no difference.

The fact that the independently established determinations of the mean density of the earth and the gravitational constant increasingly converged, added to the evidence that there are gravitational interactions between terrestrial bodies. In short, the physicists surveyed in this article had become better experimenters and their work provided increasingly *stronger* evidence for the *universality* of Newton's theory of gravitation.

6 Newton's postscript

In the analytic part of the proof for universal gravitation, Newton set out to proceed "from Motions to the Forces producing them; and in general, from Effects to their Causes, and from particulars Causes to more general ones" (Newton 1979, p. 404). In Propositions I–V of Book III of the *Principia*, Newton inferred the forces acting in the solar system and in Propositions VI–VIII of Book III he derived the theory of universal gravitation. The remainder of the *Principia* pertained to "the Method of Synthesis," i.e. "assuming the Causes discover'd and establich'd as Principles, and by them explaining the phaenomena proceeding from them, *and proving the Explanations*" (Newton 1979, pp. 404–405). Accordingly, in the synthetic part of the argument for universal gravitation, Newton set out to demonstrate that other phenomena, which were not contained in the original analysis, could be explained by the causes as established by the theory of universal gravitation. The research surveyed in this essay was a continuation of Newton's synthesis in the *Principia*. If it could be established that there are robust

105 On this matter, see Franklin and Howson (1984).

gravitational interactions between laboratory-sized bodies, which were obviously not originally included in Newton's analysis, this would add to the empirical support for Newton's theory of universal gravitation. Although it is not his present aim to explicate Newton's complex physico-mathematical methodology, the author shall, in order to contextualize his claim on the long-term experimental-methodological continuity as exhibited in the branch of post-Newtonian science which we have surveyed in this essay, briefly point to some salient features of Newton's *Principia*-style method.[106]

In contrast to the hypothetico-deductivist's attitude towards deviations, according to which deviations are either discarded or explained away by the introduction of *ad hoc* factors, Newton made discrepancies between phenomena and the mathematical results derived from ideal conditions a focal point of natural-philosophical enquiry. Newton began by establishing the physical conditions under which—*according to the laws of motion* —exact Keplerian motion would occur, so that each deviation from exact Keplerian motion is an indication that there is an additional force to the one under which exact Keplerian motion would occur. For instance, *from the perspective of the laws of motion*, any deviation from exact time-area proportionality is seen as an indication that an additional force, not included in our ideal case, is affecting the situation. Deviations thus become indicative of other forces not tracked in our initial approximation. By means of the propositions expressing systematic discrepancies, Newton was able to measure such additional forces and to trace, in Book III, additional physical sources that could account for these very discrepancies.

Moreover, in contrast with a hypothetico-deductive rendering of theory confirmation, in which the confirmation of the consequences deduced from a theoretical proposition by itself occupies centre stage, in Newton's methodology the attention shifts to a continuous exploration of residual forces and the establishment of their potential explanation. A striking feature of Newton's method is that he did not approach the empirical world through a single theoretical model or equation, but rather through a series of successive approximations.[107] This is captured by Newton's fourth rule of philosophizing, which was introduced in the third edition of the *Principia* (1726):

RULE IV.

> *In experimental philosophy, propositions gathered from phenomena by induction should be considered* [*haberi debent*]*either exactly or very nearly true notwithstanding any contrary hypotheses, until yet other phenomena make such propositions either more exact or liable to exceptions* [*accuratiores reddantur aut exceptionibus obnoxiæ*].
> This rule should be followed so that arguments based on induction may not be nullified [tollatur] by hypotheses. (Newton 1999, p. 796)

In manuscript material Newton was more explicit on the meaning of this rule. "Because," Newton wrote in a crossed-out section on what was there and then called "Reg. V:" "if arguments based on hypotheses were to be admitted against inductions, then inductive arguments, on which the whole of experimental philosophy is based,

[106] The author must stress that the features that he shall mention are not at all exhaustive.

[107] On this matter, see Smith (2002a, pp. 155–158, 2002b, pp. 46–49), and, Cohen (1982).

could always be overturned by contrary hypotheses."[108] If a proposition gathered by induction is not sufficiently accurate, then it should be corrected, not by (introducing *ad hoc*) hypotheses, but by more widely and accurately observed phenomena of nature.[109] If this turns out impossible, however, the proposition should be de-generalized.[110] The latter quote also reveals that Newton was perfectly aware of the risk involved in making inductive generalizations. Inductive-experimental arguments do not provide demonstrations, but they are stronger than arguments drawn from hypotheses (Newton 1979, p. 404). Whilst a hypothetico-deductivist endorses the view that a theoretical proposition is confirmed when the deductions from that proposition are agreeable with the phenomena at hand, Newton demanded more from a theory than empirical adequacy: in order to be accepted (provisionally), it should also be demonstrated that independent measures converge to a stable value (Harper 1998, p. 278; Harper 2002, p. 185).

The question which was put on the plate of eighteenth- and nineteenth-century experimental physics was whether Newton's theory of gravitation could be rendered "more exact" or whether its presumed universality had to be de-generalized. The research referred to in this essay, had indeed shown that the former could be reasonably accomplished. In addition, it had been shown that independently established measurements of the mean density of the earth and the gravitational constant increasingly converged. Insofar as the call for increasing accuracy and convergence of independently established measurements may be considered as being characteristic of Newton's natural-philosophical methodology, the author has brought to the fore that *in the long run* a particular branch of post-Newtonian research *developed* in line with Newton's methodological views.[111] Given what the author has discussed in the previous section, he has also shown that, whereas in the context of Book III of the *Principia*[112] increased accuracy resulted primarily from the introduction and exploration of increasingly complex physico-mathematical approximations, in the branch of post-Newtonian science which was the focal point of this essay increased accuracy resulted primarily from the experimenters' capacities to more carefully eliminate sources of external disturbances.

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[108] "Nam si argumenta ab Hypothesibus ↓contra Inductiones↓ admitterentur, argumenta ~~ab~~ Inductione ↓um↓ in quibus tota Philosophia experimentalis fundatur ~~nihil valerent, sed~~ ↓~~Nam~~↓ per Hypotheses contrarias semper everti possent." (CUL Add. Ms. 3965, f. 419^v [additions and corrections to the second edition of the *Principia*])

[109] Cf. "Si Propositio~~nes~~ ↓aliqua↓ per Inductionem collect↓a↓~~æ~~ nondum s↓it↓~~unt~~ satis accurat↓a↓~~æ~~, corrigi debe~~nt~~, non per hypotheses, sed per phænomena naturæ fusius & accuratius observa↓t↓~~nda~~." (CUL Add. Ms. 3965, f. 419^v).

[110] Cf. "Argumenta ~~ab~~↓per↓ Inductione↓m↓ ~~non [fortiora sunt quam Hypotheses~~ non sunt Demonstrationes. ffortiora tamen sunt quam Hypotheses: & pro generalibus haberi debent nisi quatenus exceptiones ab experimentis desumptæ [~~illegible text~~] occurrant. Ideoque ubi nullæ occurrunt ejusmodi ~~ubi e~~↑ex↑ ceptiones, generaliter ennunciandæ sunt." (CUL Add. Ms. 3965, f. 428^r [additions and corrections to the second edition of the *Principia*]).

[111] The italics added are meant to convey that my claim is *not* that the protagonists in this story self-consciously incorporated the *specifics* of Newton's methodology (as they can be found in Newton's introduction to the *Principia*, the Scholium to Proposition XI of Book I, the General Scholium, or the concluding parts of Query 31 in *The Opticks*) in their work. In fact, in the material the author studied in the context of this essay, the author found no evidence to support such claim.

[112] Hereby, the author does not wish to minimize the experimental parts in Sect. 6, which deals with the motions of pendulums, and Sect. 7, which deals with free fall motion in the air, of Book II.

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