Meas. Sci. Technol. **10** (1999) 470–477. Printed in the UK PII: S0957-0233(99)00227-1

Henry Cavendish: the man and the measurement

Isobel Falconer

Lumbo Farmhouse, St Andrews, Fife KY16 8NS, UK

Received 21 December 1998, in final form and accepted for publication 15 February 1999

Abstract. Cavendish's experiment to find the mean density of the earth was his last published experiment and the one which, ultimately, has proved the most significant. In a career spanning many scientific topics, this is the one which has gone down in history as 'The Cavendish Experiment'. Henry Cavendish was the grandson of the Second Duke of Devonshire and son of Lord Charles Cavendish. Reclusiveness ran in the family, and Henry Cavendish retired into a private world of science. It is entirely consistent with his personal characteristics that he removed the measurement of the density of the earth from the realm of field investigations to the privacy of his own laboratory, extrapolating his results theoretically to the wider world.

Cavendish's concern with the mean density of the earth began with criticisms of astronomical investigations which ignored the attraction of mountains: his main interest in the experiment was in the potential precision of the method and his paper reads rather like a dissertation on errors. He was an indicator of the emerging science of precision measurements that gained momentum during the following century. It is noticeable that Cavendish made no attempt to use his experiment to determine the gravitational constant. Indeed, this is a question that would have been difficult to ask within the science of the time. However, it is due as much to the subsequent importance of this fundamental force as to the precision of his work that Cavendish's experiment has outlived the problem of the density of the earth.

Keywords: torsion balance, torsion pendulum, density of earth, gravitational constant, Henry Cavendish

Henry Cavendish was 'one of the greatest scientists of his century, one of the richest men of the realm, a scion of one of the most powerful aristocratic families, a scientific fanatic, and a neurotic of the first order'. So wrote Jungnickel and McCormmach (1996) in their comprehensive biography from which much of the material for this paper is drawn.

Cavendish (figure 1) was to achieve fame in many fields of science. In chemistry he was one of the first to recognize the existence of different sorts of gases, he demonstrated that air is a mixture and showed the compound nature of water. In studying heat and energy he made important contributions to the understanding of thermometers, anticipated the law of conservation of energy and defined potential energy. He formulated a one-fluid theory of electricity and verified, experimentally, the inverse square law of electrostatic attraction. However, it is on his last and most famous experiment, that to measure the mean density of the earth, that I am going to concentrate, placing it in the context of his background and career.

Henry Cavendish was born on 31 October 1731. His father was Lord Charles Cavendish, the fifth son of the Second Duke of Devonshire and his mother was Lady Anne Grey, daughter of the Fourth Duke of Kent. By birth Henry belonged to two of the most powerful families in England. Their recent fortunes were founded in their support for William and Mary against James II 43 years previously in 1688 and they were staunch upholders of the liberal traditions that ensued from that 'Glorious Revolution'.

Among these traditions was the expectation that they should devote themselves to public service in some form, generally to liberal, Whig, politics. They believed firmly that power should not be vested in an individual (either the King or, in scientific circles, the President of the Royal Society), but should reside instead in councils of the intelligent and well informed, whose work would uphold these figurehead positions. They expected to exercise power but to avoid the limelight.

A further tradition was to aim high and to succeed. It has been said that what the Cavendishes did, they did well. Conversely, what they did not do well, they did not do at all (Jungnickel and McCormmach 1996, p 257). What Henry Cavendish did not do well was to socialize. Luckily, he was in a position to carve out for himself a successful career in which he hardly had to socialize at all. All accounts of Cavendish dwell on his agonizing shyness and reserve. His anxiety showed itself in his high squeaky voice and his total inability to face strangers. Whether his mother's death before he was two, when his brother Frederick was only a few months old, had anything to do with his reserve is, now, impossible to say. Other members of his family, although not his immediate forbears, were preternaturally shy; and recent research suggests that shyness may be a genetic trait (Kagan *et al* 1988).

0957-0233/99/060470+08$19.50

This figure is not available
in the electronic version
for copyright reasons,
please see print version

Figure 1. Henry Cavendish by William Alexander. This is the only portrait in existence, sketched at a dinner of the Royal Society Club. Reproduced in the print version by permission of the British Museum.

Even more than Henry, his brother Frederick lived in seclusion most of his life, although the reason usually given for this is the brain damage he suffered on falling from a first-floor window while an undergraduate at Cambridge. Certainly this fall rendered him incompetent, in his father's eyes, to manage his own affairs, but his few remaining letters show him to have been lucid and well able to follow Henry's scientific interests, at least in general terms. His withdrawal from society may have been as much due to reclusiveness as to brain damage.

Be that as it may, Frederick's absence left his father, Lord Charles, as Henry Cavendish's only close companion for 50 years and certainly the main influence on his life. Lord Charles Cavendish entered politics as a young man, serving as a Member of Parliament for 16 years. However, he also had an increasing interest in science, having become a Fellow of the Royal Society in 1727. He was drawn particularly to the problems of precision measurement, winning the Copley medal in 1757 for his invention of a maximum and minimum thermometer. Debarred by his position and the social code of the day from seeking to make money as a scientist, he saw science, in particular the work of the Royal Society, as an alternative stage for public service. In 1741 he resigned from Parliament and devoted the rest of his life to science and its administration.

Lord Charles ensured that his sons received the most liberal and scientific education available at the time by sending them to a private school, Hackney Academy, which emphasized mathematics, natural sciences, vocational subjects and French. From here, in 1749, aged 17, Henry went to Cambridge, becoming a member of Peterhouse.

Exactly what Henry learnt during his $3\frac{1}{2}$ years at Cambridge is unknown, for as a fellow-commoner he was not obliged to attend lectures and he left without taking his degree, a common occurrence in those days when a degree was unlikely to make a difference to a nobleman's life. However, central to Cambridge thinking at the time were mathematics and Newtonianism. In his subsequent experimental work Cavendish evinced a mastery of mathematics typical of the Cambridge graduate and which he shared with one or two others, whom he met at Cambridge and who formed an essential part of his scientific circle in later life, men such as Nevil Maskelyne, the future Astronomer Royal, and John Michell, the originator of the Cavendish experiment. However, such men were the exception rather than the rule among British experimentalists.

For these men, Newton, who died only 4 years before Cavendish was born, was a powerful guide. In the *Principia* he had shown the power of a mathematical description of nature and in the *Optics* the value of experimental enquiry. His queries provided research programmes for many years to come. Newton, in his *Optics*, also exemplified another move that was to be important for Cavendish; the move of experimental science from the outside world into the laboratory. An example, from the *Philosophical Transactions* of 1750, which likens the aurora borealis to the

Figure 2. Distinguished men of science living in Great Britain in 1807–8. Pencil and wash on paper by Sir John Gilbert, J F Skill and William and Elizabeth Walker. In this composite group portrait, designed in the 1850s, it seems significant that Cavendish, whose likeness was copied from Alexander's sketch (figure 1), occupies a central position (just left of centre), yet is isolated and sits looking at no-one. William Herschel and Nevil Maskelyne are seated by the globe on the left-hand side. Reproduced by permission of the National Portrait Gallery.

separation of colours by a prism in a darkened room, shows that this was a general trend; it shows that it was becoming legitimate to liken nature itself to a controlled experiment on nature and, furthermore, that the experiment was more familiar to men of the time than was the natural phenomenon (Baker 1750). For the reclusive Cavendish his home was to become the controllable centre from which he extrapolated to the wider world.

That home was, for 30 years, his father's house in Great Marlborough Street in London, to which Cavendish returned when he left Cambridge in 1753. Here he had his own apartment, had access to his father's instruments and shared with his father a series of meteorological and magnetic measurements in the garden. His father introduced him into scientific circles, in particular the Royal Society, of which Henry was elected a Fellow in 1760. Here he spent the rest of his life as an unsalaried, almost full-time servant, unobtrusively oiling the wheels that kept the Society running and focused on its scientific work and serving on the Council 34 times (figure 2).

In the late eighteenth century the Royal Society was engaged in working out the practical consequences of (and the symbiotic and parallel expansion in) instrumentation. At the same time that scientists began to appreciate the value of exact measurement, advances in mechanics and materials such as brass, steel and glass made the production of accurate instruments feasible, which in turn stimulated the demand for even greater accuracy (Daumas 1963). Hand in hand with the development of instruments came the understanding of how to use them most effectively. Observing procedures had to be agreed and standardized. Techniques for analysing the accuracy of the instruments and the errors of observation improved. As recently as 1755 Thomas Simpson had demonstrated mathematically the benefits of repeating observations and taking the mean in reducing errors (Simpson 1755). Cavendish came to be regarded as the Royal Society's expert on instruments: he 'routinely assessed the limits of observation and the consequent limits on theoretical calculations of physical phenomena. Cavendish's great reputation as an observer of precision depended on his mastery of the theory and practice of errors'. (Jungnickel and McCormmach 1996, p 133).

One of the Royal Society's goals was to measure the earth in relation to its place in the universe. The shape of the earth, the length of a degree of latitude, the attraction of mountains, the density of the earth, and the precession of the equinoxes constituted a tangle of inter-related problems. In Book III of the *Principia* (*The System of the World* (Newton 1973)) Newton showed how a universal inverse square law of gravitation could explain the motions of the solar system, but his theory gave the dimensions of the solar system only relatively; they needed a standard. The Royal Society's expeditions to measure the transit of Venus in 1761 and 1769 and their Schehallien expedition to weigh the earth in 1784 aimed to establish these standards. Cavendish was extensively involved in the planning and analysis of both.

By the mid-eighteenth century natural philosophers had realized that Newton's theory of gravitation implied an important potential correction to astronomical observations: the deflection of a plumb bob by mountains. Bouguer and de la Condamine had detected this during their expedition to Peru, around 1740, without being able to measure it accurately. In 1761 Cavendish's friend Maskelyne made a further attempt to measure it using a seconds pendulum on St

Helena, where he was trying to observe the transit of Venus. He concluded that a new method and a much more extensive series of experiments were needed (Maskelyne 1762).

By 1771 Maskelyne was consulting Cavendish about the theory of the attraction of mountains (Maskelyne 1771). Cavendish had been unable to reconcile Bouguer's data with those derived from seconds pendulum experiments without assuming a very unlikely density distribution for the earth, a discrepancy he thought due to inadequate allowance for the attraction of the mountains of Peru (Cavendish undated a). He also told Maskelyne that the measurement of a degree of latitude by Mason and Dixon might also be out by up to 200 m due to their neglect of the Allegheny Mountains (Cavendish undated b). Thus Cavendish's interest in the density of the earth arose out of the analysis of errors; and the experiment he proposed to quantify this error pushed observation to its experimental limits.

Having worked out theoretically the rules for finding the attraction of a particle at the foot of, and at a distance from, geometrical solids, assuming the law of universal gravitation, Cavendish proposed measuring the attraction of a mountain (and hence the density of the earth) directly by observing the deviation of a plumb line at the bottom of a mountain in comparison with the meridian altitude of stars. (Cavendish undated b). This measurement, which Newton had deemed impossibly small, Cavendish reasoned would be less affected by local irregularities in the earth's structure than were seconds pendulum experiments.

Instruments had improved rapidly since Newton's day, so in 1772 Maskelyne proposed such an experiment to the Royal Society, who agreed to finance an expedition. Schehallien, a relatively isolated mountain with an east–west baseline in the Highlands of Scotland, was chosen as a suitable site. In 1774 Maskelyne and his assistant Reuben Burrow spent the summer surveying the mountain and taking observations of 43 stars, measured from the north flank and from the south flank of the mountain. The attraction was measurable—just. Cavendish had reasoned that the deviation could be measured to within about 3 s of arc (Cavendish undated c). Maskelyne found a deviation of 5.8 s. Maskelyne concluded that his results verified the inverse square law of gravitation and the Royal Society concurred that it provided the final proof of the Newtonian system, awarding Maskelyne the Copley medal in 1775 (Maskelyne 1775). Cavendish had to wait longer, until Charles Hutton had finished analysing the survey results of the shape of Schehallien in 1778, for a figure for the density of the earth of 4.5 times the density of water (Hutton 1778).

In computing this figure, new methods of calculation had to be invented, including the invention of contour lines as a way of representing a three-dimensional shape on paper. Some of these methods Hutton acknowledged that Cavendish had supplied (but we do not know which).

One thing that is striking about the Schehallien expedition is that, although Cavendish suggested the method, took a leading role in its planning, supervised the repair of the field instruments, checked all the field data and contributed to its analysis, he remained throughout at home in London. He kept a very low profile and his contribution was noticeable only to those intimately involved. Not for him the camaraderie of a field expedition, or the wild party which, according to legend, Maskelyne threw for the locals when the survey was finished.

Figure 3. Cavendish's house on Clapham Common, now demolished. The house, with some later additions, is shown here from the back. Legend has it that Cavendish's experiment was performed in an outbuilding. Taken from Thorpe (1921). Reprinted with permission of Cambridge University Press.

He intended to observe a similarly low profile in the next great experiment to weigh the earth, the Cavendish Experiment, originally planned by his friend John Michell. The earliest evidence we have for this experiment is a letter from Cavendish to Michell in 1783, asking how his plans for the experiment were going (Cavendish 1783). How much of the original conception of the experiment was due to Cavendish, we do not know, but the pair were clearly dissatisfied with the uncertainty of the Schehallien result which depended on an assumption about the density of the mountain. They were seeking a method free of such assumptions.

A further stimulus was the attempt to verify the universal nature of gravitational attraction. This was, at the time an untested article of faith. Although the law predicted well the motion of planets, it had not yet been observed to act on the fixed stars, hand-held-sized bodies and the particles of light. During the 1780s both Cavendish and Michell were engaged in looking for such evidence, in the motions of comets, and in attempting to detect a gravitational deceleration of the light particles from the fixed stars.

The experiment Michell planned removed gravitation from the field and brought it into the controlled situation of home. This was a considerable advantage for both men, quite apart from its strictly scientific merits: for Cavendish because of his reclusive nature and for Michell because, as a clergyman in Yorkshire, he could not afford the luxury

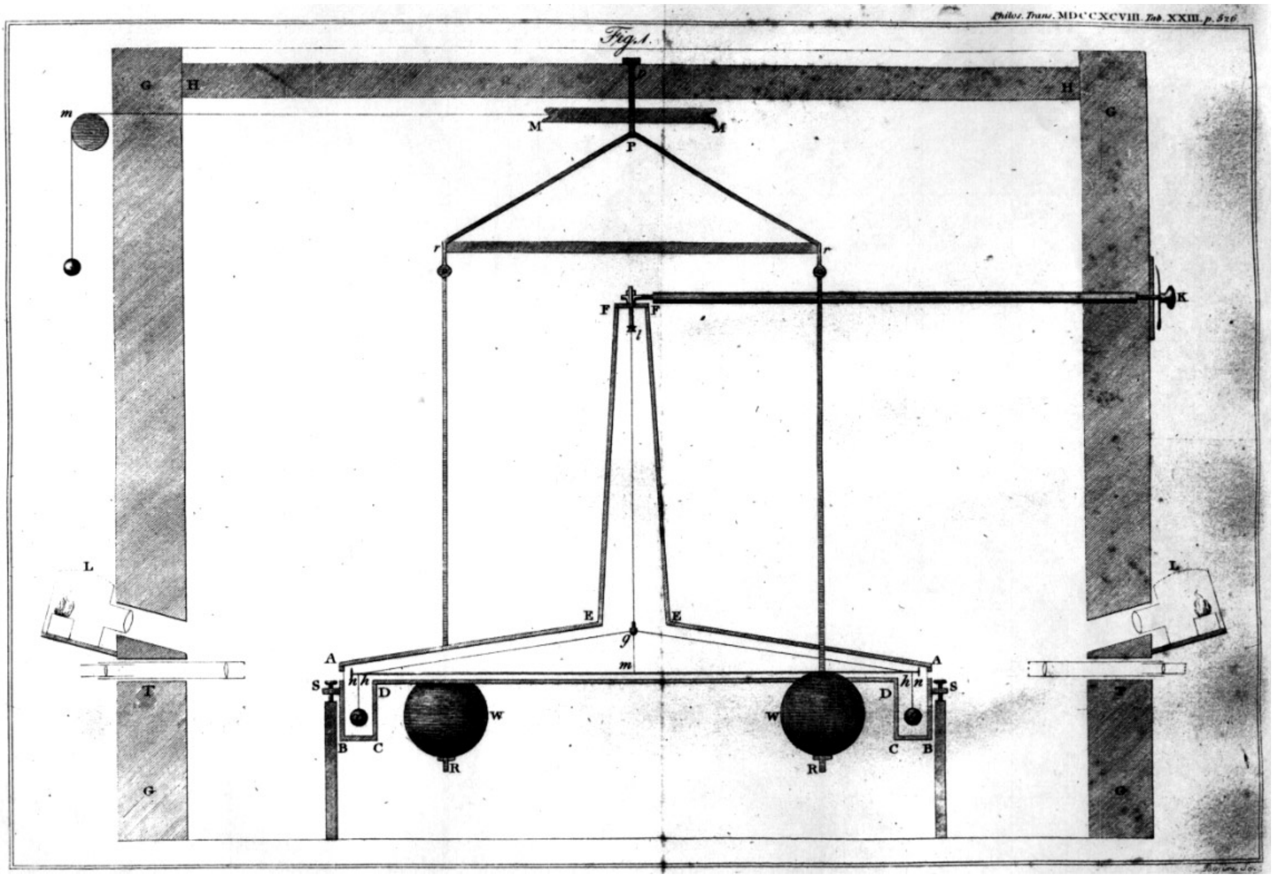

Figure 4. Cavendish's apparatus for measuring the mean density of the earth, taken from Cavendish (1798). The figure shows a vertical section 'through the instrument and the building in which it is placed. $ABCDDCBAEFFE$, is the case; x and x are the two balls, which are suspended by the wires xh from the arm $ghmh$, which is itself suspended by the slender wire gl. This arm consists of a slender deal rod hmh, strengthened by a silver wire hgh; by which means it is made strong enough to support the balls, though very light. The case is supported, and set horizontal, by four screws, resting on posts fixed firmly into the ground: two of them are represented in the figure, by S and S.... GG and GG are the end walls of the building. W and W are the leaden weights; which are suspended by the copper rods $RrPrR$, and the wooded bar rr, from the centre pin Pp. This pin passes through a hole in the beam HH, perpendicularly over the centre of the instrument, and turns round in it, being prevented from falling by the plate p. MM is a pulley, fastened to this pin; and Mm, a cord wound round the pulley, and passing through the end wall; by which the observer may turn it round, and thereby move the weights from one situation to the other'. (Cavendish 1798, pp 250–1). The beam was about $6'$ long, the suspension wire was about $40''$ long and the room about $10'$ square.

of extended field expeditions. It also brought it down to the scale of hand-held-sized objects, but ones which, unlike Schehallien, were accurately known.

The 1780s also saw a change in Henry Cavendish's circumstances, for his father died in 1783 and Henry inherited the estate. He leased the Great Marlborough Street house and moved, in 1785, to Clapham Common. It was here that he installed Michell's apparatus for finding the density of the earth following Michell's death in 1793, rebuilding the apparatus considerably in the process (figure 3).

Cavendish's final paper was published on 21 June 1798 (Cavendish 1798). It represents the results of 17 series of readings taken during the previous autumn and spring. He states at the outset that 'the apparatus is very simple', and so it was, in conception. However, it is worth noting that, however home-based it was, the experiment was 'big science', requiring construction of a special room to house the apparatus free of external disturbance. The paper has, in many ways, a very modern ring. Cavendish takes Newton's results entirely for granted and assumes that his readers are similarly familiar with them. He devotes only $2\frac{1}{2}$ of the 57 pages of the paper to a fairly compressed account of the basic theory of the experiment, but he spells out in much greater detail the errors he was guarding against and the corrections he applied.

The essential feature of the experiment consisted of using a torsion balance to find the attraction between a lead sphere 8 inches in diameter and another lead sphere 2 inches in diameter, when the distance between their centres was about 9 inches (see figure 4).

Michell had intended to move the large weights by hand from one side to the other, thus setting up an oscillation in the pendulum which could be timed. However, Cavendish, with his wide experience of experiments on heat and of analysing errors, reasoned that 'As the force with which the balls are attracted by these weights is excessively minute, not more than $1/50\,000\,000$ of their weight, it is plain, that a very minute disturbing force will be sufficient to destroy the success of the experiment; and, from the following experiments it will appear, that the disturbing force most difficult to guard against is that arising from the variations of heat and cold; for, if one side of the case is warmer than the other [such as when an observer is moving the weights], the air in contact with it will be rarefied, and, in consequence, will ascend, while that on the other side will descend, and produce a current which will draw the arm sensibly aside[...] As I was convinced of the necessity of guarding against this source of error, I resolved to place the apparatus in a room which should remain constantly shut, and to observe the motion of the arm from without, by means of a telescope; and to suspend the leaden weights in such a manner, that I could move them without entering into the room'. (Cavendish 1798, pp 249–50). This quotation illustrates not only Cavendish's clarity of expression but also the necessity he felt of explaining possible sources of error, especially those due to thermal effects, which he was one of the first to recognize. In his modified apparatus observation was by means of a telescope focused on a Vernier scale in a window of the case, which allowed him to read the scale to 1/100 inch, the scale being illuminated from outside the room.

Although Cavendish's basic method and the errors he was guarding against (principally thermal, magnetic and electrostatic) may look familiar to us today, his analysis of the experiment does not. Cavendish viewed the experiment as one of a series to measure the mean density of the earth, which was an important parameter at the time, used for fixing the density of other planets, the precession of the equinoxes, etc. Today the experiment is generally interpreted as the first laboratory measurement of G, the gravitational constant, but in Cavendish's time our familiar equation $F = GMm/r^2$ was unknown (Clotfelter 1987). Newton had not stated his law as an equation, stating merely that 'there is a power of gravity pertaining to all bodies, proportional to the several quantities of matter which they obtain' and that 'the force of gravity towards the several particles of any body is inversely as the square of the distances of places from the particles' (Newton 1973, vol 2, p 414). When Newton did calculations using the law he did not introduce a constant which could be measured; he worked with ratios rather than an equation. Cavendish was to do the same.

So how did Cavendish relate his measurements of a torsion pendulum to the density of the earth? Essentially, he compared the torsion balance to an imaginary, but equivalent, simple pendulum in a way that obviated the need to know the acceleration due to gravity. Throughout he refers to weight, rather than mass (Cavendish 1798, pp 275–77).

He likened his torsion pendulum (of half-length 36.65″) to a simple pendulum of length 36.65″ with a mass of weight W. When the mass on the simple pendulum is displaced from its equilibrium position through an angle A, the force F_0 acting to restore it to the vertical is $W \sin A$. Or, in Cavendish's terms, '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'. (Cavendish 1798, p 276). The period T_0 of this simple pendulum is given by $T_0^2 = 36.65/39.14$ s^2, where 39.14″ is the length of a seconds pendulum at Cavendish's house (he says 'in this climate').

Cavendish then wanted to find the real force F in his torsion pendulum of unknown stiffness, when it was deflected through the angle A. He did this by assuming the relation proved by Newton, namely that for simple harmonic motion the restoring force is inversely proportional to the square of the period. This then gave $F = F_0 T_0^2/N^2$, where N is the observed period of the torsion pendulum. Substituting in the previous expressions for F_0 and T_0 and putting it in Cavendish's terms, 'The force which must be applied to each ball, in order to draw it aside by the angle A, is to the weight of the ball as the arch of $A \times 1/N^2 \times 36{,}65/39{,}14$ to the radius'. (Cavendish 1798, p 276). (Note that Cavendish used commas rather than a decimal point; I follow his convention when quoting him). For apparatus of his dimensions, this meant that each scale division of displacement represented a force:weight ratio of $1/(818N^2)$:1.

Cavendish then equated the force just calculated to the gravitational attraction between the lead spheres that was twisting the torsion balance. Additionally, he wanted to do this in terms of the density of the earth related to the density of water. He introduced an imaginary sphere of water, 1 foot in diameter. Each lead weight weighed 2 439 000 grains (approximately 150 kg), which was 10.64 times the weight of the water sphere. Then the attraction, F, of the weight on the ball was $F = 10.64 \times (6/8.85)^2$ times the attraction of the sphere of water on the ball if the ball were on the surface of the sphere (this is where the factor of 6/8.85 comes from, 6″ being the radius of the sphere of water and 8.85″ being the distance from the centre of the ball to the centre of the weight). Cavendish also introduced a correction factor, multiplying his expression by 0.9779 to allow for the fact that the balls and weights were not exactly opposite each other.

Cavendish states the ratio of this attraction by the lead weight on the ball to the attraction of the earth on the ball (i.e. F/W), by assuming Newton's gravitation law, but without explaining how he got there. The steps he misses are

$$\begin{aligned} F/W &= 10.64 \times 0.9779 \times (6/8.85)^2 \\ &\quad \times (\text{weight of water sphere/radius of sphere}^2) \\ &\quad \times (\text{radius of earth}^2/\text{weight of earth}) = 10.64 \\ &\quad \times 0.9779 \times (6/8.85)^2 (D_w d_w^3/d_w^2)[d_e^2/(D_e d_e^3)] \end{aligned}$$

where D_w is the density of water, d_w is the diameter of the water sphere (=1′), D_e is the density of the earth, d_e is the diameter of the earth in feet and the constants cancel out.

Or, as Cavendish states it, 'the attraction of the leaden weight on the ball will be to that of the earth thereon, as $10{,}64 \times {,}9779 \times (6/8{,}85)^2$ to $41\,800\,000D$'. (Cavendish 1798, p 276), where 41 800 000 ft is the diameter of the earth and D $(=D_e/D_w)$ is the density of the earth relative to that of water, i.e. $F{:}W = 1{:}8\,739\,000D$. By equating his two expressions for F/W Cavendish obtained $1{:}8\,739\,000D = B/(818N^2){:}1$, where B is the number of scale divisions by which the balance has been displaced, giving $D = 818N^2/(8\,739\,000B)$†.

† A more modern analysis of the method is as follows. Let $2a$ be the length of the torsion rod, m the mass of the ball, M the mass of a large sphere and d the distance between the centres of the ball and sphere, supposed the same on each side. Then, when the spheres are moved from one side of the balls to the other, the rod moves round through an angle θ, given by $\mu\theta = 4GMma/d^2$, where μ is the couple required to twist the rod through 1 radian. μ can be found from the period of vibration of the torsion system, T, and its calculated moment of inertia, I: $\mu = 4\pi^2 I/T^2$. Then, using the relation $G = gr^2/M_e$, where g is the acceleration due to gravity, r is the radius of the earth and M_e is the mass of the earth, and defining g by the length, L, of a seconds pendulum, the density of the earth $\Delta = [3/(4\pi r)]LMmaT^2/(d^2 I\theta)$.

Observations, which might take several hours, consisted of readings of the turning points of the motion, the point of rest being calculated from three successive extreme points by taking means. The period of vibration was calculated by determining the time taken to pass two predetermined divisions near the rest point, averaged over a number of swings (each swing took about 7 min). To this data Cavendish added six correction factors, ‘first, 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 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 to be neglected’. (Cavendish 1798, p 277). He finally obtained a result for the density of the earth of 5.48 time the density of water, stating that ‘By a mean of the experiments made with the wire first used, the density of the earth comes out 5,48 times greater than that of water; and by a mean of those made with the latter wire, it comes out the same; and the extreme difference of the results of the 23 observations made with this wire, is only ,75; so 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 hereby, to great exactness’. (Cavendish 1798, p 284).

Although this result was extensively criticised by Hutton because it was considerably greater than the value (4.5) given by the Schehallien experiment, it was rapidly and widely acknowledged as the definitive experiment on the density of the earth (Jungnickel and McCormmach 1996, pp 340–42). Through it Cavendish had achieved more than simply another determination of this density. He had set new standards of precision measurement; he had demonstrated that the attraction of hand-held-sized objects could be observed; he had verified that, to within a small limit, this attraction obeyed the inverse square law which governed astronomical objects; and he had proved the validity of the approach that extrapolated from controllable, laboratory-based experiments to the outside world and, indeed, the universe. This approach was to be taken much further in the following century.

This was Cavendish’s last published experiment and, by the time of his death in 1810, it was well on the way to becoming ‘the’ Cavendish experiment, a ‘model’ which was paradigmatic in showing how natural philosophy ought to be done (Jungnickel and McCormmach 1996, p 5). However, the experiment’s enduring significance as the first measurement of the universal gravitational constant was not established for nearly another 100 years. The first move seems to have been made by George Biddell Airy who, in 1837, instigated a redetermination of the mean density of the earth, a project he handed over to Francis Bailey (Bailey 1843). Bailey criticised Cavendish for being more interested in proving the validity of his method than in giving an accurate determination of the density of the earth, and pointed out an arithmetical slip in Cavendish’s calculations. (Cavendish had written 5.88 instead of 4.88 for the result of his third density measurement and used this in his subsequent calculation of his mean results. (Note that this result has been corrected in the 1921 edition of his papers.) Bailey consequently revised Cavendish’s value to 5.448, instead of 5.48, times the density of water. Airy contributed a fascinating note to Bailey’s paper ‘On the mathematical theory of Cavendish’s experiment, part II, Investigation of the attraction of the masses and planck [*sic*] upon the balls, rod etc, on the law of gravitation with an assumed modulus of attraction’. In this he introduces something like our modern equation of gravitational attraction, writing that, ‘in treating of the attractions of particles[. . .] it becomes necessary to fix upon a unit of mass. The unit which we shall adopt is, the mass which at London weighs one grain: and for the attraction which it produces at distance 1 (estimated as an accelerating force), we shall multiply it by the modulus k’. (Bailey 1843, p 103). This led, for the first time, to an equation for the accelerating force, expressed in terms of k, the masses of the balls and plank, and the inverse of the distance between them squared. Airy further stated how k could be found from Cavendish’s (or Bailey’s) experiment, but chose instead to eliminate it, as Cavendish had done, and determine the mean density of the earth instead. The first work which specifically mentions the determination of a constant of attraction as an aim of the experiment seems to be that of Cornu and Baille in 1873; and by the 1880s and 1890s the interpretation of such experiments was a matter of hot debate (see my paper on Poynting, elsewhere in this volume).

The development of Cavendish’s experiment into a determination of the universal gravitational constant was not a coincidence. It necessarily relied on confidence in the universal nature of gravitation and the ability to make precise observations of gravitational effects. Only then was it possible to distinguish mass from weight and to define the concept of gravitational force. In this sense the experiment helped to create the context in which it was later interpreted as significant.

References

Bailey F 1843 Experiments with a torsion rod for determining the mean density of the earth *Memoirs R. Astron. Soc.* **14**

Baker H 1750 A letter. . . containing abstracts of several observations of aurorae borealis lately seen *Phil, Trans. R. Soc.* **46** 499–501

Cavendish H (undated a) Precession of equinoxes Cavendish Mss VIII, 9:14–5, Devonshire Collections, Chatsworth

——(undated b) Paper given to Maskelyne relating to attraction and form of earth, Cavendish Mss VI(b), 1:18, Devonshire Collections, Chatsworth

——(undated c) Thoughts on the method of finding the density of the earth by observing the attraction of hills, Cavendish Mss VI(b), 2 and 6, Devonshire Collections, Chatsworth

——1783 letter to John Michell, 27 May 1783, draft, Cavendish Mss, New Correspondence, Devonshire Collections, Chatsworth

——1798 Experiments to determine the density of the earth *Phil. Trans. R. Soc.* **88** 469–526, reprinted in (ed) E Thorpe 1921 *The Scientific Papers of the Honourable Henry Cavendish, FRS* (Cambridge: Cambridge University Press) pp 249–86. Page references given are to the latter, 1921, edition

Clotfelter B E 1987 The Cavendish Experiment as Cavendish knew it *Am. J. Phys.* **55** 210–3

Cornu A and Baille J B 1873 Mutual determination of the constant of attraction and the mean density of the earth *Chem. News* **27** 211; also in *C. R. Acad. Sci., Paris* **76** 954–8

Daumas M 1963 Precision measurement and physical and chemical research in the eighteenth century *Scientific Change; Historical Studies in the Intellectual, Social, and Technical Conditions for Scientific Discovery and Technical Invention, from Antiquity to the Present* ed A C Crombie (New York: Basic Books) pp 418–30

Falconer I 1999 *Meas. Sci. Technol.* **10** 525

Hutton C 1778 An account of the calculations made from the survey and measures taken at Schehallien, in order to ascertain the mean density of the earth *Phil. Trans. R. Soc.* **68** 689–788

Jungnickel C and McCormmach R 1996 *Cavendish* (Philadelphia: American Philosophical Society)

Kagan J, Reznick J S and Snidman N 1988 Biological bases of childhood shyness *Science* **240** 167–71

Maskelyne N 1762 Observations on a clock of Mr John Selton, made at St. Helena. . . *Phil. Trans. R. Soc.* **52** 434–47

——1771 Letter to Henry Cavendish, 10 April 1771, Cavendish MSS VIII, 4, Devonshire Collections, Chatsworth

——1775 An account of observations made on the mountain Schehallien for finding its attraction *Phil. Trans. R. Soc.* **65** 500–42

Newton I 1973 *Principia* (Berkeley: University of California Press)

Simpson T 1755 A letter. . . on the advantage of taking the mean of a number of observations in practical astronomy *Phil. Trans. R. Soc.* **49** 82–93

Thorpe E (ed) 1971 *The Scientific Papers of the Honourable Henry Cavendish* (Cambridge: Cambridge University Press)